SESSION SUMMARIES

SESSION A1 - GROUNDWATER PROBLEMS

Chairman Mr P. Hollingsworth (Hollingsworth Consultants)
Secretary Mr I. Hosking (Coffey & Partners)
General Reporter Mr D. Armstrong (Dept. of Mines & Energy, SA)
Panellists Mr W. Morton (Aust. Groundwater Consultants)
Mr R. Newman (Engineering & Water Supply, SA)
Dr M. Knight (Centre for Groundwater Management & Hydrogeology)
Mr W. Williamson (Retired - ex Senior Hydrogeologist, Water Resources Commission)

DISCUSSION

After reporting on the papers by Braybrooke, Whitfield and Fell et al, Mr Armstrong presented the following key questions to the panel:

(1) Do we take sufficient account of variability in *time & space* in our model exercises/project design and if not, how can we overcome the deficiencies?

(2) Do we have a valid methodology when attempting to characterise our *site* hydrogeologically at the investigation/design stage of a major construction project?

(3) Bearing in mind that groundwater may only be one of many problems existing at a project site, is sufficient attention paid to post-construction monitoring to verify predictions? Or indeed do we look far enough ahead in our predictions?

(4) Are we to some extent, fooling ourselves when we construct complex computer models, of systems which we do not fully understand, calibrate those models against inadequate real data and then use them to make predictions?

Would not simpler conceptual models be more believable if based on sound *concepts*?

The following comments with regard to question (1) were given by the panellists.

Dr M. Knight (University of NSW): With regard to variability with time, a useful strategy is to use tracers such as isotopes to track the rates and sources/destinations of groundwater. With regard to variability in space, investigations must be planned to take account of this variability. For example, (with reference to Braybrooke's paper), a long canal cannot be properly investigated with widely spaced packer tests as the scale of these tests is questionable with regard to the prototype. A test trench (as was carried out), cutting across the predominantly sub-vertical joint pattern is much more valuable. If boreholes are to be used, they must be inclined to intersect (in this case) sub-vertical joints.

W. Morton (Aust. Groundwater Consultants): In the case of long canal problems such as that presented by Braybrooke, the most appropriate general approach to assessing variability in space is to examine regional fracture systems and selectively siting boreholes in major geological structures. It might be regarded as fortuitous in Braybrooke's example that predictions based on packer testing of boreholes at 300 m spacings were actually close to performance in the field scale trial. An example of variability with time was in the data presented by Whitfield, whereby recession curves of mine inflows were found not to behave according to theory. In this case the active mining is causing changes with time - data on mining actively must be presented alongside recession curves. The importance of changes in pore pressure with time were demonstrated in the paper by Fell et al, the most important aspect being the time lag between a rainfall event and the groundwater pressure change.

R. Newman (Engineering & Water Supply): With regard to the paper by Fell et al, the modelling of time dependent pore pressure characteristics is based on 87 years of rainfall record and only 30 months of piezometer data. Given the statistical uncertainties associated with any model based on this data, perhaps the publishing of the paper is premature.

W. Williamson (ex Water Resources Comm.): Predictions of variability with time are a big problem. For example, differences in flow of up to 30% with rainfall have been experienced at an underground coal mine. Variability of rainfall with time is also a consideration. Average rainfall may be increasing or decreasing with time and this effect can be quite pronounced. With regard to variability in space, it might be argued that Whitfield's example of the Wongawilli mine takes insufficient account of boundary conditions as well as the presence of cintered coal and dykes and localised tensile zones and fractures. With regard to Braybrooke's paper, studies have shown that fracture widths tend to decrease and spacings increase in depth. However, a field scale trial such as that actually carried out is often the only practical solution for dealing with special uncertainties.

Prof. R. Fell (University of NSW) provided the following comments and answers to points raised regarding the paper by Fell et al:

"The comments by the General Reporter are appreciated and we will follow his suggestions to try STATS "9" and "10". The following should be noted

(a) In this case the geometry and mechanism of the landslide remains essentially the same - sliding along a near horizontal surface indicating water pressure, so there is no need to consider this effect on subsequent landsliding.

(b) It is recognised that the rainfall 3 km away at Cockle Creek may not be the same as at the landslide and moves are being taken to install gauges at the sites. This has not been practical up to now. The lack of data at the sites is recognised as a limitation to the accuracy of the model, but since the major rainfall events which lead to high piezometric levels are of long duration and therefore likely to be of larger areal extent it is considered that the model is not greatly affected by these effects.

(c) Recently obtained information (K. Williams-Sweeney, personal communication) indicates that the major movements of the Chelston St. landslide occurred in January 1951, 6 months after the initial movement (since the 1930's) in July 1950. The UNSW model indicates significantly lower piezometric levels in January 1951, supporting the general reporter's suggestion that disruption by the land-sliding leads to a different hydrological regime. The indications from the UNSW model are that the long period between landsliding in the 1930's and 1950 led to a similar hydrological regime being re-established. This would have been assisted by the land owner regrading and filling cracks to allow farming.

(d) Readers should note that Figure 1 in the paper is 90° out of phase. Water still runs essentially vertically down at Speers Point!

Further work is being carried out to better refine the model. This includes some of the suggestions made by the general reporter but will in the first instance be based on the available rainfall data. It is recognised that this and the heterogeneity of the landslide material may limit the accuracy to which the piezometric levels can be modelled."

Dr L. Wesley (University of Auckland): My comments refer to the type of model used by Fell et al in their paper.

I presume models like this must either assume the soil is a rigid material and water runs into and out of voids so to speak, or else they treat the soil as a clay which would respond by swelling to increased infiltration. In one case k would govern but in the other case c_v would be the important parameter. Is this correct?

Also, presumably there are limiting values of c_v or k below which a soil will not be influenced by rainfall infiltration. Clay slopes in materials like London clay for example have water tables which are not affected by rainstorms, because the permeability of the soil is so low. Have these limiting values of c_v and/or k been established."

Prof. R. Fell (University of NSW): With this particular landslide the colluvium is derived from conglomerates overlying weathered claystones, and it comprises a gravel/sand/clay mixture with open joints, tension cracks etc. Pore pressures acting on the slip surface are not only affected by vertical recharge. A coal seam, underlies the claystone and the slide area has a small upstream catchment. My expectation is that the permeability of the underlying rocks and the colluvium is relatively high and this explains the measured piezometer response to rainstorms.

S. Newcomb (SECV): Comparison of the R. Fell paper can be made to the McKinley and Raisbeck paper (Session D1) where the slip movement increases from about 45 mm/day to 120 mm/day after rainfall, with a delay of about half to 1/day after rain to commencement of increased movement.

Peter Hollingsworth (Hollingsworth Consultants, Chairman): Environmental factors are becoming an increasingly important part of engineering and groundwater studies. The three papers discussed in this session have not been directed at environmental aspects. Can anyone on the panel comment?

Dr M. Knight (University of NSW): An example is radioactive waste in Nevada USA, where the relevant time frame is 10,000 years or more. Over this time scale one problem is climatic changes - lateral thinking is required to find solutions. One concept being looked at to predict the long term dispersion of radioactive material is that of geological analogues. One method is to look at the dispersion that has taken place over geological time from radioactive ore bodies. This method has been applied to an ore body in Africa. Synroc is also an example whereby radioactive material is incorporated into mineral lattices. The examination of zircon from India of about 1000×10^6 years old has revealed that uranium isotopes have not travelled very far in this time. The climatic change issue must also consider effects such as increasing carbon dioxide levels, changing groundwater levels etc.

R. Newman (SECV): The time scale problem has also been encountered in designing strategies to dispose of salt in the Murray Basin over the next few hundred years. One approach is to engineer strategies which allow pullout in the future if monitoring reveals that the disposal scheme is behaving in a manner that is different to the predictions. One problem with such an approach is public misconceptions whereby the public who would like to see a complete solution must come to terms with the concepts of future generations pulling out of the scheme if need be.

W. Morton (Aust. Groundwater Consultants): With reference to the Braybrooke paper, one consequence of the filling of the canal could be the long term raising of the water table which could lead to environmental consequences such as activation of landslides.

W. Williamson (ex Water Resources Comm.): Environmental effects include dewatering settlement problems, surface pollution caused by saline and acid ground water, irrigation increasing salinity. Many of these effects are weather dependent both in terms of short term (seasonal) variations and also long term rainfall trends with time.

D. Armstrong (Dept. of Mines & Energy): In coastal systems, long term rise in sea level would change groundwater gradients, decrease discharge to the sea and waterlog low lying land. This is another consequence of a non-homogeneous climatic regime.

The following comments with regard to the General Reporter's question number (2) (as quoted above) were given by the panellists.

W. Williamson (ex Water Resources Comm.): For deep mines, drilling is very expensive. It may provide enough information to estimate reserves, but not enough for geotechnical predictions. For shallower mines, funds are generally available for more detailed geotechnical/ groundwater drilling. Where fracturing and faulting are the prime controls on groundwater, it is generally extremely difficult to obtain enough data for good predictions.

Dr M. Knight (University of NSW): Hydrological investigation programmes need to be planned with the resource evaluation. There is a danger in thinking there is a cook book approach - each site must be evaluated on its own merits.

R. Newman (SECV): Finite element modelling is a useful tool during the advanced investigation phase. Simple models can be used to test parameters and boundary conditions, and such modelling leads to decisions related to further investigations.

W. Morton (Aust. Groundwater Consultants): One problem area is the marriage between hydrogeologists and engineers. The geotechnical engineer can tend to see a problem as site specific.

The hydrogeologist tends to look on a broader scale. More interaction is required between the two disciplines in planning investigations.

N. Smith (University of Canterbury): A valid methodology in terms of site investigation work, with regard to the influences of reservoir filling raising ground water and thus affecting slope stability does not seem to exist. Different countries and organizations treat this problem with varying levels of investigation from practically none to extensive and this does not seem to correlate with the severity of the risk imposed by the slide.

D. Stapledon (Consultant): As comment on Mr Smith's question. I suggest that the Clyde Project is an exceptional case, where prior to the filling of the reservoir, a number of major landslipped masses, some actively moving, have been recognized. We do not have any comparable site in Australia, but if we did, I am certain that we would consider the slope stability questions just as seriously as they are being considered by the Ministry of Works, in New Zealand. For all proposed reservoirs, a careful study is made of the past and present stability of the valley slopes (starting with a geomorphological study) and predictions made about their stability during the operation of the project.

Summary by P. Hollingsworth

Three important contributions to the topic of groundwater problems have been discussed today. A fairly recent development in this area is our growing understanding of groundwater regimes in coastal areas arising from increasing development, including major buildings and resort areas, along our coastal foreshores. This topic is especially important in today's litigatious society where increasing actions are currently not benefitting the engineering profession. All of us must give due and very lengthy consideration to groundwater problems in geotechnical design and construction.

SESSION A2 - GROUND STRESSES AND MOVEMENTS

Chairman	Mr R. Jewell (University of Western Australia)
Secretary	Mr R. Coleman (Sydney Technical College)
General Reporter	Dr J. Enever (CSIRO)
Panellists	Prof. J. Booker (University of Sydney) Dr J. Simmons (James Cook University) Dr S. Bennet (Connell Group) Mr R. Walton (Mindata Pty Ltd)

GENERAL

Eight papers in the Proceedings formed the basis for discussion during this session:

Behaviour of Storvatn Dam, Norway - A Case of Prediction versus Performance - G.S.N. Adikari, T. Valstad, B. Kjaernsli and K. Hoeg.

Settlements over Bored Tunnels-Fantasy and Fact! - T.J.E. Sinclair, T.W. Hulme and D.C. Andrews.

Pre and Post Excavation Stress Measurements - J.R. Enever, C.R. Windsor and R.J. Walton.

A Study of the Influence of Anisotropy on the Response of Foundation of Multistorey Building - S.C.R. Lo, X. Zhao, I.K. Lee and G. Wu.

The Real World of Embankment Settlement - P. McDonald.

Boundary Element Analysis of Surface Movement Associated with Faulted Structures - I.V. Pedler and A.L. Skotnicki.

Finite Layer Analysis of the Effects of a Sub-surface Load - J.C. Small and P.T. Brown.

Behaviour of a Diaphragm Wall with a Top-down Construction Method - A.T.C. Goh, K.S. Wong and A.J. Burchell.

After summarising the above papers, the General Reporter listed some problems involved in measuring ground stresses -

1. Predominant reliance on displacement in prediction and monitoring.
2. "Calibration" of material properties.
3. Model matched to mechanism?
4. Ability to predict average performance but not worst case. A need for improved site investigations.

He went on to suggest three possible areas to be included in the afternoon's discussion. These were - determination of appropriate material properties, the matching of the predictive process to a physical mechanism and stress change monitoring to complement displacement monitoring.

THE PANEL

Each of the panellists in turn gave a brief presentation on aspects of the session topic related to his field of interest.

Dr John Simmons spoke on the limitations of laboratory testing with regard to the model adopted and the need to calibrate the modelling parameters to field performance.

Prof. John Booker discussed the process of numerical modelling involving formulation, analysis, interpretation and review, commenting on different types of finite elements. The summary concluded with a discussion on the achievement of simplification of the numerical modelling process while still maintaining an adequate representation of the physical reality.

Dr Sandy Bennet spoke on the specific problems of the session topic to tunnelling with examples from the Melbourne area. These included the Urban Transport tunnels which were constructed in weathered sedimentary materials. The talk included a discussion of the maximum stresses measured in these projects.

Rob Walton commented on the particular problems of measuring time-dependent stress changes in hard rock encountered in tin mines in western Tasmania and the importance of selecting the most appropriate monitoring techniques.

OPEN DISCUSSION BY THE DELEGATES

The summaries below do not include all of the discussion which took place during the session but rather attempt to give a representative sample of the main topics considered. They do, however, record at least the comments of all delegates who submitted a written contribution.

Dr Peter Brown (University of Sydney) commented on the unsatisfactory predictions demonstrated by Peter McDonald resulting from the determination of material creep properties by means of oedometer tests. He suggested that a better means of determining creep properties is a maintained-load in situ test as described by D.M.A. Smith in Session A3.

David Starr (Hollingsworth Consultants) noted that there appears to be no generally accepted method of assessing primary consolidation parameters from standard oedometer tests. He agreed with the remarks of John Simmons about regarding most laboratory tests on soils as index tests.

Peter McDonald (RCA, Vic.) directed a question to John Simmons on his point relating to the wrong things being measured in embankment settlement problems. Is this because people do not know what to do with collected information such as pore pressures? If so, what does he suggest should be measured that can be more meaningfully used?

This question led to a discussion involving all of the panel together with Tim Sinclair and Brian Little on issues including the continuous creep of very soft soils, pore pressures, finite and large strain theory, unexpected isolated events and the role of gut feelings in predictions. There was no clear-cut answer to the original question posed by Peter McDonald.

Paul Finlow-Bates (Tasmanian HEC) discussed overcoring and joint analysis and the prediction of post-excavation stress, asking what one does with the final analysis. Jim Enever suggested that this data would be used for designing the pressure lining. However, P. Finlow-Bates questioned the applicability of many of the specific predictive techniques for practical purposes. Specifically, in the King River Tunnel, pre- and post-excavation stress measurements were carried out but the results were not extensively applied to designing supports.

Dr Barry McMahon (Dames & Moore) commented that "gut feelings are dangerous things". For example, the UCS of Sydney Sandstone used to be taken as 9 t/sq ft but we now know that it is around 100 t/sq ft. He went on to point out the limitations of 3-D modelling, constitutive relationships and stress paths, emphasising the importance of a sound knowledge of site geology and stating that there are no such things as "average conditions".

Dr Mary Duncan Fama (CSIRO, Geomechanics) stated that in many cases numerical modelling has become far more sophisticated than our knowledge of input parameters can cope with. However, she agreed with J. Booker that our models must be the simplest that are adequate to give at least an approximate description of what is going on. The use of consistent parameters (e.g. - Young's modulus and Poisson's ratio) and method throughout the process of inferring stresses and numerical modelling is essential.

C.R. Windsor (CSIRO, Geomechanics) showed a series of overhead projections illustrating the instrumentation of tunnels in rock under three different geological conditions. These were massive-continuous, stratified-discontinuous and jointed-discontinuous. Instrumentation included inclinometers, stress monitoring cells and extensometer and convergence meter arrays. He pointed out that the main problem is that the behaviour mechanism needs to be known before the instrumentation scheme can be designed.

Brian Nottle (MMBW, Vic.) noted that the trend of the session was to idealise problems and not allow for real world situations where isolated features often cause problems and failures rather than the average case.

Michael Dunbavan (BHP Eng.) commented further on the problem of inferring stresses from displacements using estimated material properties and the fact that stress measurements are usually taken at an isolated point. He questioned if laboratory testing and monitoring really tell us about prediction versus performance.

SUMMARY

Session 2A on ground stresses and movements was a stimulating session with active participation from the delegates. There was a good exchange of practical and anecdotal material on case histories and problems encountered in the application of geotechnical theory to practice. The strengths and weaknesses of some monitoring, laboratory and analytical techniques were brought out in the session. Examples of these three categories were respectively, the selection of appropriate field instrumentation, the reliability of the oedometer test and simplification of finite element techniques so as not to exceed the reliability of the input parameters. From the interchange of practical information between delegates, the session would be judged a successful one.

SESSION A3 - GEOTECHNICAL TESTING

Chairman	Prof. S. Brown (University of Nottingham) (in absence of Professor I.K. Lee)
Secretary	Dr M.R. Hausmann (University of Technology, Sydney)
General Reporter	Dr M. Fahey (University of Western Australia) (report presented by Dr D. Airey)
Panellists	Dr D. Williams (University of Queensland) Dr L. de Ambrosis (Longworth & McKenzie) Mr P. Pells (Coffey & Partners) Dr M. Randolph (University of Western Australia)

This report consists of two parts. Firstly, a summary of the verbal discussion is presented. Secondly, detailed written submissions are reproduced.

1. Summary of verbal discussion

After the presentation of the General Report, discussion centred around the problem of testing natural sands in-situ and in the laboratory, specifically in relation to assessing their stiffness and relative density.

As suggested by the main speaker, first the panellists and then the audience responded to questions as follows:

* **Can in-situ tests measure the stiffness of natural sands?**

  It was clear that the academically inclined engineer favours the self-boring pressuremeter test to evaluate the stress-strain modulus for settlement predictions, while the practitioners find empirical relationships between modulus and penetration resistance adequate for most problems. It was pointed out that SPT's or CPT's are cheaper, that settlement on sand is not really a very serious problem in most cases and that design decisions do not always require perfect predictions.

  Anyway, which modulus should we be after? The initial modulus, the modulus for reloading, or the modulus for very small strains? This obviously depends on the problem in hand. For earthquake response studies, the modulus derived from shear wave velocity measurements may be the most appropriate.

  Consideration should also be given to the effect of construction activities, such as pile driving, on the in-situ modulus, warns a panellist.

  And so concludes the reporter: "The empirical camp wins the day!"

  (Also see discussion by Rallings in Part 2 of this report).

* **Can laboratory tests measure the stiffness of natural sand?**

  No, say the practitioners. The academics protest: "Lab tests are not meaningless - they throw more light on the problem, they may not give the answer, but ...".

  (Also see Lo, White, Fourie and Pender in Part 2).

* **Can relative density (or density index I_D) be measured in-situ? Is relative density sufficient to describe the in-situ state?**

  Consultants and contractors confirm the importance of I_D, citing the evaluation of the liquefaction potential and design criteria for ground improvement.

  One should be more interested in the geological processes which form the sand, insists a voice in the audience.

  (Also see Tchepak in Part 2).

* **Is there a case for site-specific (centrifuge) model testing, considering the costs involved?**

  Cambridge trained centrifuge experts exude great confidence in the relevance and validity of model testing. Others admit the centrifuge can assist in qualitative investigations on uniform soils (such as tailings) but advise caution in adopting centrifuge derived design parameters, particularly for large strain problems, e.g. large strain consolidation.

  (Also see Mostyn, and Elder in Part 2).

Concluding, the Chairman acknowledges a certain gap between research and practice. "Keep talking to each other !" is his advice. Nevertheless, since it is time for a coffee break, he closes the session, thanking the general reporter and the contributors to the discussion.

2. Written contributions

(In the same order as referred to above).

R. Rallings (DMR, Tasmania): What is meant by foundation performance?

An analytical method that may be suitable to make a design decision may be quite inappropriate to predict actual behaviour (e.g. time rate of settlement). Hence when we are judging the quality or suitability of data we have to have a clear view of its intended application.

S.-C.R. Lo (University College, ADFA): I find it difficult to agree that lab tests are meaningless. The reporter mentioned that the "elastic moduli" are stress state dependent. However, our lab test results (Lo and Lee, 1988) indicate that there is another important factor, the stress increment ratio $d\sigma_1/d\sigma_3$. The influence of $d\sigma_1/d\sigma_3$ can be up to an order of magnitude, even at small strain (shear strain $< 1\%$). This is contradictory to the elastic model. The implication is that the "elastic moduli" are fudge numbers in our elastic equations. If we still want to keep our elastic analysis, then the influence of $d\sigma_1/d\sigma_3$ must be allowed for in our choice of "elastic moduli". A possible method is described in Lo and Lee (1988). Applied to the case of an LPG tank, this method predicted a settlement of about 19 mm, whereas "conventional methods" gave predictions around 100 mm. Although we do not have the performance data of the tank, we understand the design is based on our prediction and we are not aware of any problem.

We have to agree with the reporter's comments that the effect of $d\sigma_2$, rotation of principal axes etc. etc. have not been accounted for. It is not the intention of our paper to provide an ultimate answer. Our paper has the modest message that an additional factor, the stress increment ratio, needs to be accounted for in the selection of "elastic moduli" and this may be achieved by conducting special laboratory tests.

W. White (University College, ADFA): The question was raised of the value of laboratory testing of sands. The printed discussion by the General Reporter covers this topic very well. However, the acting reporter and also the panel seemed to dismiss most laboratory testing on sands as worthless, and assume that the only way to obtain deformation properties was by field testing, especially modern direct testing such as the self boring pressuremeter.

There is no doubt that pressuremeter testing and equivalent field tests are essential in testing undisturbed sand deposits. (Is there a place for lab testing of vibroflotation-treated or recently dredged deposits?). However, recent laboratory testing, especially by Lo, has graphically illustrated the huge influence, not only of initial stress stage and stress history, but also of stress ratio increment. A perfect pressuremeter test is still exceedingly limited in that the only stress increment it can control is $\Delta\sigma_r$. Even if the soil had isotropic deformation properties (a most unlikely event in the presence of in-situ shear stress), Fig. 3 of the paper by Lo and Lee (page 162) shows that the pressuremeter-induced stress path bears little resemblance to the actual stress path (indeed for NC sands may be orthogonal to it!).

The value of lab testing (whilst limited for the consultant design engineer) is thus that it does give an understanding of the differences that can be expected between predictions based on perfect field tests and those which will actually happen. A necessary step in research on deformation moduli for sands is to rigorously relate the moduli derived by, e.g., pressuremeter or screw plate testing to the deformations produced by real stress paths.

Dr A. Fourie (University of Queensland): There is no such thing as a "unique" Young's Modulus. Factors such as anisotropy, stress level, stress path, all have an influence on the modulus. This complexity makes it difficult to use results from such sophisticated testing as are necessary to define the influence of the above parameters in day-to-day design office applications. Perhaps a preferable alternative is the establishment of a national data bank which accumulates empirical information from sites throughout the country. This data bank could then be accessed by anyone requiring such information.

Prof. M.J. Pender (University of Auckland): The following comments are related to the stiffness of sands.

Earthquake engineering requires small strain shear modulus (gives upper bound on stress modulus). Shear wave velocity measurement in-situ is likely to produce a better value for this purpose than a self boring pressuremeter (SPB). Also, in-situ shear wave velocity measurement gives a "spatial average" rather than a spot value as does the SBP. Without shear wave velocity measurement I would use the correlation between the SPT and G proposed by Seed, Idriss et al (1986, ASCE). One needs to be careful with units though, as stated by Mark Randolph with regard to I_D - q_c correlations.

S. Tchepak (Frankipile): This comment concerns the importance or need to measure relative density in sands.

A number of contracts (in particular ground improvement works) are performance contracts specifying that a certain relative density is to be achieved and that this "relative density" will be checked as a matter of routine/quality control via SPT's and CPT's. Therefore I believe the need to measure relative density is an important issue which needs to be addressed in some detail.

G. Mostyn (University of NSW): A relationship between the model and prototype must always be kept in mind. The Bass Strait models, both, centrifuge and pit tests, were on totally reconstituted surface samples. The piles are driven through a thick layer of not reconstituted material (maybe a band around the pile is). The models here may bear no relationship to the prototype. Another point is that a lot of tailings volume change is related to surface evaporation. How does the centrifuge model evaporation?

Dr D. Elder (Soils & Foundations Ltd): I have two comments to make relating to the modelling of very soft soils, and on both points I disagree to some extent with the previous speakers.

The first is with regard to centrifuge testing. It has been suggested that large strain consolidation is primarily governed by diffusion-type effects, and that the scaling factor for time is therefore $1/n^2$. I believe evidence exists(1) which suggests that at high void ratios, particularly in excess of the liquid limit, viscous effects (i.e. secondary compression or creep) may be dominant in compression-time behaviour. Since these effects are scaled directly, rather than by $1/n^2$, the modelling of large strain consolidation by centrifuge is not as straightforward as has been suggested. Although the surface settlement of deep, soft soils consolidating under self weight has been adequately predicted by centrifuge test-based analysis, the actual density changes with depth and time have not, to my knowledge, been so well described.

My second point relates to the use of critical state type models, or indeed other soil models derived empirically for particular soil types. Here I would agree with the comment by Williams that Skempton's correlation for normally consolidated soils

$$\frac{s_u}{\sigma'_v} = 0.11 + 0.0037\ I_p$$

should not be applied to soils at very low densities.

Critical State Theory provides an excellent framework for understanding soil behaviour. However, specific models, such as Cam-Clay and its various derivatives, were derived for particular soils:

- originally for Kaolin, although recently extended
- normally consolidated to lightly overconsolidated soils
- soils at water contents between the liquid and plastic limits
- soils of low activity.

Such models are of decreasing validity at water contents approaching the liquid limit and should never be used at higher water contents. Taking the example given by Williams of coal tailings (liquid limit w_L = 43), the upper limit on void ratio for application of such models is therefore about e = 1.1. This value is generally exceeded during self-weight consolidation of the coal tailings, and in most similar situations.

Reference

(1) Elder, D.McG. and Sills, G.C., "Time and Stress Dependent Compression in Soft Sediments", Proc. ASCE Symp. on Sediment Models: Prediction and Validation, San Francisco, U.S.A., October 1984.

SESSION B1 - UNDERGROUND MINING AND EXCAVATION

Chairman	Mr J. Braybrooke (D.J. Douglas & Partners)
Secretary	Mr P. Wong (Coffey & Partners)
General Reporter	Dr B. McMahon (Dames & Moore)
Panellists	Mr P. Pells (Coffey & Partners) Mr J. Barrett (Barrett Fuller & Partners) Mr B. Hebblewhite (Australian Coal Research Laboratory)

Contributions from the Audience:

Paul Finlow-Bates	- Tasmanian Hydro Electricity Commission
Chris Windsor	- CSIRO Div. of Geomechanics
Johnson St. George	- University of Auckland
Riza Gurtunca	- Chamber of Mines of Sth Africa
Jim Enever	- CSIRO Div. of Geomechanics
Margaret McMahon	- Dames & Moore

General Reporter's Comments

This session covered four papers on underground mining and excavations presented in pages 214 to 233 of the conference proceedings. The main points raised by the General Reporter, Dr McMahon, were as follows:

Paper 1 – Two Pass Drilling a 6.1 m Diameter Ventilation Shaft by Hern U. Englesman p. 224

* The two pass raiseboring method adopted for the 6.1 m diameter by 600 m deep ventilation shaft in broken quartzite appeared to be a successful technique. The method of drilling a 4.2 m diameter hole and then drilling a second pass to 6.1 m, and drilling in three stages of 200 m, overcame the machine torque/thrust limitation.

* A safety canopy was employed to protect miners when changing the cutters while the coring machine was at the base of the shaft. This safety precaution is not carried out in Australia at present and it is recommended that this idea be implemented.

Paper 2 – Mining in Proximity to the Awaba Tuff by Seedsman p. 229

* Significant roof instability and floor heave were experienced during coal extraction within the Awaba Tuff at the Cooranbong Colliery.

* The Awaba Tuff has low strength but it is also worth noting that it consists of approximately 30% montmorillonite clay. The swelling pressure measured in the laboratory ranged from 1.5 to 5 MPa. Dr McMahon raised the question "Is the problem due to clay swelling or low strength?". There seems to be an assumption made in the paper that if there is no water present there is no swelling. This assumption is questionable.

* Plate bearing tests indicated a rock mass strength of 890 kPa while analysis indicated a rock mass strength of 500 kPa is required to explain floor heave due to low strength alone. The authors applied reduction factors to the rock substance strength to arrive at a rock mass strength to explain floor heave. How the reduction factors are arrived at is not understood.

Paper 3 – Predicting and Monitoring Stress and Deformation Behaviour of Backfill in Deep Level Mining Excavations by Clark, Gutunca & Piper p. 214

* The stress field predicted by computer modelling (FLAC Program) was much higher than that measured in the field in the backfilled slopes.

* There is so much flexibility with the computer analysis (such as stress/strain law, boundary conditions etc.) that one can get any answer.

* Dr McMahon suggested that numerical modelling is more appropriate for back calculation of known situations to get an understanding of the problem, rather than for prediction.

* It appears that the void ratio of 48% to 50% quoted for backfill in South African mines is relatively low. Is there any effort being made to adopt low backfill void ratio for Australian mines and what is the advantage?

Paper 4 – Concepts of Expert Systems, Fuzzy Mathematics and Pattern Recognition Techniques in Mining Geomechanics by Dutta, Sinha & Bhattacharya p. 219

* Dr McMahon thinks that the authors of the paper are a little too generous in the analysis ability of computer methods using concepts of Expert Systems, Fuzzy Mathematics and Pattern Recognition Techniques.

* The concept of Fuzzy Mathematics is vague and adds a new level of complexity and he sees no real advantage over the present deterministic or probabilistic techniques. Fuzzy Mathematics is analogous to the ten arm geologist (i.e. on the one hand it is like this, on the other hand it is like that, but on the other hand ... etc. etc.).

* It is not appropriate to reduce a complex problem to a rule of thumb as attempted by the Expert Systems concept.

* Dr McMahon quoted an example relating to the decision making on the span of an underground opening, which, as a rule of thumb, may be governed by public usage in terms of safety and have nothing to do with the properties of the rock (i.e. it may be governed by sociology rather than engineering geology). Analysis by way of strength and displacement principles is also difficult as the stability of the opening is sensitive to minor geological variations.

* Most studies now focus on geomechanics indices. An example is the Rock Mass Rating (RMR) classification system which uses a number of parameters including RQD, average joint spacing, groundwater and strength. The classification is then used with empirical correlation such as tunnel support requirements (e.g. Bieniawski). However, Dr McMahon found there appears to be a lot of contradiction. He questions whether or not such techniques can be called Science and concludes that using short cut methods is no good.

* Dr McMahon concluded that support of underground openings should be based on good geological practice and good solid engineering geology, including field mapping and study of rock cores.

Comments from Panellists and Audience

No comments were made by the panellists and audience on papers 1 and 2. The comments made by the General Reporter on Papers 3 and 4, however, generated some lively debates. It is clear that **Dr McMahon** succeeded in hitting the nail on the head and inspired the much needed thought provoking discussions to liven up the Conference.

Most of the post general report discussions were focussed on the usefulness of rock mass indices, expert systems, computer numerical analysis versus field mapping and traditional engineering geology practice. There were only two exceptions. One was the question raised by **Dr McMahon** regarding mine backfill and **Mr Barrett** replied that there is a trend towards thick backfill, with the advantage being reduced closure and reduced stress relief. The other exception was a question asked by **Dr McMahon** on the life of rock anchors. **Mr Pells** indicated that fully sheathed anchors are supposed to last the life time of the structure. However, experience in Hong Kong indicated failure usually occurs as a result of corrosion at the head of the anchor. **Mr Finlow-Bates** indicated the Tasmanian Hydro Electricity Commission expects them to last at least 50 years, but no-one really knows.

Back to the main focus of the discussion, **Mr Barrett** started the discussion by raising the following points:

* Too many people look at core photographs only, rather than getting the parameters, such as joint spacing/orientation, from actually looking at the cores.

* Rock mass indices are rubbish. RQD of rock cores which are in sections of about 100 mm may range from 0 to 100% depending on whether the sections are just below or just above 100 mm. The use of RQD is not good enough, particularly for laminated rock. The method of analysis should depend on the stage of design. That is, it depends on whether one is after a "ballpark" figure or establishing a programme of improving the data.

* Geology and engineering need to be combined.

* Underground excavation is an involved problem. The conditions must be reviewed during construction and the data and solutions updated.

Mr B. Hebblewhite (Aust. Coal Res. Lab.) agreed with Mr Barrett. He suggested that it is a case of compromise between approach and detail. Mapping of the excavation should be carried out to build up data for each specific site to back up limited results with general correlations. Support requirements depend on the purpose of the excavation. There is a range of designs depending on whether it is a roadway for 2 weeks or 2 years. It depends on economics and safety of people. There are infinite numbers of solutions to meet the purpose.

Mr P. Finlow-Bates (Tasmanian HEC) indicated that from experience of the King River Hydro Scheme the use of rock mass indices was found to be extremely over-conservative.

Dr R. Gurtunca (Chamber of Mines Research, S. Africa) also indicated that none of the rock mass classifications and correlations work and the Chamber of Mines of South Africa are in the process of developing their own system. With regard to computer modelling Mr Gurtunca indicated that the use of elastic theory is not adequate and can give wrong information. One can conduct a parametric study to provide an understanding of the problem. There is still a long way to go before it can be used as a predictor's tool.

Mr C. Windsor (CSIRO, Geomechanics) also agreed with Dr McMahon that "indices" are a poor method of designing underground excavations. His experience is that many excavations are controlled by the interaction of the rock structure and insitu stress conditions. As the rock structures can be quite variable the mechanisms which are driven by the peculiarity of the rock structure will be peculiar to each structural zone.

Mr P. Pells (Coffey & Partners) however, pointed out that numerical analysis is essential and helps in the understanding of mechanism of failure. He felt that looking on site may not be good enough as one may not understand the mechanism by just looking. He quoted three case studies:

. a basement excavation at the corner of King and Castlereagh Streets, Sydney which caused movement of an adjacent structure,

. shearing in the roof and floor of the Malabar outfall tunnel,

. cracking of the rock mass at the base of a deep carpark excavation at The Rocks.

He pointed out that simple elastic analysis would have shown the failure mechanism whereas straight engineering geology was not adequate.

Mrs M. McMahon (Dames & Moore) debated with Mr Pells on the Malabar outfall tunnel, indicating that there were a lot of shears on the rock face with significant wedge failures along joints. She felt that the failures could have been predicted by basic engineering geology.

Mr P. Pells (Coffey & Partners) indicated that the shears at the Malabar outfall tunnel were induced by failure and were not pre-existing. Mrs McMahon disagreed.

Mr Pells also indicated that there is a lot of work involved in getting the Rock Mass Rating (RMR) and that engineering geology would have been taken into account in the process.

Dr B. McMahon (Dames & Moore) indicated that most factors in arriving at the RMR are related to the RQD, with the unconfined compressive strength accounting for only 12%. He suggested that RMR could be reduced to getting the RQD which is quite inadequate.

Mr J. Braybrooke (D.J. Douglas & Partners) agrees with Dr McMahon that RQD can be misleading, particularly if drill holes happen to be parallel to joint sets.

Mr J. St George (University of Auckland) asked Mr Pells what elastic modulus was used for the carpark excavation at The Rocks. Mr Pells replied that the governing factor is the ratio of horizontal to vertical insitu stress rather than the elastic modulus. No measurements were made but a stress field of $\sigma_H = 2\sigma_v$ was assumed.

Everyone agreed that σ_H is significantly greater than σ_v in Sydney but concluded that there is no single ruling theory on stress field.

Dr Jim Enever (CSIRO, Geomechanics) indicated that his observations during 15 years of stress measurements have been that the stress field must be placed in the context of the prevailing geology. The more data gathered, the more it becomes obvious that the notion of a single rule governing stress is ill conceived.

Mr J. Braybrooke (D.J. Douglas & Partners) indicated that an insitu horizontal stress of 2 MPa was measured at the World Square site in Sydney at 3 m depth and this high stress was causing spalling in the sandstone which has an UCS of about 30 MPa.

Mr C. Windsor (CSIRO, Geomechanics) made the final comment that we have been discussing a classical problem in rock mechanics of defining the engineering problem - that is, its geometry and loading condition or its structural geology and insitu stress field, without which we cannot solve the problem. If we assume all or part of the problem definition then the solution to the real problem is suspect. He suggested that we endeavour to measure the structural geology and insitu stress field.

In conclusion I feel it is unfortunate that there seems to be divided opinion between the engineering and geology fraternities. It is clear to me that the design of underground openings is an involved and complex geotechnical problem and neither theoretical engineering nor geological mapping could be relied on to stand alone in solving the problem. Engineering and geology methods must be integrated and I tend to agree with Mr Barrett that the design method should be dependent on the stage of design and must be updated and reviewed during construction.

SESSION B2 – GEOTECHNICS OF WEAK AND JOINTED ROCK

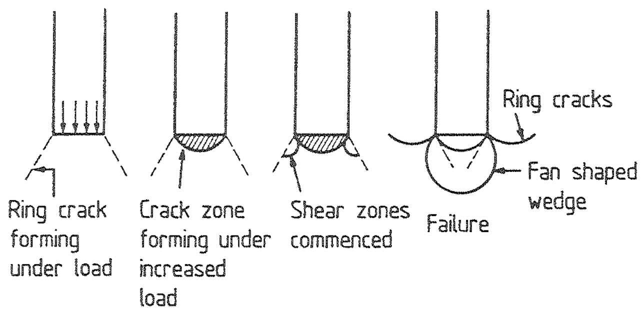
Chairman	Dr B. Burman (Coffey & Partners)
Secretary	Mr S. Tchepak (Frankipile Aust. Ltd)
General Reporter	Dr I W Johnston (Monash University)
Panellists	Professor E. Brown (University of Queensland)
	Dr J. Carter (University of Sydney)
	Dr C. Gerrard (CSIRO)
	Mr D. Jennings (Works Consultancy Services)

GENERAL REPORTER'S COMMENTS

The General Reporter emphasised that, for the purposes of the General Report, "Geotechnics" would be defined as fundamental geotechnical properties under applied loading, viz. elastic modulus (for determination of deformation) and strength (for determination of stability).

The General Reporter's comments on the 3 papers are summarised below.

Paper by Haberfield & Johnston – The mechanism of failure which was supported by model and full scale tests were depicted pictorially as follows:



Paper by Ramamurthy, Rao & Rao and Paper by Ramamurthy Rao & Singh

The General Reporter concluded that anamolies existed with the non-linear strength criterion they have attempted to develop (ref. Proceedings pp. 237–241).

PANEL DISCUSSION

The General Reporter then raised four key questions applied to weak and jointed rock (Proceedings p. 241), and the floor was handed over to Chairman, Dr B. Burman, with panellists E. Brown, J. Carter, C. Gerrard and D. Jennings.

Q. Dr Carter to Dr Johnston

(1) With respect to data relating to tension and compression moduli, are we just seeing a strain level effect?

(2) How have the laboratory values in the paper by Haberfield & Johnston, as shown in Fig. 1, compared with model tests?

Johnston & Haberfield:

(1) Very little effect as far as authors are aware.

(2) Results have been verified with triaxial tests.

Q. Dr Gerrard to Dr Johnston

Which modulus have you taken, noting that you have to be careful to distinguish between elastic and linear modulus?

Dr Johnston: Compression tests on unloading showed elastic strain to be comparable.

The tension cracks were found only on close scrutiny after loading tests. The consequences were that for intact rocks, the elastic modulus was under-estimated, leading to an over-estimate of settlement; whereas the reverse applied for fragmented rock.

QUESTIONS FROM FLOOR

Q Mr D. Starr to Dr I. Johnston

Addressing the question of when laboratory derived modulus values can be used for predictions of field deformation, a standard literature-based method entails the application of a Rock Mass Factor to an intact modulus. The reduction factor can be related empirically to discontinuity spacing and as the rock becomes more fragmented, the factor applied to intact modulus becomes smaller (i.e. less than 1 and tending exponentially to zero). How does this approach tie in with Fig. 2 of Dr Johnston's General Report, which implies a factor greater than 1 for converting laboratory modulus to field modulus, where rock material is massive and unjointed, and at the other end of the range the factor again approaches 1 for highly fragmented material? It only seems compatible with the middle portion of Dr Johnston's plot which shows a trend of a greater reduction in laboratory modulus with increasing fragmentation to obtain a representative field modulus.

Dr Johnston: All that was intended to be portrayed by Fig. 2 was for example, that in the left hand side of the curve, if tensile modulus was greater than field modulus, this may result in an over-estimate of prediction of magnitude of settlement than would actually occur; therefore suggesting that U.C.S. strength for rock could be in error and that the real answer could be indicated by field testing. Similarly, for the right hand side of the curve, there is an increasing need for field tests because of the problems associated with getting realistic and representative samples.

Q. G. Mostyn to I. Johnston

(1) Triaxial tests at Monash University are completed using traditional effective stress soil mechanics, are they not?

Ans Yes. Tests on rock do account for pore pressures.

(2) Also, is a very high tensile modulus important if tensile strength is very low? i.e. cracking occurs at very low stress and the problem now becomes facture propagation, not stiffness.

Ans Codes give low values of strength, e.g. "9 c_u" for clay or "0.5 q_u" for hard rock. 9 c_u may be satisfactory for clay and when this is applied to a soft rock of considerably greater strength, then resultant working loads are way down on ultimate, therefore producing very conservative results.

Q. **Dr Gerrard to Mr Haberfield**

(1) Relating to jointed rocks, with joints infilled; techniques are available to put together a bulk modulus and a modulus for sets of joints. The overall bulk modulus only applies with no separation or shear failure of joints.

Ans The question addresses whether a pressuremeter is no longer suitable for determination of Young's Modulus because of the joint separation problem. The answer is that it depends on the orientation of the joints. If you start separating joints then the value of elastic modulus would be suspect.

Dr Carter commented that no solution was available yet for interpretation of pressuremeter tests.

Mr Haberfield continued his contribution with a clarification of the definition of modulus in tension and compression. Compression tests displayed linear behaviour up until at least 50% of peak deviator stress. This modulus has been plotted up in Fig. 1 of the paper. Tensile tests displayed linear behaviour up to peak load. The pressuremeter induces a tensile stress field. For open, vertical joints, the load-deflection will be greatly affected. Young's Modulus can be considerably underestimated by the pressure-meter, which will then result in an underestimate of the "real" Young's Modulus for vertical loading. Thus applied vertical loading on a vertical joint has no significant effect, whereas a vertical load on a horizontal joint has an effect.

Tests performed by Mr Haberfield indicated that with pressuremeter tests up to 5 to 10% strain, the modulus of intact rock is given by the unload/reload modulus.

Q. **Mr H. Alehossein to Dr Johnston**

How can we apply tensile modulus to numerical analyses of real problems?

Ans We face major problems in application because of numerical instability. Therefore we cannot apply values yet.

Q. **Mr Mostyn to Prof. E. Brown**

Hoek and Brown have revised modulus and strength criteria?

Ans Yes, more data has led to a revision for the Hoek and Brown criteria.

Prof. Brown presented the following:

Hoek-Brown empirical rock mass strength criterion

$$\sigma_1' = \sigma_3' + (m\sigma_c\sigma_3' + s\sigma_c^2)^{\frac{1}{2}}$$

where σ_c = uniaxial compressive strength

For undisturbed or interlocked rock masses

$$\frac{m}{m_i} = \exp\left(\frac{RMR - 100}{28}\right)$$

$$s = \exp\left(\frac{RMR - 100}{9}\right)$$

where m_i is the value of m for the intact rock, and RMR is calculated assuming dry conditions and making no adjustment for joint orientation

For disturbed rock masses

$$\frac{m}{m_i} = \exp\left(\frac{RMR - 100}{14}\right)$$

$$s = \exp\left(\frac{RMR - 100}{6}\right)$$

Prof. E. Brown, commenting on 4th key question

Responding to Mr Galloway's analogy of a brick wall; the infill material – mortar – was of different modulus, resulting in tensile fracture.

In response to Dr Johnston's question on how soil and rock mechanics experience can be applied, Prof. Brown stated that some physical laws should apply for soils and soft rocks. He referred to Imperial College work and critical state soil mechanics methods. After several attempts, we appear to have a solution that works. Prof. Brown concluded by stating that we cannot extend or extrapolate numerical methods too far.

Dr R. Parry pointed out that he was not sure that laboratory criteria were of great use to predict field behaviour and referred to Wayco dam experience. Pore pressures developed in the field went strongly negative. High positive pore pressures in intact rock accentuate any weakness, further complicating behaviour, which defies simple strength criteria.

Dr Johnston agreed with this but believed that we must look at the mechanics and develop tools to deal with the problem, e.g. observation and common sense; literature using FEM is only as good as the data; c, φ data in soft rock is difficult. Dr Johnston concluded that common sense engineering was important and that criteria are appropriate.

Dr Burman closed the session.

SESSION B3 – SHALLOW FOUNDATIONS

Chairman	Mr G. Sutcliffe (Dames & Moore)
Secretary	Dr T.J. Wiesner (D.J. Douglas & Partners)
General Reporter	Dr K. Wallis (Qld. Institute of Technology)
Panellists	Prof. M. Pender (University of Auckland) Mr C. Thorne (Coffey and Partners) Dr J.C. Small (Sydney University) Dr D. Elder (Soils & Foundations Ltd, N.Z.)

DISCUSSION

Dr Keith Wallis proposed this question for general discussion:

> What is the state of implementation of the art of design of shallow foundations? Are there any new techniques, equipment or philosophies which may have assisted the authors of the papers which he reviewed in doing what they have reported better? That is developments in:
>
> . defining the soil profile for design;
>
> . selecting characteristic values for soil and rock properties;
>
> . analysis or other developments in post Terzaghi and Peck geomechanics.

The panel responded with the following comments.

Colin Thorn mentioned developments in equipment such as the Dutch cone, pressuremeter, piezocone, dilatometer and continuous sampling techniques in boreholes.

Prof. Mick Pender also mentioned the Dutch cone and discussed new settlement analysis techniques such as those provided by Schmertmann, Burland and Burbridge. Developments in bearing capacity theory have resulted in analyses for dealing with layered profiles.

Dr Don Elder mentioned the common problem with site investigation is limit of available funds. Should we carry out more tests or better tests? General opinion dictates that better tests or samples are better than more poor quality samples, particularly if layered strata are encountered. It was his opinion that more consideration should be paid to variation in soil properties.

Dr John Small considered that there was not much progress with defining soil profiles more accurately apart from the development of new tools such as the screw plate and dilatometer.

Comments were taken from the floor.

Mr W.T. Flintoff (RCA, Victoria): Several years of experience with using the piezocone in Victoria has proven it to be very useful in refining the detailed profile of the strata, in particular for identifying fissures such as sand layers intercollated in soft clays. This detailed data has allowed the settlements/consolidation analysis to be modelled with much greater confidence.

Preliminary work by The Road Construction Authority with the Marchetti dilatometer has been encouraging. Elastic moduli profiles determined with this equipment through an interlayered stiff clay/moderately dense sand profile overlying rock have been used to predict elastic settlements beneath a 6 m high embankment, with an order of accuracy greater than obtained with Schmertmann-type correlations on elastic friction cone penetrometer results. These settlements were confirmed by a settlement measurement instrumentation program during construction. The soil types identified from the dilatometer agreed well with those interpreted from cone tests and from nearby bores.

Mr R. Herraman (University of Adelaide): A significant advance in the areas of definitions of soil profile and selection of characteristic soil properties has been the use of the CPT and screw plate tests. This combination has been used to obtain information for the design of footings for several multi-storey buildings in stiff clays in Adelaide. In several cases there has been reasonable agreement between measured settlement and the settlement predicted using values obtained from CPT and screw plate testing.

Dr P.T. Brown (University of Sydney): Satisfactory use of static cone results for prediction of soil modulus depends on the factor α used in the equation $E = \alpha q_c$, and there will usually be appreciable uncertainty involved in the selection of α. More reliable modulus values will be produced by means of in situ load-deflection type tests.

For predictions of foundation settlements, we require values of vertical modulus. Because most soils are somewhat anisotropic, results produced by pressuremeters and Marchetti dilatometers which measure horizontal modulus, will be of limited value. Hence, the most satisfactory test will be the screw plate test which measures vertical modulus.

Dr John Simmons (James Cook University): Proposed the question to the practitioners in the audience: When is a layer a layer for the purpose of analysis, particularly given the "squiggly lines" which occasionally occur on CPT results?

Dr David Larmour (Australian Construction Services): In response to the last speaker: experience at Brisbane Airport is that cone penetration tests, either mechanical or electric, sometimes predict stratigraphy well when compared with continuously sampled boreholes and sometimes are completely inaccurate. Thus there are reservations about reading too much into the cone penetration tests.

Initially at the airport, continuously sampled boreholes with intense laboratory testing plus instrumented test embankments to check the effects of scale were carried out. As the project developed and testing became less detailed, the initial data base was increasingly used for prediction purposes-supporting Mr Thorne's comments on the value of detailed testing.

Prof. Robin Fell (University of NSW): I wish to defend the use of the Burland method for estimating settlement from SPT. It represents one of the few published methods which gives the actual scatter of data on which the method is based - so that you know the potential degree of error.

The other methods all have scatter in their data, but are often quoted in equation form, e.g. $E = \alpha q_c$ and the practitioners (at least the inexperienced ones) start to believe the answers are accurate. The profession would be better served if such correlations were always presented in graphical form with the original data points shown.

Mr Gordon Sutcliffe (Dames & Moore): Proposed the question - "Is there a need for practitioners to do much better? To produce results on a lower unit cost data basis? As a profession are we deluding ourselves by trying to make accurate predictions when the data is all over the place. Should not we be looking also at the soil- environmental deposition, and stress history".

Other members of the panel also commented on variability in sediments and the importance of having local calibrations for in situ testing and knowledge of local conditions.

Mr Alex Litwinowicz (Queensland Department of Main

Roads): Consolidation is primarily governed by site conditions which are never adequately quantified. Therefore, acceptance of all sand layers from the CPT as drainage layers has generally given reasonable results; actual settlements generally occur faster than predicted in the author's experience. I agree with the Chairman; there is often little use for high resolution of settlement magnitude estimates in roadworks as provision of the facility often far exceeds most surface imperfections.

Mr Tim Sinclair (Tonkin & Taylor):

(1) In reply to Gordon Sutcliffe's challenge – what do we need to better? I believe the most important area of greater effort should be in obtaining more feedback – more monitoring data – there is no substitute for experience.

(2) The second point is to answer the General Reporter's comment on the use of the Skempton and Bjerrum correction for settlement. I understand that the correction factor falls out of the mathematics. But my co-author (Dr Wesley) disagrees and should be asked to comment on why he believes it does not apply with such soils as the residual soils or volcanic soils of the Auckland area.

Dr L.W. Wesley (University of Auckland): My reason for not applying the Skempton/Bjerrum correction in our paper (in relation to the Corner Building) is that I am not convinced that we ever get a pore pressure rise beneath foundations on residual soils, or at least residual soils of the sort which exist in Auckland. There does not appear to be any data in the literature to suggest that there is such a pore pressure rise. Most, if not all papers on foundations on residual soils conclude that such loading is "drained" and pore pressures dissipate as rapidly as they are generated. Values of k or c_v seem to be 100 or 1000 times higher in residual soils than sedimentary soils.

A second point I would comment on in relation to differential settlement of multi-storey framed buildings, is that there is the great difficulty in obtaining tolerable settlement figures. Some structural engineers are happy to accept quite large settlements, while others have apoplexy at the suggestion of even 10 mm or 15 mm of differential settlements.

Dr Peter Brown (University of Sydney): Referring to the last speaker describing the calculation of settlements of a building which resulted in 60 mm at the centre and 25 mm at the edge, was the applied loading uniform or was some account taken of stiffening effect of the superstructure?

Mr Ian Smith (Hollingsworth Consultants): In reply to questions on the range of conversion factors to obtain undrained shear strengths (c_u) and coefficients of consolidation (m_v) from CPT cone resistances, the paper by Litwinowicz and Smith states an average conversion factor of $c_u = q_c/18$. This was a mean value and is certainly applicable to the very soft or soft silty clays at specific sites. There is no question that such conversions are only relevant to very special and site specific conditions and it is pointed out that for all soil types, of whatever strength, over the Gateway project the conversion factor ranged from 3 to 30 (from memory).

In reply to the question as to how the undrained shear strengths were derived, these were from field shear vane tests and c_u values derived from CPT results. All results were graphed and mean values adopted.

For m_v values derived from CPT curves, the curves at any one site were divided into blocks of average cone resistance and a conversion of $m_v = 1/5q_c$ used. For settlement predictions, layers of thickness less than 50 mm were either ignored or aggregated.

Panel discussions proceeded on developments in post-Terzaghi and Peck Soil Mechanics.

Dr John Small (University of Sydney) mentioned development including Skempton and Bjerrum's method, the pioneering efforts of E.H. Davis and Harry Poulos in applying elastic theory to 3D analysis for prediction of settlements, numerical techniques such as finite layers, finite elements and boundary elements. Other techniques such as non-linear analyses, structure-soil interaction, methods to accommodate variability in soil deposits are representative of developments since Terzaghi and Peck.

Colin Thorne (Coffey & Partners) thought it most important to get the mechanism straight before getting too involved in an analysis and cited elasto-plastic analysis where it may not be possible to determine a factor of safety because it is difficulty to decide what constitutes failure. He also advised the use of more than one method, in fact a redundancy of methods:

. in situ testing has resulted in great strides in the prediction of settlements on stiff soils;

. soft ground predictions are still undeveloped.

Mr Thorne also tabled some results of preload settlement measurements and posed the question "How do we distinguish between classical primary and secondary consolidation"? – because it is not easy!

Prof. M. Pender (University of Auckland) cited as developments in post-Terzaghi soil mechanics, developments in bearing capacity analysis such as Vesic's work, model tests on calcareous soils and techniques used to establish an upper limit.

Mr K. Seddon (Pak Poy & Kneebone) (from the floor) drew attention to the problem of the relative importance of bearing capacity as opposed to settlement in determining allowable bearing pressures.

Dr M.R. Hausmann (University of Technology, Sydney): This session and the Conference as a whole is dominated by discussions of stress and strain. Little thought is given to problems of flow of fluids and other liquids in soil, even less consideration is given to the effect of the variability of hydrological and environmental factors. Virtually nothing is mentioned about predicting the desirability of new materials used in earth structures, such as steel strips and geosynthetics.

In Reinforced Earth, prediction of corrosion is an integral part of design. Lack of long term experience is still hampering predictions of durability of synthetics. The paper by Hausmann, Adler and Boyd attempted to draw attention to this problem.

E.G. Trustcott (Gutteridge Haskins & Davey Pty Ltd): Various discussions have separated settlement and stability. Furthermore, Colin Thorne has pointed out the high creep factors measured in several jobs. I suggest that settlement and stability are in fact interrelated through the foundation stress state. In our paper we, in fact, addressed stability by ensuring that nowhere did the foundation stress state greatly exceed the shear strength. This was done because on other jobs we had found high measured creep which we felt was due to the high local shear stresses in the foundation. At Gibson Island, the creep measured (although only a few measurements have been taken) was of the expected magnitude of 1% to 1.5%.

D.J. Douglas (D.J. Douglas & Partners Pty Ltd): I draw attention to the expected standard of accuracy of cone gear currently used in Australia. In very soft soils we may be operating at less than 1% of full scale deflection and unless very carefully and regularly calibrated, cone results may be grossly in error.

Secondly, in many situations it may be more appropriate to use "regional values", than to attempt to measure them. Sometimes the range of uncertainty in the measured value can be greater than the actual value.

SESSION C1 - MINING SUBSIDENCE

Chairman	Prof. B. Hobbs (Chief, CSIRO, Div. of Geomechanics)
Secretary	Mr R. Jeffery (Jeffery & Katauskas)
General Reporter	Dr J. Galvin (Newcom Collieries)
Panellists	Dr M. Coulthard (CSIRO, Div. of Geomechanics) Dr L. Holla (Dept. of Mineral Resources) Mr J. Braybrooke (D.J. Douglas & Partners)

(Session summary by J. Small and J. Braybrooke).

GENERAL REPORT

The General Reporter, Dr Jim Galvin, began the Session by summarising and commenting upon the papers which were included in the Mine Subsidence category. He then asked some key questions of the panel:

Q (1) The first question involved modelling and the problem of trying to represent the real mechanism. Dr Galvin said that many different types of software had been tried but none were fully successful.

Lax Holla said that subsidence prediction often required empirical methods for which there were hopes for good accuracy but this was not always possible. In the UK ±10% was an acceptable accuracy. The ultimate objective was to get the order of magnitude of the subsidence correct; to be able to predict if it would be 1 mm or 100 mm.

Michael Coulthard said that the Dams Safety Committee encouraged the need to model and predict. Their thinking was guided by physical modelling in the laboratory which suggested distinct element modelling i.e. interacting blocks.

John Braybrooke (D.J. Douglas & Partners) said that in the Electricity Commission a series of different material models had been looked at. The elasto-plastic models were not successful but limited tension models showed reasonable results.

For Angus Place Colliery, which is a difficult case, this model produced fairly good predictions. Horizontal shear modulus appears to be the more important parameter.

Lax Holla said that in a Department of Mineral Resources subsidence project, anchors were placed at different horizons in a borehole and this produced some interesting results. These could be used for comparison with predictions from models.

Q (2) The second question related to pillars.

Dr Jim Galvin said that mining underwater, where strain must be a minimum, can be successfully carried out by mining a single seam and subsidence can be restricted to <50 mm.

Further, using a width to height w/H ratio of 8-10 the accepted wisdom is that the pillars are stable. However, what happens if 3 seams are mined by first workings with pillars stacked one on top of the other. For example, this has been done at Myuna Colliery, Lake Macquarie where the top seam is overlain by about 70 m of virtually unjointed conglomerate. However, between the first and second and the second and third seam there are zones of claystone. In places this claystone collapses above the pillar.

In one place a 12 m high fall has been measured which drastically reduces the w/H ratio.

Individually the w/H ratios for each seam may be acceptable should no collapses occur, however what is the effective w/H ratio for the stacked pillars should falls occur?

John Braybrooke said that although the convention was that a w/H ratio of 8-10 was acceptable he had seen records of w/H ratios >8 where failures were still occurring. If fall-in does occur then this provides a minor confinement to pillars, however he felt that wider pillars were the answer.

Lax Holla said that the general answer was very difficult, however if the depth of extraction is greater than 200 m, then stable pillars are not required (for example there is 400-500 m cover in the Southern Coalfields).

Jim Galvin added that w/H = 8-10 is questionable especially with deep mining.

Contributions from Floor

Dr A.K. Bhattacharyya (University of N.S.W.):

1. Stability considerations for pillars provided for restricting surface subsidence should include the aspect of possible penetration of the loaded pillars into the floor material. Such occurrences have been considered to be responsible for the absence of humps in the (transverse) surface subsidence profile above the chain pillars separating longwall panels at West Cliff Colliery.

2. The use of more complex models e.g. 'distinct elements' instead of simpler elastic models in the simulation of subsidence of strata over completely extracted areas in coal seams, would introduce problems relating to

the requirement of more detailed data for example, the height of collapsed strata compared to bridging strata.

Mr Don Kay (Dept of Mineral Resources) said that he had provided status advice on the Joint Mathematical Modelling Case Study of a Longwall Panel at Angus Place Colliery.

He advised that modellers should be able to learn from comparing the performance of their models with the measured results and then learning what type of model simulates the subsidence mechanism best.

Professor David Stapledon (Geotechnical Consultant) agreed with the General Reporter, that an important weak link in his Figure 1 flow chart, is the geomechanical work aimed at identification of subsidence mechanisms. One problem that faces the "Geomechanics" is the inability to monitor displacements, water pressures etc. at great depth in the rock mass (say the lower third) once extraction has taken place and the mass has suffered sufficient disruption to cut instrument cables. Prof. Stapledon said he understood that remote-reading instruments are now in use in high earth embankments. Development of more powerful remote reading gear to allow monitoring from surface to (almost) seam, before, during and after extraction (i.e. through up to 300 m of rock) would greatly assist our understanding of subsidence (and potential water flow) mechanisms.

Dr Laurie de Ambrosis (Longworth and McKenzie): said

1. Aspect ratio of pillars in a caving roof

   We require a symmetrical caving on either side of an existing pillar to decrease the aspect ratio.

   However, in this situation with an obvious "weak" roof what in fact controls stability the coal pillar or the roof?

2. Surface Geotech Features

   These are very significant. Our case history in the Hunter Valley shows that the centre line translated 100 mm. This obviously affected surface strains. In predicting, most of the discussion has been on subsidence whereas strain and to a lesser extent tilt will cause damage to surface structures.

   Surface geotechnical features will control these latter aspects to a much greater extent than does subsidence and hence will become very important in this next level of prediction.

SESSION C2 - OPEN CUT MINING

Chairman	Dr M. Dunbaven (BHP Engineering)
Secretary	Mr B.F. Walker (Jeffery & Katauskas)
General Reporter	Mr J. Trudinger (Dames & Moore, W.A.)
Panellists	Dr S. Priest (University of Adelaide)
	Mr T.D. Sullivan (Coffey & Partners)
	Mr F.E. Kaesehagen (MIM Holdings Ltd)

COMMENTS BY GENERAL REPORTER

The General Reporter introduced the session by raising the question of "Why are we concerned with instability in open pit mines?" Factors cited included:

- safety of the work force,
- industrial relations issues,
- damage to plant and equipment,
- sterilisation of ore,
- additional waste to be removed,
- loss of production,

all of which have an impact on costs and profitability.

The published General Report on the papers in the Conference was reviewed and the questions arising from each paper briefly addressed. The General Report noted the lack of case histories in the published papers that included topics such as rock reinforcement, effect of blasting on slope stability, and engineering geological input.

Suggestions for research areas and questions to be investigated were made as follows:

* Characterisation of discontinuities (in the absence of large exposures)
  - origin and effect of tectonic history
  - effects of texture
  - unusual orientation and how they are related to stress fields
  - continuity problems in-situ versus evidence in rock cores.
* Stress Relief
  - is it good or bad for stability
  - reduction in friction angles
  - increases in infiltration
  - improvements in permeability and drainage.
* Other directions for stabilisation techniques
  - use of cables, bolts, dowels or others
  - drainage by use of; low angle drill holes; hydro-fracturing or pre-splitting prior to excavation
  - reduction in infiltration
  - disruption of potential failure planes by blasting prior to excavation.
* Effects of Blasting
  - how far into face does damage extend
  - does it reduce stability other than superficially

- does loosened rock mass relieve stresses in similar way to intact rock mass.

* Velocity of Failure

- what causes rapid failures

- under what circumstances can we assume rapid failure will not occur

- need to understand mechanisms operating.

After introducing the panellists, the session was then opened for discussion.

DISCUSSION

Characterisation of Discontinuities

Dr S. Priest (University of Adelaide): made the distinction between *prediction* and *characterisation.* He considered characterisation to be important and to involve field measurements to put numbers into the engineering geological picture. Objective sampling correcting for bias was therefore needed. Prediction required extrapolation of our knowledge into areas of the unknown. As adoption of geostatistical methods had been unsuccessful for prediction, we have used characterisation to predict face performance.

He then summarised the needs for future research to include consideration of:

- prediction variability e.g. sizes of failures
- spatial patterns e.g. termination of joints
- discontinuity shape models e.g. circular
- water pressure models e.g. linear distributions
- validation.

In response **Mr Garry Mostyn (University of NSW)** made two comments. Firstly, as far as he could tell all references to discontinuity shape seem to trace back to McGregor and Robertson (1970) who measured a few joints and found strike length approximately equalled dip length. This has almost become universally accepted. Mostyn considers it needs to be re-examined to better determine shape.

Secondly, Figure 1 of Priest and Samaniego seems to show a lot of long discontinuities. This contradicts normal field observations but could be a result of length bias in a planar section. Priest commented that this appears to be due to bedding planes which cross over in the model; a computational quirk still to be resolved.

Dr Barry McMahon (Dames & Moore) commented that failures tend to occur on major joints rather than minor ones, yet we tend to measure all the minor ones. Observation of data plots shows maxima correspond to the major joints. Need also to consider symmetry as jointing is often orthorhombic and maxima are parallel to the major stress relief effects. Consideration needs to be given to how joints are measured and observation of the character of the joints.

(Contributor from Floor): Steve Priest's concentration on discontinuity characterisation rather than prediction is probably appropriate from a practical point of view e.g. in strip coal mines where you are locked into a strip orientation and the important issue is to characterise the discontinuities you will meet as you approach them.

Mr J. Beal (Dept. of Mines & Energy, SA) commented on the success or failure (or future possibilities) of down-the-hole camera techniques for identifying suspect major failure feature(s).

Slope Stabilisation

Mr T. Sullivan (Coffey & Partners): In commenting on cable bolting as a slope stabilisation measure he felt it was an abused technique; it was hard to see how 10 to 15 m cable anchors could have magical properties in stabilising failures at 20 to 30 m behind the face. Cable anchors do, however, have a part to play in supporting slopes, being effective for surficial support.

Dr B. McMahon (Dames & Moore): In a recent tour of many open pits I gained the impression that one third of cable anchors installed are doing a good job, one third are failing on deep-seated weakness planes and one third are not necessary.

There are two types of rock slope:

(i) Those which are stable with respect to deep failure mechanisms and which are controlled by surface instability. These are the slopes in which rock anchors are applicable.

(ii) For those which are subject to deep seated modes of instability, patterns of 25t anchors 15 m long have no effect. Long anchors with capacities of hundreds of tonnes may be applicable but at the present time in many cases they are not economic compared to waste excavation.

Mr C. Windsor (CSIRO, Div. of Geomechanics): The CSIRO Division of Geomechanics has been involved in the research of cable dowels for over a decade, predominantly in underground mining. In this area it appears that cable dowels have been most useful in confining either superficial or structural controlled failure near or at the surface of the excavation. I think it is reasonable to expect that cable dowels used in the stabilisation of surface slopes will operate in a similar manner and should be quite useful in controlling superficial type instability as opposed to deep seated type failures.

We call cable dowels 'reinforcement' in contrast to 'support' for we feel that they operate predominantly by 'reinforcing' structural discontinuities within the rock mass. For this reason each slope should be mapped such that a specific design that suits the specific structure of (a specific) slope can be evaluated. I would suggest that this approach is better than adopting a 'standard' pattern design for all slopes.

Future Developments

Mr G. Mostyn (University of NSW): At Mei Moh mine (Thailand) blasting had been adopted to disrupt failure planes. Canadian literature cites doing the same to relieve pore water pressures. This method is considered to be useful for well defined failure planes.

Mr T. Sullivan (Coffey & Partners) agreed that blasting to reduce pore water pressures by drainage was beneficial. He considered that the cheapest option was often groundwater control as this was a major destabilising force.

Dr McMahon (Dames & Moore): One technique for stabilisation of some rock slopes is deep blasting to break up the continuity of weak seams. A trial of this method at the Mei Moh Mine was successful in stabilising a low wall slab slide on a clay seam. Depths of up to 30 m were used and up to 100 m are being considered.

The deep blasting tends to bend the bedding planes around the blast holes upward in a shape similar to tepees, thus disrupting the failure planes. In addition, drainage was achieved to the base of the blast holes. The method was successful in stopping the slide.

Mr Davies (BHP–Utah): I suspect that Mike has invited my comment due to the current situation at Goonyella Mine where more than 2 million cubic metres of low wall are on the move. In considering remedial measures we first tried the cheapest option – horizontal drains. Once this proved unsuccessful we were faced with three options:

(i) blast to disrupt the well defined bedding plane shear which forms the base of the slide,

(ii) reinforce the rock mass through the shear surface, and

(iii) earthworks, particularly unloading.

The earthworks option has been chosen due mainly to the reliability and the feasibility of modifying the method as required during monitoring. Blasting was considered too risky with the possibility of greatly accelerating the relatively low rate of movement (10 mm/day). Reinforcement was likely to have the same order of costs as earthworks with less reliability and feasibility.

Dr John Eckersley (James Cook University): Experience at Goonyella Mine was that blasting in strips east of the high wall (excavation) simply weakened the weak shear surfaces just above the coal seam, and allowed infiltration of rain water through the blast fracutres and then softening of the slake-prone high wall rocks. So the effect was disastrous rather than beneficial to stabilising.

Dr S. Priest (University of Adelaide) commented that for dams, erosion cavities were usually not resealed and failure resulted. In slopes the reverse often occurred; if a slope moved, a drainage path opened up and the block stabilised. Subsequent resealing of the drainage path leads to future reactivation of the movement.

Mr S. Newcomb (SECV, Vic.): In relation to groundwater control the S.E.C.V. has a practice of clay covering batters, installing horizontal drains and covering heavily jointed areas where a drain passes through by using old conveyor belt.

Mr F. Kaesehagen (MIM Holdings) commented that economics was the main driving force for mine managers; they were prepared to spend but it must be profitable.

Mr Newcomb (SECV): In reply to use of numerical methods to predict movements, S.E.C.V. have used the Boundary Element method to predict movements which can follow current trends, however, we are not using it to predict catastrophic failure due to stabilising methods employed.

In relation to the velocity of slides, the East German's at their open cut brown coal mines allow the slopes at the edge of the basin to slip then go back and dig out the remaining coal. They have good monitoring and when movements reach a certain rate the bucket wheel excavators are pulled away.

SESSION C3 – EARTHQUAKES AND VIBRATIONS

Chairman	Mr A. Olsen (Worley Consultants Ltd)
Secretary	Dr I. Moore (University of Newcastle)
General Reporter	Prof. M. Pender (University of Auckland)
Panellists	Mr D. Jennings (Works Consultancy Services) Mr W. White (Aust. Defence Force Academy) Dr P. Moore (University of Melbourne) Mr T. Sinclair (Tonkin and Taylor Ltd.)

DISCUSSION

Prof. M. Pender (University of Auckland) commenced his report by noting that earthquake hazard is not rated highly in Australia. The Bay of Plenty earthquake in New Zealand in 1987 and the Northern Territory quake in 1988 were both significant events. The latter would have generated considerable interest in New Zealand, but strangely did not do so in Australia. He also noted that while predictions for static problems are difficult, dynamic systems are much more difficult since errors are magnified with integration in time.

Professor Pender discussed the papers listed in this session (refer to written discussion). He then initiated the panel discussion by introducing the subject of estimating small strain soil stiffness. He queried whether it was best to use shear wave velocity measurements to estimate small strain modulus, and asked for comments on current practice.

Mr Tim Sinclair (Tonkin and Taylor Ltd) indicated that he thought there was no argument, since sonic velocity testing is the obvious approach. However, other techniques had been tried, for example, the torsional borehole stiffness device developed by Dames and Moore.

Dr Peter Moore (University of Melbourne) suggested that it was popular with practitioners to follow the Japanese (and Bolton Seed in the U.S.A.) by using correlations between Standard Penetration Test N values and maximum shear modulus. This was a result of the costs of making field measurements for velocity, although measurement of shear wave velocity would be better. It requires experience to conduct the velocity test and to interpret it, and this knowledge did not yet seem to have reached practitioners.

Mr Weeks White (Australian Defence Force Academy) suggested that if a hyperbolic relationship, say, could be fitted to the data then this would be adequate, and it is not necessary to determine maximum shear modulus.

Mr David Jennings (Works Consultancy Services): Supporting Dr Moore's comment, Jennings indicated that practitioners use Standard Penetration Test correlations, although the use of shear wave velocity does look promising.

Mr John Bosler (Snowy Mountains Engineering Corporation): Professor Pender has taken me to task in his review for not measuring the shear modulus of the Yonki Dam embankment at small strains.

In defence of S.M.E.C.'s procedure for the dynamic analysis of Yonki Dam, I would like to make the following comments:

1. Measurement of the small strain modulus was attempted in-situ on the rolling trial material using geophysical methods, however, this was not successful owing to an inadequate thickness of material to test.

2. The small strain modulus is mainly of academic interest. Of paramount importance is the modulus at strains that will be induced in the embankment during the Maximum Credible Earthquake (M.C.E.). These strains are of the same order as the strains measured in cyclic triaxial tests (as used at Yonki) or cyclic simple shear tests. Hence it is argued that the cyclic triaxial data are appropriate for predicting the dynamic response of the dam during the M.C.E.

Dr Laurie Wesley (University of Auckland): With respect to the Yonki Dam use of laboratory tests to measure modulus. It seems to me that for the compacted fill, one may be forced to use laboratory tests rather than field wave velocity measurements because of the difficulty of doing field measurements which would involve construction of a trial embankment.

Mr Kevin Mills (South Australian Institute of Technology): Were sensitivity analyses performed by S.M.E.C. on the Yonki Dam to determine the effect of shear modulus on dam performance? What is this sensitivity?

Mr John Bosler (SMEC): Some work was undertaken, but the details are not at hand. Shear modulus does have a significant effect, and there is a need to have reliable values.

Dr Tam Larkin (University of Auckland):

1. Triaxial tests are not a direct measure of shear modulus. Shear modulus is inferred from data using an estimation of Poisson's ratio etc. Blending of shear tests (resonant column or in-situ wave velocity) with triaxial tests usually leads to a mismatch of data in the mid-strain range.

2. There is potential for downhole devices to measure in-situ wave velocity at high strains. Work has been carried out in this direction at Auckland University where wave velocities were measured at strains in the nonlinear range.

Prof. S.F. Brown (University of Nottingham, United Kingdom): Care needs to be taken in the laboratory evaluation of small strain stiffness using cyclic load triaxial tests illustrated by instrumentation for axial and radial deformations allowing displacements down to 10 microns to be measured. This allows the gap between earlier triaxial and resonant column testing to be bridged.

Consideration of stress level effects on stiffness, both stress history and level of cyclic deviator stress, may be important. Data was presented by Professor Brown for a silty clay to illustrate the relationship between stiffness and stress level.

Dr J.C. Small (University of Sydney): Are there any laboratories or companies in Australia which have resonant column devices or equipment capable of doing down borehole shear wave tests to determine dynamic shear wave modulus?

Mr Weeks White (ADFA) indicated that he did know of some cross-borehole and downhole testing being undertaken.

Prof. Mick Pender (University of Auckland): The University of Auckland has equipment for measuring shear modulus at different strain amplitudes.

Mr Peter Goss (Richard Heggie Associates) commented on availability of cross-hole survey work: Richard Heggie Associates has carried out cross-hole shear wave velocity determinations, in-situ.

Mr David Jennings (Works Consultancy Services): The Yonki Dam is interesting in that softening occurred during the earthquake. Has the natural period returned to its initial value since the earthquake occurred?

Mr Colin Newton (Works and Services Development Corporation): In reply to a question from David Jennings regarding the softening and increase of period during shaking of the Metchine Dam during the 1987 earthquake.

I am unaware whether the first period of the dam has returned to its initial pre-earthquake value. Further analysis of the accelerometer records is underway at present and this may answer this question.

Dr Peter Moore (University of Melbourne): In the resonant column test, what assurances do we have that sample disturbance effects are not too severe? The effect may be more profound than we are used to even in static testing.

Prof. Mick Pender (University of Auckland) initiated discussion on a second question, that of dependence of rheological constants on frequency. He asked if anyone was taking account of the frequency affect.

Dr Peter Moore (University of Melbourne): No, this level of sophistication is not needed since frequency effects are less important than other nonlinearities.

Prof. Mick Pender (University of Auckland): As a final discussion topic, consider factor of safety. Should it be defined using an interaction diagram? Should we just evaluate deformation using F = 1 during an earthquake?

Mr David Jennings (Works Consultancy Services): Codes of practice give advice concerning limiting loads which can be applied to the foundation. This also limits the maximum moment which can be applied. We need to be aware of the impact of the total situation.

Mr Tim Sinclair (Tonkins and Taylor Ltd.): I disagree. There is a problem of "soil engineer" - "structural engineer" interaction. Structural engineers merely want the maximum bearing stress, but bearing capacity depends on the combination of soil with structure.

Dr Peter Moore (University of Melbourne): We unconsciously have a pseudo-static approach to analysing the problem, using limiting equilibrium.

This has no meaning for the dynamic problem, where there is a need to focus on displacements.

Mr Weeks White (ADFA): I agree with Dr Moore. Factor of safety has no meaning and we need a more rational approach. This is a situation where reliability theory would demonstrate the problems with using normal static concepts.

Mr John Galloway (Consultant): A somewhat wry suggestion to Messrs Sinclair and White:

> When a structural designer insists on the geotechnical engineer providing a "safe bearing value", perhaps the proper answer is:
>
> > "the safe bearing value is the value which you, as designer, with your understanding of the overall problem, and the service requirements your client wants, consider to be safe".

Dr Don Elder (Soils and Foundations Limited): I have a question about design of foundations where liquefaction is predicted, and where I believe there is currently a lack of direction. As a simple example, which applies as a general principle:

> If a shallow foundation would otherwise be suitable for a low-rise structure (say 1-3 storeys) but liquefaction potential exists - perhaps in the upper 10 m of sediment - how does one design the foundation? What is the best advice that the geotechnical engineer can give the structural engineer and the client?
>
> Options might be:
>
> * Piled foundation to carry all foundation load.
> * Vibro-floatation to improve the site.

These are somewhat inconsistent with the general principles of seismic structural design, which require ductile response from a structure during an earthquake. Displacements and deformations are therefore acceptable provided catastrophic failure does not occur.

If we recommend full piling, or full site improvement, we are therefore not being consistent with structural design philosophy and are probably being very conservative.

The options I prefer are:

1. Tie the foundation together very rigidly with foundation beams, increased steel, etc.
2. Use a raft foundation.
3. The option I often prefer is to use "partial piling" to reduce excessive settlements, and particularly under heavily loaded areas. Piles are designed to carry part or all of the gravity load only under earthquake conditions - with end bearing - and with factor of safety of unity. These piles will not attract significant load until liquefaction occurs.

Would the panel, or anyone else, like to comment on this philosophy?

Dr Peter Moore (University of Melbourne): If you are building on sand which can liquefy, then either don't do it, or use piles.

Dr David Jennings (Works Consultancy Services): Towns in New Zealand have liquefiable deposits where there is building. Perhaps we should look at bridge design philosophy. Design the superstructure to limit the loads on the foundation. Make sure that if damage occurs it is accessible, and does not cause collapse.

Mr Tim Sinclair (Tonkin and Taylor Ltd): I agree. Many buildings in New Zealand would be ruled out otherwise. Deep foundations are suitable for five storeys or greater, but do you rule out a site due to the small possibility of a problem? Tie the footings together and hope for the best.

SESSION D1 - STABILITY OF SLOPES IN SOIL AND ROCK

Chairman	Prof. D. Stapledon (Geotechnical Consultant)
Secretary	Mr M. Dale (Peter Burgess and Assoc. Pty Ltd)
General Reporter	Mr G. Mostyn (University of New South Wales)
Panellists	Prof. R. Fell (University of New South Wales) Mr P. Burgess (Peter Burgess & Assoc. Pty Ltd) Mr W. White (Aust. Defence Force Academy)

REPLY BY AUTHORS

1. Jiro Kuwano - Kuwano, Yoshida & Ishihara, "Change in the stability of slopes with degree of saturation".

Failure conditions determined by stress ratio are not so easy to use on a daily basis. However, the same criteria is to be used. I agree to it.

Excess pore pressure at failure is to be predicted, if the effective stress analysis is carried out. From the practical point of view, it is very difficult, so such difficulty is put into a black box, i.e. total stress analysis.

2. T. McKinley - McKinley & Raisbeck, "Monitoring an active landslide at Howlett's Road, Yallourn North"

1. Horizontal drains were considered but experience in this area in fine grain materials has shown that this method would be unlikely to work.
2. Contractors would be unlikely to work at the toe of such an active landslide.

General Comments from Weekes White

I would like to make a comment for the record on the definition of reliability index in probabilistic analysis. The index β can be expressed as:

1. (mean F - 1)/(standard deviation of F) or
2. (mean safety margin)/(standard deviation of safety margin)

where the safety margin SM is in essence equal to the resisting moment M_R less the demand moment M_D. (The factor of safety $F = M_R/M_D$).

Now M_R and M_D tend to be normally distributed (because of the central limit theorem); hence SM is also normally distributed but F is *NOT*. In practice, a "design" β is often chosen assuming that the underlying friction is a normal distribution; if β is based on F then what values of β do we use?

Secondly, choosing β based on SM gives a value fairly close to that obtained from Hasofer and Lind's "invariant" definition. Finally, defining β using the SM allows us to take advantage of the analytical expressions for the derivatives of the SM in a Morgenstern and Price formulation of the limit equilibrium method.

With respect to the paper by Chowdhury and Zhang, if β is defined using the SM, a Morgenstern and Price analysis of homogeneous slopes (Figures 2 and 4 on p. 454) indicates much closer critical failure surfaces for minimum F and minimum β than those shown in the paper.

PANEL DISCUSSION

Q (1) What is the value of back analysis in slope design?

Professor R. Fell (University of New South Wales) is a devotee of back analysis and discussed the need to keep the analysis to a particular set of variables (viz strength and pore pressure). Strength can be divided into cohesion and friction angle and, combined with pore pressure, these three variables should be mixed and analysed, then designs should be checked. Correct slope geometry is also important.

Weekes White (ADFA): Nothing to add.

Peter Burgess (Peter J. Burgess & Assoc. Pty. Ltd.): Back in the real world, you *must* develop a realistic geotechnical/geological model. From my experience with back analysis of dam failures it has been necessary to keep increasing the strength variables to equate with the observed failure and phreatic surface conditions.

Gary Mostyn (University of New South Wales) voiced the concern that practitioners were using back analysis essentially to prove their initial conceptions.

Robin Chowdhury (University of Wollongong) cited an example from the UK where a research student was examining case histories of failure surfaces. Dr Chowdhury had then used these examples, together with various strength data which gave the same factor of safety, to predict which was the correct slip surface.

Michael Dunbavan (BHPE): Back analysis will give a family of strength values. If a variety of slopes can be analysed, the intersection of the strength loci should give a better indication of the appropriate strength values rather than guessing from a single back analysis.

John Eckersley (James Cook University - Townsville): Thorough attention to the slope geometry and slip mechanism is essential.

1. It is important to realise that normal stress range has an important effect on the combination of c & φ - and the values selected need to be appropriate to the stress range in the failure surface being analysed.

2. While most failures can be assumed to fail at an overall F = 1, failures occurring immediately after excavation may have failed at F < 1.

3. One must also be aware of the consequences of changing the parameters (say to a higher friction angle) in a back analysis because different parameters may relate to a completely different slip surface.

(A contributor from the MBW, Victoria) (No contribution sheet): With factors of safety <1.15 creep may occur which leads to progressive failure. On a failure surface, parts of the surface may have a F of S of 0.8 and other parts have a F of S of 1.2 which averages out to a F of S of 1, i.e. failure, so one must be also careful of where the slope is analysed.

Q (2) Is the difference between 2D & 3D modelling important?

Peter Burgess (P.J. Burgess & Assoc.): When it comes to modelling, the average practitioner does not have the time or the budgets to develop a 3D model. Rock slope models, say wedge analysis, are generally 3D models but on a soil slope a 2D model is generally adequate except for say arching between abutments.

Weekes White (ADFA) considered there are two problems with 3D analysis:

(a) What factor of safety is acceptable for design using a 3D model?

(b) We must reconfigure original 2D parameters so that they are suitable for the 3D situation.

Prof. Robin Fell (University of NSW) agreed with W. White, saying that he does not believe you get much more out of a 3D analysis.

Dr Robin Chowdhury (University of Wollongong) believes 3D modelling should be used in the appropriate cases e.g. modelling of the Vaiont Dam Failure.

Keith Seddon (Pak Poy Kneebone) believes 3D modelling has its uses and cites an example of a trackscavator working on the edge of a trench; 2D modelling would have indicated that the trench would have collapsed where as the 3D modelling gave a factor of safety of 1.35 - 1.4. The factor of safety for 3D analysis is approximately 2 × 2D F of S.

Q (3) How would you treat non analysable slopes?

Weekes White (ADFA) stated that he would call his geologist.

Prof. Robin Fell (University of NSW) gave the example of house sites in the local shires and the need to predict stability of a 2 to 4 m thick colluvial layer. With individual land owners you cannot afford the time or the money to analyse the stability of their slope so you have to rely on your own judgement.

Peter Burgess (P.J. Burgess & Assoc.) concurred with Prof. Fell's remarks and added that it may be difficult with councils requiring that the geotechnical engineering guarantee that any remedial works will last the life of the structure i.e. predicting the unpredictable.

Paul Finlow-Bates (Tasmanian Hydro Electricity Commission) continued by saying that the geotechnical profession is guilty of giving answers when answers cannot be given. The profession generally has a gamble on the answer or gives a very conservative design.

Ian Pedler (SECV) said that charges can also be laid against the profession for being too conservative.

Prof. David Stapledon closed the session stating that it was both stimulating and productive and went on to thank the General Reporter Gary Mostyn for a systematic, thorough, honest and thoughtful review of all the slope stability papers.

SESSION D2 - DEEP FOUNDATIONS

Chairman	Prof. I. Donald (Monash University)
Secretary	Dr I. Swane (Dames & Moore)
General Reporter	Dr A. Williams (SMEC)
Panellists	Dr M. Randolph (University of Western Australia) Mr B. Rodway (Australian Construction Services) Mr B. Chandler (Maunsell & Partners)

GENERAL REPORT

The general report was delivered by Dr Adrian Williams, who summarised the papers and made comment upon them (see elsewhere in this volume), after authors made replies to the general reporter's comments, he posed some key questions for the panel and audience to consider.

REPLY BY AUTHORS

Dr Tony Phillips (Arup Geotechnics) stated that had local "code" values been used to design piles at Darling Harbour they would have had a third of the capacity adopted. The approach used was to classify the rock in accordance with the recommendations and experience of the local branch of the AGS which resulted in some of the heaviest pile loads ever on Frankipiles.

Back analysis of the test results indicated that this approach was justified.

Mr Don Douglas (D.J. Douglas & Associates): The real problem is the presence of large fragments causing problems in interpreting the SPT values. SPT's did not indicate the weak nature of the material.

The main purpose was to illustrate:

1. SPT's in coral debris overestimate soil density and any analysis based on them will overstate actual capacity.
2. Analysis was based on static values.
3. Site control by the Hiley formula was backed up by static loading.

Prof. Harry Poulos (University of Sydney) replied to the comments of the general reporter concerning the paper by himself and C. Hewitt by saying that:

(a) the soil modulus and degradation parameters were determined from model tests on single piles and applied to analysis of pile groups;

(b) the mechanism of degradation of skin friction along pile groups is that degradation commences at the region of greatest shear stress concentration. In single piles, there is a stress concentration at the top of the pile and hence degradation tends to commence at the top and work its way down the pile. In contrast, for pile groups, shear stresses tend to be concentrated over the pile tip, and hence, under cyclic loading, degradation commences near the pile tip, and moves upward along the pile.

Anthony Goh (Nanyang Technological Institute): One of the problems of determining soil parameters of residual soils in Singapore is the partially saturated nature of the soil. This makes conventional triaxial testing rather more complex. As these materials are rather stiff, cone penetration tests have to be ruled out. Hence SPT and pressuremeter tests are the main methods that can be used. The heterogeneous nature of the materials also makes determination of the soil parameters directly next to the pile relatively difficult.

S.C.R. Lo (ADFA): A clarification in response to the comment by the reporter on the paper by Chin et al (1988).

The radial variability of the soil properties is in fact taken into account inherently in the formulation of the random field model (Vanmarke, 1978) used and the assumption that the domain is statistically homogeneous (i.e. variability given by the same trend and variance). This means that the penetrometer testing need NOT be adjacent to the pile. However, the analyses do assume that scale of fluctuations δ in the vertical and horizontal direction are identical. This is justified becaused the result of the analyses is not sensitive to δ, provided that δ is small compared to pile length - a requirement consistent with the values of δ reported by Lumb.

PANEL SESSION

Q (1) Why do we need to carry out static load tests if we have performed dynamic pile tests? Also, what are the uncertainties?

Mr Brian Chandler (Maunsell & Partners): The following contributions on the use of PDA/CAPWAP were made by Brian Chandler:

1. Field Data Acquisition must involve both strain and acceleration measurements.
2. You must conduct CAPWAP analysis to accurately determine pile capacities.
3. You must hit the pile hard enough to mobilise the resistance required and if the ultimate capacity is to be determined the pile must achieve a set of at least 2 to 3 mm.
4. You must consider pile 'set-up' and hence do restrike testing to compare CAPWAP with direct load tests.
5. Even with consideration of 3 and 4, you must not anticipate a CAPWAP to direct load test correlation to be much better than ±10%.
6. I believe the PDA/CAPWAP system provides sufficient testing accuracy and quality assurance to use Factors of Safety of 2.0.
7. I believe the PDA/CAPWAP system can be

confidently used as an alternative to direct load testing.

He also detailed the increasing usage of the stress wave method throughout the world including countries such as the USA, Canada, Sweden, Germany, Singapore, Malaysia, Hong Kong and Australia. Recognition of the method in these countries was increasing and it was being considered for inclusion in codes of practice including our own AS Piling Code.

Variation of CAPWAP results can be a function of the operator, and to demonstrate this he showed overhead transparencies which illustrated the variations in pile capacity obtained (for the same pile) by 18 different predictors. He also presented their predictions of the static resistance of the pile with depth, pointing out that although there was some variation in the predictions, the general shape of the distributions were usually similar. A CAPWAP analysis is also highly dependent on the data acquisition system used, he added.

Dr D. Williams (University of Queensland) asked "Was a static pile load test done in the comparison study?"

B. Chandler (Maunsell): Yes. The results fell within the range of CAPWAP results.

Mr Jim Millar (Frankipile Australia): Commenting on the presentation of Dynamic Pile Test results from all operators, Mr Millar said that clients are generally presented with a prediction of ultimate capacity whether this has been mobilised or not. He advocated the greater promotion of the use of load settlement curves so that clients can interpret test results in the same way that they would a static load test. Ultimate capacity should not be mentioned unless it has actually been mobilised.

Results are generally interpreted by structural engineers who are not used to dealing with highly variable results and who consider the results as acceptable or not without consideration of any variability. Along with other electronic testing, there has been an overselling of what Dynamic Testing can do. Dynamic Testing is a good system for the industry if used within its limitations.

Dr G Chapman (Wagstaff Piling): With reference to FDA testing of cast in place piles, the problem is knowing pile impedence (EA/c) for the shaft. Cast in place piles have greater irregularities in cross-section and modulus which are difficult to model.

Also there is often a problem with testing at high enough energy levels to mobilise full resistance without inducing unacceptably high tension stresses in the pile shaft.

However, I believe that the system can be used to proof load test, and to integrity test cast in place piles.

Mr P. McDonald (RCA Victoria): Two comments were offered by Peter McDonald:

1. Astoundingly good correlation between deflections based on SPT synthesised p-y curves (Goh & Lam) and load-deflection response of a pile may be obtained. Agreement was not as good for West Gate Freeway lateral load tests where self-boring pressuremeter tests were used to establish p-y curves.

2. In response to Tim Sinclair questioning the need for accurate lateral deflection predictions, he added that on West Gate Freeway project there was a need for accurate knowledge of lateral deflection, head rotation and pile moment, which had significant influence on the erection sequence for the precast segmented bridge units and also on the design of anchor piers to resist in-service lateral loads along the bridge.

Mr Slav Tchepak (Frankipile Aust.): Frankipile's experience to date indicates that PDA/CAPWAP appears to be more reliable for precast or preformed piles.

Dynamic testing of cast-in-situ piles, in particular, hammered shaft enlarged base Frankipiles often produced dynamic predictions very much different to static tests.

Similarly, poor results have been obtained to date for grout-injected piles.

The degree of modelling of a pile shaft, particularly one with an irregular shaft and base size, together with concrete strengths/moduli which may differ significantly, appears to produce considerable problems to CAPWAP modelling.

Results of a symposium in Brussels in 1987 indicated that a wide range of results were predicted for a number of pile types (Fundex, Atlas, Augercast, Precast concrete).

The conclusion drawn by Holeyman and his committee was that, although a lot of progress has been realised during the last few years, quite a lot of work is necessary to adjust the simulation models of the contact between the pile and soil. Thus, it seems still wise to actually carry out a calibration static load test to determine actual pile performance.

Dr Tony Phillips (Arup Geotechnics) made the following comments:

1. Pile tests at Darling Harbour were spread over a large number of contracts with different clients and different consultants and each needed to be satisfied individually.

2. PDA testing was carried out at Darling Harbour as part of a limited demonstration but it was not adopted by the project manager.

3. Driven cast in place piles have an irregular shape and it is understood that this creates problems of interpretation.

Mr Sam Plesiotis (RCA) asked "Are static loads necessary?"

1. We must ask whether the reported results are all class 'A' predictions, or were the static load test results known to the CAPWAP analyst.

2. The CAPWAP analysis is an interactive iterative process in which the operator assesses whether a satisfactory "match" has been obtained. There is some degree of judgement necessary and although generally there is agreement between different

operators there are still sometimes significant variations as reported by B. Fellenius at the 3rd Stress-Wave Conference (Ottawa, 1988).

3. The number of static load tests at a site can be dramatically reduced if a CAPWAP/static load test correlation is performed.

Mr W Flintoff (RCA Geotechnical Group): Commenting on the question of whether dynamic testing can be used on variable section cast in situ piles, Mr Flintoff said that experience with dynamic testing using CAPWAP techniques and a large hammer to mobilise the pile, on large diameter bored piles socketted into Melbourne mudstone, on the West Gate Freeway project in Melbourne showed the technique to be very effective in measuring capacity, settlement and integrity of these long piles. The piles were generally cased through soft sediments, then socketted into rock, loads of up to 30 MN being mobilised during the tests. Comparison of the technique with static load test results was reported by Balfe (1984) and Seidie and Plesiotis (1985).

Balfe B.J. (1984). Dynamic Testing of Piles Socketted into weak rock. Proc. 4th ANZ Conf. on Geomechanics (Perth).

Seidei J.S. and Plesiotis, S. (1985). Two Case Histories of Dynamic Testing of Piles. Proc. NAASRA Bridge Engineering Conference, Brisbane.

Mr Bruce Rodway (Aust. Construction Services): Is the PDA system inherently conservative? If it is then it may gain wider acceptance in design practices.

Mr Jim Millar (Frankipile Australia): Generally, the PDA system does provide a lower value than a static load test. More attention should be given to reporting the load-deflection curve as well as capacity in test reports. The PDA has much potential but its limitations should be recognised.

Q (2) Do we have an adequate degree of sophistication in representing soil behaviour?

Mr Tim Sinclair (Tonkin & Taylor N.Z.): For evaluating lateral pile deflection, we often do not need to go to sophisticated methods of analysis or testing. For small investigations, SPT's have a place.

The pressuremeter is the obvious test for obtaining soil behaviour for lateral pile analysis. However, my own personel experience has found agreement not to be as good in comparison with the cruder p-y approach.

Dr T. Phillips (Arup Geotechnics): On the subject of simple tests, the design of foundations for overhead electrification systems on railways requires high overturning moments to be carried in a wide range of soil types. Ideally simple tests are required at each stanchion location. Over design of standard foundations which will be built many times over is extremely costly.

Mr W. Flintoff (RCA) commented that it should always be remembered that the SPT produces index data, not absolute data, for pile design, and regard should be taken of local experience when designing using standard formula based on SPT results.

For example, SPT's taken in gravels in N.E. Victoria are often high, but this may be due to scale effects of large stones being impacted on by a similar sized SPT tube. Pile driving experience shows that the actual pile resistances are much less than those derived using methods such as Meyorhof's using the field SPT value as input.

SESSION D3 - FAILURE AS A YARDSTICK FOR PREDICTION ABILITY

Chairman	Prof. M. Pender (University of Auckland)
Secretary	Mr P. Andrews (D.J. Douglas & Partners Pty Ltd)
General Reporter	Mr J. Galloway (Consultant)
Panellists	Dr D. Andrews (Dames & Moore) Dr L. Wesley (University of Auckland) Dr S. Priest (University of Adelaide) Mr A. Olsen (Worley Consultants Ltd, N.Z.)

Mr Galloway summarised his report on the six papers presented at the Conference on geotechnical failures (ref. p. 557 of Conference Notes). In his opinion there are three main categories of failure:

(a) Overt or Conspicuous Failures - due to a tendency to focus on the aspect of blame, these are not effective yardsticks for day to day use.

(b) Secret Failures - even if these failures are eventually revealed, their use is limited as the reasons for secrecy usually overshadow the geotechnical factors.

(c) Day to Day Events - which do not necessarily lead to collapse or overt failure - it is the information to be gained from these events which is the most important. It is unfortunate that it is usually "in-house" and often not publicly available.

Three questions were proposed to the panel and the audience -

Q (1) How can we ensure that the day-to-day failures of prediction that we are all subject to become available to the profession as neutral technical information? Is our individual reluctance to expose our feet of clay a greater hindrance than our keenness to feel smug about the misfortunes of others?

A. Olsen (Worley Consultants): Information regarding failures, whether minor or major, is often not available until technical and/or legal enquiries have been completed. Even then, there is often reluctance, professionally or personally, by those closely involved to disclose reasons for the failure. Fortunately this reluctance often diminishes with time.

Dr S. Priest (University of Adelaide): Publication of case histories in the literature and seminars are presently the most useful source of information, but these often contain poor geotechnical information.

Expert data base and retrieval systems need to be developed.

L. Wesley (University of Auckland): Cost over-runs are often considered by a client as a fault. These are not necessarily a technical fault, but are often an administrative fault.

There is also the case of the reverse fault, for example, where a gross overestimate of settlement may result in higher foundation costs than necessary.

D. Andrews (Dames & Moore): Owners are often wary of releasing sensitive data relating to failures. The recent Rankine Field case is an exception to be applauded.

A problem on the majority of projects is the lack of funds and opportunity to properly monitor performance. It is often the case that sufficient funds are available for an investigation only when you are looking into the causes of someone else's failures.

But are geotechnical engineers really in the business of prediction? Or should they be?

J. Galloway (Consultant): What is a 'failure'? Except in the obvious cases the term is difficult to define. The failure of accuracy of a performance prediction is not necessarily a (public) failure. Quite often the design function appears to be 10% effort making sure that a building stands up, and 90% making sure that it does not fall down.

From the Audience

Mr C. Newton (Works & Services Devel. Corp.) suggested that it could be a function of the professional bodies to catalogue and record failure data and maintain a data base for future reference.

This theme was continued by Professor E. Brown (University of Queensland), pointing out that considerable success has been achieved recently in a limited area by the Commission on Rock Failure Mechanisms around Underground Openings of the International Society for Rock Mechanics. Success in such voluntary, cooperative undertakings depends very much on the enthusiasm and persuasive skills of a number of key individuals. The President of the ISRM Commission has been most successful in setting up productive Working Groups in a number of countries, including Australia. These enthusiasts have overcome the expected initial reluctance to make data on failures available, and have begun to publish reports that will be of great value to the profession in an area which is more limited than, but which forms part of, the topic of this Session.

D. Jennings (Works Consultancy Services) made the point that all engineering judgements require predictions of strength, of deflection etc. Predictions are unavoidable to ensure that the performance of the structure meets design criteria.

Dr D. Elder (Soils & Foundations Ltd) agreed with the panel that failures are expensive and that so too is excessive conservatism. He recalled a talk regarding a state highway extension in the U.S.A., involving about 100 km of road embankment over soft ground. The client 'expected' that local failures would occur as an indication that the design was not excessively conservative.

Q (2) How do the precision of mathematical analyses and the rigidities of fundamental concepts compare with the rather fuzzy information about the geotechnical context that is available. Should we replace the term "factor of safety" with one such as "margin for uncertainty"? And what margin should be allowed in predicting performance from the results of analysis?

When introducing the second question Mr Galloway proposed that engineers must not accept the responsibilities of the owner, nor his risks. Surely we are the servants of society, not its guides.

A. Olsen (Worley Consultants): Our ability to analyse a situation (number crunching) far outweighs the quality of data that we can input and far outweighs the practitioner's need for accuracy, given the extremes of variability of ground conditions on many sites, especially in a geologically young country such as N.Z. Mathematical analyses are certainly essential, but more emphasis should be given to back analysis or sensitivity analyses, rather than obtaining a definitive answer after using a limited amount of sometimes doubtful data. Often the use of a substantial amount of simple or crude data is more useful that a very limited amount of precision or high class data. Often it is more economical to obtain more data of the simple type than less data of the precision type and this is important to the practising engineer and to the owner of the project.

As to margin of uncertainty, we are stuck with the concept of factor of safety to some extent. The margin to be used in any particular practical situation depends on the amount and quality of data available and the consequences of failure. There is no one answer.

Dr S. Priest (University of Adelaide): In many situations, such as slope analyses, it is extremely difficult to assign 'factor of safety' concepts. In these cases, probablistic methods should be more widely practised than they are at present. The probabalistic statement should be given to the client to show him the level of risk of a particular course of action. It will often lead to more investigation work being carried out and/or monitoring of the work to reduce the risk of failure. The safety margin can be related to the cost of failure. The probabalistic statement will allow the client to make an informed judgement and accept the risk.

Dr L. Wesley (University of Auckland): Many problems are not amenable to numerical analysis, for example, the cracking of liners in water retaining structures. In these cases it is more important to determine the correct failure mechanism.

D. Andrews (Dames & Moore): The quality of data obtainable in an investigation is often the limiting factor. The client should be made aware of the risks and costs of not obtaining accurate data. A probabality analysis may assist in this. The safety margin would depend upon the judgement of the engineer, considering the consequences of failure.

From the Audience

Professor E. Brown (University of Queensland) felt that, as technical advisers, we have a professional responsibility to reach our own conclusions and to give the best advice of which we are capable. The owner is free to accept our advice or not, to make his own decisions and accept the consequences. Nevertheless, we are not free of the responsibility to make recommendations to him about the risks which are considered to be involved in various alternatives.

Professor M. Pender (University of Auckland) agreed with **S. Priest** that probabalistic approaches to the design of slopes and foundations are intellectually most appealing. However, we need more data – not only best estimates of properties, but also scatter of data for variance etc. There is rarely enough testing at a given site. There may be some hope if data is shared among sites in a given region.

B. North (A contributer from the floor) said that geologists often carry out investigations and provide some input into models hard to 'convert into numbers', and hard to communicate to design engineers if trying to describe more than average properties. Regarding factor of safety, why not adopt a factor of safety with an error range due to the data? Engineers are usually able to select design data, and with little difficulty could select a data range and hence get a feel for the error range in the factor of safety.

Professor H. Poulos (University of Sydney) referred to S. Priest's statements on the desirability of use of probabalistic methods, and made the comments that:

. Those who carry out research in probabalistic methods make it too complex for practising engineers.

. There is a lack of appreciation of what type of data is required – mean, standard deviation, variance, etc.

. Lack of appropriate software to do the probabalistic analysis. This should be viewed, for example in a similar way to equation solvers, as a tool for calculation which can be reliably used by the practising engineer.

Dr S. Priest (University of Adelaide) in reply, agreed that:

. the 'aura of complexity' fostered by some statisticians must be resisted,

. that engineers do need to know more about the methods,

. the majority of the software is private and that there is a need for more commercial software to be developed.

C. Newton (Works & Services Devel. Corp.) felt that there was too much emphasis on the use of computers and not enough on judgement in recent times. In the field of geomechanics, the crux to problem solving is not the mathematical model used or input values, but to correctly identify the mechanism. This was shown in the culvert prediction discussed earlier at this Conference, where a simple calculation based on the correct mechanism gave better results than the use of finite element methods. The question of pooling data is a good concept. Unfortunately clients pay the fees and therefore they own the data. In the increasingly commercially orientated world, clients are not prepared to make this data available to the community without reward or compensation.

J. Hodgson (Consultant) commented that modern trends in structural engineering are away from "factors of safety" towards limit state design, load factors and "fudge" factors. Geotechnical consultants have led the way, as they have always used limit state design and fudge factors.

A. Nelson (Murray North Ltd) discussed the large number of failures on small projects such as houses. Are engineers giving the man in the street the information on risks in a form which he can understand, and so be able to make an informed judgement? Are we providing a professional service by not giving a risk assessment of say 1% or 5%? Would the public understand such assessments?

On this last topic the concensus of the panel was that it is often not possible to quantify the risk, for example in stating "70% risk" of failure rather than a "high risk". Matters such as this can be called failures of communication, and these can often be the greatest failures of all.

Q (3) What techniques are available for the identification and correlation of thin defects or weak layers in geological formations and how can their significance on a large scale be better recognised in specific cases?

Dr S. Priest (University of Adelaide): Current investigation techniques cannot be expected to find all weaknesses. Remote sensing systems need to be developed for use in areas of known or potential weakness.

A. Olsen (Worley Consultants): The best techniques for identifying thin defects or weak layers are inspection pits or trenches for shallow situations, or shafts and adits elsewhere. Seismic techniques are also occasionally applicable. In the case of the Abbotsford failure reported in this session, the culprit was a bedding plane some 20 mm thick and at a depth of 25 m. Even the most confident of our profession is unlikely to say that his investigation programme would have located this layer and recognized its significance, given that the site was merely a residential subdivision.

The Chairman was reluctantly forced to close the session at this point as time had run out. This was unfortunate as this last question looked to be leading to the most interesting discussions.