

**PROCEEDINGS OF THE
FIRST AUSTRALIA-NEW ZEALAND CONFERENCE
ON GEOMECHANICS**

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Australian Geomechanics Society

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Preface

The Proceedings of the First Australia-New Zealand Conference on Geomechanics have been published in two Volumes. Volume 1 contains the 63 papers presented at the Technical Sessions of the Conference. Volume 2 contains the General Reporters' reviews of the papers, the discussion on the papers, and the six addresses to the Symposium on "Geomechanics - a Tool in National Development", which was held on the opening day of the Conference.

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The Role of Geomechanics in Water Storage Projects

G. T. COLEBATCH

(Hydro-Electric Commission, Tasmania)

1. Dam structures and reservoirs are responsible for the largest man-made loads on the earth's crust. These loads cause settlement and displacement of the dam foundations and of the valley walls of the reservoir and it is most important that these deformations be predicted.

Geomechanics is the science we use to help us in these predictions. It combines the two earlier sciences of soil mechanics and rock mechanics and, in a wider sense, involves other sciences - such as geology, geophysics, seismology, hydrology and chemistry.

2. I will refer now briefly to some major failures which have occurred to water storages in the last 12 years. These will serve to illustrate the utmost importance of using the most modern investigation techniques available to ensure adequate knowledge of foundations of dams and of the hillsides of the storage.

On 2nd December, 1959, France's Malpasset Dam, a thin concrete arch structure, 218 ft. high, failed suddenly with a full reservoir of 7,300 million gallons. 396 lives were lost and property damage was estimated at about \$50 million when an enormous flood passed down the valley. It was concluded that the poor rock condition of the left bank had not been sufficiently explored below the ground and the results of studies made had not been properly assessed.

Turning to a still more recent failure, in U.S.A. the Baldwin Hills Reservoir in Los Angeles, California, an earthfill structure, 160 ft. high and containing 293 million gallons, failed with only a few hours warning, on Sunday, 14th December, 1963. As a result of emergency measures taken promptly the reservoir was partly dewatered before the breach occurred and only five lives were lost. Property damage however approximated \$10 million. The failure resulted from a combination of settling of the land and of major movements along a known tectonic weakness (i.e. a fault) in the rocks. The settling caused water to leak through the asphaltic lining of the reservoir and this leakage quickly eroded some of the underlying materials. The leakage went undetected until a few hours before the collapse of a section of the retaining wall when all the remaining 40% of the water cascaded out of the reservoir into the largely evacuated metropolitan area.

It is clear from these two catastrophies that joints and faults and the general behaviour of the

foundations under the dam wall itself and in the abutments must be explored to see if critical conditions exist. A design engineer cannot accept any weakness in foundations which might lead to such failures - unless the weaknesses are adequately strengthened. And it must not be forgotten that most rocks are weaker when saturated by water.

Failures can also occur starting from a small seepage, which develops into a serious leakage through the foundations of a dam site which have not been made sufficiently impermeable to water under reservoir pressure.

As one example of this type of failure I refer to the Mequinenza Dam in Spain, a 259-ft. high concrete gravity structure. During construction it was found from in-situ shearing tests that the factor of safety against possible sliding was insufficient. It was necessary to completely re-design the spillway section, adding some 80 ft. in length to avoid the danger of the whole section sliding downstream. And this was not all. After the dam was completed, the reservoir filled and the much needed power station operating, water was found to be seeping through layers of lignite and marl rock on the right hand side of the dam. Gradually the leakage increased until the flow was about 500 cusecs (about 250 million gallons per day) equivalent to the daily water supply to Melbourne. A possible major washout of the abutment threatened the Mequinenza village below. By draining the large reservoir of most of its 1,239,000 ac. ft. of storage a serious failure with possible loss of life was avoided.

When the faulty right abutment was excavated a horizontal hole or slot about two feet deep and twenty feet wide was found. The weak rock had been completely eroded. The cost of the subsequent repair work ran into millions of dollars.

A second example occurred in Greece where the 90-ft. high Kremasta earth dam was built to supply water to three important industries. When heavy spring rains came with good runoffs into the reservoir it was found that the water seeped out as fast as it came in. No worthwhile storage has resulted since that date. The problem here was that there were horizontal beds of conglomerate in impervious silt stones. Undetected solution channels on the bottom of the conglomerate zones permitted serious leakage. Large sums have been expended over many years in a largely unsuccessful effort to prevent the leakage.

These two examples show that in addition to being strong enough to carry the considerable loads placed on them, the foundations around a dam must be reasonably watertight, or be made so by injection of cement grout or by other means.

Let us look now at the adequacy of the storage area away from the dam structure, where the valley walls become inundated and the stability of slopes may lessen as a consequence.

On 9th October, 1963, the most tragic water storage disaster in history occurred when the Vaiont Dam in Italy, a thin concrete arch structure 858 ft. high, was overtopped by a huge wave of water caused by a massive landslide into the reservoir. It has been estimated that the volume of the material in the landslide was more than 250 million cu. yd. - at least 20 times the volume of the Eildon Dam - and that the wave of water resulting from the slide rose nearly 500 ft. over the crest of the dam. Yet the dam itself did not fail.

Nearly 3,000 lives were lost when a major village was flooded downstream. Although a landslide had been expected the quite extensive exploration studies carried out over several years on the soil and rock conditions in the unstable area were insufficient to disclose the magnitude and suddenness of the slide which finally occurred. Clearly the stability of slopes in the valley of the reservoir must be assured before a storage is constructed.

By definition a water storage must hold water. Yet 40 years ago, in Spain, a beautiful arch concrete dam was built in karstic limestone country. It now stands silently with its reservoir always empty.

More recently in southern Turkey, the May Dam - an earthfill structure just under 100 ft. high - was built to hold 3,500 ac. ft. (950 million gallons). Water storage commenced in 1960 and after a short time several sink holes appeared. When filled with impervious material others appeared. After some five years of studies the authority reluctantly concluded that the May reservoir could be used only for flood control and not for useful storage.

3. While such major failures as I have described are infrequent there are many other cases where weaknesses of a serious nature have been detected but fortunately remedied in time to avoid a possible catastrophe.

There are over 10,000 large dams, i.e. dams over 50 ft. high, known to be in operation in the world today. In 1968 an analysis of 650 of these dams, all of which had a weakness considered to be serious, showed that over 40% of the defects had occurred in the foundations on which the structures were supported. It is quite clear therefore that site explorations, tests and analyses of foundation materials play a major part - often the most important part - in the safety and stability of a dam.

4. How then does the engineer - assisted by specialists - ensure that the foundation materials of a dam will safely withstand the loads to be placed on them?

A great deal has been learnt in this century through the sciences of soil and rock mechanics. It is now relatively easy to win soil and rock

materials from quarries and borrow-pits and to sort and process them satisfactorily for placing in a dam in a predetermined manner. These materials can be readily inspected above ground, sampled, tested and analysed for strength and other important characteristics. Using accepted design formulae and proven construction techniques, very large structures of earth and rock can now be built with confidence - provided that we have first proved the competence and stability of the foundations and valley walls.

Considering now the rock and soil under the dam, where they exist in their natural state, there are special problems to overcome. Rock and soil underground are subject to all sorts of natural variations due to their geological history. Exploration and study of these materials "in situ", i.e. without disturbing their natural state, is much more difficult, and therefore most costly, than a study of the same materials removed from the ground or of man-made materials such as concrete or reinforced concrete. This is mainly why the subject of geomechanics has not developed to the same extent as the sciences applicable to man-made materials. Although there is yet a long way to go substantial progress is now being made in geomechanics as is evident from the papers submitted to this Conference.

5. To give a picture of the surface and subsurface exploration considered necessary before an important dam can be designed and built with confidence, I will comment on a few dams of different types where modern techniques have been or are being applied.

Naturally the extent of geomechanic studies will vary greatly with the size of structure and storage and the quality of the foundations.

Considering first concrete arch dams, the Hydro-Electric Commission in Tasmania is currently building an arch structure 460 ft. high in a deep quartzite gorge on the Gordon River as part of the Gordon River Power Development. At the site are several major faults, joints and considerable folding and great care had to be exercised in siting the structure. Up to date exploration techniques have been employed not only to ensure safety but also to obtain the minimum cost of construction at the site selected.

To obtain preliminary information about the foundations, the surfaces of the dam abutments were cleaned off by sluicing with high pressure water jets to permit detailed surface geological inspection. Sluicing has become a standard practice of the Hydro-Electric Commission. About 100 diamond drill holes - totalling four miles in length were bored with diamond drills, the rock cores being recovered, stored and closely examined. Seven small horizontal tunnels - adits - were excavated - a mile in total length - and the rock exposed in drill cores and adits was tested in various ways to determine its strength and other characteristics so that engineers could assess what loads and stresses the foundations would withstand. Geologists recorded all information and kept up to date a perspex model showing the location of drill holes, adits, faults and joints etc.

Water tests in drill holes also indicated the permeability of the foundations. Information was obtained on the amount, quality and pressure of water which is always present to some extent underground. From such data assessments can be made of what areas

underground should be drained (to reduce water pressure) and what areas must be plugged or grouted to ensure adequate watertightness.

With the knowledge gained on the strength and other properties of the rock, the whole design of the dam and foundations considered together was checked by several models and different methods and appropriate adjustments made to the shape of the dam, its thickness and location until an optimum solution had been obtained from the viewpoint of safety and cost. Even then, as construction has proceeded adits have been extended and more holes drilled so that the most up to date knowledge is available before the final details of the concrete arch are settled.

A few years earlier similar exploration methods had been employed on the foundations of the Cethana dam - part of the Mersey-Forth Power Development in Northern Tasmania. Here, an arch structure, 360 ft. high, sited in quartzite and conglomerate rocks, was proposed and in fact had been virtually designed. As exploration proceeded, however, it became apparent that large blocks of rock in the foundations could slide out, theoretically at least, under the action of forces imposed by the dam. This study shed serious doubts on the economic practicability of constructing a concrete arch dam, with safety, on the site selected and, as a result, the design was changed completely and a rockfill dam was built instead. Subsequent exposure and examination of the foundation showed this to be a wise decision.

In U.S.A. the Auburn dam, a 685 ft. high arch structure with a crest 4,000 ft. long, is being designed with the most rigorous examination in foundation analyses ever made by the Bureau of Reclamation. The finite-element analysis has been complemented with a new "joint-shear index" technique - a method which can be used to evaluate any structural foundation. Discontinuities in the rock include joints, shears and major faults and these are all carefully studied, and their characteristics obtained. On a model embracing both the dam structure and its foundations different sections representing the varying foundations have been made proportional in strength to the actual rock underground. The influence of joints and shears are taken into account in the final analyses. The methods used are considered to be a major break-through in the evaluation of dam foundations. They are described in an article entitled "A new method for evaluating foundations" in Water Power, March, 1971.

With rock and earth fill dams poorer foundations, which would not be suitable for high concrete structures, can usually be accepted. However, problems associated with settlement or sliding or with pore pressure may necessitate special studies.

One of the highest fill dams in the world is nearing completion in British Columbia, Canada. The Mica dam will be about 800 ft. above the lowest foundation bedrock. Above this rock about 150 ft. of deep glacial and alluvial deposits exist. The bedrock of precambrian schists and gneiss has a number of minor faults but no major fault and is reasonably watertight. Nevertheless sub-surface exploration comprised:-

2 miles of seismic lines
73 holes drilled through alluvium up to

285 ft. deep
8 miles of drilling into rock
over 200 triaxial shear tests and a
multitude of others.

Of interest also in the Mica storage area are the extensive studies carried out with large models over several years to assess the risk of major rock slides into the storage. As a result the dam design was modified so that if a slide did occur wave action could not endanger lives or the structure itself.

I will mention only one other dam, originally called the Mission Dam, to indicate very briefly what can be achieved on very poor foundations by scientific study of foundations and to refer also to its brilliant designer Karl Terzaghi.

Located on the Bridge River, also in British Columbia, this dam is built on very permeable river gravels with a highly compressible thick clay stratum below. Terzaghi's design for this 200-ft. dam provided for a grout curtain to go down 500 ft. to bedrock in order to make the foundations reasonably watertight. To prevent water penetrating cracks expected to develop on the surface while the settlement was occurring, polyvinyl chloride sheeting was placed over an area of two acres. From the studies, he predicted accurately that a major settlement of the foundations would occur with a maximum of 11 ft.

That this dam is behaving so very closely to his prediction is a worthy tribute to Dr. Terzaghi. It was the most difficult and daring project of his career and understandably, the dam was re-named the Terzaghi Dam.

7. From the first examples that I gave of dams and storages which failed in one way or another, lessons have undoubtedly been learnt - but at an unacceptably high cost in lives and property damage.

In Australia we have been fortunate that in recent years no large dam has failed with loss of life. We cannot afford to be complacent about this because it is known that serious defects - now remedied or being remedied - have occurred to a number of large dams. But for the remedial work these defects might well have led to serious deterioration and in some cases, failure. Moreover it is recorded also that many smaller dams have failed completely due to lack of knowledge of the foundations and how they will behave under load and under water pressure.

From the descriptions I have given of foundation explorations carried out with modern dams, it must be apparent that the science of geomechanics is already playing a most important role in the building of safe dams and water storages.

Because the earth's crust and the foundations of a storage area change with time, however slowly, it is most important with large dams to monitor the changes in settlements, movements, leakages, pressures etc. after the structures are completed and throughout their lives. Geomechanics can also help significantly in interpreting the results of these measurements.

Australia will require many more water storages in the years ahead for various purposes. Unless we apply the continually improving techniques for

foundation exploration to design of water storages, hazards to life and property will tend to increase further because:-

- (i) as population increases, larger storages and higher dams are needed;
- (ii) more storages are built in and above populated districts;
- (iii) less favourable dam sites are available.

The methods available are now scientific and highly sophisticated and can only be employed effectively by engineers, geologists and other specialists who are experienced in the application and analysis of geomechanics as we know it today.

While all will agree on the safety aspect it may not be so apparent to the layman or to the administrator that it is economical to meet the cost of intensive exploration of the foundations of water

storage areas before a project is committed. Normally this cost will be of the order of 2% of the total cost of the structure - most surely a worthwhile insurance. The worse the foundation conditions the more justification there is for testing them thoroughly.

Administrators and engineers alike have a fundamental responsibility for public safety and economy. My concern is that the need for foundation studies, in depth, is not yet appreciated by many persons responsible for the decisions to build quite large structures. My hope is that this and other similar conferences will improve communications between the specialists primarily responsible for safety and the administrators concerned primarily with the cost. Not until the value of thorough investigations is understood and appreciated will the full benefit of the science of geomechanics be obtained and the country be blessed with safe and economical water storages in the future.

Transportation — Application of Geomechanics to Roads and Bridges

D. H. AITKEN

(Commissioner of Main Roads, Western Australia)

1. Geomechanics, which is the comparatively new science of soil and rock mechanics, is playing a significant and increasingly important role in the development of transportation in Australia, and particularly of roads. It is true, of course, that roads have been successfully built without the aid of such scientific methodology since the days of the Roman Empire. The Romans realised the importance of good communication in their quest to conquer new territories, and the greatness of their empire was due largely to the excellence of their land and water transport systems. Those early road builders were fortunate in that labour and materials were plentiful, and consequently their roads were seemingly built to last forever, so much so that many are still capable of carrying present day traffic. By today's standards, of course, their roads were very much over-designed. Today we must be much more rational in our design of road pavements.

2. With limited resources we are faced in Australia with the task of providing an expanding road system to meet the needs of a steadily increasing population with a steeply rising rate of vehicle ownership. Although the opening up of new agricultural areas, to which much of our road building efforts have been directed in the past, has virtually ceased, new basic developmental work must continue in order to service the expanding needs of a mineral boom which is taking place in many remote areas, and at the same time provide for the backlog of road needs in our capital cities. Geomechanics is a most important tool which is helping to make this formidable task possible. By providing a more basic understanding of the characteristics and behaviour of soils and road materials encountered in road and bridge foundations geomechanics has made it possible to achieve economies in design and improve construction methods.

3. The multi-layered flexible road pavement system is a complex structure made up of materials whose engineering properties themselves vary and are very much affected by the loading to which they are subjected, and also by environmental conditions. Despite our vastly increasing knowledge, present methods of pavement design are largely empirical. Considerable research effort is being applied both overseas and here in Australia by the Australian Road Research Board, universities and others to develop a better understanding of the behaviour of pavements and the soils underlying them. The aim is to be able to design a road pavement in a similar manner to that used in other engineering structures

whereby stresses and deformations at critical points may be limited to acceptable values. While this aim is unlikely to be achieved in full in the foreseeable future it is a fact that any significant increase in our knowledge in this field of geomechanics will have a considerable impact on road and airfield engineering, and might well lead to a considerable saving in national investment.

4. A most important application of geomechanics in connection with road pavements is its use in determining the limitations to be placed on wheel and axle loads and overall vehicle weights. Such limitations have nation-wide economic implications. While increased axle loading could result in significant savings in transport costs any saving must be offset against the increased cost to the community of new roads and the strengthening of existing roads and bridges. The member authorities of the National Association of Australian State Road Authorities are at present conducting a survey in each State to determine the axle load distribution on the Australian road system.

5. From these few remarks on the application of geomechanics it can be seen that this science has become an important tool in highway engineering. Besides the uses mentioned it is used to assess varying pavement materials sometimes involving alternative methods of construction, and it is also used in bridge foundations. In recent years the theory of consolidation, originally propounded by the founder of soil mechanics, Terzaghi, has been extensively used in highway projects. This theory enables the amount and time of settlement to be determined for each increment of load. A spectacular achievement in this application of soil mechanics theory by the Main Roads Department of Western Australia has been the Narrows Interchange which forms part of the Mitchell Freeway project in Perth. There are many features of this project which are believed to be unique in Australia and New Zealand.

6. Urban freeways, and more particularly interchanges, have an almost insatiable demand for land which in developed areas is very expensive, so it is often necessary to use areas which were previously avoided because of poor foundations. Today the cost of strengthening the soil or of providing complex foundations in these areas is usually much less than the cost of acquiring already developed land. The land reclamation project on the Swan River estuary for the Narrows Interchange in Perth is an interesting

example of this.

7. A complex traffic interchange involving freeways and important arterial roads at the south-west corner of the city near the Narrows Bridge required an overall area of about 90 acres which includes an area for a city car park. In order to avoid resumption of heavily built-up areas and adjacent parkland it was decided to reclaim this area from a shallow and somewhat stagnant area of the river where there was an underlying very soft organic silty clay layer, varying between 40 ft. and 100 ft. deep. The mud layer was so soft in its original state that many of the cylindrical samples almost collapsed under their own weight. After blanketing the area with a few feet of dredged sand the maximum height of embankment which could be built in this situation without failure was less than 7 ft., yet it was necessary to contemplate an embankment height of over 40 ft. to accommodate a third level bridge. Had it been possible to gradually add more filling over a period of 100 years or so the area could have been stabilised without any special measures being taken. A maximum settlement of 26 ft. would have occurred in the mud layer.

8. The fact that embankments under these conditions would most certainly have been avoided not so many years ago gives a meaningful indication of the advances which have been made in geomechanics in recent times. Even at the planning stage ten years ago the problem of constructing stable embankments in this area was considered unrealistic by many engineers who would have preferred to build the whole interchange in structure.

9. However, building the interchange in structure was not favoured because of higher cost and the fact that it would inevitably create a concrete jungle. By contrast, embankments could be landscaped to give a spacious park-like appearance. In fact, I believe it is of interest to note that the whole of the interchange is being developed as a park with two man-made lakes, a fountain and extensive pedestrian walkways accessible via special subways and bridges. This feature is possibly unique in the world for a traffic interchange of this magnitude.

10. Tests were carried out in 1962 and a feasibility study undertaken to determine if it were possible to stabilise the area and strengthen the soil in the time available before construction of the interchange was required to start. Advanced geomechanics theory was applied to these studies which, backed by large-scale drainage and failure tests, confirmed that it was feasible to construct the embankments and that the post-construction settlement could be reduced so that they could safely carry the heavily-trafficked roads of the interchange.

11. 43,000 sand drains were sunk in staggered rows. These were 18" in diameter and had an average depth of 58 ft. The total length of sand drains was about 500 miles. They were spaced between 5 ft. and 10 ft. apart, depending on the height of the embankments and the time available for consolidation. Four million cubic yards of sand filling was imported from 11 miles away by truck and placed over the reclaimed area. The weight of the sand filling in effect squeezes moisture out of the mud into the sand drains. This is accompanied by settlement and an increase in the strength of the mud. Surcharge loading, i.e. filling in

addition to that provided by the embankments, was used to accelerate the process.

12. Geomechanics theory was constantly applied during the course of construction. Various measurements were made using sophisticated instruments to determine the effectiveness of the operation and to control the rate of loading to avoid construction failures. The study predictions were largely borne out in practice and the embankments completed within the time allowed. Total depth of fill including up to 15 ft. of superload exceeded 70 ft. in some places with a settlement of up to 23 ft. taking place. The 60-ft. mud layer was reduced to approximately two-thirds of its original thickness.

13. The whole reclamation project has proved a most economic exercise. Land for the interchange has been obtained at a cost of about \$52,000 per acre, which is very much less than city property values.

14. Geomechanics theory has continued to be applied to the unusual problems encountered in the design and construction of the foundations for the five bridges in the interchange.

15. Although most of the expected settlement had taken place before the bridge structures were commenced, residual settlement of 2 ft. or more is expected to occur over a long period. Such settlement will cause significant horizontal movements within the clay stratum which could cause large unacceptable stresses in piles supporting the bridge piers. The solution finally adopted after a study of alternatives was to build the pier foundation within a concrete caisson and provide an annular space between them. The estimation of the horizontal movements to be designed for was a difficult problem on which only limited work had been done previously.

16. A unique feature of the caissons was that they were built in segments each of which could slide horizontally with respect to adjacent segments. This was to accommodate differential horizontal movements at various depths. Each segment was 12 ft. high and was separated from the adjacent segment by a one-inch-thick neoprene sliding joint.

17. The internal diameter of the caissons varied between 25 ft. and 30 ft. depending on the diameter of the pier shaft and the annular space varied between 9 inches and 32 inches. The wall thickness of up to 39 inches included 3 inches of sacrificial concrete to allow for predicted bacterial attack. The skin friction during sinking was reduced by the provision of a sleeve filled with bentonite slurry around the caisson.

18. Many difficult construction problems were encountered in the sinking of the caissons. These problems and their solution will be described in technical literature to be published later. Ten of the thirteen caissons have now been sunk to the required depth and three are nearing completion.

19. Geomechanics has played an important role in the development of the Australian road system. This is due not only to the diverse geological and climatic conditions which occur in this continent but also on account of compelling economic factors. In order to meet the heavy demands for roads in the rapidly

developing regions of Australia it has been necessary to develop and make maximum use of low-cost construction methods. Considerable success has been attained in achieving relatively low costs by the use of natural locally-occurring materials. These techniques made the construction of large mileages of road possible during the early development of the Australian road system at a time when funds were grossly inadequate. Besides extensive use on lightly-trafficked roads, these methods were also used on some important arterial roads. On the Hume Highway in Victoria, for instance, one of Australia's most heavily-trafficked rural roads, long sections of sealed loam and non-standard gravels which were built for light traffic have only recently been reconstructed after thirty years of satisfactory service. These are many similar examples in New South Wales, Western Australia and the other States.

20. Low-cost methods have certainly played a dominant role in the development of the Western Australian road system. Fortunately the earliest demands for roads in the beginning of the motor-car era were for agricultural development in the south-western corner of the State where very good lateritic gravels are readily available. Outside this area, however, normally acceptable roadmaking materials are scarce, and so-called substandard materials have had to be used. Satisfactory construction procedures have been established for these substandard materials following extensive laboratory investigations and field trials. It is of interest that for some years the Australian Road Research Board has been sponsoring a research project at the University of Western Australia to determine more realistic parameters for the selection of such base-course materials for use in low-rainfall areas.

21. The highway to Carnarvon, some 600 miles north of Perth on the coast, provides a good example of the use of mechanical stabilisation using local materials. The pavement of a 180-mile section of this road where there were no gravelly materials was constructed by stabilising the in-situ fine sands with silty clays and loams imported from nearby deposits. The maximum size of these materials was about the size of a pin head. This construction cost considerably less than the more usual methods of stabilisation using additives. The construction and sealing of the road was completed about ten years ago and has given satisfactory service. One still-unsolved problem which has been experienced with this type of construction is that the shoulders are susceptible to scour and are consequently difficult and expensive to maintain. The Department is continuing to investigate ways of overcoming this difficulty. Similar soil-stabilising techniques for low-cost road construction which have been developed and widely used over a long period of years have made an important contribution to the agricultural and pastoral development of the extensive light lands in the coastal regions of Western Australia from north of Port Hedland, more than 1,000 miles north of Perth, to Esperance situated on the south coast some 450 road miles from Perth.

22. An interesting example where geomechanics has been used as a tool in evaluating the economic feasibility of various forms of road treatment concerns the construction of the Western Australian section of the Eyre Highway — some 450 miles in length — between Norseman and the South Australian border.

The section between Balladonia and Cocklebiddy, a distance of 140 miles, was completely devoid of suitable gravels. The natural soils along this section are highly calcareous. They disintegrate to "bull-dust" when dry and are very sensitive to moisture, becoming boggy after even limited rain. An extensive laboratory testing programme on various alternative stabilisation treatments using bitumen, cement and lime, followed by field trials, was carried out. Cement and lime stabilisation proved most satisfactory, but because of the high cost of transporting these materials to this remote area they were discarded in favour of crushed rock base.

23. Another problem encountered on the Eyre Highway project arose from the need to use highly saline water for pavement and subgrade compaction. Normally such water is considered unsuitable. In this case, however, it was found that because of the relative absence of soil moisture movement due to the arid conditions, it was possible to use water with up to 2,000 grains of soluble salt per gallon without harmful effects, provided that the quantities of water used were closely controlled.

24. In Queensland, geomechanics is being used to develop more efficient methods of construction on the highly expansive black soils which are so prevalent in that State. In these areas conventional road-making materials are very scarce, making road development in these potentially highly productive areas most expensive.

25. The dramatic increase in car ownership and growing urbanisation in Australian cities has led to the adoption of large freeway and expressway programmes costing many millions of dollars in most States. Because of the very high cost of these facilities a high standard of construction, which would not be possible without controls based on geomechanics, is required. The Sydney-Newcastle expressway — one of the most exciting and sophisticated road projects in Australia, constructed by the Department of Main Roads, New South Wales — is a striking example. The expressway has been carved through rugged terrain requiring many large cuttings through sandstone. Some of these rock cuttings were up to 150 ft. deep, while the largest fill was 215 ft. high and measured 1.7 million cubic yards. The closely controlled compaction of these fills and the pre-splitting techniques for the rock cuttings are but two of the construction operations which have been greatly assisted by geomechanics.

26. Airfields have much in common with roads. The most important difference is the greater magnitude of aircraft wheel loads compared to those of trucks, and the fewer load repetitions which airfield pavements carry. The development of larger jet aircraft with the consequent need for heavier and longer runways constitutes a continuing and difficult problem for airfield engineers. The extension of the main north-south runway of the Kingsford Smith Airport is an interesting example of such a problem. The runway had to be lengthened by 3,000 ft. which extended it into Botany Bay. A tunnel had to be included in the project to carry an important six-lane highway that would otherwise have been cut off by the extension. The construction of the embankment which involved about $4\frac{1}{2}$ million cu. yards of sand dredged from Botany Bay, its protection against erosion, stability considerations together with

de-watering and site construction problems associated with the tunnel, were some of the problems which were overcome by the application of geomechanics.

27. Similar geomechanics problems are being encountered in the construction of a causeway $2\frac{1}{2}$ miles in length linking Garden Island with the mainland south of Fremantle in Western Australia. The causeway which will carry a 24-ft. road is being built by the Federal Government as part of a naval defence complex.

28. The basic problems involving geomechanics which are encountered on roads are also experienced on railways, one difference being that these problems are often more severe because the critical gradient requirements of railways result in considerably larger cuttings and fills and often traverse terrain which would be avoided for roads. In recent years Australia has experienced an active period of rail construction. First there were the rail standardisation programmes in Western Australia and South Australia, followed by several new railways in the Pilbara in Western Australia for the transport of iron ore. Geomechanics has been a valuable tool in these projects.

29. Australia's increasing trade and commerce necessitates frequent enlargement of its ports. As in other transport facilities which have been mentioned, geomechanics is again involved — particularly in the dredging of navigation channels and the foundations of wharf structures. The ports for the iron ore industry in the Pilbara in Western Australia are an interesting example. The viability of the industry depends on economic transport. Carriers of up to 150,000 tons which can be accommodated at existing ports are able to transport the ore economically to Japan. Export to more distant countries, however, will require carriers of up to 300,000 tons. Planning for a port at Legendre Island capable of handling these ships is being undertaken. The design of such a port will depend very heavily on geomechanics.

30. These few examples of the use of geomechanics give some indication of the importance of this comparatively new science in the construction of roads, railways, harbours and airports. Significantly, its importance has been increasing — for recent developments in massive earth-moving equipment have enhanced the importance of soil as an engineering material. Increased efficiency in earth-moving equipment has caused a dramatic shift in the balance between the cost of earth and rock compared to other types of structures. Thus today it is generally

more economical to build an earth embankment for a road or railway than an additional span on a bridge. Typically also the large Ord Dam in Western Australia is being constructed of crushed rock rather than concrete for largely economic reasons. This increased importance of soil as an engineering material is inducing much new research into geomechanics.

31. I should like to make some brief reference to the work of the Australian Road Research Board which is playing an important role in carrying out or promoting research in fields of geomechanics related to highway engineering. In addition to its own research the Board sponsors a significant amount of research at Australian universities.

32. One of the important tasks carried out by the Australian Road Research Board was the determination of those characteristics and physical features of the Australian environment which govern pavement behaviour. The most notable of these characteristics relate to soil moisture movements on which much of the earlier work was carried out by C.S.I.R.O., some of which was done in conjunction with NAASRA. ARRB has made a useful contribution in the study of structural analysis of layered pavements. Besides such theoretical work, a wide range of compaction studies and several field evaluation trials involving a variety of natural materials and dry compaction methods have been carried out in various parts of Australia. In developing non-destructive testing techniques the Board has developed a roughness meter for assessing the structural condition of roads. The Board has also made useful contributions in other branches of geomechanics. Studies were performed on various aspects of the support provided by soft sediments along the length of slender point bearing piles. The principles established by these studies have already been applied in the design of important road bridges and of building foundations.

33. In conclusion, I believe that of the many factors which have contributed to the remarkable growth and development that Australia has experienced since the war there is little doubt that the development of transportation — by road, rail, sea and air — has been one of the most significant. Geomechanics has been an important tool in making this development possible. By providing a basic understanding of soil characteristics and performance with which the pavements of roads and airfields, rail tracks, bridge foundations and harbours are so intimately connected it has made possible major improvements and economies in design and construction methods which have contributed very largely to more efficient transportation.

Geomechanics in the National Development of New Zealand

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The theme of this symposium is "Geomechanics - A Tool in National Development" and I was asked to talk specifically on "Earthquake Engineering and Geothermal Power". But, bearing in mind that the stated purpose of the symposium is "to help define the relatively new term Geomechanics in the mind of the profession and the public" I feel that such an approach is too narrow. I might have thrilled you with tales of earth-shaking powers and hellish energy; of Hephaestus forging his divine armour in the bowels of the earth; of Ruauumoko, the earth-shaker, cleaving the mountains. But in my view Geomechanics is much more than the awesome or spectacular. It underlies all man's economic development and is the literal and metaphorical foundation of every nation's wealth. Therefore I have chosen a much wider theme and attempted to give a review of the place of Geomechanics in New Zealand's national development; if it is the fundamental basis of national development where the national wealth is almost wholly tied to agriculture, how much more it must be the foundation of economic wealth in countries where mining and manufacturing provide the main occupations.

New Zealand has only two major resources, an equable climate with an abundant, well distributed rainfall and a fairly fertile soil. The whole history of the development of the country has revolved round the processing of these two major resources into saleable commodities, a development which to date has been based upon pastoral farming.

Following in the English yeoman tradition New Zealand agriculture developed as many separate holdings each farmed by the owner. There was no corporate farming, and corporate investment and development were largely limited to the freezing works, dairy factories and like facilities necessary to process the products of the land.

This almost total dependence on individual pastoral farmers has proved very profitable for New Zealand. A high standard of living was built up and advanced social legislation developed. But now that the terms of trade for pastoral products are changing the dangers of almost total dependence on a few animal products are evident and strenuous efforts are being made to diversify. This diversification is leading to corporate development of other ways of using the two major natural resources of New Zealand but to date these corporate projects have hardly begun to be significant and I doubt whether they will contribute more than a sizeable minority of the nation's exports by the end of the century. Certainly in the year 2000 climate will still be the basic

ingredient of our exports (after all, aluminium ingots and newsprint are only ways of packaging an abundant rainfall for sale!) and no likely development of manufacturing or discovery of mineral wealth will contribute more than a few percent of our exportable surplus.

The present high standard of living in New Zealand arose because not only did the farmers produce enough to support their families and continue to develop their land but a substantial surplus was available for investment in community and national development. National development started early because Central Government recognised the need and set up the Public Works Department to build roads, railways and public buildings. As the needs of national development grew so did the responsibilities of the P.W.D. to include irrigation, hydro-electric, water supply, airport and large-scale development projects till by the end of its first century the Department, now called the Ministry of Works, had become consulting civil engineer to Government. In this capacity it supervises about \$250,000,000 worth of works every year, the vast majority of which are designed and a sizeable proportion constructed by the Department. Thus the history of national development in New Zealand is largely the history of the Ministry of Works and it seems likely that the Department will dominate this sphere for many years to come. It is thus particularly appropriate that I, a civil engineer of that Department, should be discussing the place of Geomechanics in national development, and it is a happy coincidence that 1971 is the centennial year of the Department.

To a civil engineer like myself it is almost axiomatic to regard Geomechanics as the basis of national development. True I have spent the bulk of my career in the study and application of soil mechanics but apart from this almost everything done by the engineer uses the natural resources of the earth and relies upon the earth for support. To the layman this dependence may not be so obvious as even the names of the three main disciplines of Geomechanics - Soil Mechanics, Rock Mechanics and Engineering Geology - may not be familiar to him.

It is apparent that soil mechanics - the art of using the natural properties of soils to best structural advantage - must be of great importance in conceiving and building a large earth dam, and that the skills of engineering geology - the application of the knowledge of geology to engineering problems - are fundamental to assessing the water-tightness and

safety of the reservoir basin.

It is also fairly apparent that both rock mechanics - the art of using the structural properties of rocks to best advantage - and engineering geology are the basis on which tunnels and underground caverns are designed and constructed. Both these structures feature in hydro-power, mining and underground railway developments.

It should be obvious, but is often forgotten, that every building or structure must rest upon a foundation. The design of these foundations is one of the main pre-occupations of soil mechanics experts.

It is perhaps not so obvious that the development of geothermal power depends heavily on Geomechanics. Once the natural steam has been brought to the surface its conversion into electricity or use as a source of heat involve more or less routine problems of mechanical engineering. But the location of the steam-bearing strata and the proving of its extent are very much in the province of geology while the tapping of the steam reservoir and the bringing of the steam safely to the surface requires all the skill and experience of the oil well driller.

In the same way earthquake engineering depends heavily on Geomechanics. No matter how expert the structural engineers are in calculating the strength and safety of their structures they have to rely on the geomechanics disciplines to provide them with information about the foundations beneath those structures and the earthquakes they may be called upon to resist. The response of a structure to an earthquake depends not only on the structure itself but also on the properties of the foundation on which it sits. And the earthquake shaking felt at the site is itself greatly influenced by the nature of the foundation soils, the ground water level, the depth to rock and the nature and surface configuration of the rock itself.

Less obvious again is the role Geomechanics plays in our roads, railways and airports. All vehicles produce rolling loads on the earth beneath and soil mechanics helps clarify the way these loads are dispersed by the structure of the pavement so that they can be safely carried by the underlying natural soils. Certainly durable roads could be made without the help of Geomechanics but they would not be economical roads.

Least obvious of all perhaps is the influence of Geomechanics on our buildings. Concrete has become the sine-qua-non of modern civil engineering and concrete itself is basically an exercise in Geomechanics. The whole art of concrete design - and make no mistake good concrete is much more than a random mixture of stones, water and a grey powder which goes hard in a few days - is to produce the desired properties at minimum cost. Cement is the essential and most costly ingredient, but in terms of volume, only a minor one. The majority of the concrete consists of shingle and sand and the quality of the finished concrete depends greatly on the judicious selection and blending of these ingredients. Geological study of the minerals in both shingle and sand will indicate their likely strength and durability and show whether any unwanted reaction with other ingredients of the concrete or the reinforcing steel might arise. Rock mechanics plays a considerable part in

determining the most economical, effective and safe way of operating the quarries which produce the shingle and in predicting its likely quality and surface texture. The theories and practices of soil mechanics are called upon in determining the most economical blend of the available sand and shingle. This blend is usually the one in which the maximum amount of these relatively cheap materials is packed into a unit volume thus minimising the space which has to be filled with expensive cement. So even the home handyman who lays a concrete path to the clothesline is, unwittingly perhaps, making use of Geomechanics!

I am sure that what I have said about the fundamental position of Geomechanics in New Zealand's national development is quite typical of other countries too. That the task of national development is divided in different ways between Central Government, Local Government and private industry in different countries is a political fact, but this political arrangement only overlies the basic Geomechanics principles upon which man's economic development depends.

I hope I have now given an adequate picture of the fundamental position I believe Geomechanics occupies in national development and would like to move on to more specific treatments and to ride a few hobby horses of my own.

Firstly I wish to thank my Department for permission to attend this Conference and to mention some of its achievements in Geomechanics. Currently something like \$80,000,000 to \$100,000,000 of the work it supervises (and mostly builds) is directly in the sphere of geomechanics - roading, airport works, railway tunnels and hydro-power developments. Some of these are in world class for size and in a country as geologically complex as New Zealand, few have been simple. At present we are driving tunnels through thermally active ground, building a hydro-power development in the fringes of a volcanic rubbish tip, setting up a hard-rock tunnelling machine to drive a six-mile railway tunnel and have under design a dam founded on deep peri-glacial materials to raise a large glacial lake by 150 feet, all developments in the forefront of geomechanics technology. Quite a number of our dams and tunnels were, at the time of their building, high in the world lists of "greats", either for sheer size or because of geological complexity. For example there must be few dams in the world where the rock abutments are underlain by large depths of uncemented sediments yet we have several such. Perhaps this is what is meant by antipodean!

At present we have in hand a most interesting series of studies of the dynamic behaviour of buildings. These rely on the vibrations of wind, traffic etc. to excite the buildings and hence allow us to measure their natural frequencies in the elastic range. They show quite clearly there is normally a significant component of foundation compliance which will substantially modify behaviour in an earthquake. All modes of compliance can be present and there will be much interesting analysis to be done in relating site properties to measured compliances.

I would also like to be able to make claims for our current work in the field of geothermal power. Regrettably this is not a vintage year for this activity and at the moment our prospecting work is closing

down. The truth of the matter is that blocks of 100 to 200 electrical megawatts that could be developed from known steam fields are too small to be significant in the national electricity scene, but too large and remote to interest industry. There have been corporate proposals recently to use geothermal steam for sulphur mining but this development has not yet started.

This catalogue only includes some of the current works of geomechanics interest of the Ministry of Works and much else is under way not only in the public but also in the private sector. Much of this is the normal run of geomechanics - building foundations, slope stability, tunnels, dams, oil exploration, mining developments, roading - but one aspect that should be given further mention, to give it due place in the New Zealand scene, is earthquake studies. Rightly earthquakes are a cause of continuing concern and occupy a good deal of our thinking. But it is only fair to point out that the total loss of life in New Zealand as a result of earthquakes since European settlement began is less than one year's road toll. In fact the statistical risk is so slight that it does not equal that of accidental drowning or compensate for the small difference between the Australian and New Zealand road death rates. But it is only by continual vigilance that this record can be maintained and there is economic loss as well as loss of life to be considered. Two ways in which this vigilance is being maintained are the studies in the dynamic strength properties of soils and the vibrational characteristics of foundation strata at present in hand in our Universities and the seismic microzoning being undertaken by the Department of Scientific and Industrial Research (Ref. 1). Seismic Microzoning is a technique in which all the factors which influence the ground shaking liable to be experienced at a point on the earth's surface are assessed so that a rating of the risk can be made. Involving soil mechanics, rock mechanics, geology, seismology, and geophysics, seismic microzoning is of the very essence of Geomechanics.

Secondly I wish to talk in my capacity of Chairman of the N.Z. National Society for Soil Mechanics and Foundation Engineering. In this capacity I wish to thank the Society for the honour they have done me in electing me Chairman and for the confidence they have shown in nominating me to contribute to this Symposium.

For years soil mechanics in New Zealand was haunted by the spectre of "overseas standards". These standards had been developed against a background utterly different from ours and when applied here in New Zealand were quite inappropriate. What could one make of a material whose optimum moisture content changed with the moisture content at which the test started or whose density lay above the zero air voids line? Or of a clay whose "secondary" consolidation vastly exceeded its "primary" consolidation? Precious little in terms of "classical" soil mechanics and as such materials continued to turn up with alarming regularity one became a little sceptical of the worth of soil mechanics. It was something of a relief when Terzaghi published his paper on the Sasamua dam to find that many of the contributors to the discussion had run across oddities similar to ours and that, therefore, we were not alone in our troubles. I hope we have now got past the stage of slavish dependence on overseas standards but as we have not

made as much progress in publishing standards of our own as I would like to see, we may not yet be out of the wood. But one lesson I hope we have learned is that there is not enough communication of ideas at an informal level. We wait too long attempting to produce the "perfect" publication with all the loose ends sewn up when our ideas, imperfectly formed though they be, might be of immediate use to others.

In an attempt to remedy this lack of communication the N.Z. National Committee has recently expanded its newsletter and given it the title of N.Z. Geomechanics News. This will appear from time to time as material accumulates and is intended primarily as an informal forum in which society members can try out ideas, do a bit of coat trailing, offer opinions and generally discuss their experiences. This, we are sure, can only benefit Geomechanics as will other attempts to bring about a genuine exchange of ideas. One such is the recent formation of the Australian Geomechanics Society to whom we offer our congratulations and hope shortly to emulate.

But I have rather strayed from the theme of this Symposium and what real use is mere talk of co-operation and communication? I therefore now will endeavour to show that informal co-operation in Geomechanics between disciplines and countries already exists and that there are good reasons why it should continue. I will do this by quoting a few case histories of which I have personal knowledge.

My first case refers to a recent tentative Australian standard for measuring the in-situ density of soil (Ref. 2). About 17 years ago I was involved in the construction supervision of an earth dam and one of the things we happened to come across was an article in a magazine describing the Washington "Dens-o-meter". The device looked viable so with the help of the plumbers' shop we made up something on the same lines from an assortment of scrap. The thing worked, (though we found out later when fuller details of the Washington device appeared that we had subtly altered the principle of operation), and I can remember showing it off with pride to my friends in the Snowy Mountains Authority when they visited us. During succeeding jobs our version of the device developed from an "ad-hoc" collection of bits into a fully manufactured article while our understanding of the philosophy of use deepened so that we became aware of the substantial change for the better involved in our originally unconscious modification of the operating principle. Meanwhile the S.M.A. people had built a device of their own based on our original drawings and had good success. By the time they were prepared to accept it as a really useful tool our formal development programme was complete and we were able to offer them full working drawings and operating instructions which, I believe, are virtually indistinguishable from those in the new standard.

Thus an idea originated in U.S.A., travelled across the Pacific to New Zealand (suffering a sea-change en route), developed there for a time and finally appears in an Australian standard. Surely an excellent example of international co-operation, mostly done through the "old-boy network".

My second example concerns a problem in road stone durability. Over the years we had found that in some parts of New Zealand there was a strong tendency for road stone to degrade badly when stock-

piled and this could happen even with the "superior" grades of stone used for sealing chips. The material gave "acceptable" results in such overseas standards as Los Angeles abrasion and the B.S. 10% fines test but would not stand up to the apparently minor rigours of being stockpiled. Frost riving was not a significant factor but it was known that some New Zealand rocks while highly competent in the mass suffered badly when separated from the living rock. Blocks cut from the river banks in stable gorges of the order of 500 to 1,000 feet deep would crumble to fragments if left out in the weather for a few months. Some powerful degrading effect was at work but its exact nature was unknown. With the exception of the sodium sulphate test, which was far too severe since few New Zealand rocks would pass, the only published test that seemed relevant was the New South Wales Department of Main Roads "shale test". This obviously was not wholly applicable as it applied severe mechanical shock to each test piece and this was certainly not a major factor in our case. But it did provide a starting point from which a simple ten-cycle drying and soaking test, in conjunction with light mechanical rolling, was evolved. The amount of break-down in this test, coupled with the cleanness value of the material after test provide a reliable indicator of how well the material behaved in the road. The test is now commonly used in assessing the quality of road stone in New Zealand and can also be applied to concrete aggregates. It has not been much publicised though it was described in a paper by J. Clelland in the New Zealand Roading Symposium, 1967. It could be of use to Road Authorities in Australia and I was pleased to see a recent request for further details from an Australian quarrying company.

The point of this example is not that a new, definitive test has been developed but the way in which it evolved. An Australian idea was adapted to meet a New Zealand problem; the development work was done in a laboratory primarily involved in concrete technology; and now is becoming of interest to Australians, surely a good example of the benefit of international and interdisciplinary co-operation.

My third example also illustrates the benefit to be gained from an informal exchange between disciplines. It is generally accepted that the higher the solids content the greater the stability of a road base (provided there is no question of pore fluid pressures being developed). Almost always natural gravels or crushed stone mixtures are not ideally graded for maximum solids content and some form of adjustment is necessary. Traditionally this has been attempted by specifying a grading which closely follows an exponential curve. Controversy has raged as to what exponent value was the best but this theoretical battle was quite beside the point to the quarry manager faced with producing the best result from the materials available to him. The exponentialists would have him instal ball mills and other expensive devices or else reject "excess" sizes to satisfy their preconceived ideas of grading but no one was looking at the justification for these preconceptions. The exponential curve certainly is the "ideal" one provided the assumption that all particles have the same shape, surface texture and packing arrangement regardless of size holds. But this assumption is so unlikely to be correct that, in most practical cases the exponential theory is highly suspect. Not only can it cost a lot to produce an

exponential grading, but also this grading may in fact be inferior to one more readily obtained.

While working with the design of grouts for prestressing ducts it was discovered that the suitability of a sand for this use could be assessed without actually mixing it with cement and water. The mass rate of flow of the dry sand through the standard grout cone was in fact a reliable indicator of the suitability of the sand. The higher the mass rate of flow of the dry sand the lower the water content (and hence cement content) required for a given grout fluidity. As the grout would only be fluid while there was an excess of pore fluid (in this case a cement-in-water suspension) it was therefore apparent that the mass rate of flow reflected the packing ability of the sand grains. Harsh angular grains, excess of fines, and a narrow range of grain sizes all led to poor flow rates while best results were found with well graded materials free of angular particles or silt sized fines.

When this knowledge was applied to the road stone grading problem it was soon apparent that in the sand flow rate we had a tool for directly assessing the packing ability of the sand fraction and that the exponent controversy could thus be relegated to limbo. It proved quite simple to show that with a range of natural and artificial sands better packing could be achieved (and at little trouble) by using the sand flow rate than by any exponential grading. In fact one could demonstrate that where a size fraction was particularly angular better packing could be achieved by replacing it with other sizes even to the extent of leaving it out entirely. It only remained then to measure the unit weight of the sand (by allowing it to fill a container of known volume as it fell from the grout cone) and a simple grading optimisation technique was available. The optimum blend of the coarser sizes is determined by trial and error (only a few size intervals are involved so the problem is not very complex) while the optimum blend, and the resultant unit weight of the sand fraction, is determined by use of the grout cone. Sufficient of this sand can then be added to the combined coarser fractions to take care of the void spaces left. The optimum blend should of course make allowance for the relative abundance of the sand sizes available as it can be economic nonsense to strive for maximum unit weight irrespective of expense or the rejection of large quantities of certain sizes.

It appears that the mass rate of flow idea could be extended to include the coarser sizes but this we have not yet done. The logistic problems of handling the much larger samples needed seem a bit daunting and could very well prove limiting. The method has been used on several occasions with good success and is described more fully in the N.Z. Standards Association Bulletin (Vol. 14, No. 4, October, 1968). It was recently used to optimise the concrete mix design for an arch dam. In this case, with a rather coarse, but within specification exponentially graded sand savings of up to 20% of cement (70 lb. per cu. yd.) were achieved by adding 20% of fine dune sand to the mix. The resultant sand grading was hardly conventional but a denser, more workable and economical concrete was achieved without any loss in strength. An incidental bonus was the considerable reduction in temperature rise that will occur in the dam structure.

My final example is of an informal exchange of

ideas between Canada and New Zealand. Over the years we have experimented with a variety of ways of measuring the in-situ unit weight of soils. Some of these proved successful, others not. One rather successful idea was the use of Suckling clover seed as a density "sand" in the sand replacement technique. The seed had many desirable properties in this application, it had a very narrow size range, was essentially spherical with a slippery slightly oily seed coat and gave an almost constant calibration irrespective of the method of pouring or the shape and surface texture of the calibration vessel. It was even quite resistant to the short term effects of moisture. But somehow the idea of using it as a calibrated material was slightly ludicrous and we were always a bit different about pushing its use.

Imagine my surprise and pleasure to discover from a chance conversation with the laboratory staff of the Saskatchewan Prairie Farms Rehabilitation Administration that they had found similar desirable characteristics in wheat grains. They too seem to have been diffident about publicising their findings, probably for much the same reasons as ours. Yet, between us we had gone a long way towards establish-

ing that a calibrated sand has no need to be made of mineral grains and that, in emergency, any one of a number of common grocery lines such as sugar, coffee beans, rice or sago could serve perfectly well.

I hope these examples convince you of the benefits of informal exchange of information and ideas and that we in New Zealand are keen to foster such exchange. I also hope that I have done something towards establishing a clearer understanding of the scope of Geomechanics. But I fear I have done little to affirm the thesis of this Symposium. Geomechanics is not just a tool in national development, it is the fundamental basis of all economic development.

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Geomechanics in Underground and Open Cut Mining

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I.- INTRODUCTION

My talk is intended to tell something of the history of the involvement of Geomechanics in mining, particularly in Australia.

During the 20th century, the growth in the development of the tools and machines used in mining, and the harnessing of greater and greater quantities of energy to these machines, together with the continued improvement in technology and materials used in blasting, has enabled the size and depth of excavations both surface and underground to be increased out of all proportion from those possible in the days of hand tools and manual labour.

As excavations have scaled up in size, it has become more and more important to understand the forces that cause the rocks, soils and minerals to remain in situ in the earth's crust without movement, to understand what change in circumstances will cause them to move, and when they do move what is the nature of the movement and the forces involved in the movement.

As these matters are better understood the mining engineer is better able to plan and design the sorts and size of openings he may make in the earth, specify the types and nature of drilling and excavation tools he requires for surface and underground excavation, and use with maximum economy the explosives required to loosen rocks for excavation. Conversely, as his understanding grows it will enable him to predict more accurately the stability and safety of ground and structures adjacent to mining operations, and to understand the longer term consequences of such operations on the surrounding country.

In development of large-scale mining enterprises in Australia, geomechanics research related to the huge Bougainville open-cut copper project, the brown coal developments of the S.E.C. of Victoria and work at the Snowy Mountains Authority and Mt. Isa Mines as well as other mines, has played a significant part in national development.

II.- HISTORY

The science of Geomechanics, by whatever name it might have been called, has always been a fundamental art or science in mining because of the very nature of the processes of excavating valuable minerals and rocks from the earth's crust. The miner has to determine the tools and application of forces necess-

ary to excavate the opening, to judge or determine the stability of rocks or soils surrounding the opening with or without support, judge the effect of excavation in an area on adjacent openings or the adjacent natural surfaces. The breaking of rock, controlling of caving in underground operations, the flow of materials in caving operations and ore passes, the maintenance of permanent openings for haulage of ore, the support of ground are among the many matters that have been subjects of study as long as mining has been carried out.

The mining engineer up till the 1930's took less interest in the theories of soil properties and behaviour being developed, tending to devote his investigation to development of simple "rule of thumb" principles for excavation in soils or rocks, leaving it to the civil engineer to undertake the development of theories which became essential as earth and rock dams, foundations for structures, road construction etc. all increased in size throughout the world.

Since the 1950's there has been a marked increase in theoretical study amongst the mining companies and many universities and institutes throughout the world and in the time and effort spent in seeking to develop a greater depth of understanding of Geomechanics. Rock mechanics, until comparatively recently, has largely developed from somewhat empirical rules based on mining experience throughout the world.

In the 1950's more and more effort was put into investigation work aimed at developing a proper understanding of the mechanics of rocks and soils, structures and stability, in order to achieve the proper balance of economy and safety in mining operations. In Europe, America, South Africa and Australia, at a number of universities and institutes oriented towards mining, investigations started which were for the first time really geomechanics, seeking to bring together the theories of soil mechanics with the empiricism of rock mechanics.

As in all the applied sciences, the major efforts had in the first instances to be devoted towards observation and measurement of stress and movement. To date rock and soil mechanics theory has only been able to proceed as rapidly as the collection of reliable data of stress and movement will permit and the development of tools and techniques in these fields has been, and still is, fraught with problems.

The initial fundamental academic research work in geomechanics as related to mining was being undertaken

in Australia at the Australian National University by Professor Jaeger and associates, working particularly in conjunction with the Snowy Mountains Authority and the coal mining industry. At the University of Melbourne work was being carried out under sponsorship of the State Electricity Commission of Victoria in connection with the large-scale open-cut brown mining.

Later the requirements of mining at Mt. Isa and Broken Hill led to further research being undertaken within companies and in the Universities of Queensland and New South Wales.

Since the mid-1960's geomechanics investigation work has gained in momentum in Australia. The C.R.A. group work, under the guidance of Professor Hoek, is establishing design criteria for the very-large-scale Bougainville Copper open-cut operations, where a whole variety of problems in relation to geomechanics are of very great significance; better slope design, overburden stripping and removal by sluicing, the design of the mine and plant in an earthquake area with live volcanoes comparatively nearby. In my own company the low-cost caving operation at the Mt. Charlotte mine of Gold Mines of Kalgoorlie has been based on investigations both in relation to the optimum drilling and blasting schedule to cave and break up the ore and also design for stopes and for drawing off the broken ore to obtain maximum recovery with minimum dilution. In mentioning the C.R.A. and Western Mining work, I have started to discuss in more detail the particular areas in mining where theoretical research into geomechanics has started to play a more significant part. Perhaps it is worthwhile at this stage to continue to look more closely into the specific fields where such research is and will prove of considerable use in mining.

I would group the geomechanics work into two main areas, what one might call micro-geomechanics, comprising work seeking to determine the way the grains, particles and components that make up rocks (or soils) of all types are held together and the way forces should be applied by tools and machines to break up the rock to facilitate the further steps involved in winning the ore. The second area, which could be named macro-geomechanics, consisting of work related to the evaluation of factors which affect the stability of rocks and soils, surface or underground. There are two aspects to this: firstly, the creation of unstable conditions to bring about the process of excavation and, secondly, the continuance of stable conditions either temporarily or permanently such as in the permanent batters and surrounds of large open cuts, the haulage shafts, drives, tunnels etc. in underground operations. In both these areas, the significance of the roles played by contained water either interstitial or pressure water in crevices still warrants further research. The two divisions of research naturally overlap and a proper understanding of what I have called micro-geomechanics is obviously necessary in the study of the macro field.

Perhaps I can illustrate this best by examples from my own experience which tell in themselves the story of the continuing and deepening involvement of geomechanics in open-cut mining in Australia.

Incidents involving substantial movements of massive coal blocks and overburden in and around the excavation areas had been a regular feature of open-cut mining operations in the Yallourn and Yallourn

North open-cuts prior to 1950. The incidents underlined the need for greater understanding of the causes of these movements, particularly as it was proposed to take the new Morwell open-cut to a depth of over 400 ft. and future development of the huge Loy Yang field could involve multiple-seam mining to a depth of 1,000 ft. and more in unconsolidated or semi-consolidated sediments.

At the same time early in the 1950's, due to problems involved in reaching design outputs from new bucket-wheel excavators, there was a growing awareness of the limited knowledge and experience of German mining engineers and excavation manufacturers in relation to the effective design and utilisation of the bucket wheels for digging our local clay overburdens and coal. Rhineland overburden consisted of sands and fine gravels and their coal is friable with no pronounced planes of weakness in one direction or another.

A programme of research was instituted in the mid-1950's to provide proper bases to establish design criteria for the plans to mine the deep brown coal seams of the Latrobe Valley and also the most efficient way to dig with bucket-wheel excavators in our clay overburden and brown coal. I omitted to mention that just under the bottom of the 300-400 ft. Morwell coal there was known to be an almost artesian aquifer which would tend to lift the coal and probably promote serious movements, and which could cause flooding of the lower portions of the open-cut as the depths of working increased.

The research programme consisted of the following elements:

- (i) The usual literature research and investigation of the possible existence of similar mining problems elsewhere. This led to the discovery that a great deal of similar work was in fact getting underway elsewhere in the world, in particular in the Bingham Canyon huge copper open-cut in Utah, U.S.A. - but more of interest to us was work undertaken at the University of Aachen in West Germany.
- (ii) The development of a very comprehensive system to survey horizontal and vertical surface movement in and adjacent to the Yallourn and Morwell open-cuts and endeavours to observe and measure what movement occurred at depth in the coal adjacent to operations as excavation occurred, particularly at coal/clay interfaces.
- (iii) Research into the physical characteristics of the brown coals of the Latrobe Valley at the University of Melbourne. Several papers by K. Rosengren, separately and collectively with Professor Trollope and E.T. Brown, detail the results of this work. Their paper, "The Mechanics of Brown Coal" published in *Geotechnique* in December, 1965, would possibly still be the classic paper in theory in this field.
- (iv) Research into the development of instruments and techniques for determining movement stress and strain within the brown coal deposits before and during mining. Much of

this work was carried out by Professor Newstead, now of the A.N.U., and at the Monash University under the direction of Professor I. Donald.

- (v) Laboratory and field experimentation related to the resistance to cutting and fracture of brown coal and overburden in the process of digging by bucket-wheel excavators.
- (vi) Operational research work on the application of digging forces and the methods of continuous digging with these excavators.
- (vii) Investigations to establish the extent and recharge rate of the sub-artesian aquifer underlying the Morwell seam, to determine the measures that would need to be developed to minimize the effects of these aquifers on mining operations at depth.

Over the last six or seven years the planning of the development of the huge Bougainville open-cut copper project similarly led to a comprehensive programme of research under the overall guidance of Professor Hoek of the Imperial College, London.

III.- UNDERGROUND

In underground coal mining, the introduction of mechanised mining has created the need for application of geomechanics to the design of coal cutting machines and design and support of mine openings. Roof bolting techniques, the use of temporary roof supports and greater control of roof caving as operations proceed has led to use of larger openings, greater economies from bigger machines and also higher extraction percentages. In Australia the work done on arch support and roof bolting theory and application by the Snowy Mountains Authority and Professor Jaeger and associates at the A.N.U. undoubtedly has been of considerable benefit throughout both coal and metalliferous mining.

I think the applications for geomechanics in underground metal mining can once again be best

illustrated by an example with which I have some familiarity.

Operations at the Mt. Charlotte mine in Kalgoorlie, with guidance from the Mining Engineering Department of the University of Melbourne, have utilised the results of geomechanics research and theory in several aspects.

Firstly, in the design of the stopes and supporting pillars so that maximum extraction of ore is achieved without prejudicing either the safety of operations or involving excessive overbreaking of surrounding rock which would produce dilution of the ore. Secondly, the location of draw points so that the ore can be drawn from the stopes, achieving maximum recovery without flow problems or dilution by waste rock or filling from the caved area.

The cut-and-fill method which was developed using thorough geomechanics studies and planning has resulted in a very-low-cost operation permitting until recently the profitable extraction of low-grade gold ore which would not be otherwise possible.

The planning of the operations are based on determination of the characteristics of the ore-bearing rocks and the development of mathematical models to simulate the mining operation.

Measurement of the inherent stresses in the rock showed that the field stress along the strike of the ore body was of the order of 2,500 p.s.i. as compared with 1,500 p.s.i. across the ore body.

The design of the stope and supporting pillar were undertaken in such a way as to avoid high stress concentration, minimize the possible danger of the roof failure and poor ground conditions in the North and South ends of the ore body.

In conclusion, I am conscious that this paper does not tell anywhere near adequately the story of geomechanics in mining in our national development, nor have I mentioned or done justice to the initiative and efforts of many people involved - work at North Broken Hill, the coal mining industry, and elsewhere.

Planning and Control of Building Foundation Work in the Seventies

R. M. JOHNSON

(Milton Johnson and Associates, Melbourne)

INTRODUCTION

There is emerging a feeling that our traditional approach to this matter needs modifying to take into account new environmental changes that have occurred recently. Economical and technological factors will produce confusion unless properly understood and incorporated in the programme.

Business Executives throughout Australia are very critical of the inability of Foundation Engineers to control foundation progress and cost. The failure of some property ventures can be traced to under-estimates of foundation conditions. It is quite clear that while the Quantity Surveyor can estimate the cost of the superstructure fairly accurately from sketch plans, he cannot do the same with foundations. In order to produce an estimate of costs of the same order of accuracy, the Geomechanicist has a vital role to play prior to the project being initiated.

There are signs of change; Foundation Engineers are becoming aware of and applying new management techniques and have found the traditional phasing of operations prior to the start of construction needs re-thinking.

ENVIRONMENT

Owing to Government policy of restricting loan funds for Public Works, there has been a slowing down in the economy. There is increased competition between companies providing housing and office space. Analysis has shown that buildings must be bigger to be competitive. Margins are thus reduced to a point where foundation costs must be known far more accurately.

The speed of construction has hastened greatly of late. Prefabrication of building components is necessary to reduce construction time. It is no longer possible to do all the design before calling of tenders since this would unnecessarily delay the start of construction. Instead, planning and building economists are used to ensure the financial success and therefore it is possible to phase design and construction in parallel with one-another.

Buildings are higher, foundations are thus deeper to cater for increased loading on the soil. Horizontal shoring is necessary to prevent damage to adjacent buildings. The movements to be prevented are small, in the order of $\frac{1}{2}$ inch, and sophisticated methods of analysis are required.

Foundation construction methods are more diverse and in the hands of many different specialist firms who cater for such techniques as rock bolting, vertical sand drains, slurry trench retaining walls, ground freezing, to name a few. Many of these processes require as much as a year to get mobilised and therefore require early warning.

In this environment it is increasingly evident that a one-shot investigation, design and tender would require a considerable time to carry out and it is very likely that the building would be obsolete when erected. The following problems are created by the environment:-

1. Cost escalations due to delay and wage rises, loss of profit and the like, are greater now than ever before.
2. A foundation designing programme which would supply soil parameters for the whole range of foundation expedients would be very costly to perform. For example, commercial testing laboratories offer as many as 30 standard laboratory tests involving over 100 soil parameters for each class of soil.
3. Most foundation investigations at present prove inadequate because of alteration in either the Client's intention, the building regulations, or the range of contractors interested at any one time.
4. Analytical techniques for solving soil mechanics problems are greater in number and diversity than ever before. Considerable research in a number of places throughout the world and the advent of computer programming has made the subject very large and difficult to keep up-to-date.

PRESENT TRENDS

We must now accept the fact that the foundation investigation should now be conducted in stages. It is necessary to have qualified engineers continuously assessing and up-dating the investigation programme. Simultaneously, there must be dialogue with the structural engineer designing the foundations and with the specialist contractors. The following principles will be apparent:-
Plan a feasibility study aimed at getting the cost of the foundations to the same order of accuracy as the rest of the superstructure. In order to do this, one would need to do sufficient drilling and testing

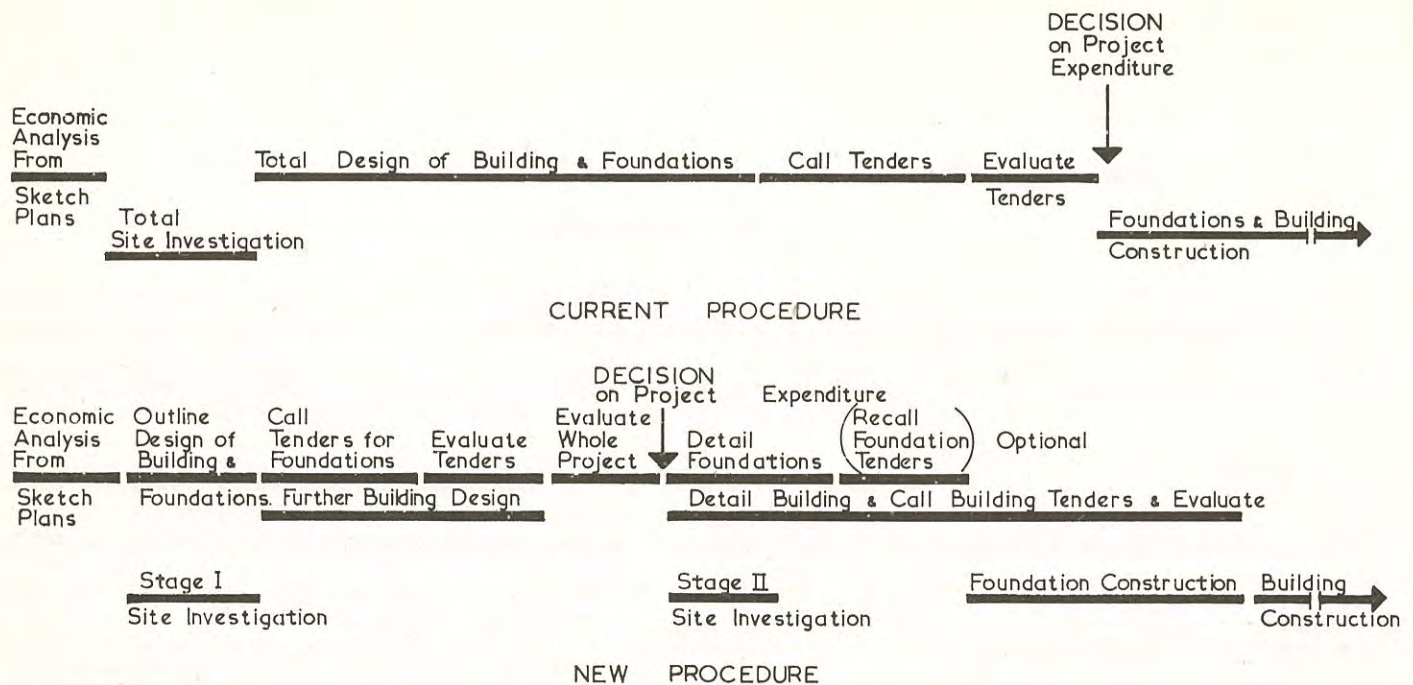


Fig. 1. - Design - Construction Procedures for Building Projects.

to determine the most economical foundation method. The foundations should be designed in some detail, and tenders called; and this sum combined with a Quantity Surveyor's estimate for the remainder of the work. If the project is approved, it will only be necessary to firm up the foundation design and construction started well before the superstructure design is completed.

This whole process is illustrated in Fig. 1 which compares current procedure with the new procedure just described.

SUMMARY

- Abandon one-shot investigation design and replace with at least two shots and perhaps three.
- Call tenders and select foundation sub-contractors with firm rates for their work before committing your Client to the project.
- Parallel as many activities as possible to save time.
- Use statistical methods to quantify the probability of variations in soil strata from the norm, e.g. the occurrence of dykes, soft or hard layers, perched water tables which are not possible to locate in the early stages.

CONCLUSIONS

The way is now open for improvement in the management of foundation construction for high rise developments. Major Australian companies, most of whom are design-construct organisations are now using these techniques with great cost savings; many more will follow.

It is most important for all foundation engineers to supply the data, energy and knowledge to the planners not fully acquainted with foundation problems. If the planner is old-fashioned, he must be told; if the soil data is insufficient, additional appropriations must be obtained.

The foundation engineer is a very important partner together with the quantity surveyor, structural engineer and architect in conducting the feasibility study. It is not sufficient to be an expert in geomechanics, but it is necessary to know something about what the other members in the team are trying to achieve. The foundation engineer must therefore acquire an interest and skill in planning techniques to fully utilise all the latest developments that geomechanics can provide.

Research and Development in Geomechanics

G. D. AITCHISON
(C.S.I.R.O. Division of Applied Geomechanics)

In the other papers presented by my eminent colleagues to this introductory session of the First Australasian Geomechanics Conference there is a great deal of evidence to prove that our progress in this field has been based upon sound original thinking and the willingness to exploit new ideas. These parameters of progress - new concepts and relevant innovation - are also in fact the attributes of any programme of research and development.

It has always been a difficult matter to define what is meant by the 'research'. Research, in turn, is not independent of development for the relevance of research is determined by the stage of development in any service or technology and the value of research is often determined by the adequacy of the developmental processes which follow.

But one thing is certain - the term 'research' and the term 'technology' have now lost their magic. We have all seen some part at least of the extraordinary emotional entanglement between science and technology on the one hand and the public and governments on the other. For two or three decades there has been a sort of fascination associated with science and particularly with scientific research. Many people have been mesmerised by the sheer ability of other men to reach out and grasp new concepts; others have seen research and technological development as an avenue to easy affluence - a kind of cargo-cultism, but all - or nearly all - have tended to regard 'the research worker' as a person apart - as a sort of minor priest in a heathen technological society: someone to be fed and feared; someone to be treated with awe if not with complete respect; someone who would brook no interference; someone whose motives could not be understood by the layman; and someone who did not always accept the total responsibility for developments arising from his work. It is easy to place such a person (or category of people) on a pedestal: and it is equally easy to be disillusioned when the ordinary human limitations of such people lead to social or economic blunders which can affect a whole society.

We are in just such a stage of disenchantment at the present time. The public has become aware of the rape of the environment by this demi-god of technology and governments have become aware of the damages to society arising from uncontrolled expenditure on expanding technology. These are matters with little apparent connection with the subject of this address but they are in fact entirely relevant since they set the tones and define some of the

themes for research and development in the seventies. We can be very sure that, whatever the past has held the future will certainly require of us a great sense of purpose, a public responsibility, an ability to communicate our desires and achievements and an appreciation of the economics of our position. Perhaps those of us in geomechanics research have always operated under some of these restraints since some of the controls of expenditure on our research has been exercised by hard-headed engineers. Be that as it may, the omens for the future are that we will certainly have to justify our place in the sun.

It is no mean place that we occupy already. Perhaps before we look to the future we should take a long hard look at what we are costing the community already so that we can measure the achievements of our subject with the cost of research and development. (Naturally there will be phase differences between any such expenditure and the benefits therefrom but the figures may be useful nevertheless).

Geomechanics research and development in Australia and New Zealand today is probably costing well over \$10 million per year. Naturally any such estimate of cost is a little imprecise for in most cases (for example in Universities and in State engineering instrumentalities) it is difficult to separate research from other activities. However the sum of \$10 million is probably quite a conservative estimate of the current rate of expenditure - and this expenditure is increasing rapidly each year. Whatever the correct figure - and it is certainly more than \$5 million and less than \$25 million - this sum of money has the dimensions of an industry and should be viewed in that light.

But before we pay attention to the present or proposed situation in geomechanics research it may be as well to review the past to some small extent.

It is a feature of the pioneering stages of any technology that innovation tends to precede research. We, in these countries, despite our technological achievements of the past four decades, are not long out of the pioneering era - and it is not surprising therefore that we still see the evidence of innovations and practices developed in that era. We still have many earth dams and many rural roads which were constructed using techniques in advance of their time and which still represent sound practice today. Some of our mines, opened more than a quarter-century ago still represent practices which have changed more due to the advent of modern

machinery than due to advances in engineering understanding of the properties of soils and rocks. All credit must be given to the brilliant engineers - the innovators of the past - who made significant progress without the aid of organised research.

But innovation based upon an individual's observations and experiences results in slow progress and society has demanded a more rapid rate of progress. Consequently research activity became essential and as society demanded more and more technology so research activity accelerated - leading to an even greater pace of technological progress.

Forty years ago, in these countries, research in any aspect of geomechanics was virtually non-existent. An interest in soil mechanics then developed but research expenditure had probably not reached a level of more than the equivalent of a few thousand dollars per annum before it was checked by the war. The technological stimulus arising from the war opened the way for research expansion on a variety of fronts and the pace of progress has continued to increase ever since.

It has been a feature of a great deal of geomechanics research that it has been linked to technological progress. Any usable research result has been accepted immediately by the engineering profession and often has been applied in an engineering project almost before the research paper has been published.

The extraordinary changes that have come over the research scene in the past thirty years can be seen in the growth pattern of the relevant portion of C.S.I.R.O. (The choice of C.S.I.R.O. as an example is partly because of the author's direct connection with the whole of the programme during this period and partly merely because this is possibly the only research group to have spanned this period. It is not suggested however that C.S.I.R.O. has any more claim to fame than any one of a number of other groups in Australia and New Zealand. The Snowy Mountains Authority in Australia and the Ministry of Works in New Zealand - as two examples only - have accomplished great things in this time. The Universities generally have also made giant strides and in many instances have achieved an entirely high academic status).

C.S.I.R.O.'s role in geomechanics research, like so many other new developments in science and technology, emerged from the travail of World War II. The need to build heavy-duty airfields, in a country which still used grass surfaces to handle the air traffic at some of its major capital cities, created an unprecedented demand for materials and technique.

Soil was the one material available at all of the scattered sites chosen for strategic airfields and, since transport of other materials was out of the question in wartime Australia, it was therefore soil that had to be used for airfield construction. Soil in its natural condition was unsuitable to carry the ever-increasing loads applied by aircraft wheels and so it became necessary to improve, or to stabilise, the various natural soils. C.S.I.R.O. therefore embarked on a programme of investigation of soil stabilisation, closely linked to the day-to-day defence requirements of the country. Studies were made throughout much of Australia and a good deal of New

Guinea.

After the war C.S.I.R.O. (then C.S.I.R.) naturally withdrew from activities associated with defence but immediately found itself involved in the post-war building boom. The experience gained in the war-time studies of the engineering behaviour of soils for airfield construction was soon put to good use coping with problems of building foundations. New areas of land had to be evaluated in terms of their comparative foundation difficulties and, as a consequence of these appraisals, it became apparent that architects and engineers lacked the knowledge of soil properties necessary to permit them to design economic building foundations. C.S.I.R., through its Division of Soils, therefore undertook a long-term programme of research to define these foundation properties of soils and to develop a technology for the application of knowledge of these soil properties to proper design procedure for building foundations.

This work on building foundations was sparked off by the urgent post-war requirements of domestic construction but before long, as Australian cities and industrial areas grew apace, there was an inevitable demand for the extension of the work of C.S.I.R. to cope with the foundation design problems of major buildings, and of course other structures such as bridges, wharves, and roads.

During this same post-war period the interest of University Engineering Schools in the subject of soil mechanics was growing and in 1952 C.S.I.R.O. joined with the University of Melbourne in a cooperative programme which established the first post-graduate school of soil mechanics in Australia.

The increasing commitment to engineering research associated with these soil mechanics activities prompted the Executive of C.S.I.R.O. to recognise the need for a separate entity - leading to the disassociation of this work from the agriculturally oriented Division of Soils and to the establishment of an independent Section of Soil Mechanics, later to become a Division of Soil Mechanics and then a Division of Applied Geomechanics.

The years that followed the establishment of the Soil Mechanics Section were extraordinarily eventful and exciting. Engineering in Australia was booming, leading to many new opportunities for consulting engineers. Universities were proliferating, usually with strong growth in engineering departments. These developments had two consequences - firstly that every new technological development arising from C.S.I.R.O. research was seized upon and exploited immediately - and secondly that every research scientist in soil mechanics in C.S.I.R.O. became fair game for the recruitment efforts of the other expanding groups. Turnover became high - but since turnover of knowledge to industry roughly matched the turnover of staff the end result was anything but detrimental. In fact the outcome was merely that the whole research programme passed through a continuing process of rapid change and thereby maintained its vitality.

It is interesting to look at this pattern of change in C.S.I.R.O. in the context of the patterns of technological development in associated or dependent areas of engineering.

The early involvement (i.e. from 1945 onwards)

in the soil problems associated with building foundation design led firstly to a joint study with the South Australian Department of Mines of the interactions between geology, soil occurrences and foundation design for small buildings. As this work progressed the Department of Mines took over firstly the role of consultant to apply existing knowledge and then undertook further programmes of site investigation and mapping. By 1960 or shortly thereafter development had proceeded to the point at which private engineering consultants were ready to accept responsibilities for investigation and design of building foundations on expansive soils. Since that time the role of C.S.I.R.O. in this field has been one of research in support of the specialist problems of the consultants (which role may well continue for as long as such problems are brought forward at this level).

Early in the 1950's, with the expansion in engineering construction there was a dearth of knowledge concerning the soils strata at depths of up to 300 ft. in areas of Melbourne, Adelaide, Launceston and elsewhere. C.S.I.R.O. accepted the task of organising adequate site investigations for several major projects and although some aspects of these tasks could not be defined as research they represented a necessary stage in the upgrading of the procedures adopted in these areas for such site investigations. Within a space of only a year or two, commercial and consulting organisations were available to take over this type of work but C.S.I.R.O. has retained from these studies a research interest in and an international responsibility for the attainment of quality in the processes of soil sampling.

One of the relatively unique soil problems of Australia is that associated with the aridity of much of the country. From the inception of its soil mechanics interests C.S.I.R.O. has made extensive studies of the soil moisture variables and its effect on soil properties. The unfamiliarity of the concepts of soil physics has led engineers to accept these studies with caution but there is now a trend for the interpretation of research results into the designs of highway engineers.

Other topics on which C.S.I.R.O. has expended considerable research effort, but which have now largely passed into direct use in the engineering industry or into the engineering consulting profession, include studies of techniques for soil stabilisation and development of procedures for the engineering evaluation of terrain.

Naturally these semi-practical studies were backed up by fundamental research - into the nature and magnitude of interparticle bonds; into the mechanics of fracture; into the properties of unsaturated soils; into advanced computational techniques for the prediction of the stress-deformation behaviour of earthen materials.

Two important issues emerged from these basic studies - these were firstly that - in terms of principles - there was a complete continuum between the behaviour of soils and of rocks; and secondly that it was imperative that the predictive capacity generated by the use of basic principles should be supported by real observations - involving real materials and real structures.

Inevitably therefore, C.S.I.R.O. was led to expand

its interests to include rock mechanics as well as soil mechanics (this transition also coincided with pressure from the mining industry for new geomechanics knowledge) and as a second development the Division of Applied Geomechanics became closely involved with the whole process of the use of geomechanics knowledge in engineering design and construction.

The end point of this programme of development of the pattern of research in C.S.I.R.O. has been the establishment of integrated geomechanics research projects in which an attempt is made to include every aspect of the interactions between:

1. A (mathematical) model of material behaviour.
2. The measurement of the material properties (the parameters of the material behaviour).
3. Computational procedures to apply the predictive capabilities of the model and the measurements to a real situation (involving possible combinations of two or more structural elements, e.g., a building frame, a foundation slab and an underlying soil; or a series of mine openings in rock with some openings backfilled with hydraulic fill, etc.).
4. Measurement procedures to provide a check on all of the parameters of prediction involved in 3.

Feedback processes between each stage permit the assessment of the validity of the model, the measurements of material properties, the computational procedures or the techniques of measurement of physical behaviour of the system.

It is an important feature of such an integrated project that not only are all of the above technical interactions involved but also the interactions of various professional groups (the research workers, the consulting engineer, the site investigation engineer, the construction engineer and often indeed the technicians) are brought into play with exceedingly valuable effect.

Such integrated projects are extremely costly but since the cost is shared by all collaborating (and benefitting) parties, the true expenditure is often unknown. However, it is pertinent to note that since C.S.I.R.O. became involved in such projects, its expenditure on geomechanics research has climbed steeply until today it is not far below the figure of \$1 million per year (including contributions from the relevant industries).

I have stressed earlier and I wish to stress again that this pattern of growth in C.S.I.R.O. is not abnormal. Other groups in this country have forged ahead in a similar manner while some of the leading groups overseas are firmly ensconced on a much higher level of activity.

If one now looks to the future, bearing in mind the probability of close scrutiny of future projects for expansion in technological research it seems that we, in these countries, cannot afford the fragmentation that has characterised much of the past. We believe, quite obviously, that we have a valuable

contribution to make but we must make sure that we treat our industry - for such it is - with respect and pride and this we can do if we plan to operate efficiently.

Accordingly it may be useful to set out some guide lines for the planning of our future:

1. Research must have a clear identity (usually defined in relation to well established categories within the arts and sciences). Therefore research in geomechanics must be identified as a component in the pattern of progress of civil engineering and of mining engineering.
2. Research (in the useful arts) must have a definite purpose - usually defined in terms not unrelated to its identity. Research in geomechanics is predominantly pragmatic - it seeks to fill those gaps in engineering knowledge which are or may be important to the engineers of the time.
3. Research will always assume a definite pattern - with characteristic components usually determined by the purpose of research. Research in geomechanics follows the engineering pattern in having as components:
 - (1) basic studies of an experimental or an analytical nature,
 - (2) application of the results of basic studies to generalised engineering situations (leading to the statement of design principles or to innovations of a generalised character),
 - (3) application of the basic studies or the design principles to specific engineering situations (leading to specific designs or innovations of a detailed nature - each associated with a specific performance),
 - (4) observations of the actual performance vis-a-vis the predicted performance.

4. Research, if it is to be efficient, must have a structure involving proper relationship between all of the constituent components of research. Feedback between all components is essential.

None of the components of the research structure must be permitted to lag for this will limit the progress of the whole field of knowledge.

Obviously there are dangers in stating such a philosophy of research - for there is the clear implication that the time-honoured freedom of the individual will be subjected to the requirements of technological progress - or possibly to the requirements of optimising expenditure against a specified technological goal.

But restraint is not an evil of itself any more than freedom is a virtue of itself and I, for one, cannot see that an organised programme of research and development - in the national interests - is in any way abhorrent.

Possibly the optimum structure for such organised research is one in which a limited number of institutions need not have specific geographic entities but may consist of many individuals or groups working within a collaborative scheme.

Whatever the pattern that we may adopt for our research structure it is unlikely that we will treat our responsibilities lightly. This Conference is the first Australasian Geomechanics Conference but it is also the sixth in the series of Australasian Soil Mechanics Conferences. The first of these Soil Mechanics Conferences arose from the deep awareness of the need to cooperate in our search for a better understanding of soil problems in Australasia - and for soils engineers and research workers in soil mechanics the results of that meeting have been far-reaching indeed. I feel sure that after this conference we will move just as strongly ahead into the new field of Geomechanics research and development.

CORRECTIONS

(Volume 1)

Page 137: The following paragraph should be added:

In the undrained creep test illustrated in Fig. 8, the deviator stress was increased temporarily to induce rapid strain. It was then decreased to the point where the stress ratio was identical to that prior to the increase and maintained at this level for some time. It will be seen that the rate of increase of both the pore pressure and the axial (or shear) strain with time was reduced as a result of the rapid strain, but eventually both curves became coincident with curves which appear to be a continuation of the original curves. These results complement those in Fig. 3 and lend further support to the contention made in the paper that the internal resistance to shear increases as a function of previous shear strain rather than as a function of time.

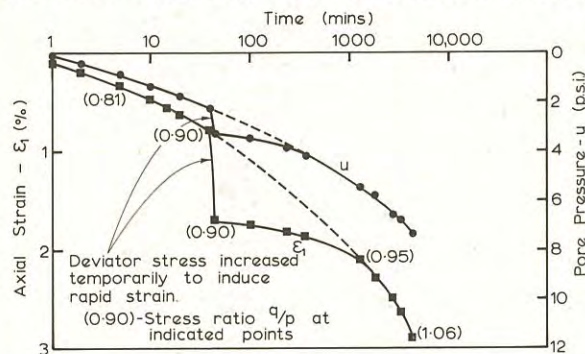


Fig. 8. - Effect of Transient Increase in Deviator Stress on the Ensuing Creep Behaviour.

Page 220: The caption to Fig. 7 (a) should read "Radial ridges (F)".

Page 302: Fig. 7, the distance between the lines $u = 0$ and $u = 0.3 h$ should be shown as 50 feet. This dimension had been omitted.

Page 305: Section X(d) "RESULTS", first paragraph, second last line, $F = 1.6$ should read $F = 2.6$.

Page 317: "(e.g. joint 3 in Fig. 4)" should be deleted.

Pages 406 and 407: To facilitate evaluation of the terrain components the horizontal rows of the table on p. 407 should align with those of the table on p. 406.

Page 425: In some of the columns of Tables II and III there should be $<$ and $>$ signs.

TABLE II.

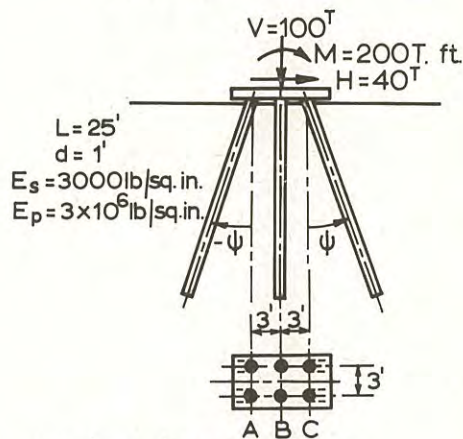
Grade IV	The P.I. should be < 10 rather than 10 as shown. The % finer than 2 microns should be < 15 rather than 15.
Grade VI	The P.I. should be > 30 rather than 30 as shown. The % passing the 200 sieve should be > 80 rather than 80.

TABLE III.

Grade IV	The compression index should be < 0.1 . The preconsolidation pressure > 20.0 T/ft ² The friction angle $> 35^\circ$.
Grade VI	The preconsolidation pressure should read < 10 T/ft ² The friction angle should be $< 15^\circ$.

Page 273: (i) Fig. 3 is incorrect and should be replaced by the corrected version shown below.

(ii) The last sentence in the first column should now be deleted.



(a) $\psi = 0$ (vertical piles)

Piles	V (T)	H (T)	M (T.ft)	ρ_v (in.)	ρ_h (in.)	θ (rad.)
A	-1.63	9.09	-10.33	.202	.178	.00235
B	12.50	1.82	-1.65	.287	.178	.00235
C	39.13	9.09	10.33	.372	.178	.00235

(b) $\psi = 15^\circ$ (battered piles)

Piles	V (T)	H (T)	M (T.ft)	ρ_v (in.)	ρ_h (in.)	θ (rad.)
A	1.98	11.54	-13.50	.165	.124	.00203
B	12.67	5.85	-3.75	.239	.124	.00203
C	35.35	2.61	17.12	.313	.124	.00203

DISCUSSIONS AT TECHNICAL SESSIONS

TECHNICAL SESSION No. 1—STRENGTH BEHAVIOUR

"Residual Strength Determination in Direct Shear", R.M. Cullen & I.B. Donald.

"Undrained Shear Strengths in Clays", R.H.G. Parry.

"Effects of Salt Content on Thixotropic Behaviour of a Compacted Clay", Surachat Sambhandharaksa & Za-Chieh Moh.

"The Deformation and Yield of Clays in Direct Simple Shear", L.K. Walker.

"Brittle Fracture of Rock at Low Confining Pressures", E.T. Brown.

"The Prediction and Measurement of the Long-Term Strength of Rock", D.P. Singh & W.E. Bamford.

"Liquefaction of Saturated Granular Soils", M. Kurzeme.

The Collapse of Sands Upon Saturation", P.J. Moore & D.V. Millar.

GENERAL REPORTER - Mr. P.L. NEWLAND:

The authors of the paper on "Effects of Salt Content on Thixotropic Behaviour of a Compacted Clay" make use of variables which are not clearly defined but appear to be as follows:-

Salt content% = wt. of salt per 100 gm. of dry soil.

Dry unit wt. = wt. of dry soil plus salt per unit vol. of wet soil.

Water content% = wt. of water per 100 gm. salt plus dry soil.

The results in the authors' Fig. 1 have been replotted in Fig. D1 using more conventional definitions of dry unit weight and water content, in both of which the weight of salt is excluded. It will now be seen that the results present an entirely different picture.

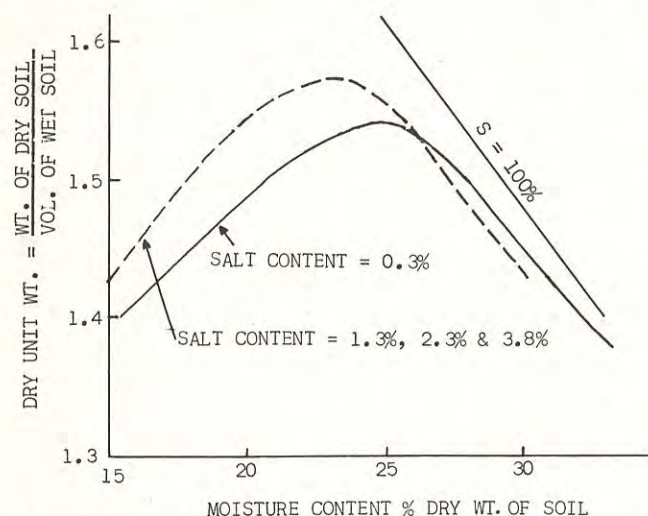


Fig. D1. - Compaction Curves for Different Salt Contents.

The quantity, salt content, is retained in Fig. D1 but a preferable quantity would be salt concentration (as used in the later section of the paper) which refers to the weight of salt per litre of pore water. Thus, in Fig. D1 a salt content of 3.8%, the salt

concentration varies between about 250 gm./l. for a water content of 15% and 110 gm./l. at a water content of 35%.

Since the authors analyse their results in great detail and provide very complex explanations in terms of interparticle forces, etc., for the observed behaviour, the appropriate choice of variables is a subject for discussion.

Dr. Walker's paper evaluates the results of simple shear tests on Kaolin in terms of the theory associated with the concept of a unique state boundary surface.

The major part of the paper presents results on both normally and over-consolidated samples which seem to accord fairly well with that part of the theory concerned with the principle of a unique surface. There is, however, one major discrepancy, namely in Fig. 8 where the results of constant τ_{xy} tests are presented. A curve linking the initial points of these tests, i.e. ABFEDH, diverges considerably from the curve AX for the state boundary surface. This will be seen by reference to Fig. D2. It would be interesting to know if the duration of the consolidation increment preceding application of τ_{xy} in these tests was long enough to allow some secondary consolidation to occur, since similar behaviour has been observed by the Reporter under such circumstances.

Also shown in Fig. 3 are the results of simple shear tests carried out by Thuraiarajah (Ref. D2) on the same Kaolin as that used by Dr. Walker, together with the curve predicted by Roscoe and Burland (Ref. D1) on the basis of their modified Cam-clay theory. It would be interesting to hear the author's comments on this.

The final section of the paper deals with the all-important stress-strain behaviour on which the main justification for the Cam-clay theory rests. Unfortunately, this section is considerably condensed and therefore difficult to follow. This is compounded by a reference to the author's Ph.D. thesis.

Focusing attention on the normally consolidated state, the author states among other things that

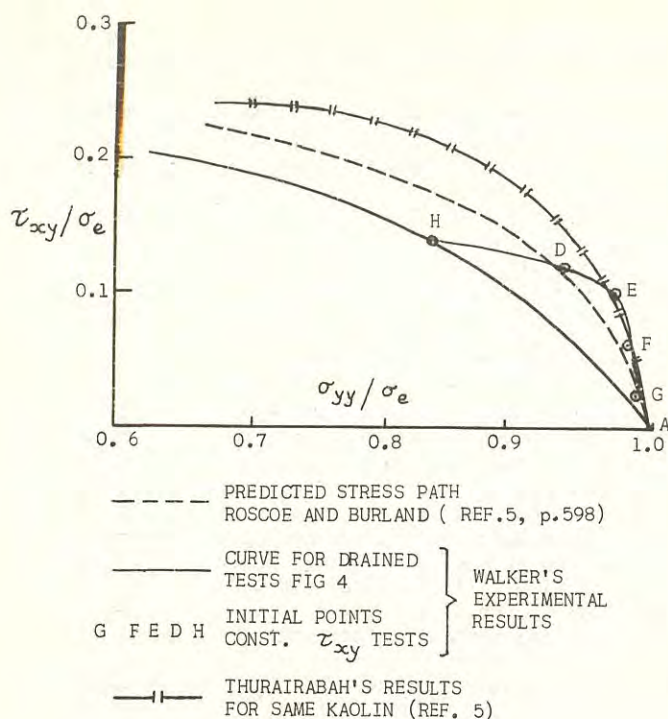


Fig. D2. - Results of Simple Shear Tests on Normally Consolidated Kaolin.

essentially the same stress angular strain curve is obtained in both drained and undrained tests. This is certainly a surprising result, particularly in view of experience with the results of triaxial tests. It would be of considerable interest if the author could present the results for the undrained tests so that they may be compared directly with those for the drained test in Fig. 10 in the paper.

The above observations, if true, certainly show up a deficiency in the Cam-clay theory. On the other hand, Roscoe and Burland (Ref. D1) demonstrate reasonable agreement between results predicted on the basis of their modified theory and the results of drained tests (Ref. D2) which are very similar to those presented by the author over the range concerned. Perhaps these anomalies may be resolved by discussion.

Dr. Kurzeme presents an interesting review of the literature on the liquefaction of saturated granular material. It will be seen from Table II that the type of loading simulated in tests on laboratory-sized samples falls in two categories; shear waves in the first and compressional waves in the second.

Most of the workers cited seemed to have concentrated on the first of these categories. Without exception, the ambient stresses in their tests were either hydrostatic (triaxial tests) or corresponded to the K_0 condition (simple shear). No attempt was made to investigate the effect of higher stress ratios to simulate the existence of a building. This is rather surprising since it requires little reflection to realise that quite profound changes in behaviour might result. For example, loose samples might be expected to undergo quite large strains once the effective stress had fallen sufficiently to allow shear

failure to occur. This would precede the state of general liquefaction where the effective stress had fallen to zero.

The above deficiency has probably arisen from a preoccupation with the rather dramatic phenomenon of liquefaction; from a practical point of view, however, it is the deformations associated with earthquakes that are of primary importance, as Schroeder and Schuster (author's Ref. 18) point out. These workers concerned themselves with the second category of loading, namely compressional waves. The technique used by them to simulate these waves was to vary the cell pressure sinusoidally in a triaxial apparatus with a $\frac{3}{4}$ in. dia. ram. The net effect of this was to impart a varying deviator stress to the sample in the vertical direction because of the varying upthrust on the ram. Shear stresses are, of course, the only stresses which can cause changes in the effective stresses under the undrained conditions which are assumed to prevail during the transient passage of earthquake waves. If the above workers had placed more significance on this aspect, their investigation may have taken a different direction. However, their tests did cover a range of initial stress ratios and they demonstrated that large deformations occurred in certain circumstances without the attainment of general liquefaction.

It would appear that a profitable direction for future research would be to investigate the axial deformation behaviour of triaxial samples under differing stress ratios with cycling shear stresses either vertically in compression or horizontally by displacing one end of the sample relative to the other. Neither of these avenues appears to present great difficulty from the experimental point of view.

With regard to the phenomenon of collapse which forms the subject of the next paper the causes of contact instability following inundation of an unsaturated soil are twofold:-

- (1) A decrease in the normal force leading to a decrease in the resistance to shear, provided there is no accompanying increase in the coefficient of friction, and
- (2) a net decrease in the shear resistance.

The second case involves two possibilities, namely, a decrease in the coefficient of friction of the solid material or a destruction of the resistance to distortion provided by the lenticular film of water at particle contacts.

As far as the first of these latter possibilities is concerned, if the added moisture already coats the particles before they are brought into contact during preparation of the sample, it is difficult to imagine that the coefficient of friction of the solid material is not already modified, if at all, by the presence of the film of water. It is unlikely, therefore, that the process of inundation will produce any further change.

On the other hand, if the sample is first assembled dry and water then allowed to condense around the contact points, instability would arise only if the water penetrated the points of contact and in so doing reduced the coefficient of friction. In this case, of

course, collapse would occur in the unsaturated state. If collapse did not occur, there is no reason to suppose that inundation would change the situation.

Thus meniscus effects remain as the sole source of whatever volume instability there is due to inundation.

The important question of how the samples were prepared is not clear from the authors' paper. Fig. 6 shows all the water located around the contact zone but this would depend on the method of preparation. This may not be significant if the effective stress is relatively insensitive to water content in the lenticular state, as Fig. 7 shows, although it is not clear how Eq.(6) was derived.

The equivalent effective stress calculated by the authors as being due to the lenticular water is admittedly small. However, there is no question that removal of this stress will qualitatively lead to collapse. The question is how much, since the change in effective stress can provide no indication. Some light might have been thrown on this question if tests had been carried out with different values of σ' .

The above comments have been concerned with the phenomenon of collapse under conditions of constant (zero) externally applied shear stress during the change in water content. The question of shear strength is another matter again since particles are constrained to move by an increase in the externally applied shear stress.

The method of sample preparation in the unsaturated state clearly now becomes of importance. There is also the matter of what happens to the menisci when particles move relative to each other (the work of Horne and Deere cited by the authors might well be reviewed in the light of the above statements although they do not clearly set out their method of preparation).

Can the authors explain the astonishingly large change in shear strength of the saturated sand with pre-shear void ratio (ϕ' varies from about 21° to as low as 11°)? A further question relates to the variation in e at failure (0.81 to 0.76). All the samples appear to have been prepared at an initial voids ratio greater than the critical so that one would have expected they would reach much the same strength and voids ratio at failure at a single value of σ' .

Just what the difference in strength between the "moist" and saturated samples means under these circumstances is difficult to assess. An interesting test would have been to inundate the moist sample after shear under the prevailing stresses and observe the ensuing collapse if any.

In Dr. Brown's paper on the brittle fracture of rock at low confining pressures, there appears to be an error in the last paragraph on p. 31 in the classification of rock types. The unloading curve for class 1 rocks should lie to the right of ACD and class 2 to the left.

The author's main concern is with the measurement of the post-peak stress-strain behaviour of rocks, particularly those in class 2 where failure tends to

be explosive. He describes several methods by which this can be achieved with varying degrees of success.

In discussing the dense ultra-fine grained rocks in class 2(b), the author states "If violent fracture is to be prevented, large amounts of energy must be extracted from the specimen very quickly and this has not been achieved to date". A possible solution to this might be obtained by allowing the confining pressure to rise rapidly in a closed stiff system in response to lateral deformation. This could be combined with the lateral displacement monitoring described in the paper or the pressure rise itself could be used for monitoring.

By contrast with the previous paper, the paper on the long-term strength of rock is concerned with the pre-peak stress-strain properties. For one who is more familiar with the properties of soils, it is pertinent to ask, with reference to Fig. 1, whether the volumetric strain and pore pressure curves are obtained in a single test or whether they apply to tests with differing drainage conditions. If the former is the case, is there some mechanism such as partial saturation to explain this?

With regard to the prediction of the long-term strengths, the loading rate, stress-strain and log stress-log strain methods involve the same two variables, stress and strain. In principle, therefore, the methods should give the one result. The differences recorded in Table I must surely arise from the graphical procedure in making the determination. Greater precision may be claimed from a given plot but this is not necessarily synonymous with accuracy.

The volumetric strain method gives higher predicted long-term strengths than those obtained using the above methods. The authors used samples with a 3:1 length-to-diameter ratio to minimise end effects but seemingly no special measures were taken to reduce end friction. It is possible that the microfracturing which contributes to lateral deformation is particularly sensitive to end restraint. It is also interesting to note that the departure of the transverse deformation curves from linearity in Fig. 4 occurs considerably earlier than in the case of load curve. The predicted long-term strength would be about 85% on this basis which is closer to the measured long-term strength than the 88% from the stress-strain method.

References:

- D1. ROSCOE, K.H. and BURLAND, J.B. - On the Generalised Stress-strain Behaviour of "Wet" Clay. Symposium on Enngg. Plasticity, Cambridge University, 1968.
- D2. THURAIRAJAH, A. - Some Shear Properties of Kaolin and of Sand. Ph.D. Thesis, Cambridge University, 1961.

Paper by SURACHAT SAMBHANDHARAKSA and ZA-CHIEH MOH:

The Authors in Reply:

The General Reporter has raised a basic question about the definitions of terms used in the paper. Since the soils considered in the paper consisted of

three components, i.e. the soil particles, the salt, and water, any definitions adopted for defining the unit weight of the soil and the "moisture content" must be compatible and consistent with each other. There are three possible ways to define these terms as shown below

(1) Salt as part of solid:

$$\text{Dry unit weight} = \frac{\text{wt. of dry soil} + \text{wt. of salt}}{\text{total volume}}$$

$$\text{Moisture content} = \frac{\text{wt. of water}}{\text{wt. of (dry soil} + \text{salt)}}$$

(2) Salt as part of fluid:

$$\text{Dry unit weight} = \frac{\text{wt. of dry soil}}{\text{total volume of soil}}$$

$$\text{Volumetric moisture content} = \frac{\text{volume of (water} + \text{salt)}}{\text{volume of dry soil}}$$

(3) Salt not included:

$$\text{Dry unit weight} = \frac{\text{wt. of dry soil}}{\text{volume of soil}}$$

$$\text{Moisture content} = \frac{\text{wt. of water}}{\text{wt. of dry soil}}$$

The first two were the ones used by the authors in the paper. The third set is the one suggested by the General Reporter. Although the latter terms appear to be the so-called conventional definitions, they have excluded an integral component of the system, i.e. the salt. In fact, when engineers deal with marine clays, no-one excludes the salt present in the soil when computing the dry unit weight and moisture content; usually the first set of definitions is being used.

Despite the differences in opinion about the definitions to be used, the effects of salt content on the compaction characteristics of the clay studied are still the same as shown by comparing Fig. D3 with Fig. 1(a) and 1(b). There appears to be some mistake in the General Reporter's plot, perhaps because he did not have the access to the original data.

The salt content can be either expressed by percent weight of the dry soil or as concentration in the pore water. It is just a simple conversion between the two. In a number of the graphs the authors have shown both. To the practising engineers, and in particular, when one deals with compaction, it appears to be simpler if the salt content is expressed in percent by weight.

Fig. D4 presents the micrographs of three samples, all compacted at a moisture content on the dry side of the optimum but with different salt content. These pictures which show differences in the particle orientation as well as the inter-particle contacts due

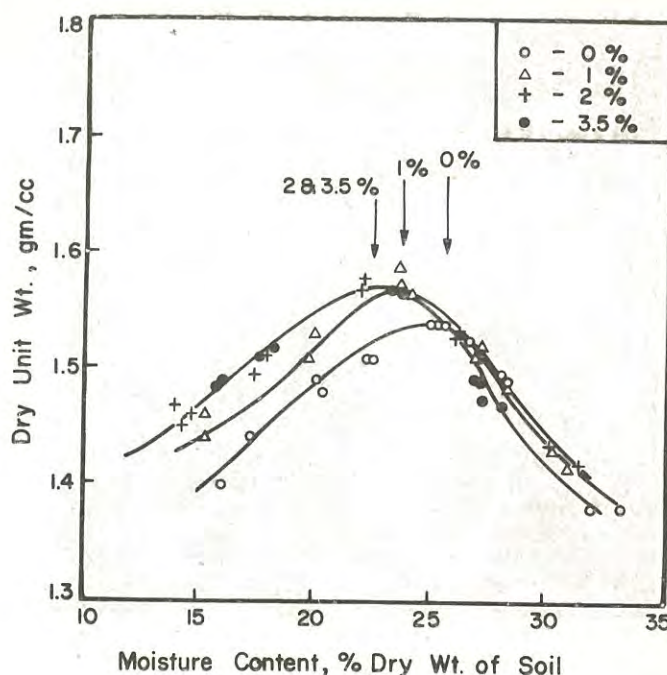


Fig. D3. - Effect of Salt Content on Compaction Characteristics of Bangkok Clay - excluding the weight of salt.



(a) Salt Content = 0.3%.

Fig. D4. - Electron-micrographs of Salt-Treated Bangkok Clay.



(b) Salt Content = 2.7%.



(c) Salt Content = 3.7%.

Fig. D4. - Electron-micrographs of Salt Treated Bangkok Clay.

to different salt content lend some support to the arguments put forward by the authors. The micrographs were taken by Mr. H.P.W. Polding of the University of Salford, Britain.

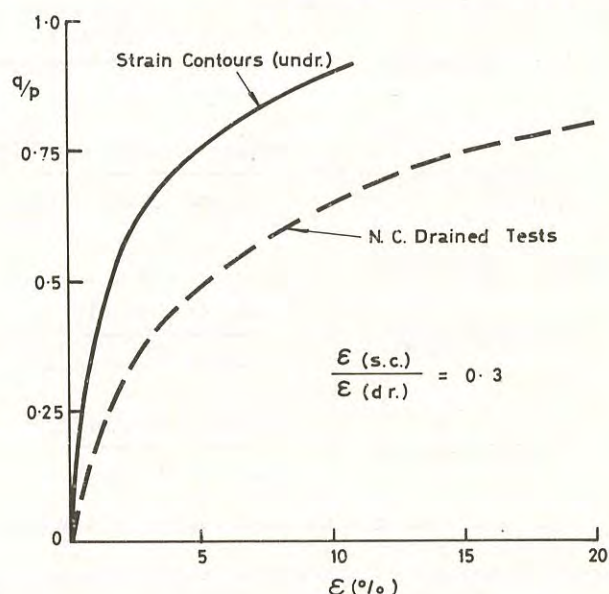
Paper by L.K. WALKER:

The Author in Reply:

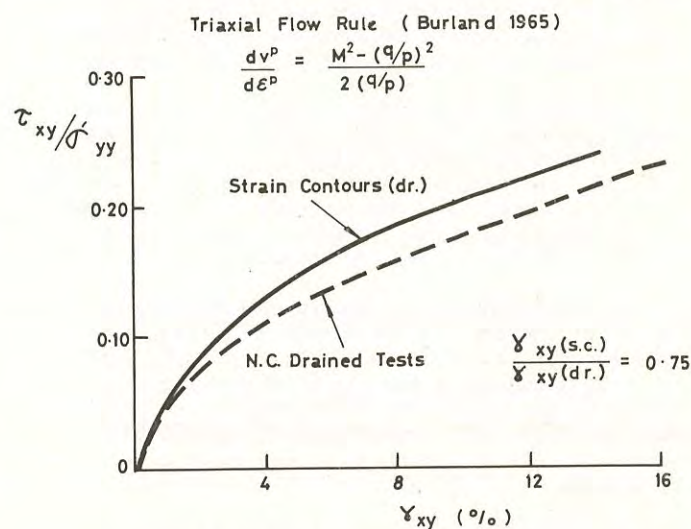
The General Reporter has touched on a significant feature of the author's results, viz. the comparison

between stress-strain curves for drained and undrained tests. The author has clarified this point by presenting in Fig. D5 below S.S.A. test data replotted from the paper, together with triaxial test data published by Burland (1967). The purpose of the figure is to illustrate the relative significance of the two components of shear strain discussed in the paper, these being

- (a) a strain contour component which is independent of plastic volumetric strain. This can be obtained directly using drained tests on overconsolidated samples, or indirectly by using the undrained stress-strain curve.
- (b) a shear strain component related to plastic volumetric strain by a flow rule similar to that shown on Fig. D5.



TRIAXIAL TEST DATA (BURLAND 1967)



S.S.A. TEST DATA (WALKER 1967)

Fig. D5. - Effective Stress Ratio - Strain Data for Kaolin.

The data plotted on Fig. D5 show a comparison between the strain contour component, and the total stress-strain curve obtained in drained shear tests on normally-consolidated samples. For the S.S.A. data, the strain contours have been taken from drained shear tests on overconsolidated samples and represent about 75% of the total shear strains. This figure would be lower (approx. 60%) if the undrained stress-strain curve had been used.

For the triaxial test data, the strain contour component has been based on the undrained stress-strain curve. This component represents, on the average, 30% of the total drained shear strains.

There are two major conclusions emphasised by the data, bearing in mind that the triaxial data are considered to be subject to less scatter caused by experimental factors. It is suggested (as in the paper) that the S.S.A. data show little difference between drained and undrained stress-strain curves. The scatter in individual test results overlaps the respective mean curves. The triaxial data, on the other hand, show the usual major difference between drained and undrained curves.

The reason for the different behaviour between the two test methods can be ascribed qualitatively to the different stress paths, which induce greater plastic volumetric strains in the triaxial test than in the S.S.A. test. The mathematical support for this proposition is contained in the paper by Roscoe and Burland (1963). The test data in Fig. D5 suggest that the application of plasticity theory as developed at Cambridge is of limited value for samples undergoing plane strain, as the theoretical strains so predicted form a small part of the total drained strains.

Paper by D.P. SINGH and W.E. BAMFORD:

The Authors in Reply:

The answers to the General Reporter's questions are as follows: The volumetric strain and pore pressure curves may be obtained in a single test, using appropriate apparatus (see A.S.T.M. Special Technical Publication 479, 1970, pp. 604-612). The initial increase in pore pressure reflects the closure of cracks and pores under increasing pressure; the decreased pore volume leads to increased pore fluid pressure. After the onset of unstable fracture propagation the rapid increase in microcracking leads to an increase in pore volume, and so to a decrease in pore pressure.

The Reporter's comment regarding the differences recorded in Table I is partially covered by paragraph 3 on page 41, and it is agreed that the graphical procedure can lead to errors. In principle, the log stress-log strain method should minimise these errors, as changes in curvature of the pen records should show up as changes from one straight line to another: by extrapolating from points on each straight line section the transition point should be precisely determined, even if no record was made of this point during the process of transcribing values off the recorder chart.

No measures should be taken to minimise end friction of rock specimens other than to polish the ends of the specimens on a surface grinder. Use of

soft capping material, having lower deformation modulus than the rock, has been shown to greatly reduce the apparent strength of the rock. It has also been shown recently that the failure process in a cylindrical specimen initiates at the contact with the platens, but that provided the length/diameter ratio is greater than 2, the transverse deformations measured at the centre of the specimen are "true". The departure of the transverse deformation curves from linearity, as shown in Fig. 4, should indicate the point where the volumetric strain curve departs from linearity, as in Fig. 1. That is, it coincides with the fracture initiation stress level, and the onset of stable fracture propagation. It should not, according to Bieniawski's hypothesis, also be the long-term strength. The apparent coincidence in the case of the marble specimen cited cannot be explained except by invoking natural variability, and presuming that the marble specimens tested under sustained loading were a few percent weaker than the marble specimens tested under "quick" compression.

No criterion of fracture was used to determine the sustained-loading strength: direct observation of the specimens showed that abrupt brittle fracture occurred, with the failure modes (mainly shear failure) being usually identical with those observed in the short-term compression tests. None of the long-term tests were terminated before failure occurred. One of the loading frames was used for tests which were expected to take more than a week to complete, while the other was used for sustained loadings at stress levels which it was expected would cause failure after less than one week. The longest sustained loading test completed to date was of 3 months' duration.

Paper by M. KURZEME:

Discussion by J.H.H. GALLOWAY:

A good deal of interest in Dr. Kurzeme's review of the problems of liquefaction of sands has been apparent in the oral discussion. This interest, it shows a growing awareness of earthquake problems in Australia but it seemed to me that there is a tendency to let emotion cloud the issue. Like any other engineering factor, earthquakes should be assessed objectively so that neither unwarranted risks nor excessive costs arise. It is not easy to decide what risks are unwarranted or which costs are excessive but a recent paper by Hollings (Ref. D3) gives some useful guidance. Working from simple basic concepts Hollings shows that blanket building code requirements, such as those in force in New Zealand, may provide unnecessary protection, or good protection at reasonable cost, or be almost totally ineffective depending on the circumstances of the case. By applying these concepts to each case a rational choice of protective measures can be made.

Reference:

- D3. HOLLINGS, J.P. - The Economics of Earthquake Engineering. Proc. N.Z. Nat. Conf. on Earthquake Engg., Wellington, May, 1971.

Discussion by C.T.J. BUBB:

The author has discussed prediction rather than prevention of liquefaction but a careful reading of the

paper hints at surcharging as a possible means of prevention rather than densification which is usually suggested.

Ambrasey and Sarma (Ref. D4) have suggested increasing the total normal stress and thus - as can be seen from the author's Eq. (1) - reducing the likelihood of liquefaction or substantial loss of bearing capacity for a given increase in pore water pressure that is for a given seismic event.

They suggested that this be done by surcharging the whole site with a permanent blanket of free draining granular fill.

Atkinson (Ref. D5) has further suggested that, as the aim is merely to increase the total normal stress, the free draining requirement might not be significant.

Provided the change in level of the site is acceptable it would seem to offer a very economical means of avoiding damage from full or partial liquefaction. In addition on coastal sites where tsunamis such as recently experienced in New Britain are to be expected the consequent elevation of the site might be a further advantage.

The author refers to the Kingston, South Australia, earthquake of 1897. In view of the fact that there have been no other events detected in the last 10 years or reported for the previous 70 years the writer has suggested elsewhere (Ref. D6) that this might well be an example of sporadic as opposed to continuous seismicity as recently suggested by Richter.

Finally in view of the known very high and continuous seismicity of New Guinea has the author any programme for carrying out his suggested field experiments in liquefaction there?

References:

- D4. AMBRASEYS and SARMA - Liquefaction of Soils Induced by Earthquakes. Bulletin Seismic Soc. America, Vol. 59, 1969.
- D5. ATKINSON, J.H. - The Behaviour of Foundations During Earthquakes. M.Sc. Degree Report, Imperial College, University of London, 1970.
- D6. BUBB, C.T.J. - On the Seismicity of Australia. Commonwealth Dept. of Works Publication, 1971.

The Author in Reply:

To Mr. J.H.H. Galloway:

The author agrees that the extent of any protective measures for reducing liquefaction damage should be assessed objectively. Two steps are involved. Firstly, whether the soils encountered are prone to liquefaction, could be investigated in-situ by the use of the standard penetration test or in the laboratory under cyclic loading conditions. Secondly, it is necessary to determine whether for the level of seismic activity found in the area in question protective measures to reduce liquefaction damage at the time of construction are an economical proposition. Two alternatives are either to ignore the possibility

that damage may occur and make good the damage if it does occur, or to take out some form of insurance to cover possible damage during the lifetime of the structure.

Generally, in areas of high levels of seismic activity, the cost of actual damage and the resulting loss of service of a structure due to liquefaction during its lifetime appear to be high, if compared to the initial cost of protective measures. However, in the recognised seismic zones of Australia, the level of activity is very low by world standards. The return period of events of significant magnitude in these areas are long compared to the lifetime of engineering structures. It is therefore probably more economical to take out insurance to cover the cost of possible damage as opposed to taking engineering measures to reduce that damage.

A method such as that of Hollings, referred to by the discussor, would provide an objective assessment of the course of action to be taken under a particular set of circumstances.

To Mr. C.T.J. Bubb:

The use of a permanent surcharge blanket over liquefaction-prone soils would certainly reduce the damage due to partial or total liquefaction response resulting from a particular earthquake. The economics of surcharging as opposed to in-situ densification or alternative foundation methods, would be highly dependent on the particular circumstances encountered at a site. A decision on the economics of protective measures can be arrived at objectively as outlined earlier.

If surcharging is the most economical means of protection, and the site in question can tolerate the change in surface level involved, a free-draining granular fill is preferable. The primary purpose of a surcharge blanket is to increase the total normal stress, but with an impervious blanket, any rise in pore water pressure as a result of shaking will take a correspondingly longer period to dissipate.

With a loose, saturated granular material, any shaking will result in some rise in the pore water pressure, that in turn causes reduction in strength (see Eq. (1)). Consequently, if the surcharge blanket is impervious such a reduction in strength will persist for a longer period than if it were free-draining which, while avoiding total failure, may result in unacceptable total or differential settlements of structures.

In his verbal introduction the author referred to the Kingston, South Australia, Earthquake (Ref. D7) of 1897, to draw attention to an early description of liquefaction response on the Australian continent. The description is of interest because many of the indicators of liquefaction response were already given at a time when the term liquefaction and the mechanisms producing the phenomenon were unknown. The soils in the Kingston area appear to be prone to liquefaction, but from the discussors work the nature of seismic activity in the area seems to be sporadic. Engineering protection against liquefaction response in such an area would not be necessary.

Paper by P.J. MOORE and D.V. MILLAR:

The Authors in Reply:

Regarding the mechanics of collapse the authors are in complete agreement with the comments of the General Reporter, namely that if the added moisture already coats the particles during preparation then it is difficult to imagine that the coefficient of friction between the solid particles could be further modified by inundation. In fact this point is discussed in the paper. However the test data shows that further change did occur following soaking of initially moist samples. One possible interpretation of this is that not all contacts were wet initially.

The authors cannot agree that "meniscus effects remain as the sole source of whatever instability there is due to inundation". The General Reporter has not demonstrated this point and the discussion in the paper suggests that meniscus effects played a minor role with the samples tested.

A variety of preparation methods for the samples was tried before the method described in the paper was adopted. The moist sand was thoroughly mixed to produce a uniform distribution of the water throughout the sample before it was placed in the oedometer.

In reply to the General Reporter's question relating to Eq. (6), this was derived simply by satisfying vertical force equilibrium for all forces and pressures at the contact illustrated in Fig. 6. The derivation given by Donald (Ph.D. thesis, University of London, 1961) namely

$$\sigma' = \frac{\pi T}{2r (1 + \tan(\theta/2))} \quad \text{for open packing}$$

is in agreement with Eq. (6).

Other tests have been carried out at values of the vertical stress of 20 lb./in² and 45 lb./in². These test results showed similar types of collapse behaviour to those observed with the 10 lb./in² value of vertical stress. In all of these tests the change in equivalent effective stress due to dissipation of the capillary stress following soaking is very small in comparison with the vertical stress (less than 1%). It is difficult to see on the basis of the data presented how the General Reporter can claim that removal of the capillary stress will lead to collapse.

The authors cannot explain the large magnitude of change in shear strength shown in Fig. 9. With other tests that have been carried out the changes in strength have not been as marked but there was always a relatively lower strength with the saturated samples at the higher void ratios.

During discussion of the paper some verbal criticism was levelled at the direct shear test data, a sample of which was presented in Fig. 9. Because the lowest point plotted in this figure corresponds to an angle of shearing resistance of approx. 11° it has been suggested that the friction of brass on brass was actually measured. Even if it is assumed that the two halves of the direct shear device were in contact, an evaluation of the criticism indicates that it is invalid.

A normal stress of 10 lb./in² over the area of the sand sample corresponds to a load of 55 lb. This load may be transmitted downwards through the sand or laterally to the upper half of the direct shear device. Test measurements show that the maximum load which could be transmitted to the upper half of the device is about 17 lb. which means that at least 38 lb. must be transmitted downward through the sand. If the full angle of friction for brass is assumed to be mobilised between the two halves of the device then a maximum possible shear stress of about 0.6 lb./in² over the area of the sand sample could be generated. With the normal stress of 10 lb./in² on the same sample this shear stress corresponds to a maximum of 3½° (the actual value is certainly much smaller) and not 11°, which may be attributed to brass friction. This maximum frictional effect was found to be equally applicable to dry sand, moist sand and saturated sand, so the conclusions which have been drawn from the direct shear data, such as in Fig. 9, remain unchanged.

To confirm methods of predicting liquefaction response, the field experiments suggested in the paper will have to be carried out in an area of known, high and continuous seismicity and in soils prone to liquefaction, to obtain results within a reasonable period of time. New Guinea certainly has high and continuous seismicity and also areas of liquefaction-prone soils.

At this stage the problem of pursuing the suggested programme is one of cost, primarily due to difficulty of access.

Reference:

- D7. GREGORY, J.W. - The Geography of Victoria. Whitcombe and Tombs, 1903, pp. 174-176.

TECHNICAL SESSION No. 2—UNDERGROUND WORKS

"Stresses Induced by Mining Operations at Mount Charlotte", by W.E. Bamford.

"A Rock Mechanics Survey and its Use in an Underground Stability Analysis at Kambalda, W.A.", by L.A. Dyson.

"Horizontal Pillar Extraction at Mt. Isa Mines Limited - Some Rock Mechanics Aspects", by D.B. Edwards.

"A Comparative Theoretical Study of the Effects of Inclination and Progressive Mining on Stope Behaviour", by C.M. Gerrard & W. Jill Harrison.

"The Gravity Flow of Material in the Sub-Level Caving Mining System", by G.D. Just & G.D. Free.

"The Establishment of Optimized Design Parameters for a New Gypsum Mine", by I. Weir-Jones.

GENERAL REPORTER - Dr. A.J. HARGRAVES:

A group of only six papers classified as Underground Works necessarily falls short of a full cross-section of geomechanics problems in underground works. Nevertheless they form a spread of problems across the cross-section, and the problems are approached with a sophistication of design virtually unknown in underground works twenty years ago. An apology is due from the coal mining industry, which is not represented at all. Another obvious absence is the tunnelling industries which have such advantages in comparative simplicity of analytical approach and generally with geometric openings in rocks more grossly homogeneous, more isotropic and less disturbed than are found in mining environments.

The paper by Bamford on stresses at Mount Charlotte profits from an unintended pillar failure which provides the opportunity for deduction of failure stresses, the checking of stability of remaining pillars and the use of the deduced data in future design. It is a more positive approach than the first attempt eight years ago at scientific design in rock mechanics at Mount Charlotte. This ended with the arbitrary practical over-riding of the designs, and their substitution by a virtually traditional empirical design method long established in the mining industry.

Several points in the paper could be explained more fully:

1. The apparent anomaly in the use of S.M.A. stress determinations indicating no change in stress between depths of 300 and 500 ft. and in the use of Hast's relationship clearly showing significant increase in stresses with depth to extrapolate stresses at greater depth. The data for the extrapolation were the similar stress values at 300 and 500 ft. depth and the mean depth.

2. A survey of a blasted excavation is necessarily approximate, and a selected cross-section is necessarily a sample which is intended to approximate the average or the worst condition. The selection of the best fitting geometric shape to represent a chosen cross-section is a further approximation. Has allowance been made for all of these factors?

3. The estimation of stress concentrations was based on elastic theory which probably is feasible as long as the limitations are understood. In a two-dimensional solution this should be

satisfactory for a stope block of limited extent. Undoubtedly there are some end effects which are ignored.

4. Nobody wants a support pillar to fail but capital has been made from this (unintentional) pillar failure to use it as a large-scale in-situ test. On this basis the likely strengths of other crown pillars in the same region have been determined. But at the same time factors of safety for the remaining pillars were determined using Mohr's envelopes. What part of the Mohr's circle was used to determine this mathematical expression of factor of safety?

5. What were the results of the finite element study mentioned - has sufficient progress been made to allow the hoped-for comparison to be made? Bamford's concluding remark modestly hoped for greater precision in the future finite element investigation at Mount Charlotte compared with the deduction method of the paper.

The paper by Gerrard and Harrison studies theoretically the effects of progressive mining on stope behaviour. The aim is similar to that of Dyson - to study sequential mining by the finite element technique. Two stopes at Cobar are the examples taken. Firstly, a crack-tip theory is employed on the premise that the theory can be extrapolated to a large orebody of mine dimensions. The Griffith crack theory is an extremely general one, and this extrapolation is open to question.

Secondly, an extremely general approach is made to the finite element technique in the stoping model. Some might question the inference that this work could be extended to other areas for progressive mining of stoping areas because it is rather more specific to Cobar than a general rock mechanics study. There is a caution regarding edge effects when using the finite element technique.

Whether specific or general, a most intensive and thorough study of the problem has been made, giving encouragement for work in similar problems elsewhere. Nevertheless, several matters would be improved by classification:

1. Is the finite element technique really suited to long, thin excavations?
2. It is stated that stresses were independent of

modulus effects, but displacements were inversely proportional to modulus. Elsewhere both stresses and displacements have been found to be dependent upon modulus.

3. It would be interesting to have field results to correlate with theory. Has this work progressed to a stage where it is possible to compare theory with measurement?

4. What would be the effect of different initial stress fields?

5. As every mining situation is unique, how can a useful general theory be created from finite element work?

6. The solution to this study not only depends on stress and deformation in two-stope systems — it also depends on the strength of structure, the rock mass. It appears that whereas this is stated in the conclusions, these considerations appear to be overlooked in the work done. Does the crack-tip theory allow for any of these things?

The paper by Just and Free has a strong theoretical and practical basis. It is important for a university project to have clear practical application, yet a high degree of theoretical content and whilst being academic, have good prospects of a successful conclusion.

The previous examples of drawing of broken ore have been more a documentation of experiences than an investigation of the mechanics of movement of the broken ore. However, sufficient qualitative material existed to provide a description of the movement patterns.

The authors have made an intensive background study before embarking deeply into a study of the factors underlying gravity flow. However, whilst the development is thorough, there are several points which should be raised:

1. At several points in the text, an expression is presented, largely without introduction, and usually without deduction or reference. Whilst the expressions appear logical and their mode of development is probably empirical, are they based on the authors' analysis or the work of others?

2. It is admitted that the model suffers the limitation of a two-dimensional approach to a three-dimensional situation. But more specific detail is required about the general extrapolation from two- to three-dimensional results.

3. Regarding the effect of overburden pressure, after a statistical analysis a linear regression expression is developed between the eccentricity factor and the height of overburden, indicating very little change due to the height of overburden. The qualitative assessment that an increase of overburden pressure might be expected to decrease eccentricity does not appear obvious nor the grounds for the inference. From the regression analysis it appears that overburden pressure might not be mathematically significant in any case.

4. In the longitudinal model of draw through the chute gate with variation of the parametric angles, the independence of the three angles does not appear to be clear either in the diagram or the text.

5. The two-dimensional limitations stated require some three-dimensional work to test the extrapolations made. In the conclusion the imminence of three-dimensional work was mentioned. Have these progressed to the stage of confirmation of the extrapolation?

A point of interest in summing up the papers of the session is that several authors have employed the finite element technique, apparently with varied success. Perhaps time could be taken to review what has happened — how far the method has advanced. Quite a considerable capital outlay has been made on these investigations of finite element — what has been gained?

In general, there have been some valuable investigations made of strength etc. properties in the laboratory and geological, geometric, failure and safe design situations in the field. The general situation of the geomechanics design in mining and its successful application in the field has been brought closer by the valuable papers presented in this session.

Paper by W.E. BAMFORD:

The Author in Reply:

1. Because of the limited number of stress determination results available, and the fact that these did not, as noted, show any increasing trend with depth, it was decided to average the stress values and also to average the depths below surface at which they were obtained. The vertical distance between stress determination sites — 200 ft. — was small by comparison with the depth to which these figures were to be extrapolated. This led to assumptions of the stresses at a depth of 400 ft. below the surface. It was felt reasonable to assume that the stresses would increase with increasing depth below surface, and as the horizontal stresses fell close to Hast's relationship a modified form of this was used to predict virgin field stresses at greater depths in the mine.

2. For the purposes of the approximate calculations, as outlined in the paper, representative cross-sections were selected. It is conceded that errors may arise due to the approximations involved. For the more detailed analysis, which is continuing, East-West cross-sections are selected for analysis at 5 or 10 ft. intervals; this should reduce approximation errors; it should also be noted that failure was found to have occurred due to concentration of the easterly field stresses, so a closer spacing of East-West than of North-South cross-sections is indicated. A survey error of as much as one foot should lead to only insignificant changes in axis ratios of the 100-200 ft. wide holes, and so of the stress concentration factors associated with the holes.

3. The fact that three-dimensional openings were being modelled in two dimensions was regarded seriously.

While a modelled slice normal to the axis of a long tunnel can obviously be regarded as leading to a fairly close analogy to the stresses in the field prototype, the dimensions of the stopes at Mount Charlotte meant that the dimension normal to the plane of each cross-section could not be regarded as infinitely large. However, the position of the most critically-stressed unit (Rib Pillar B2, from 500 to 600 ft.) was close enough to the centre, measured in a northerly direction, for the stresses calculated in an East-West section to be reasonably valid. The stresses in North-South sections were adjusted, in an attempt to allow for the small length of openings normal to the section. The diminution of stresses near the ends of the stopes was also allowed for.

4. If shear failure was observed to have occurred in the pillars, the factor of safety of other pillars could be expressed by

(i) Finding the point on the circle at which the normal distance to the failure envelope was a minimum.

(ii) Measuring the value of shear stress at this point (S_c) and also at the point on the failure envelope (S_e) corresponding to the same normal stress ($N_c = N_e$).

(iii) Factor of safety = $\frac{S_e}{S_c}$

If, as was the case here, the pillar failure was by axial cleavage, the factor of safety is obtained by measuring the radius of the Mohr stress circle (R_c) and the minimum radial distance, measured normally to the failure envelope, between the centre of the Mohr circle and the failure envelope (R_e).

Then factor of safety = $\frac{R_e}{R_c}$

5. Unfortunately, the final results of the Mount Charlotte stress study can still not yet be presented. It can, however, be stated that the finite element method is producing results in acceptable agreement with the approximate elastic stress concentration method first adopted.

Paper by C.M. GERRARD and W. Jill HARRISON:

The Authors in Reply:

- (a) Crack-tip theory can be extrapolated to a large orebody of mine dimensions under the assumptions made in the paper. Provided the rock mass is assumed to be linearly elastic, and that the mine opening is geometrically similar to a crack (that is, breadth of the opening is less than about 1/10 of its length), it is only a question of changing the units of length.
- (b) Finite element technique can be applied to long thin openings provided care is taken with stress concentrations around the ends of the opening. To obtain accuracy of solution in these regions, a high concentration of triangles was used around

the stope backs. About a quarter of the 380 nodal points used in the mesh were in the pillar region and within 50 ft. of the back of the upper stope.

A detailed comparison of the crack-tip and finite element results was carried out round the back of the upper stope and the two results agreed quantitatively. This indicates that the finite element mesh used can cope with the high stress gradients near the ends of the long thin stopes.

- (c) From the graph presented in the papers by Mr. Bamford and Mr. Dyson of the world-wide stress results, our assumption that the initial stress field falls off linearly with depth from the value measured at the 1200 ft. level to zero at the surface is suspect and will probably need revision when more information regarding the initial stress field is available. If the initial stress field were to decrease at a slower rate with depth, stresses above the upper stope would be increased. There would be a slight increase as well in the stresses in the pillar region.

Another possible initial stress field based on some earlier stress measurements of Carrard and Alexander has principal stresses parallel to the N-S, E-W and vertical directions. This would decrease considerably the 'shear' component of initial stress parallel to the stope walls. Hence the resultant stress field would be more like the 'compressible' stress field shown in Fig. 6(a) of the paper.

The solution to this study not only depends on stress and deformation in two-stope systems— it also depends on the strength and structure of the rock mass. Detailed consideration of these matters is not given in the paper because of the very limited amount of available data relating to the strength of the rock. The only strength tests results available to date are ones carried out by Stephenson at the 1200 ft. level. Stephenson found for tests on samples, a uniaxial compressive strength from 8000 p.s.i. to 25,000 p.s.i. with an average of 16,000 p.s.i.

On the basis of this very limited data, values of cohesion of 4000 p.s.i. and an angle of internal cohesion of 34° were assumed for the rock mass. For the case of two fully-developed stopes with a pillar width of 50 ft. (as shown in Fig. 11(d), contours of excess stress were plotted for the finite element results. 'Excess stress' is defined as the maximum value at a given point of

$$|\tau| - \mu\sigma - c$$

where μ is the coefficient of friction, and c is the cohesion. If the excess stress is positive the failure criterion for the rock mass has been violated at that point. The contour plots indicated that this occurred in the pillar region in regions of high compressive stress.

Also from the values of maximum tensile stress shown in Fig. 7(b) for the pillar, tensile failure is likely on the other side of the pillar region.

However these results are only preliminary and only apply to the fully-developed 600 ft. stopes. They are also heavily dependent on the assumed initial stress field.

Paper by G.D. JUST and G.D. FREE:

The Authors in Reply:

The mode of development of the expressions presented in the paper is largely of a theoretical nature. The majority of the expressions are derived from a study of plane geometry and calculus and have been developed by the authors. A limited number of the expressions presented have been developed by Professor Janelid (1966).

The model studies presented in the paper were solely of two-dimensional systems in which the flowing

particle size was greater than one-third of the third dimension. No attempt was made to extrapolate any results obtained from the two-dimensional models to a three-dimensional flow system. Small-scale model tests have been conducted recently by Bishop (1971) at the University of Queensland. The limited number of these three-dimensional tests so far completed indicated that an elliptical draw shape definitely exists.

References:

- D1. BISHOP, G.A. - Report to A.M.I.R.A. on Sub-level Caving Project. Department of Mining and Metallurgical Engineering, University of Queensland, October, 1971.
- D2. JANELID, I. and KVOPII, R. - Sub-level Caving Int. Jour. Rock Mech. Min. Sci., Vol. 3, 1966, pp. 129-153.

TECHNICAL SESSION No. 3 — CREEP AND CONSOLIDATION

- "Non-Linear Consolidation and the Effect of Layer Depth", by E.H. Davis.
- "Recompression Characteristics of Perth Overconsolidated Clays", by R.J. Frost, L.K. Walker & I.K. Nixon.
- "Aspects of the Consolidation of Morwell Brown Coal", by D.C. Green.
- "The Primary/Secondary Transition during the Consolidation of Clay", by J.G. Hawley.
- "An Investigation of the Three-Dimensional Creep Properties of a Clay", by P.L. Newland.
- "Application of Rate Analysis to Settlement Problems Involving Creep", by A.K. Parkin.
- "Deformation of Walls Retaining Soft Clay Backfills", by G.K. Spencer & P.J. Moore.

GENERAL REPORTER - Prof. I.K. LEE:

Introduction:

Although time has not diminished the importance of Terzaghi's original contributions to the prediction of the magnitude and the rate of primary consolidation, it is clear that the applicability of this "conventional" one-dimensional theory has to be examined rather carefully. Terzaghi did, of course, state the basic assumptions and was aware of some of the limitations of the theory, but as time elapses there is a tendency to ignore the limitations and to concentrate on the application of the analytical solutions of the conventional equation without sufficient thought being given to the applicability of the theory. Fortunately, the continued research on the topic of consolidation and the progressive publication of these studies has provided the opportu-

unity to review the limitations of the conventional concepts and to study the developments of theories which are no longer restricted to a one-dimensional strain state.

Certain of these developments, concepts, and applications of the consolidation theories are given in the papers which form the present session. Of the seven papers, three concentrate on different aspects of secondary consolidation or creep (Newland, Parkin, Spencer and Moore) and the need for clearly defining what is meant by primary and secondary consolidation is evidenced by some of the apparent conflicts of concept. For example, the paper by Hawley proposes an interpretation of secondary consolidation based on strain rate which is vastly different from the commonly accepted interpretation.

On a more practical level, the paper describing certain consolidation properties of the Perth clays (Frost, Walker and Nixon) also makes some reference to secondary consolidation and the importance of settlements arising from this source.

In recent years there has been greater interest in the problem of predicting creep, or perhaps, greater confidence that the studies may lead to a rational analytical technique for predicting creep, under one and possibly three-dimensional strain states. It is significant to note that there was a similar degree of emphasis on creep at the Mexico International Conference and this was clearly reflected in the submissions to the specialty session on Advances in Consolidation Theories for Clays.

In contrast to the papers mentioned above the contributions by Davis and Green concentrate on studies of primary consolidation or the settlements associated with primary consolidation. The first paper is a theoretical study of non-linear effects, whereas the latter is an account of certain consolidation properties of brown coals.

When reporting on a session of this type it is worth recalling the major aspects of consolidation theories not covered by the papers as this completes a brief state-of-the-art. Of the more notable exclusions are the three-dimensional studies which have developed from the original contributions of Biot and his co-workers (Refs. D1 and D2). Some of the numerical results of these studies are now available (e.g. Ref. D12) and a coverage of the useful practical solutions is gradually being published. Although these solutions are based on the linear stress-strain model this is not necessarily a severe limitation on the effectiveness of the theory to predict immediate and total final settlements and the rate of settlement (e.g. Ref. D7). Furthermore, the development and availability of finite element programmes has provided the opportunity for the use of any proposed constitutive law and examples of this are appearing in the literature (e.g. Ref. D5). The flexibility of this approach has not only removed the restriction of non-homogeneity (which reflects the non-linear character of the problem) but the usual difficulties involved in handling anisotropic material properties in an analytical solution do not greatly complicate the analysis provided, of course, the degree of anisotropy is specified. Similarly, the analysis can handle any initial stress state and it is important to take the initial stresses into account when attempting to predict load-settlement relationships.

Another development which has been not only recognised but evaluated analytically is the non-homogeneity of the soil structure (e.g. fissures, layering, laminations etc.). In the particular case of one-dimensional consolidation the effect of layering on the rate of consolidation has been evaluated (Ref. D6) and the work of Rowe on the effect of laminations under a three-dimensional strain state has shown very clearly the enormous errors which can occur in rate predictions if the usual assumption of homogeneity is accepted (Refs. D13 and D14). This has led to the recognition of the role of tests on samples of large dimensions and emphasises the need for proper field study plus the laboratory tests when financial expenditures are sensitive to the rate parameter.

The most disappointing feature of the present session is the lack of any contributions to the understanding of the consolidation behaviour of unsaturated soils. It has been stated and restated that a knowledge of the behaviour of unsaturated soils is of particular importance in Australia, but even the world-wide problem of predicting the consolidation behaviour of unsaturated soil in, say, a compacted earthen embankment requires a great deal of further study. Perhaps this is a reflection not only of the difficulty of the problem and the effort involved in making progress, but also the difficulty of obtaining adequate financial support for the experimental studies which unavoidably involve expensive equipment.

Consolidation Due to Pore Pressure Dissipation:

As mentioned in the Introduction there appears to be some difference of opinion regarding the origin of creep and for this reason it is considered expeditious to first discuss the papers based on the usual concept of primary consolidation, and then consider the papers which discuss creep.

As well as the analytical solutions now available for the rate of consolidation of a layered soil under a one-dimensional strain state, various investigators have studied and made significant contributions to non-linear consolidation. When only small strains are being considered the prime causes of non-linear behaviour are the variation in permeability and compressibility. The magnitude of these variations obviously depends on the type of soil, and when analysing the rate behaviour of any particular soil the possibility of significant non-linear behaviour should be examined.

One of the original contributions to this aspect of consolidation was due to Davis and Raymond (Ref. D8) and in his present paper Davis has summarised the original contributions and extended the analyses to certain cases which could arise in a real situation. It will be noted that the more generalised differential equation (13) (compared with the corresponding Terzaghi equation) arises because the variation of permeability is taken into account. The solution of this generalised equation requires the same assumption as Terzaghi, namely, that the coefficient of consolidation is a constant during the consolidation process. However, in the non-linear case this assumption only required constancy of the ratio of permeability to coefficient of volume decrease. Experimental evidence for the constancy of the coefficient of consolidation is given in the paper for an effective vertical stress range in excess of two tons per square foot (Fig. 1). Frost and his co-authors also show one soil type which manifests a constant coefficient of consolidation, but, in contrast, also show the results of oedometer tests on other soils where this constancy was not recorded. This raises the question of effect of experimental technique such as the influence of the "fit" of the sample in an oedometer ring and whether a test such as the K_0 test would not be a more satisfactory technique from this and several other aspects.

The difficulty of obtaining reasonably correct field values of the coefficients of consolidation was mentioned above with reference to the work carried out by Rowe on nonhomogeneous soils subjected to three-dimensional strain state. One of the most promising techniques for the measurement of this coefficient under

a one-dimensional strain state requires the measurement of the excess pore water pressures over the depth of the consolidating layers at suitable time intervals. Provided the intervals of time are adequate, use can be made of the rate theory (non-linear if appropriate) to establish the field value of the coefficient. It was clearly shown in the original work of Davis and Raymond and in the present paper, that the distribution of pore pressures are a function of the loading ratio in the case of a uniformly stressed layer, (Case 1) (see Fig. 2). This feature is worth mentioning since evaluation of the field coefficient of consolidation requires the effect of the loading ratio to be taken into account.

Published case records are rather meagre although it is likely a great deal of unpublished data has been collected. Mention is made in the Davis paper of some studies of this type in association with an evaluation of the performance of sand drains. The technique was recently attempted on some highly compressible silty clays in the Sydney area which happened to be subject to an essentially one-dimensional strain state. However, insufficient attention was paid to the sealing of the piezometers and the results were of little consequence from the point of view of establishing the field value of the coefficient of consolidation (Ref. D11). This project is mentioned not only as an example of an attempt to measure the field value of the coefficient of consolidation but it also shows very clearly the need for proper instrumentation and careful installation at all stages of a field investigation. Adequate resources to purchase or develop suitable equipment to record the relevant data, and the satisfactory installation, recording, and interpretation facilities are necessary before reliable data can be obtained. The full-scale programmes carried out by M.I.T. are outstanding examples of what can be achieved by proper conduct of field studies.

The difficulties of obtaining reliable and complete experimental data are also exemplified in the last section of the paper by Frost and his co-authors when they attempted to compare predicted and observed settlements.

They considered that the total final settlement due to primary consolidation was more closely predicted by the use of a recompression index instead of a "conventional" compression index - the term conventional presumably referring to some "average" slope of the unloading or recompression curve obtained from the specimen subjected to an overconsolidation pressure well in excess of p_c of their Fig. 1. This "average" slope would be that used in the Schmertmann field e -log p construction and would be relevant to the stress range shown, if the usual interpretation was adopted.

All unloading and recompression curves can only be approximated by straight lines and the slope of the unloading curve increases as the overconsolidation ratio increases. It is therefore not surprising that the value of C_s is less for the stress range shown than the "average" slope defined by unloading from a large consolidation pressure. The question of whether the slope obtained by the authors (0.04) is closer to the field value than the "average" slope is a difficult one to answer and the authors have made a commendable attempt to assess the validity of this approach. As indicated above final conclusions could

not be obtained on the basis of the field data which was available to them. One of the major difficulties in this study and indeed in many studies is the difficulty of differentiating between the settlements due to dissipation of pore pressure and to secondary consolidation settlements. Some investigators have suggested that the settlements which occur in the overconsolidated range are primarily due to creep and to support this interpretation experimental data has been established which shows a linear strain-log time relationship. Thus the conclusion of how long the loads were applied to the sample is of particular importance as well as the actual load and the increment.

Another aspect of the paper which requires comment is the attempt to find some means of indexing the compressibility properties of the Perth soils. One wonders whether there is any real justification for attempting to relate compressibility properties of the soil in the natural state to a quantity obtained by testing the soil in remoulded state. Some general trends, possibly indicative of physico-chemical effects may be expected, but if quantitative results are required for settlement calculations the philosophy behind the use of such data as presented in Figs. 2, 4, and 6, is questionable.

The aspect of Green's paper which promised greatest interest was the section on effects of temperature on compressibility. It is very disappointing that the data published is insufficient to establish definite conclusions regarding the effect of temperature on the compressibility properties of brown coal and it is hoped that more complete results will be available in the near future. Similarly, the data on the field loading test were somewhat disappointing and it is difficult to assess the value of the embankment test.

Primary, Secondary Consolidation, and Creep:

It is common to accept as a definition of the strains due to secondary consolidation as "the strains recorded under a condition of constant effective stress". Parkin for example accepts this definition if the term creep replaces "secondary consolidation". Newland proposes a more specific definition of creep and secondary consolidation. He considers that creep should be defined as "the process where any one of the quantities - pore pressure, volumetric strain, and shear strain changes with time in a manner which differs from that normally associated with primary consolidation where pore pressure gradients due to hydro-dynamic lag are set up in the sample". His concept of secondary consolidation is "the specific case where volumetric and shear strains accompany each other".

Hawley reinterprets secondary consolidation as "essentially 'small strain rate' consolidation" and primary consolidation "to be 'that during which all elements of soil within a stratum follow the same path in e - σ' under conditions of high strain rate". On the basis of the material given in the paper it is difficult to comment on the validity of the interpretation as the analytical development and experimental verification are not given. One can only presume that such information exists in the (unavailable) Refs. 6 and 7 of this paper. While responsive

to the "non-uniqueness of the $\epsilon - \sigma$ ' relationship" for a one-dimensional strain state, and some of the concepts presented in the paper, a realistic discussion can only be made when the evidence is assessed on the basis of the "conventional" as well as this new theory.

Although Parkin does not present experimental data to support the analytical and philosophical concepts presented in his paper, his review is extremely interesting and should provide the basis for stimulating discussion. A comparison of the concepts used to describe the creep behaviour of a range of materials is a commendable approach. It is well recognised that many of the concepts used in describing the behaviour, for example, of metallic materials, can form the basis of understanding certain aspects of soil behaviour. Parkin attempts to find a theoretical model for creep behaviour which is based on a fundamental consideration of material behaviour as distinct from the "curve fitting" procedures (e.g. using rheological models, Refs. D4 and D9). The problem of predicting creep settlement in the field on the basis of the inevitably limited information gained from short-term laboratory tests has not been solved. Until a satisfactory analytical model for creep behaviour has been established, any predictions must be treated with caution combined with experience and common sense. This remark has also some relevance to the studies described by Newland where again the question of inter-relationship between laboratory and field times is of prime importance.

The conclusions made by Parkin that predictions might be based on a creep rate equation appears to be one promising approach and is, for the reasons he gives in the paper, likely to be superior to the conventional approach to this problem.

Spencer and Moore make use of the creep rate equation in an attempt to predict the rate of outward movement of a model retaining wall. The authors are rather vague about the preparation and placement of the clay mass and how they established when primary consolidation due to surcharge was effectively completed. Furthermore, when the wall was subjected to a reduced lateral load there would have been an associated change in pore pressures. The recorded displacement at any particular time was therefore due to a primary component of consolidation as well as a creep component. It is difficult to see how definite conclusions regarding the long-term movements based on the creep rate equation can be made unless one first attempts to isolate the strain components. It would be reasonable to expect, for example, that the rate observed in the early stages of the model test (see Fig. 2) was significantly affected by the change of pore pressure.

Presumably the authors have more definite data than that presented in Fig. 2 to justify their conclusion that the strain rate is time-independent in Test 1 and time-dependent in Test 2, as the results in this figure do not make the stated differences in curvature evident to the reader.

One could also question the assumption of a single, specified surface of rupture and the validity of assuming that a torsion test simulates the strain state along the rupture surface. In brief, while commending the authors' attempt to solve this difficult problem the Reporter feels that a more fundamental

approach is necessary.

If adequate computing facilities are available to the geotechnical engineer he can make full use of the finite element method to solve load-displacement-time problems provided he has a constitutive equation which adequately describes the stress-strain-time behaviour of a typical soil element. At the present time we have therefore an effective analytical technique but to make full use of this tool a great deal of further research into the basic stress-strain-time properties is required. The type of work described by Newland could lead to the establishment of some appropriate constitutive equations.

The paper also reflects the degree of sophistication required in the experimental studies in order to establish reliable data and the importance of the care required in experimental work of this type cannot be overemphasised. It is analogous to a surgical operation in more ways than one.

It is not apparent from the paper the area of applicability of the undrained creep studies if one assesses this on a practical basis, although it is presumed that the conditions are approximated within certain regions of a deep clay layer for at least a limited period.

The experimental work has illustrated several aspects of creep behaviour and one can see a parallel between this study under a triaxial stress condition with some of the concepts put forward by Bjerrum in his Rankine Lecture (Ref. D3). The paper does require a considerable degree of concentration combined with a suitable background to absorb the significance of the data, and it would not be relevant here to quote out-of-context some of the finer details. However it would be fair comment to say that Newland has a basis for questioning the applicability of the simple visco-elastic type model for the triaxial stress condition, and he proposes as a qualitative depiction of some of the observed phenomena a "lazy contact" model (Fig. 7). This model or an equivalent non-linear version of it may provide a suitable basis for some analytical studies.

Concluding Remarks:

When examining each of the papers in detail one could spend a great deal of time and paper in praising, questioning, and criticising the contributions. Some of the aspects which appeared to be of some general significance have been raised in this review.

There does appear to be some basic topics for discussion which apply to a great or lesser extent to each of the papers. It has become evident that the conventional concept of primary and secondary consolidation needs further examination. In the Reporter's opinion the most useful topics for general discussion for a one-dimensional strain state are

- (i) the strain rate concept of Hawley, the evidence for this concept and the relationship to conventional theory.
- (ii) the analytical prediction of creep from the strain rate equation, or alternative approaches.

In view of the importance of the more generalised

stress state it is considered that discussion could also be usefully concentrated on analytical techniques for predicting creep under a multi-dimensional strain state. Such a discussion may well take into account the observations of Newland, and the approach proposed, for example, by Koizumi and Ito (Ref. D10) who consider separate deviatoric and volumetric strain components. Is there any evidence to support the latter's simplification of an exceedingly complex situation? It is hoped that the discussion will include an evaluation of the techniques at present available to the geotechnical engineer who wants to make use of existing knowledge to give him the best possible prediction of the long-term behaviour of his structure.

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Paper by R.J. FROST, L.K. WALKER and I.K. NIXON:

Discussion by P.L. NEWLAND:

At the building sites considered in the paper, the clay deposits are located under sand layers ranging in thickness from 27ft. to 55ft. The clays are therefore likely to be subjected by the raft loading to a system of stresses which places the clay closer to failure than the K_0 condition. Taking account of the magnitude of the stresses and of the fact that the clays are overconsolidated, the resulting pore pressures are likely to be sufficiently small for settlement to arise from undrained creep alone.

Following on the work on normally consolidated clay described in the paper to this session by the writer, tests on overconsolidated clay have been carried out. The results suggest that the lack of primary consolidation observed in the authors' Fig. 7 is to be expected. The results also indicate that the observed C_a value is likely to be higher than that for one-dimensional consolidation, consistent with the field observation.

In any event, the writer considers that better predictions of the field behaviour could have been obtained had triaxial instead of oedometer tests have been carried out.

The Authors in Reply:

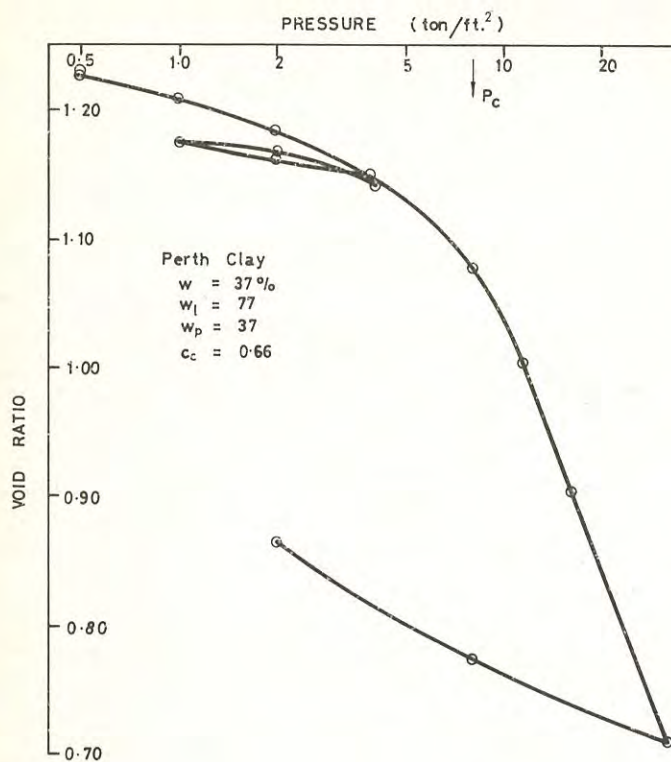
To the General Reporter:

The question of selecting a recompression index requires further comment following the General Reporter's remarks. The situation for Perth clays has been clarified in Fig. D1. below which analyses in greater detail the compression curve shown in Fig. 1 of the authors' paper. Three possible pressure increments have been considered - 1 to 2 ton/sq. ft., 1 to 3 ton/sq. ft. and 1 to 4 ton/sq. ft., and possible recompression indices calculated by applying these pressure ranges to the e vs. $\log p$ curve in two ways:

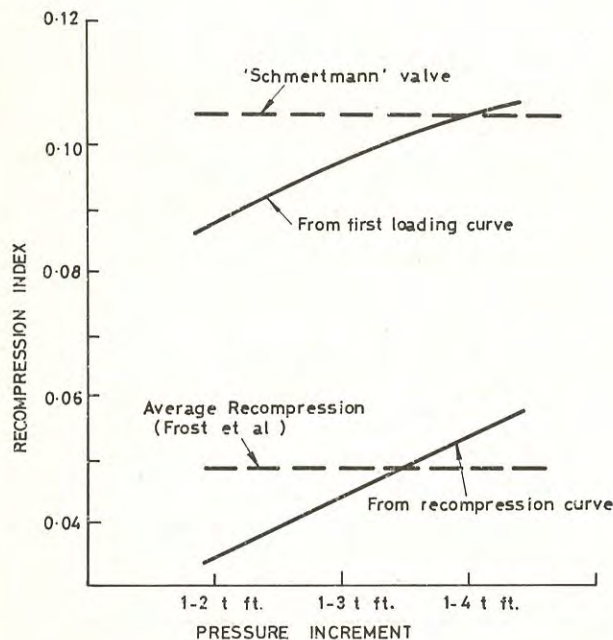
- (a) using the first loading curve;
- (b) using the recompression curve.

The resulting recompression indices increase with pressure increment as shown by the solid lines in Fig. D1. Values from the first loading curve are about three times values obtained using the recompression curve.

Two possible recompression indices have been calculated -



OEDOMETER COMPRESSION CURVE



ALTERNATIVE RECOMPRESSION INDICES

Fig. D1.

- (c) using an average slope of the hysteresis loop shown in Fig. D1;
- (d) using the "Schmertmann value", being an average slope of the unloading curve at the end of the consolidation test.

These two values (c) and (d) are independent of pressure and are illustrated in Fig. D1 by the broken lines. The Schmertmann value is close to the highest of all values, while the value from (c) above, which was used by the authors in the paper, is an average of the values obtained in (b) above.

Strictly speaking the authors feel that the most reliable compression index should be obtained using method (b), with a pressure increment appropriate to the stress increase anticipated in the field. The authors' proposed method is in approximation to this value. As noted in the paper, neither method can be adequately assessed until adequate field data is made available.

However, the existing evidence, although far from complete, suggests that the more conventional methods (a) and (d) can lead to gross overestimates of settlements in stiff clays.

To Mr. P. L. Newland:

The design of rafts in Perth with 30ft. or more of sand above the clay deposits is such that high factors of safety (certainly greater than 2) exist against shear failure in the clay. Under such conditions the oedometer test has been shown to give a representative value of C for application in the field (Ref. 12 of the paper). Cases where the effect of shear strains on C may be significant are likely to be provided by embankments on soft clays, for which low factors of safety (less than 1.5) are used.

It must be stated that C values are very sensitive to test conditions and are difficult to reproduce accurately in either field or laboratory. The paper has pointed out the limitations applying to the estimates of C made for the field cases studied. With the evidence available at present, the authors consider that there is little justification for the refinement of using triaxial tests to predict C .

Paper by J.G. HAWLEY:

The Author in Reply:

In his concluding remarks the General Reporter states "it has become evident that the conventional concept of primary and secondary consolidation needs further examination". He also expresses interest in "the strain rate concept of Hawley, the evidence for this concept and the relationship to conventional theory". Although these topics are dealt with formally in the Hawley and Borin reference it seems appropriate to indicate some of the main points here.

Theories of consolidation comprise six basic relationships;
 (1) continuity,

- (2) Darcy's law,
- (3) the effective stress equation,
- (4) equilibrium of stresses, i.e. self weight, and two more equations relating permeability, voids ratio and effective stress. If the postulate is made that for a given soil the permeability in a given small region at any given instant may be expressed as a function of voids ratio alone then these two equations become:
- (5) a permeability -voids ratio relationship, and
- (6) a description of the stress-strain properties of the soil skeleton.

There is no reason to suppose that the first five of these six equations are any the less applicable to 'secondary' than they are to 'primary' processes. The division of the phenomenon into primary and secondary stages has been necessary when particular assumed sixth equations have given an incomplete description of the stress-strain properties of the soil skeleton.

Two examples of sixth basic relationships are given as Eqs. (11) and (12) in the author's paper (p. 129). These equations were derived from the results of predominantly high strain rate laboratory tests. It is not therefore surprising that their introduction into the theory leads to solutions which are obeyed by the soil only under similar conditions. Under conditions of low strain rate such as occur at large times in conventional laboratory step loading tests, theories incorporating these or similar sixth relationships cease to describe the behaviour of real soils.

The common definition of the 'primary' and 'secondary' stages as those which occur respectively before and after the dissipation of excess pore pressures does not stand up to critical examination. The first two of the basic equations may be combined to give

$$\frac{\partial e}{(\partial e) \partial t} = \frac{\partial}{\partial \xi} \left[\frac{k}{\gamma_w} \cdot \frac{\partial u}{\partial \xi} \right] \dots \dots (D1)$$

where ξ is distance measured on the (moving) soil skeleton. With the exception of the condition where a significant amount of the strain rate might be due (momentarily at a given point) to $\frac{\partial k}{\partial \xi} \cdot \frac{\partial u}{\partial \xi}$ this equation shows the close relationship between strain rate $\frac{\partial e}{(1+e) \partial t}$ and curvature of isochrones $\frac{\partial^2 u}{\partial \xi^2}$. Any pattern of strain rate ('primary' or 'secondary') implies therefore a corresponding pattern of excess pore pressure variations.

(For non-Darcian flow the form of the above equation will be different, however it is still the case that the two laws of nature - (a) continuity and (b) the finite viscosity of fluids - will ensure that any pattern of strain rate within a soil stratum is matched by some corresponding pattern of excess pore pressures.)

The magnitude of the pore pressure at any given point at any instant has on its own nothing to do with the progress of consolidation at that point at that instant. Such pore pressures could be

- (a) Back pressures.
- (b) Hydrostatic pressures.
- (c) Pressures associated with seepage.
- or (d) Pressures associated with consolidation.

In case (c) it is the magnitude of the pore pressure gradients $\partial u / \partial \xi$ which control the seepage. Consolidation has associated with it finite values of $\frac{\partial}{\partial \xi} \left[k \cdot \frac{\partial u}{\partial \xi} \right]$ which imply finite values of $\frac{\partial u}{\partial \xi}$ and of u . Though finite values of $\frac{\partial}{\partial \xi} \left[k \cdot \frac{\partial u}{\partial \xi} \right]$ imply finite values of these other two quantities the converse is not necessarily true, i.e. an excess pore pressure or even a pore pressure gradient at any point within a soil stratum does not guarantee that any consolidation (primary or secondary) is in fact taking place.

The only practical purpose served by any theory of consolidation lies in the derivation of a scaling law or method whereby the results of tests on thin laboratory samples may be extrapolated in order to predict the behaviour of thick strata. For any given value of mid-plane excess pore pressure the values of $\frac{\partial}{\partial \xi} \left[k \cdot \frac{\partial u}{\partial \xi} \right]$ at corresponding points within the stratum will be much smaller in a thick field stratum than in a thin laboratory sample.

It is the form adopted for the sixth basic relationship which causes the phenomenon to be divided into two stages. This sixth relationship describes the stress-strain behaviour of the soil skeleton and has nothing to do with the pore fluid. It is unreasonable to postulate that elements of this soil skeleton 'know' anything of the location of the origin for the measurement of the excess component of the pore pressure - if indeed they can 'know' anything of pore pressure at all! (A basic assumption behind the use of back pressures in soil testing is that the stress-strain properties of the soil skeleton will be unaffected).

It is of interest to see whether, by postulating a form for the sixth basic equation which is less restrictive than those adopted in 'primary' theories, both 'primary' and 'secondary' stages may be united under the control of a single set of the six basic relationships. It has been found (Hawley and Borin) that by postulating a (sixth) relationship between effective stress, voids ratio and strain rate this objective can be achieved. Because the theory can then model the low strain rate laboratory behaviour as well as the high strain rate laboratory behaviour the authors consider that it is more likely to predict satisfactorily the (predominantly low strain rate) behaviour of thick field strata.

The six basic relationships cannot of course be solved explicitly. The implicit solutions must be found by numerical methods and for this the services of a small computer are essential.

The effect of strain rate has been considered by many rheologists. I consider that unless all spring and dashpot constants are expressed as functions of voids ratio and effective stress, such models cannot describe the behaviour of real soils. Many rheological models have implied in them the fallacious 'pore pressure' definition of the 'primary' and 'secondary' stages referred to above. No simple rheological model

can describe the behaviour of a whole stratum - only the behaviour of a typical thin element of soil within a stratum. The postulate of an effective stress-voids ratio-strain rate relationship and the adoption of numerical 'element-by-element' methods of solution allow these pitfalls to be bypassed.

Some advantages in adopting a numerical method of solution are:

1. Self weight effects (which may predominate) may be taken into account.
- 1a. The particular self weight effect, that of sedimentation, may be handled by a simple extension of the method.
2. All six of the basic relationships may be incorporated in the calculations without modification for mathematical expediency.
3. Variations in permeability both with depth and time in accordance with any assumed or independently determined permeability-voids ratio relationship may be incorporated.
4. The effect of variations in time between increments of step loading are handled automatically.
- 4a. The method of computation is not restricted to step loading conditions.
5. Computations relating to stratified soils may be made. Field strata are commonly stratified to a significant extent.
6. The method of solution is not restricted to soils which obey particular forms of the basic relationships. For different soils both the form of and the constants in several of these equations will in general be different.

Unlike most extensions of classical theory the unified theory described here is no more complex than classical theory - once the necessity of employing a computer has been accepted.

In general, for any given real soil, the thicker the stratum the more 'non-primary' is the predicted consolidation behaviour. Excess pore pressures are predicted for all stages of the process (both 'primary' and 'secondary') in accordance with Eq. (D1) above.

Paper by P.L. NEWLAND:

Discussion by L.K. WALKER:

The author has produced interesting experimental data which can be compared closely with those obtained by the writer for a similar clay (Ref. 4). Of particular interest is the author's Fig. 4 which illustrates the effect of separating out volumetric and shear strain components and examining their variation with effective stress ratio. The use of invariant stress and strain parameters obviates the necessity of defining principal axes of stress and strain.

The author's results indicate a significant reduction in volumetric creep rate with increasing stress

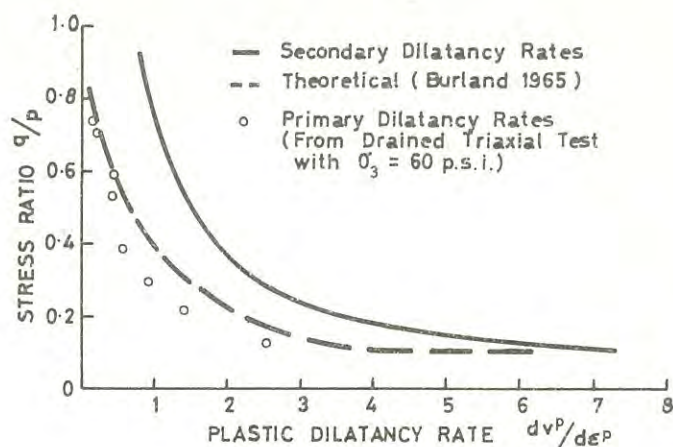


Fig. D2. - Comparison of Observed Triaxial Primary and Secondary Dilatancy Rates for Kaolin.

level, in conflict with results shown in Figs. 7-12 of Ref. 4. The writer would appreciate any discussion which might explain such observed differences.

The writer has also established the value of applying a flow rule concept to the process of secondary compression. This is an indication of the fact that the ratio of volumetric and shear creep rates can be represented as a function of effective stress ratio in a similar form to primary deformations. Results from the writer's triaxial tests on kaolin are shown in Fig. D2 above, confirming the existence of distinct flow rules for the two types of consolidation. Similar behaviour has been observed by the writer from tests on Leda clay. Any further data along these lines obtained by the author would be welcomed.

The Author in Reply:

To Dr. L. K. Walker:

In reply to Dr. Walker's first point, it is difficult to see any physical explanation for the difference between his results and those presented in the paper. They must be accepted as observed differences.

There is one point which can be made, however. In his Figs. 7 and 10 in Ref. 4, the volumetric creep rate is shown as constant and independent of stress ratio. Presumably, this continues up to the stress ratio corresponding to failure. This would appear to conflict with the recognised tendency of clays to fail at constant volume. The decrease in volume associated with a finite volumetric creep rate would tend to 'harden' the clay and therefore enable the stress ratio to be increased. The results presented in the paper indicating a trend toward zero creep rate at failure would therefore appear to be the more likely. (The volumetric creep rate obtained from Dr. Walker's Fig. 5 appears to be about 3.3×10^{-4} for $\sigma_3 = 60$ p.s.i. and $q/p = 0.7$. If this is so, the circle in his Fig. 10 for this point should be displaced to the left. In addition, if the bottom three points left of the full line are discounted, a line can be fitted to his results which shows the same trend as that in the present paper. It should also be noted that changes in the volumetric creep rate affect the associated shear creep rate).

With regard to Dr. Walker's second point, it is important to note that, in his 'drained' tests, the deviator stress was applied in finite increments. Thus, in the early stages after application of an increment, pore pressures would develop. The associated increase in stress ratio and the more or less undrained conditions would both lead to high shear strains relative to volumetric strains or low ratio between volumetric and shear strains. As consolidation proceeded, with associated decrease in stress ratio, volumetric strains would increase relatively

and lead to a higher ratio between volumetric and shear strains. This would appear to provide an alternative explanation for the results in Fig. 13 of Ref. 4.

If primary and secondary dilatancy rates are to be compared as in Fig. D2 presented by Dr. Walker, the onset of consolidation must be delayed as long as possible in order to reduce the contribution from shear strains which occur under undrained conditions. A different picture then emerges, as is discussed in detail in Ref. 3.

TECHNICAL SESSION No. 4—THE INFLUENCE OF THE STRUCTURE OF SOILS AND ROCKS ON THEIR ENGINEERING PROPERTIES

- "Rock Structures and Associated Rock Alteration, Danjera Creek Dam, N.S.W.", by A.D.M. Bell.
- "Some Effects of Structure on the Behaviour of Argillaceous Sediments", by P.M. James.
- "An Investigation of Moisture Changes and Soil Structure in Earth Dams", M.S. Kotowicz & S.N. Kiek.
- "Fabric Symmetry and Mechanical Anisotropy in Natural Soils", by D. Lafeber & D.R. Willoughby.
- "Estimating the Strength of Jointed Soils", by P. Lumb.
- "The Use of In-situ Tests in a Study of the Effects of Fissures on the Properties of Stiff Clays", by A. Marsland.
- "The Role of Porosimetry in Geomechanics", by B. Shackel & A.F.S. Nettleton.
- "The Influences of Structure on the Dilatation of Clay", by J.D. Nelson & K.L. Siu.
- "The Collapse of Clastic Aggregates", by D.H. Trollope.

GENERAL REPORTER - Dr. G.D. AITCHISON:

It seems that the pattern of growth and development of soil mechanics and of rock mechanics has itself followed a path which is compatible with the 'rules' (if such they be) of modern engineering research. There was firstly the stage of observation to define the existence of a problem and the need for new knowledge; secondly, the stage of hypothesis (involving a behavioural model) and relevant experimentation in idealised circumstances; thirdly, the stage of prediction followed by the application of the partially proven hypothesis to real circumstances; fourthly, the observation of the adequacy of the working hypothesis in the light of defects made apparent in the behavioural model by exposure to real circumstances; and finally the adjustment of the model and of the hypothesis to cope with the defects noted.

Soil mechanics separated from engineering geology

because of the need for improved techniques for quantitative predictions of behaviour. The geologist's careful observations and qualitative explanations of soil behaviour were supplanted by the engineer's mathematical models and measurable parameters leading to quantitative explanations of soil behaviour. This apparent predictive capability was coupled to a great extent with laboratory studies and the validity or otherwise of the predictions was not exposed adequately to examination in real circumstances for quite a long time (in fact, this examination is still far from complete in many cases).

As evidence has accumulated, however, from such real studies it has become more and more apparent that the simple models of soil behaviour originally adopted must be modified. Some improvements to the behavioural model have been shown to emerge from considerations of mechanics, e.g. stress-path and strain-path studies,

but a very large part of the total required improvement appears to be related to the recognition of soil as a non-ideal material.

Soil in nature is, in physical terms, often a grossly discontinuous medium characterised more by its complex array of defects than by its uniformities. Rocks may be described similarly.

In such circumstances, what is the value of soil and rock mechanics and what is the pattern of soil or rock engineering?

At the present time it seems that there are two answers to these questions. To the practical engineer (and this category probably includes the leading experts in both soil and rock mechanics), the answer is simple: rationalise through the processes of mechanics but decide not only on the basis of such rationalisation but also on the grounds of judgement based on direct observations of the materials.

To the research worker, attempting to quantify all processes of computation and of material description, the answer is not so simple. He must attempt to recognise all significant defects in the material structure and to quantify these - as parameters of performance - in a manner which is practicable and which permits the statement of an improved behavioural model.

In so doing both the practical engineer and the research worker have taken a step backwards (or is it forwards?) towards the re-recognition of the value of the geologist's careful observations.

It is difficult to define the point in time at which the capabilities of geologists to describe the structural features of rocks first became adequate but it seems that the first serious attempts to formulate the structure/mechanical behaviour problem and to study its implications were initiated around 1910 by Bruno Sander in Innsbruck, Austria, while he was studying the genesis of metamorphosed rocks in the Tauern Massif and the Tyrol. His continued studies over more than half a century have culminated in a large number of publications by himself, his co-workers, and followers, and have led to the establishment of a whole new school of thought in structural and metamorphic geology. The impact of Sander's original work was reinforced around 1920 by the work of Hans Cloos and his school in Germany on the genesis of joint and fault patterns, and the influence of both Sander and Cloos is still discernible in the European rock mechanics work of Leopold Muller and his co-workers.

The first mention of the fabric/mechanical behaviour interaction problem in soil mechanics seems to occur in Terzaghi's 'Erdbaumechanik' published in 1925. This was followed by a further discussion and slight expansion of the hypothesis by Casagrande in 1932. In both instances, however, hypotheses only were presented. Towards 1930 the study of the real structure or fabric of undisturbed natural soil was initiated by Walter Kubiena, remarkably enough again in Austria. Unfortunately, Kubiena's studies were very strongly biased towards pedogenetical problems. Consequently, they are hardly known in soil mechanics, even today.

After 1930 the study of the relationships between soil fabric or structure and soil mechanical properties

and behaviour went through a peculiar phase of development due to a number of extraneous factors. Partly as a result of the earlier development (Terzaghi, Casagrande, Mattson, and others), in both soil mechanics and pedology, studies began to centre around and concentrate on the clay fraction of soils, assuming this to be the ultimate control in soil behaviour. Consequently, the study of such topics as pore geometry or pore geometry patterns did not progress very far, and the possible role of the coarser soil constituents or soil skeleton was ignored altogether. This tendency to focus attention on the clay fraction of the soil was reinforced by the simultaneous vigorous development of X-ray diffraction techniques for the general study, and particularly the identification, of clay minerals. Even today this trend towards an uncritical concentration of effort on the clay fraction of soils is very obvious, although in many instances the place of the X-ray diffractometer has been taken over by the still more powerful electron microscope. Unfortunately, the majority of natural soils, including most of the so-called 'heavy' clays, just do not consist of clay minerals only. Moreover the influence of the structural arrangement of the clay particles often may be of minor importance compared to the influence of mechanical properties of coarser fabric elements.

More recently and particularly during the past five or ten years, natural soils have been observed critically both at natural scale and with low-power microscopy in place of the high magnifications of electron microscopy. In these studies some modified fabric concepts related to the earlier work of Sander and others have been adopted. Soils have been considered in terms of complex systems consisting of a limited number of 'primary' components, such as individual pores, skeleton grains, clay mineral individuals, etc. Characteristically, these primary component individuals join together at various levels, to form compound units of a 'higher order', and the evidence appears to be growing that these compound structural elements and their associated complementary defect structure patterns are in fact the determinants controlling the engineering behaviour of natural soils. The similarity with a corresponding trend developing in rock mechanics could hardly be more striking.

One of the most valuable results to emerge from the fabric studies on soils and rocks has been that these materials are almost always anisotropic, i.e., that the magnitudes of many of their properties are direction-dependent. The concept of anisotropy has long been known in crystallography and mineralogy, where it has been described in terms of combinations of a small number of symmetry elements.

Around 1890 Pierre Curie emphasised the value of applying the notions of symmetry to physical phenomena in general, and formulated a number of significant principles. His principle of superposition is still of fundamental importance for modern fabric analysis 'Quand plusieurs phenomenes de natures differentes se superposent dans un meme systeme il ne reste plus comme elements de symetrie dans le systeme que ceux qui sont communs a chaque phenomene pris separement.' (When several phenomena of different nature are superposed in the same system there only remain as symmetry elements those that are common to each phenomenon separately).

Sander introduced the symmetry concept very early in the field of rock fabric analysis to describe (statistical) anisotropy in rocks and rock properties, and the same development has been initiated recently in engineering soil fabric analysis.

The introduction of the symmetry concept in rock anisotropy has led to the recognition of only a very limited number of possible anisotropy patterns. These are:

1. axial symmetry: one n -fold axis of symmetry (usually vertical), associated with n (vertical) symmetry planes intersecting in the axis ($n = \infty$). This pattern is known in soil mechanics as 'cross-anisotropy';
2. orthorhombic symmetry: three mutually perpendicular symmetry planes, associated with three 2-fold symmetry axes normal to them;
3. monoclinic symmetry: one symmetry plane (often vertical) associated with one 2-fold axis, perpendicular to it;
4. triclinic symmetry: centre of symmetry.

All these types of anisotropy have been found to occur in natural rocks and rock masses, although their frequency of occurrence is strongly variable. On the other hand, in soil mechanics, it has often been assumed that the only type of anisotropy to occur is 'cross-anisotropy' with axial symmetry, (despite field and experimental evidence of the very frequent occurrence of (geometrical) anisotropy with monoclinic symmetry in sediments ranging from recent to very ancient deposits).

The fact that a symmetry pattern may be established by studies at a chosen scale, e.g., using low-power microscopy in a clay soil, any in turn be subject to the same criticisms as those directed (above) to X-ray or electron microscope studies of the clay fraction. It is becoming apparent in fact that any definable symmetry condition is scale-dependent. More than one symmetry may be observable in the same material depending on the scale of observations. For example, consider a deposit of a rather 'heavy' marine clay selected for a construction site. As a result of its origin, it is reasonable to expect that the majority of the clay mineral platelets or domains will show a strongly preferred orientation parallel to the horizontal plane, i.e., seemingly an ideal case of 'cross-anisotropy'. If, however, this clay deposit in its present position slopes only a few degrees (which is quite common), an additional, large-scale fabric feature expressing this slope, will be superimposed on the 'cross-anisotropy', and consequently reduce the overall symmetry of the system to monoclinic.

This additional feature may be only expressed in the anisotropy of the ('large-scale') crack pattern developing in the particular clay deposit when it dries out, but it will certainly maintain itself in a defect-structure pattern when the clay is re-moistened again. As a consequence of the similarity in scale of an engineering construction on the one hand, and of such defect structure pattern on the other hand, it seems logical that the latter fabric feature would be of primary significance, whereas the 'small-scale cross-anisotropy' may come in the second place, if at all. Many examples of a similar nature can be found and have

been found in natural rock masses. It follows therefore that any fabric analysis and any definitions of symmetry must be made at a scale which is appropriate to the dimensions of the engineering process to be applied to the soil or rock.

This principle, which does not depart in any way from ordinary engineering concepts, requires that the fabric analysis be undertaken as an integral part of an engineering study and not as an independent measurement of a supposedly 'intrinsic' property of the material.

Turning now to the papers submitted to this session, one must feel a sense of disappointment in the fact that the number of submissions on this important topic is so small and further in the lack of cohesion in the totality of these contributions. The disjointed nature of the material available to this session, of course, does not reflect any discredit on the authors but must be attributed to the lack of prior planning by the Conference organisers. In this Reporter's opinion, it should be the responsibility of an organising committee to spell out the intentions behind a nominated theme and to seek out the best possible contributions on each aspect of the selected theme. The mere nomination of the title of the selected theme for the session followed by the publication of any relevant voluntary contributions is not a process which lends itself to advancement in the state-of-the-art.

Nine papers are listed for attention in this session.

The papers can be considered under five topic headings. These are:

1. Techniques for observing and quantifying the fabric elements of soils and rocks.
2. Geological explanations of observed defect structures.
3. Techniques for expressing the observations and measurements of defect structure patterns.
4. Models to represent defect structures in soils and rocks to permit computations of dependent physical properties.
5. Measurements of the real values of defect-structure-dependent physical properties of soils and rocks.

Within the first topic 'Techniques for observing and quantifying the fabric elements of soils and rocks' the papers by Lafeber and Willoughby and by Shackel and Nettleton are relevant. Lafeber and Willoughby have extended their previously reported work on fabric observations in soils to make greater use of symmetry concepts as a prerequisite to the establishment of useful mathematical models for computation. Probably the most important feature of this paper, other than the matters covered in earlier comments in this report, is the factual evidence advanced on anisotropic features in natural soils. The gradual accumulation of such data (and associated dependent physical properties) must serve as a basis for the future emergence of a subject of defect-structure mechanics.

The paper by Shackel and Nettleton discusses the use of mercury intrusions porosimetry to define the distribution of pore sizes in soils. The technique proposed is not new and has been applied previously to other similar materials (e.g. brown coal) as well as to clays. The authors' conclusions also do not appear to depart significantly from those stated by earlier authors (e.g. Ingles, Refs. D5 and D6). One serious limitation in such a method if applied without other supporting techniques is that it gives no account of pore system anisotropy. A further limitation is that it applies only to small pores and as mentioned previously, the importance of these small-scale features may be questionable if one is concerned with the structure of natural soils and rocks in engineering circumstances ..

The second topic 'Geological explanations of observed defect-structure' is well treated in the paper by Bell.

This is a well-documented case history linking rock structure and associated rock alteration with subsequent dam foundation design.

The third topic 'Techniques for expressing the observations and the measurements of defect-structure patterns' is again well served in an entirely realistic manner - this time in the paper by Lumb.

Lumb has applied a probabilistic treatment to the consideration of the influence of planar discontinuities on the strength of residual soils.

Although he does not suggest that his present studies do much more than open the way for further probabilistic studies, he has in fact made an extremely useful contribution by presenting his data and his reasoning as well as his comments on the need for many more such studies.

The fourth topic 'Models to represent defect structures in soils and rocks to permit computations of dependent stress properties', is elegantly, if somewhat metaphysically, treated by Trollope in his paper 'The collapse of clastic aggregates'. The term 'metaphysical' is not used in any derogatory sense but rather to suggest the degree of difference between Trollope's concepts and those used by many other workers in geomechanics. His use of an array of systones as the basis for computation of stress-strain behaviour of granular media appears to be quite logical although the language is not familiar.

In the opinion of this Reporter, a paper such as that presented by Trollope tends to lose much of its value (as a conference contribution) due to the absence of other relevant papers. It would be very useful to have other contributions based upon other idealised systems of arrangements of particles so that comparisons could be made of the relative significance of various concepts. This point may be covered in discussion.

Another quite different paper to this topic is that of Nelson and Siu on 'The Influences of Structure on the Dilation of Clay'. In earlier comments a critical line has been taken towards the emphasis placed over the past several decades on the structure achieved in laboratory samples of clay and the effects of such structure (or the processes involved in producing a presumed structure) on the properties of such laboratory specimens.

Although this critical opinion is strongly maintained - if consideration is being given to the effects of real structures in real soils and rocks - there is obviously still a need for fundamental studies of the reasons for specific stress-strain behaviour patterns in all materials. Nelson and Siu's paper adds significantly to this area of knowledge.

The fifth topic 'Measurements of the real values of defect-structure-dependent physical properties of soils and rocks' received valuable impetus from the papers by James and Marsland.

James has contributed a useful review of the excellent work which has been undertaken at Imperial College and has added some limited information on observations and tests at the Maroon Dam site in Queensland. Perhaps the principal value of his paper lies in his demonstrated awareness of the importance of defect structures and in his strong recommendation that adequate studies of such defects should be made in advance of construction.

Marsland has submitted an exceedingly useful paper in which the principle has been adopted of adjusting the scale of measurement of physical properties to the scale of the defect-structure pattern. Although some data are given to define the structure of the clay this information is rather scanty and there is no connection stated (in terms of a principle or a mathematical model) between the actual defect-structure pattern and the actual scale adopted for testing.

The suggestion therefore is that engineering judgment must be the basis for selection of the scale of the test procedure. Within the limitation imposed by this arbitrary process it is evident that the increase of dimension of the stressed zone in any test provides data of increasing reliability.

Marsland's procedures in fact provide an excellent 'engineering solution' to the immediate problem of providing valid stress-deformation in soils. The technique may be less applicable in rocks due simply to the further increase of dimension and load which could be required.

The title of the remaining paper, that by Kotowicz and Kiek, suggests that it could be pertinent to this topic. However, despite the title no evidence is adduced to define the structure or structural changes in the soil or to suggest that there is any relationship between a measurement procedure and any definable soil structure. The other points in this paper - relating to variations of the optimum moisture content in a compacted sample could best be discussed as a separate issue.

In reviewing the papers as a whole it is evident that since contributions have been made to the five topics listed above there must be some research activity in each aspect of the total subject. However, it is also apparent, unfortunately, that almost all of these studies are proceeding independently and without any real chance of cohesion.

It is the firmly held opinion of this Reporter that real progress will only be made in this complex subject if all aspects of the study can be fully integrated, i.e. if observations, explanations, quantifications and representations of defect-structure

patterns in a selected soil or rock can be coordinated in an engineering context and if the further stage of measurements of the real values of defect-structure-dependent physical properties can be made on the selected soil or rock for comparison with the predicted values based upon the behavioural model used in representing the material.

Such integrated studies are necessarily costly but in terms of cost-benefit ratios they may represent excellent value. The possibility of collaborative integrated studies on this topic should not be ignored and in the opinion of this Reporter should be considered as a challenge for the immediate future.

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- D7. MATTSON, S. - The Laws of Soil Colloidal Behaviour. (Pts. I to XXI incl.), Soil Science, 1929-1940, Vol. 28, pp. 172-220, to Vol. 49, pp. 109-153.

Paper by M.S. KOTOWICZ and S.N. KIEK:

Discussion by J.W. HILF:

The complete absence of data on dry density, geologic origin, and clay mineralogy of the soils makes it difficult to diagnose the causes of the changes in optimum moisture content with different initial moisture contents and with different curing times that were reported by the authors. However, it has been known for many years that there are two major causes of change in Proctor compaction values in soils; (1) the progressive change of gradation from coarser to finer (breakdown) of soils containing friable particles under repeated compaction and (2) the presence of certain types of clay minerals, particularly halloysite, which undergo irreversible changes in properties due to drying.

Breakdown:

The effect of breakdown on the interpretation of

field compaction results was reported in 1957 by Hilf (Ref. D9):

"The validity of the field-density-test procedure depends on no change in the gradation of the compacted soil after it is removed from the fill, reworked, and recompacted in the cylinder. For soils that were transported over great distances by wind or water, disintegration is usually minor. On the contrary, some residual soils continue to break down into finer gradations on repeated compaction. Finer gradation would tend to decrease the cylinder densities of such soils.

"The breakdown of the particles results in a higher optimum water content for the soil. This is reflected by an increase in the difference between the water content of the fill (which is independent of particle size) and the laboratory optimum determined in the field-density-test procedure".

The authors have discounted the breakdown phenomenon as a cause by stating on page 161:

"The soil was taken from the impervious core of the dam after compaction by an average of eight passes of a heavy sheepsfoot roller. Thus, it was ensured that breakdown of soil particles would have already occurred before laboratory compaction tests".

It is unfortunate that this assumption of no further breakdown was not checked by gradation tests before and after the final compaction tests.

Let us examine further the effect of breakdown on the interpretation of compaction control tests. If a cohesive soil in a compacted fill is not susceptible to breakdown and a compaction control test indicates that it is at Proctor laboratory optimum moisture content and that the dry density of the fill is close to Proctor maximum laboratory density, we can expect the behaviour of the fill to conform with predictions of compressibility, shear strength, and pore pressure development based on laboratory test values. However, if the soil is susceptible to additional breakdown on recompaction, as many residual soils are, compaction control test results indicating that the fill is at optimum moisture content and close to Proctor maximum laboratory density would be misleading in that the soil in the compacted fill would actually be somewhat wetter than optimum and might very well develop higher pore pressures and lower shear strength than anticipated, as was reported by the authors to be the case at Talbingo Dam.

Moisture control tests (such as a 3-point rapid compaction test) made on the loose layer of fill prior to compaction would predict the correct relation to moisture content in the compacted fill to its optimum moisture content. Hence, when density control tests made after field compaction indicate that the soil is consistently drier than indicated by the moisture control tests made prior to compaction, breakdown of the soil should be suspected. In any event, whenever residual soils are used in major earth embankments the implications of breakdown on the interpretation of control test results should be carefully investigated by means of a series of gradation tests.

Initial Moisture Content:

With respect to the effect of initial moisture content on the optimum moisture content of certain soils, Walker (Ref. D12) summarized the results of investigations of the weathered Weaverville gravels in borrow material for Trinity Dam. More complete data on these materials were reported by Hilf (Ref. D9) as follows:

Weathered Weaverville Formation:

"This material is an ancient gravel deposit which has undergone decomposition from the surface downward. The upper, more weathered portions have been completely decomposed into a brownish-red, fine-grained, nonplastic soil containing remnants of rounded friable gravel particles. The average grain size distribution obtained from mechanical analysis was:

Clay-silt fraction (minus No. 200 sieve)	61%
Sand (200 sieve to minus No. 4 sieve)	20%
Gravel (No. 4 sieve to 3 inches)	19%

"However, many of the sand and gravel particles were friable, and excepting for the less weathered soil at great depths, or along erosional cuts, these particles could be expected to break down into silt sizes by normal construction operations. All of the samples tested, plotted well below the 'A' line on the plasticity chart. Characteristic values for the completely decomposed gravels were:

Liquid limit	52%
Plasticity index	12%

which makes the soil fall in the MH, or elastic silt group of the Unified Soil Classification System".

About 25% of the soil consisted of the clay mineral halloysite. Average compaction characteristics of this soil were Proctor maximum density 83.7 p.c.f. at optimum moisture content of 33.6%. The specific gravity of grains was 2.88, $\tan \phi$ was 0.46, cohesion 16 p.s.i. at optimum moisture content, and 3.5 p.s.i. after saturation. At 300 p.s.i. applied pressure, volume change was 12% in the drained confined compression test. Average permeability was 0.4 ft. per year.

Normally, laboratory tests are performed on specimens prepared by adding moisture to materials that have been air-dried during storage and shipment. When the results of several tests in the laboratories of the Bureau in Denver indicated that the optimum moisture contents of the samples were very much lower than the natural moisture contents reported by field personnel, an investigation was made. A sample was obtained from a road cut in the Weaverville formation, sealed in a plastic-lined bag, and tested in the laboratory for compaction characteristics with and without predrying. Fig. D1 indicates a 4% reduction in optimum moisture content and an increase in maximum laboratory dry density of 3 p.c.f. resulting from air drying which are typical values. Because of the presence of the halloysite mineral and its high compressibility, the Weaverville formation soil was not used in the construction of the 537-ft. high Trinity Dam.

Terzaghi (Ref. D11) reported that Sasumua Dam,

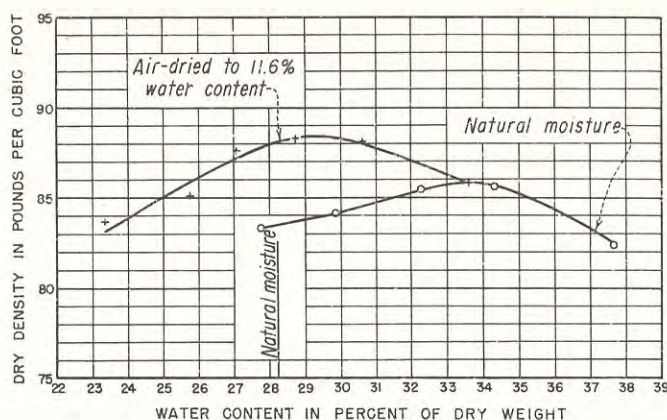


Fig. D1. - Effect of Air Drying on Compaction Properties of Soil Containing 25% Partially Hydrated Halloysite

Nairobi; Tjipanoendjang Dam, Java; Lumot Dam, Luzon; and Silvan Dam, Australia, were successfully built of soils containing appreciable quantities of halloysite which exhibited unusual properties. Walker (Ref. D12) included data on the Fena River Dam, Guam, which was successfully constructed despite soil with Proctor values of 62 p.c.f. and optimum moisture content of 54%. This soil consisted of 75% halloysite mineral.

Anomalous Water?

The most interesting finding reported by the authors is that the moisture content of soil samples became progressively smaller during curing periods exceeding 24 hours. Assuming that the soil was inorganic and that normal laboratory precautions were taken to avoid loss by evaporation, the results indicate that some of the water apparently changed its character.

A possible explanation of the authors' finding is the phenomenon of "anomalous water" which was reported by Deryagin (Ref. D8) to form after a period of time in small diameter quartz capillary tubes. Since this water (specific gravity 1.04 to 1.4 and viscosity between that of molasses and heavy motor oil) is reported to boil at temperatures greater than 500°C, the determination of moisture content at 200°C, as was done by the authors, would not include the portion that might have become "anomalous water". The authors did not report the corresponding densities with the changes in optimum moisture contents, hence one can only conjecture whether the complete test data provide any indication of water changing from the ordinary to the anomalous state. It should be noted that the experimental results obtained by Deryagin have been reproduced by other investigators (see, for example, "Polywater", by E.R. Lippincott, R.R. Stromberg, W.H. Grant, and G.L. Cessac, *Science*, Vol. 164, No. 3887, June 27, 1969, pp. 1482-1487). However, there is considerable controversy as to whether the substance formed is actually another form of water or a phenomenon related to impurities. Further study of "anomalous water" with respect to fine-grained soils appears to be warranted.

Conclusion:

The writer does not agree with the conclusion of the authors that construction of trial embankments remains the only means of accurate assessment of soil properties for major dam projects. Laboratory investigations of proposed embankment materials using the standard compaction test can provide adequate design and construction control data for major earth dams, provided that for residual soils the effects of breakdown and irreversible changes on drying are properly evaluated.

References:

- D8. DERYAGIN, B.V., and N.V. CHURAYEV. - Do We Know Water? Priroda (Nature), 1968, No. 4, pp. 16-22, (In Russian. Translated by Joint Publications Research Service (JPRS 45989) Washington, July, 1968. U.S. Department of Commerce, National Technical Information Service, Springfield, Va. 22151).
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- D12. WALKER, F.C. - The Use of Residual Soils in Earth and Rockfill Dams. Proc. First Panamerican Conf. on Soil Mechanics and Foundation Engg., Mexico City, Sept. 7-12, 1959, Vol. 2, pp. 589-656.

Discussion by A.I. LAWRENCE:

The authors have clearly demonstrated with the aid of carefully controlled laboratory techniques the variable nature of O.M.C. of soil, and have stressed the dangers in adopting a unique value of O.M.C. as related to performance of soils in engineering structures.

In the Talbingo study into high pore pressure it was found using rapid (Hilf) method of compaction control, that moisture variation from optimum ($w_o - w_f$) was constant with respect to time after initial moisture conditioning, up to a period of 3 months for the 16 samples tested.

Were any observations made on moisture variation from optimum ($w_o - w_f$) in the study covered by the paper?

Substitution of actual compaction values (Talbingo av. compaction 102% of M.D.D., Blowering av. compaction 101.9% of M.D.D.) in the Hilf one-dimensional consolidation method of pore pressure determination in place of the 98% of M.D.D. used for the Design pore pressure forecast, gave excellent correlation with measured pore pressure at Talbingo and Blowering, where for typical sample @ O.M.C. = 16.2% & M.D.D. = 115.6%, $B = \frac{\Delta u}{\Delta \sigma} = 0.17$ @ 98% of M.D.D. & 1.00 @ 102% of

M.D.D. (for $\sigma' = 50$ p.s.i.). I would therefore query the authors' explanation of pore pressure anomalies at Talbingo and Blowering being due to O.M.C. changes.

It is suggested that the residual soil-insitu weathered granite of Jindabyne in which the mechanical fracturing of the parent rock matrix could produce drastic structure re-orientation is a vastly different soil category to the transported (slopewash) soils of Blowering and Talbingo.

Discussion by J.D. HODGSON:

The test procedure used is not clear when related to Fig. 1 of the paper. If moisture was added from an initial starting moisture content then is it not possible to have starting moisture contents in excess of the optimum as shown in Fig. 1. Perhaps the authors could explain this further.

It has been known for a considerable period that the curing period alters the compaction test results and for many years standard test methods differed considerably on this point. I think most laboratories now cure over-night. The results from this investigation seem to indicate that a fixed time should be used. The authors suggest that the compaction test may need review and it would be interesting if they might suggest either how it might be changed or with what it might be replaced.

One of the problems of compaction control is that the laboratory optimum rarely coincides with the roller optimum and therefore field air voids can vary markedly from laboratory air voids. Pore pressures are determined generally by the volume of air voids and possibly this should be the control used rather than optimum moisture content.

On the question of the moisture content varying with storage time in air-tight containers could the authors say whether a check was kept on the bulk weight of the containers during storage. The writer has found with many supposedly air-tight containers that loss of weight occurs with prolonged storage.

Discussion by E. GERMANIS:

While disturbing fluctuations of moisture content in the field are known from observations, the attempt to trace their cause and the elimination of some of the factors which would probably affect the O.M.C. are of considerable interest and value for dam builders.

The paper illustrated once more the inability of small test specimens to adequately represent the properties and behaviour of large earth masses placed in dams. While this representation may be improved by an increased frequency and by improved methods of sampling, some aspects still are beyond the capability of such representation.

One possible partial reason for the observed variations in the soil moisture contents and the following increase in or reduction of the construction pore pressures at different levels might be the temp-

perature gradients developed in the fill surface layers during placing and the subsequent thermal moisture transfer. This effect might be accentuated when construction extends over several seasons.

A study of moisture movements towards moisture equilibrium using the Thornwaite's Index could be carried out as suggested in the paper. It is doubtful, however, whether any quantitative relations useful for practical application could be found by such study. Local conditions can also considerably change the relations represented by the Index.

Some doubt as to the validity of the standard compaction test has been expressed by the authors of the paper. A change or adjustment of this test to take account of probable variables (e.g. the initial moisture content, etc.) can be considered only after sufficient clarification of the discrepancies associated with the test and their relative significance.

When conditions for cracking the fill are not likely to be important, it seems at present that a generally safer procedure is to compact dam material slightly below the O.M.C.

An ample curing time should be given before compaction to secure a uniform distribution of moisture within the sample and thus to eliminate variations in test results from this course.

As trial embankments are an expensive method for investigation, proper instrumentation should be installed in dams for observation of pore pressure changes during construction. In cases where unexpectedly high construction pore pressures occur, a change in construction programme to allow for some pore pressure dissipation within the fill may help.

Discussion by D.C. GREEN:

The authors are to be congratulated on a very interesting paper. It is my experience that many people engaged in compaction control for various purposes come to regard optimum moisture content as almost an inherent property of the material concerned, qualified usually (but not always!) by recognition of a change in O.M.C. for a change in compactive effort. Reference has been made before to the difficulties of compaction control for residual soils where O.M.C. changes with working due to structural breakdown (e.g. McKenzie, Proc. Third A.-N.Z. Conf., Sydney, 1960; Howard and Holliday, Civ. Engg. Trans. I.E.Aust., Vol. CE10, No 1, April, 1968), but it is interesting to have the authors extend our awareness of the factors affecting O.M.C., even if their conclusions as to the mechanisms involved are incomplete at this stage.

Could the authors elaborate on the following items?

- (a) What is the nature and geological origin of the soil reported in the paper?
- (b) Do the authors consider the phenomena they have observed to be generally applicable, or applicable only to a certain class of soils - say, residual soils?
- (c) What changes in maximum dry density accompany changes in O.M.C. of the order they have described?

- (d) Reference was made to the extreme sensitivity of economic and engineering considerations to changes in moisture content (p. 164). Could the authors quantify this comment briefly for the case they have reported?

The Authors in Reply:

To Dr. J.W. Hilf:

Geology and Mineralogy:

The authors regret that the data on geological origin and mineralogy of the soils was not included in the paper. The geological origin of the soils were as follows:

Jindabyne: Residual decomposed granite.

Blowering: Slopewash and alluvium were derived essentially from phyllite and quartzite with minor amounts derived from porphyry and tuff.

Talbingo: Andesitic basalt slopewash. Staining tests were carried out on the clay fraction of the Talbingo soils which indicated that about 30% consisted of the expanding lattice type clay minerals, montmorillonite and degraded illite, with the remainder being the non-expanding minerals, kaolin and illite. Traces of vermiculite and pyrite were also identified.

Talbingo soils were also examined by X-ray diffraction (by courtesy of Mr. H. Berry of the Australian National University). The results indicated a chlorite-feldspar assemblage containing mainly plagioclase, chlorite, hydrated iron oxides and occasional clay particles in nodular form together with small amounts of calcite, kaolinite and epidote. Halloysites were not detected.

M.D.D. and Breakdown:

The results of the investigation testing described in the paper showed a strong relationship between M.D.D. and the initial moisture content. However, this relationship followed a very pronounced maximum value curve shaped like a normal distribution curve and could not be considered comparable with the O.M.C. curve (Fig. 1). As a result the authors felt that the effect of initial moisture content on M.D.D. warranted a separate study.

The effect of the breakdown of the soils either during rolling or during laboratory compaction was initially considered and eliminated. The authors were familiar with work by Hilf (op.cit.) and many others, and evaluated this phenomenon with the greatest care. Further breakdown, i.e. that occurring during laboratory compaction of soils obtained from rolled earthfill was indeed checked by sieve and hydrometer analyses after each test. No further breakdown was observed. It may be added that the original laboratory investigation testing also followed the

standard practice of simulated breakdown. In order to detect anomalies, field control testing was carried out under close scrutiny with moisture contents from 3-point moisture tests and field densities being continuously checked and compared.

Thus, in the absence of any evidence of soil breakdown the authors must contest the statement that results obtained from Proctor compaction tests on soils not susceptible to breakdown will necessarily predict the behaviour of fill compacted to similar values.

Initial Moisture Content:

As mentioned above, halloysite was not detected in Talbingo soils. At Jindabyne, tests were carried out on oven-dried soils (Hilf, Terzaghi and Walker, op.cit.) but the results did not indicate any abnormality nor the presence of halloysite in detectable quantity.

Anomalous Water:

The authors were aware of the extensive literature on 'polywater' or 'anomalous water'. Were its protagonists more completely convincing the subject would bear further investigation in connection with the unexplained changes in moisture content observed. Its occurrence would certainly provide a possible explanation.

Conclusion:

The authors dispute the view that breakdown and irreversible changes on drying, if properly evaluated, will exonerate the shortcomings of the standard compaction test as reported in the paper. These factors were fully evaluated and found to have no detectable influence on the fluctuations of O.M.C.

To Mr. A.I. Lawrence:

The results mentioned in the discussion were included in Whatley's paper and were accepted by the authors without repetition. However, the effect of $w_0 - w_f$ on O.M.C. was studied further and a highly significant relationship was found, except where $w_0 - w_f$ was small.

The authors agree that the use of actual field density, moisture and observed consolidation values gave good correlation values with measured pore pressures (calculated using the Hilf method), particularly in the initial stages of construction, i.e. for relatively low stresses imposed. However, the fact remains that the actual fill moisture contents were much higher than those used in design because of considerable O.M.C. variations. Reduction in the density of compacted fill did not produce a noticeable reduction in pore pressure, but reduction in placement moisture content by an amount equal to calculated difference in O.M.C. produced immediate and appropriate results. It is quite obvious that pore pressure anomalies at Talbingo were solely due to an apparent increase in O.M.C. in field testing.

Whilst there are many differences between residual

and transported soils, both the Jindabyne and Talbingo soils showed a discontinuity in the shear strength - moisture content relationship near O.M.C. which could be interpreted as a structural change. However, this discrepancy, which was checked by electron micrograph, was not responsible for the observed moisture changes.

To Mr. J.D. Hodgson:

The soils were tested at a wide range of initial moisture contents. The authors disagree that it is impossible to have a starting moisture content in excess of O.M.C. The compaction test, by definition, requires the location of density-moisture points on both sides of O.M.C. to determine the M.D.D. - O.M.C. intercept.

Curing time, as mentioned in the paper, seemed to reduce somewhat the O.M.C. fluctuations. The authors feel that a minimum fixed curing time, if adopted, should be first established for any given soil.

As mentioned in the paper, the compaction test, or rather its hitherto unquestioned acceptance, may need a review. A review of factors (other than maximum size, compacting effort, etc.) affecting the two basic resultants, O.M.C and M.D.D., is in the authors' opinion necessary. Until those factors are evaluated either empirically or by theory, there seems to be little point in discounting the test altogether, though in the authors' view, our present understanding (and acceptance) of the results as such, could be grossly misleading.

With regard to the comment concerning the roller versus laboratory O.M.C., this confirms the authors' suggestion that where time and expense warrant it, trial embankments should be used. The authors do not see any advantage in using the volume of air voids rather than moisture for pore pressure prediction as the two are mathematically linked anyway. Finally, a close check was kept throughout the investigation on the bulk weight of the containers during storage. No loss of weight was detected.

To Mr. E. Germanis:

The authors agree that a change or empirical adjustment to the standard compaction test can be considered only after sufficient clarification of the discrepancies associated with the test. However, they feel that determination and elimination of factors which affect the O.M.C., whilst of considerable interest to dam builders, are of more fundamental value in research work, as affecting our understanding of M.D.D. - O.M.C. concept.

With regard to the importance of the cracking of a brittle fill, the authors consider that placement of core material wetter of O.M.C. is still both preferable and safer. However, the full discussion on the merits of such practice is beyond the scope of this reply.

The authors agree that ample curing time should be allowed before compaction to reduce O.M.C. variations but this, unfortunately, is not always possible in field work.

The suggestions that moisture transfer on account of temperature gradients in the dam could be respons-

ible for the moisture changes observed in the dam cannot be verified in the absence of the required temperature measurements. However, this would not explain the similar (although smaller) movements of moisture content in the laboratory stored samples, always in the same direction as in the dam.

To Mr. D.C. Green:

Query (a) is answered in reply to Dr. Hilf.

Query (b) is, of course, asking what can be at best an opinion only.

The authors have a large number of test results on three soils only; two transported and one residual. It is not known whether the findings apply to other soils but the greatly differing parent materials make it appear possible that these moisture and O.M.C. changes may be universal.

Query (c) is answered in reply to Dr. Hilf.

Query (d). It should suffice to say that in the case of a large earth dam, for example, higher than anticipated pore pressures could affect the stability of the structure. To remedy this, construction may need to be interrupted for a period sufficiently long to allow dissipation of the excess pressures or structural changes (berms, flattening of slopes) may be necessary. In either case, the cost of doing this for a dam of Talbingo size could be very significant.

Paper by D. LAFEVER and D.R. WILLOUGHBY:

Discussion by P.L. NEWLAND:

A perfectly oriented assemblage either of slightly flattened spheres or of discs would appear as a line in a plane containing the shorter axis when plotted on a polar diagram such as in the authors' Fig. 4. Intuition suggests that, if the properties of materials in different directions are indeed affected by anisotropy in the shape of the particles, the effect will be minimal in the first case and most marked in the second. Thus, some measure of the degree of anisotropy in shape of the particles would seem to be a necessary adjunct to the polar diagram in order to assess the significance of the orientation of the particles.

The authors do not present any quantitative measure of the shape of the Portsea sand grains but the impression gained from Figs. 2 and 3 is that they are not strongly anisotropic. In addition, fabric factors delineating the bedding plane are not particularly evident, nor is it at all easy to see how such factors might influence the stress-strain properties or the orientation of the failure planes.

It would therefore appear reasonable to anticipate that the fabric or structure of the Portsea sand would exert a weak influence only on the stress-strain properties at low strains and probably no discernible influence on the orientation of the failure planes since strains necessary to produce observable failure planes might be large enough to destroy the initial structure for the very reason that it is weak.

Examination in the authors' laboratory of the actual samples after testing revealed several significant features. Unlike the diagrams in Fig. 6, the central zone between the failure planes had undergone large lateral strains while the end zones were relatively unaffected. These observations pointed strongly to the existence of end restraint which is not surprising since standard ceramic end plates were used. Furthermore, the angle of the failure planes indicated a drained angle of shearing resistance as low as 22° . This is likely to have been due to the use of carbowax as the pore fluid.

It is difficult to counter the argument that the above factors exerted a strong influence on the location and orientation of the failure planes; strong enough indeed to completely mask any effect due to the original weak fabric. A further contributing factor is that drained tests were carried out so that volume changes are likely to have occurred during the course of the test.

The authors concluded from their observations that the original fabric does indeed control the behaviour of the sand. In view of the above discussion, it would be interesting to have the authors' comments. It would help in assessing their conclusions if they presented actual measurements of the orientation of the failure planes to substantiate the diagrammatic representation of these in Fig. 6, as well as the stress-strain curves from which the secant moduli were derived. It would also be of help if they presented available data on the initial relative density and the volume changes during testing as well as any observations of the fabric after stressing, particularly at the point when the failure planes first became visible.

The Authors in Reply:

As emphasised in the summary the paper is primarily based on symmetry considerations, where factors such as the nature of the pore fluid, relative density, and end restraint are not relevant unless they affect the symmetry of the experimental arrangement. The argument involving the shape of the Portsea sand grains is altogether missing the point under discussion, namely that of the relationship between the anisotropy of mechanical properties and the geometrical anisotropy of an assembly of particles, and not of individual particles. Elementary considerations already indicate that the latter two are not necessarily identical or even similar. This is demonstrated simply by the fact that a single perfect sphere has a symmetry completely different from an ordered assembly of perfect spheres (vide symmetry axes!), even if the external boundary surface of the latter is not taken into account. The pore fluid (molten Carbowax) is isotropic, the relative density is a scalar, and the end restraint possesses axial symmetry, whereas the deformation pattern shows only monoclinic symmetry that is strictly and simply related to the symmetry conditions present at the sampling site. In other words, the pore fluid, the relative density and end restraint have no influence on the symmetry of the mechanical properties. Their possible influence on the magnitude of these properties is irrelevant in present context. In view of the observed fact of the strikingly identical symmetrical arrangement of the sample failure surfaces in regard to the known position of the coast-

line, there is no room left for intuition and/or anticipation of what may or may not happen as a result of theoretical assumptions and considerations.

Paper by D. H. TROLLOPE:

Discussion by A.K. PARKIN:

Several algebraic errors are noted in Section VI, concerning the strength of the ideal sphere system. Eqs. (6.2) and (6.3) should, respectively, read as under:

$$\frac{\sigma_1}{\sigma_3} = \frac{3 \sqrt{3 + 4 \tan^2 \phi_\mu} + (\sqrt{3} + 4 \sqrt{2} \tan \phi_\mu)}{3 \sqrt{3 + 4 \tan^2 \phi_\mu} - (\sqrt{3} + 4 \sqrt{2} \tan \phi_\mu)}$$

$$\tan \phi = \frac{\sqrt{3} + 4 \sqrt{2} \tan \phi_\mu}{2 (\sqrt{6} - \tan \phi_\mu)}$$

Concerning Eq. (6.1), quoted from Rennie (Ref.2), it is noted that the equation, as originally published, contains an error, which may become significant for higher values of ϕ_μ . The equation should read:

$$\frac{\sigma_1}{\sigma_3} = 2 + 2\sqrt{6} \tan \phi_\mu + 7 \tan^2 \phi_\mu + 8 \sqrt{\frac{2}{3}} \tan^3 \phi_\mu$$

+ higher terms.

TECHNICAL SESSION No. 5—DRILLING AND BLASTING

- "Application of Oriented Drill Core in Structural Geology at Mount Isa", by M.C. Bridges & E.J. Best.
 "Damage Induced in a Smooth Work Piece by a Sliding Diamond - an Approach to Hard Rock Drilling", by J. Graham.
 "Some Basic Aspects of Diamond Drilling", by D. Rowlands.
 "Reduced Soil Strength and Stiffness at the Top of Tube Samples", by J.G. Lang.
 "Model Studies of Fragmentation of Explosives", by G.D. Just & D.S. Henderson.

GENERAL REPORTER - Mr. M.D. HUGHES:

It is good to note that Geomechanics is being used to see what can be done to make drilling and also blasting more efficient. This is the main contribution from the work described in the papers at this session. Drilling and blasting are expensive mining operations. Drilling is expanding its scope as it embraces raise and tunnel boring. The field is a large, complex and exciting one requiring work in several aspects.

Drilling is also a tool of the structural investigator and our session also covers this aspect mainly in the first paper, which is about oriented core at Mt. Isa.

Before we go looking through the microscope at the papers, let us first look through the telescope at the field.

1. Classification. - Usually if there is a drilling problem ahead, whether for a diamond drill contractor quoting a job or an engineer appraising likely cutter costs for a raise borer, the first question is "show me a typical piece of rock". At present this question is meaningless, but the problem is capable of analysis. Whether it is worth it for the sake of the odd drilling contractor is another matter. Some means of classifying rock on the basis of its drillability by various methods is desirable.

2. Structure. - Now, even if there is a completely uniform rock-type in a mine, it will not necessarily drill or blast uniformly because of the effect of structural weaknesses, such as joints that might give short runs to a diamond driller or which might allow easy fracturing by a raise borer or might give excellent fragmentation to a blast. I wonder if the structural work done for other purposes may help drillers and blasters.

3. Drillability. - Now, knowing what the workpiece is, how do we drill it efficiently, quickly, straight enough and with minimum cutter cost or maximum cutter life by various methods, either filing with diamonds, chipping with jack hammers, gouging with button bits, etc? Can we use additives equivalent to a steel turner's soluble oil to increase our efficiency?

4. Blasting. - If the holes are blast holes, should there be a lot of thin ones or a few wider ones? What pattern is right? How full should they be of explosive? What is the best explosive?

5. Scale of Tests. - All testing can be done in a

workshop, on a tombstone size block or an actual drilling or blasting situation. Correlations between the three techniques are desirable.

So much for our look through the telescope. Let us see what our authors have done.-

1. The Mt. Isa paper tells us how an application of drilling helps structural investigators.
2. The paper about a sliding diamond takes us right back to fundamentals.
3. The paper on basic aspects takes over somewhere about where the previous one leaves off and gives some interesting summaries on diamond drilling theory and describes how penetration rate, torque and specific energy varied during some tests.
4. The paper about samples tackles a specific and relatively simple problem and leads to a definite answer to it.
5. The blasting paper was based on being able to predict the size distribution of blasted rock.

So the papers range widely in their subjects and their scope.

Some of the papers spark off many conjectures and questions, as well as comments. Others treated a limited field and do not lead to much extension beyond themselves.

Now let us look briefly at the papers and their implications:

The paper by Bridges and Best comes from Australia's largest underground mine and it is only fitting that they should be our leaders in structural underground examinations. The authors say that they drill core and orient it and lay it out as oriented and then make many measurements of structural defects, their extent, frequency, continuity, cohesions, etc.

This provides a tremendous amount of data which they classify and analyse on stereograms, etc.

They give examples of an investigation of a crusher station excavation and a slope wall to illustrate how they applied their techniques.

They conclude with a statement which all mine structural men might do well to heed. One of their examples "demonstrates the necessity for probing the rock mass rather than only mapping nearby development and assuming rock conditions will be the same. In this instance, the distribution of defects in the rock mass changes markedly four times within 220 ft."

Questions on this paper range more over the whole problem of analysing the structure of a large mine rather than following the paper too closely.

Here are some ...

- (a) Is it necessary to examine the structure of all potential stope walls at Mt. Isa and all major openings? If so, is a pattern emerging which simplifies the task with time?
- (b) Is the appraisal a continuing task like mineral drilling?
- (c) For stopes, can general figures be quoted indicating how many tons of ore are proved by each foot of mineral drilling and how many tons of ore are structurally investigated by each foot of structural drilling?
- (d) What is the order of the additional cost of oriented core compared to mineral core?
- (e) Is there a future in examining mineral holes with cameras or periscopes?
- (f) What steps were taken to ensure the stability of crusher excavation and the stope wall quoted as examples?

Mr. Graham aimed to extend previous work in this field to embrace the effects of various lubricants

of diamond radius
of different loads on soda glass, silica glass and quartz.

He concluded that even from such a simple fundamental test as this, no broad generalities could be established. He noted that bentonite as an additive seemed to induce more fracturing than other additives. This is odd, because bentonite is used to avoid scouring rather than to improve cutting.

Diamond drillers talk of a maximum figure of 12 lb. per diamond point as its working load, regardless of radius or diamond size, although such diamonds have a diameter of 1 to 2 mm. Some called "hard core" or "processed material" are rounded like river gravel and are something like spherical. Hard core diamonds are claimed to be thoroughly polished in addition.

Here are some questions ...

- (a) Once the apparatus has been prepared can this method give quick, inexpensive results?
- (b) Why did the author choose two kinds of glass for study rather than natural material?
- (c) Would the author comment on the early part of Mr. Rowlands' paper including Fig. 1. - the theory of how a diamond cuts, diamond exposure and size,

bit thrust, etc?

- (d) Why did the author use spherical diamonds? Rowlands refers to the sharp points of octohedrons.
- (e) Did the author observe the blunting of the diamond?
- (f) What happens as the load on a diamond is increased?
- (g) What mechanism is responsible for pulling apart the fragments of Fig. 8. and getting them away?
- (h) Can the author see why soluble oil or other "hardness reducers" might be effective?

Turning now to the paper by Rowlands, a diamond driller cannot move too many ways to improve his penetration. He can vary -

the cutting tool,
lubricant (soluble oil, etc),
bit speed,
axial thrust,
rate of flow of circulating fluid.

As he does this, the rate of penetration, size of cuttings, torque and specific energy and blunting of the bit will vary.

After examining some basic diamond drilling beliefs, the author carried out a series of tests to show what happens.

Bit Speed. - Tests showed that with other things constant, penetration rate is directly proportional to bit speed and that torque, and hence specific energy, are independent of bit speed. This fortunately eliminates bit speed as a complicating factor. By the same token, it indicates that the faster we spin the faster we advance. There must be some factors to guide us in the upper limit to this. The author might comment as to what they are.

Axial Thrust. - Then the tests showed that as axial thrust is increased drilling becomes more efficient than we might at first expect. This may be because there is a minimum thrust below which only elastic deformation occurs. A graph of penetration rate vs. thrust at constant r.p.m. (say 1,000) would illustrate this and it would be good if the author would draw it.

Specific Energy. - The figuring showed that specific energy is proportional to $\frac{\text{bit speed}}{\text{penetration rate}}$ which is a term which can be expressed in turns per inch.

Underground diamond drillers often use screw-feed machines in which bits advance a set number of turns per inch and they usually have a choice of four feed gears fitted to the head; so these feed gears set the specific energy.

Incidentally, the gears in a drill are commonly 200, 300, 400 and 500 turns per inch for drilling rock. It is noted on Fig. 4 that at a thrust of 404 lb. the feed rate was about 35 turns per inch which is an almost unheard-of feed in igneous rock.

Bit Wear. - The author refers to wear on bits and his

method of correcting it. Could he draw a graph of penetration rate under standard conditions vs. total drilling done by bit to show us how blunting affected drilling? Did he make any observations of flats on the diamonds? We would hope that efficient drilling with low specific energy goes hand-in-hand with reduced blunting. Is there evidence of this? Is the author's correction to sharp bit performance likely to provide a set of information which holds for the special case of a new bit but not for the more general case of a bit somewhere between new and blunt?

Practical Results. - Much EX drilling in mines is done at bit speed around 1,000 r.p.m.

Torques up to 50 lb. ft.	(Paper around 10)
Thrusts to 3,000 lb.	(Paper around 450)
Feeds around 300 turns/inch	(Paper around 30)
Machine H.P. around 16	(Paper 2 H.P.)

These seem to be very different.

Hardness Reducers. - Has the author done work on the effects of hardness reducers?

The paper by Lang is the case of an investigator with a specific and limited problem to which he got a conclusive answer, but which did not open readily into wider fields.

The author noted that when he used a 3-in. auger soil sampler and took runs either $1\frac{1}{2}$ or 2 ft. long, he got consistently different results from the top half of the samples from those from the bottom half. So he set out to see why and which half was right. He did sets of readings taking long samples and compared results to sets of readings with short samples.

He got much more conclusive agreement than common when analysing the earth. He showed that the bottom half of the long sample was close to the strength and stiffness of the short sample and that the top half was weaker:

When a sample of this nature is taken, not much force is needed initially but the force to penetrate further increases with extra length of sample, so that the bottom half of the sample is taken with quite a high force which might be expected to disturb things and give a false reading.

The author said that it did disturb something - the zone immediately below it. This formed the top of the next sample and this was weakened by the disturbance from the preceding sample.

The author aimed to check his conclusion by taking a long sample and then taking steps to clear away any material which it disturbed and then taking another sample.

We only have one paper on the complex operation of blasting which has all sorts of variables and which is subject to changes as we use jumbo drills, large carrying vehicles and ANFO instead of air legs, chutes and gelignite. The paper by Just and Henderson tackles an interesting aspect in that it aims to give data to predict the size distribution resulting from a blast.

The authors used models and they worked in dimensionless constants proportional to the optimum depth,

i.e. the depth of charge which yields the largest crater.

They achieved some neat relationships between some of the blasting variables which ought to help simplify design of blasting. Here are some examples ...

The authors found logarithmic size distributions for different depths of charges and they illustrated this in Fig. 2. The slopes of the lines in Fig. 2 are called "fragmentation gradients". Then they plotted the fragmentation gradients against depth ratios (the ratio of charge depth to the optimum depth) to log scales and got a straight line.

The work was done with both concentrated and linear charges using concrete and sandstone blocks. In due course, full-scale tests will be carried out.

Here are some questions ...

- (a) Whatever breaking is not done by the explosion will have to be done by the primary crusher. What are the relative costs of breaking 4-ft. blocks into, say, 6-in. blocks by the two methods?
- (b) The paper treats the toughest job for an explosion, in that it does not include breaking due to falling, or for collisions of rocks from adjoining blasts or of structural weaknesses in the rock. Can the authors give some examples on the effect of those factors on size distribution?
- (c) Is the rock close to a concentrated charge shattered more than rock which is close to the free face?
- (d) Would blocks have to be significantly bigger to get meaningful results from model tests with ANFO?
- (e) Is this work fairly fast, giving plenty of results for a day's work?

Paper by M.C. BRIDGES and E.J. BEST:

The Authors in Reply:

- (a) The geological structure of the rocks around our bedded lead-zinc orebodies is believed to be strata controlled, that is, relatively uniform along strike or down dip but variable across dip. Each orebody is relatively uniform in structure but each is different from others. Ideally, it is only necessary to drill into each orebody at several points in a stoping block to establish this uniformity. Special features, such as folds, need to be individually investigated.

Copper orebodies are enclosed within a massive rock whose structure is fairly uniform throughout. A few thousand feet of drill core can be applicable to a large area such as a 1000 ft. cube.

In all cases sufficient drilling is done to establish uniformity or regular variation of structure.

- (b) Yes it is. As new mining areas are opened up along strike and down dip, structural geology investigations, including diamond drilling, are undertaken. However, as more becomes known of an area, both structural and mining, the intensity of structural investigation decreases. New techniques of drilling and of analysis and presentation of data are continually being investigated.

- (c) For a stoping block the percentage of exploratory drill core which is structurally logged depends on whether there has been mining experience in this rock and whether there are development openings in the area.

At the proposed Hilton mine, about 14 miles north of Mount Isa, there are no development openings as yet. In an initial mining block about 33% of exploratory diamond drilling was structurally logged, so that, overall, each foot of logged core investigated about 15,000 tons of ore.

In another orebody with no exposures we logged about 39% of exploratory drill core. A third area has been mined up dip and there are some exposures in the exploratory block and we plan to log about 15% of core. In an operating copper orebody, with many exposures, the amount is less than 1%.

- (d) Oriented drill core of good quality costs approximately double that of usual exploratory core. It also costs more to log and process the data.
- (e) In most underground mining situations the core provides all the information that is required from the drill hole. Borehole cameras and periscopes are not thought worthwhile at Mount Isa. There may be a future for down-the-hole geophysical surveys. In special cases, where there are thought to be open cracks, then these down-the-hole devices may be worthwhile. However, there is no substitute for the recovery of good quality core.
- (f) The planned orientation of the crusher excavations was such that the principal set of fractures would be parallel to the largest walls. During excavation there would tend to be slabbing from these walls and support would be costly. As a result of the exploratory programme, the excavations were turned through an angle of 20° to lessen the possible slabbing. The investigation enabled an effective rock bolting pattern to be drawn up. The structural information was incorporated in a plaster model which was constructed and tested at the Australian Coal Industry Research Laboratory at Wollongong.

Production has been deferred in the stope whose hanging wall was investigated.

Paper by J. GRAHAM:

The Author in Reply:

Experiments and theory applicable to drilling range from the friction results of Bowden and his school, through the static indenter described in

Rowlands' paper, the low-load single diamond work of Schlössin (Ref. D1) and of Tabor et al (Ref. D2), and the "ploughing" experiments of Appl et al (Ref. D3) to attempted instrumentation of a complete drilling bit in the manner described by Rowlands.

The present work was intended to be a bridge between the 1 mm dia. slider used by Schlössin at loads of 2-5 kg (1 or 2 lb), and that of Appl et al, who used a normal force of 1 to 60 lb. to cut limestone.

The optical technique used in the present paper limits the material to glass, or transparent crystals such as quartz or feldspar, and limits the load on a 1 mm diameter diamond to about 10-12 kg (5 lb.). This is a severe restriction, but this range could be more sensitive to variations in the conditions than the more heavily loaded ranges, and the technique has enabled the gathering of new information.

At light loads, Schlössin has interpreted his results on the basis of elastic Herzian fracture; the forward component of stress due to the movement of the diamond prevents the crack opening ahead of the indenter, and a series of semi-circular cracks like those of Fig. 3 (e) is left behind. Tabor et al predicted that plastic behaviour would result in cracks forming in front of the diamond, and this may be the reason for the reverse cracks shown in Figs. 8 and 9. Thus increased normal loads give greater plastic behaviour, inverse cracking, and greater depth of damage. Extending this even further, Appl et al ploughed a furrow in limestone under very heavy loads, and interpreted the mechanism as a plastic deformation, followed by fracture to produce the debris, somewhat like metal turning.

An obvious extension of the present work would be to include higher loads more akin to the loads used in practice. We hope to do this when a technique for evaluating the debris has been developed. In this case, the technique would also be suitable for polycrystalline samples of natural hard rocks, so that the extra variable of intergranular failure can be evaluated.

The apparatus used is simple and inexpensive, and the experiments may be carried out quickly and easily. However, some limitations have already been pointed out, and these make the application to field work very difficult. Spherical diamonds were used so that comparison could be made with previous work and with hardness theory, and because the diamonds used in drill bits are usually rounded as indicated by Mr. Hughes. Absolutely spherical diamonds, however, are not used in drilling as they require higher than normal loads.

The low loads used in our experiments had important effects both on diamond wear and on removal of material from the work piece.

The effect of wear in a bit is not understood, although it is known that wear and fracture of the diamond are extremely important in affecting the efficiency of drilling. According to one theory, wear is associated with the temperature of the interface, high temperatures resulting in graphitisation. A flat worn underneath a diamond will affect the depth of cut, by reducing the local pressure there.

To obtain removal of material with our low loads,

it is necessary to make multiple passes. Schlössin believes this to be a real mechanism for drilling, but if most material is removed by a ploughing action, its effect will be minimal. Even in this case, however, a deep cracking network may form below the fracture surface, which will make the work of the next diamond easier.

Various possible explanations have been advanced for the effect of soluble oils and lubricants. Joris and McLaren (Ref. D4) suggest that the primary function of the lubricant is to keep the diamond cool. Polishing and nickel plating of the diamond have also been attempted to accomplish the same end. Other possible effects of the lubricant are to lower the surface energy of the rock, thus decreasing the energy required to break it; to lubricate all the components of the drill string, and to remove particles as quickly as possible. Another effect sometimes advanced is that of chemical corrosion of the rock near the drill bit, although this seems very unlikely to the present writer in view of the time available. Joris and McLaren say that soluble oils improve all these functions. In view of the excellent results achieved by Rowlands in his paper, I would tend to rate particle removal ahead of the other effects.

We have found almost any lubricant to be effective in drilling glass, and suggest that only where a large volume of work is done would turps or bentonite have any advantage. This would need to be evaluated in the workshop.

I am not competent to comment in detail on Mr. Rowlands' theory, although I think that the shape of the diamond and the presence of a tangential force will profoundly modify the ideas expressed. One theoretical treatment of a diamond bit which takes these points into account is given by Hamilton and Goodman (Ref. D5). Most of the Herzian theory applies to lighter than practical loads, as mentioned in my previous remarks, but one very elegant experiment shows the difference between a normal and inclined force at this level (Ref. D6). A ball bearing is dropped from a height onto a flat surface and the crack shape is observed as a function of the inclination of the surface.

Equation (1) of Rowlands' paper is only meaningful to me if P is the mean penetration per revolution, and this is not simply related to the embedment of the diamonds. The exact mode of cutting of a multi-diamond bit has not been established in detail, and even the number of diamonds contacting the rock is in doubt. In some of his experiments, Rowlands achieved a penetration rate of $\frac{3}{4}$ mm per revolution, and this must mean a very appreciable diamond embedment in the rock.

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- D3. APPL, F.C., ROWLEY, D.S., and BRIDWELL, H.C. - Theoretical Analysis of Cutting and Wear of Surface Set Diamond Cutting Tools. Report of Christensen Diamond Products Company, Salt Lake City, Utah, 1967.
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- D5. HAMILTON, G.M., and GOODMAN, L.E. - The Stress Field Created by a Circular Sliding Contact Trans. A.S.M.E. (Jour Appl. Mech.), Vol. 33, June, 1966, pp. 371-376.
- D6. LAWN, B.R. - Partial Cone Crack Formation in a Brittle Material Loaded with a Sliding Spherical Indenter. Proc. Roy. Soc., Series A., Vol. 299, 1967, pp. 307-316.

Paper by D. ROWLANDS:

The Author in Reply:

Bit Speed. - The upper limit of bit speed is probably governed by the rate of heat dissipation and/or cuttings removal.

Axial Thrust. - Any vertical line through Fig. 4 will give a graph of penetration rate vs. thrust at constant r.p.m.

Specific Energy. - The gear feed rates will indicate the specific energy if the thrust remains constant.

Bit Wear. - Bit wear, in the form of flat areas, was observed on the leading diamonds but it was not measured.

Equation (6) gives a curve of penetration rate against depth drilled for 200 inches of drilling; from such a curve one can standardise for any depth of drilling.

Work on Hardness Reducers is currently being undertaken.

Paper by G.D. JUST and D.S. HENDERSON:

The Authors in Reply:

The relative costs of breaking rock by blasting and crushing will depend upon the nature of the operation. Although these costs are important other unit costs such as drilling, loading and haulage costs should be considered. Very little information is available from Companies on such costs. However, in one large underground mining operation the following relative unit costs have been found to apply—drilling 33%, blasting 8%, loading 32%, haulage 19% and crushing 8%.

With regard to shattering near the hole, it is generally accepted that most of the very fine

particles are produced from this region. The larger particles are produced from the region near the collar of the hole. It is recognised that structural weakness planes will have a significant effect upon the degree of fragmentation. Model tests have been planned to investigate this phenomenon in detail.

Model testing is a slow process and field testing on even a moderate scale is even more costly and time-

consuming. However larger-scale tests are essential due to the lack of knowledge regarding scaling factors as applied to size distributions. A limited number of such tests have been conducted.

In conclusion it should be noted that the prediction of size distributions from blasting may also be relevant to Civil Engineering operations where a particular grade of rock fill is required.

TECHNICAL SESSION No. 6—FOUNDATIONS

"An Analysis of Pile Loading Tests in a Stiff Clay", S.B. Bromham & J.R. Styles.

"Model Tests on Piles in Clay", N.S. Mattes & H.G. Poulos.

"A High Capacity Load Test for Deep Bored Piles", J.D. Moss.

"Analysis of the Movements of Battered Piles", H.G. Poulos & M.R. Madhav.

"The Bearing Capacity of Strip Footings from the Standpoint of Plasticity Theory", E.H. Davis & J.R. Booker.

"Uplift Testing of Prototype Transmission Tower Footings", R.J. McKenzie.

"Stresses Beneath Granular Embankments", I.K. Lee & J.R. Herington.

GENERAL REPORTER - Prof. P.W. TAYLOR:

Of the seven papers presented at this session, five are concerned with piles, one with spread footings and one with embankment stresses. Your Reporter considers that this emphasis on piled foundations is desirable for, despite the fact that civil engineers have employed piled foundations for centuries, estimates of load deflection relationships and ultimate bearing capacities are still subject to a wide margin of error. For this reason, high 'factors of safety' are commonly used. More exact knowledge will undoubtedly lead to more economic design. The three papers dealing with full-scale loading tests will be considered first.

That by Bromham and Styles is of considerable interest as it attempts to compare three methods of estimating ultimate bearing capacity with results of field loading tests. Concerning field and laboratory

soil tests, it would be of interest if the authors could give the liquid limits for the soils tested and a definition of 'friction ratio' for the penetrometer. Perhaps the authors could state also whether or not 'N-values' were corrected for vertical effective stress (Ref. D1) and whether lubricated end platens were used for the triaxial tests on samples with a height/diameter ratio of unity. Referring to Fig. 5, showing time-dependent deflection under constant load, it is difficult to accept the statement that "secondary consolidation effects begin after about 30 minutes". One would expect the start of secondary consolidation to be indicated by a reverse curvature, as shown in Mattes and Poulos' paper (Fig. 2) for model piles.

In the pile loading test at 38 ft. depth, failure is clearly defined but, for that at 76ft., the load was still increasing rapidly at 0.35 in. deflection and while the elastic limit may have been passed,

true yield has not occurred, and the ultimate bearing capacity could well have been considerably greater than the maximum load applied.

For the estimation of 'skin friction' in clay, the remoulded shear strength would seem, to your Reporter, to be an obvious though perhaps conservative value to choose, rather than to apply some arbitrary value of C_a/C_u . This ratio (used by Bromham and Styles and also by Mattes and Poulos) must be strongly influenced by the sensitivity, which is not stated in either case. Bearing capacity factors are strongly dependent on the value of ϕ_u used. It seems surprising that the soil strength parameters, and hence bearing capacity factors, at 76 ft. depth (in medium sand) were taken as the same as 38 ft. depth (in very stiff clay). Analysis in terms of effective stress, using the results of drained tests, might have been more appropriate for the sand. As no strength tests are reported on samples from below 50 ft., an estimated value of ϕ' for the sand at 76 ft. depth might have provided a more realistic approach. Conclusion number 2, it is respectfully suggested, is a slight overstatement.

The paper by J.D. Moss describes a loading test in which up to 400 tons could be applied to a single pile, the reaction being obtained, not by dead weight, but from deeply anchored prestressing cables. While this is not the first time the method has been applied on the Waitemata sandstones in Auckland (Ref. D2) it is surprising that wider use is not made of the system, which appears far less cumbersome than the provision of kentledge, supporting beams, etc.

A wide variation in strength with depth (at depths only a few inches apart) is a well-known feature of the Waitemata series, clearly shown in Fig. 1, though the usual tendency for the strength to increase with depth does not appear. Your Reporter's own experience has shown that one factor strongly influencing the measured compression strength of these soft sandstones is the accuracy to which the ends of the samples are trimmed. Both Ministry of Works, and the Reporter's own tests have usually shown ϕ_u nearer 20° than the high values given.

The paper by R.J. McKenzie includes full-scale tests on piles (both parallel and belled) and on 7 ft. square spread footings at 14 ft. depth. The record of these carefully - conducted tests is a welcome addition to the literature on a topic which is otherwise sparsely documented.

The use of the Brazilian indirect tensile test is noteworthy. Almost without exception, we assume that soils fail in shear, but there are some problems (such as this) where tensile strength is of importance. In a 'deeply fissured' clay, as at the Keilor site, a wide scatter of results in both tensile and compression tests is to be expected.

The relations between theoretical and actual results are clearly set out in the conclusions to the paper. The author could, perhaps, enlarge on the statement in Section VII that piles with enlarged bases are not acceptable because of low reserve capacity. Can this not be overcome by applying an increased safety factor in design?

The paper by Mattes and Poulos is an experimental evaluation of earlier work, carried out at the

University of Sydney, in which elastic theory is applied to estimate the load settlement relationships of single piles, and of pile groups, while that by Poulos and Madhav, "Analysis of the Movements of Battered Piles" is a continuation of this theoretical work. The latter paper will be considered first.

The theory is based on the use of Mindlin's equations for displacements caused by forces acting within an elastic medium, rather than on Winkler's assumption, which had previously been used in analysis of pile groups. It is, perhaps, only to be expected that displacements of piles caused by axial or normal loads or moment are not appreciably affected by slopes of up to 30° but it is comforting to have one's intuition reinforced by theory.

The example of a single pile shown in Fig. 2 is interesting. The vertical and horizontal loads (40T. and 8T. resp.) may be combined into a single load (of 41T.) acting at 11° to the vertical. If the point of application of this load is 1.2 ft. to the right of the pile top, the moment of 50T. ft. is also applied. The two loads and moment are thus replaced by a single force. Considered in this way, the reasons for the variations in displacements and rotation with batter angle may be better appreciated.

The extension of the theory to groups of piles (some sloping) should make it a useful tool for the practising engineer. The approximation used to deal with the case where the planes of the loads and of the sloping piles are different, appears to be a reasonable one. The term "fixed-head" refers to a pile which is fixed in direction but not in position. Have the authors looked into the question of just how "massive" a pile cap must be, for the fixed-head assumption to be reasonably accurate? With regard to the example of group analysis given, the authors state: "the beneficial effects of the battered piles may clearly be seen". While this is true in the example quoted, it is not universally so. The fairly common practice of including a few sloping piles to "carry horizontal loads" when there is a much larger number of vertical piles, may have effects which are far from beneficial.

Returning to the paper of Mattes and Poulos, the elastic theories for pile deflections are here put to the test, with model piles. The authors overcome the difficulty, noted by Bromham and Styles, of determining soil moduli by using their theory (in reverse, so to speak) to find E and E' from tests on models at one length-to-diameter ratio, ($l/d = 25$) and applying these moduli to check the theory at other ratios ($l/d = 10$ and 40). The difficulty of applying elastic theory, which assumes a linear stress-strain relationship, to soils is that real soils invariably have a non-linear stress-strain relationship. At low stresses, however, the non-linearity is small enough to make the assumption of elasticity a reasonable approximation. This can be seen in Fig. 4, showing the static test results to have been made at about one-quarter of the failure load, that is, within the 'linear' range. The opinions of the authors regarding the estimation of moduli for soils in the field, would be of value.

For the static load tests on single piles, deformations were very small. Your Reporter wonders how (without some imagination) the detail of the

graphs shown in Fig. 2 was obtained, when the settlement between 0.1 and 10 minutes amounted, in each case, to about one scale division on the dial gauge.

The pile spacing in the group tests ($= 2d$) is rather less than might be considered desirable in practice. The 3×3 group of $\frac{1}{4}$ in. diameter piles might be expected to act as a single $1\frac{1}{4}$ in. square pile, at this spacing.

The conclusion that elastic theory "used with discretion. . ." provides a wise comment, which could well refer to many of the theories we apply, sometimes unthinkingly.

The paper by Lee and Herington dealing with stresses beneath granular embankments also compares theory with experiment by the use of a model. While the principles employed in the deformable base are clearly described, it is hoped that the authors may provide photographs or diagrams, during presentation, so that the physical arrangement might be better appreciated. To the practising engineer, the knowledge that the vertical stress beneath an embankment is within about + 5% and - 15% of that usually assumed (that is, the weight of the material directly above) will be comforting. Three construction sequences are described but two only appear to be reported in the results section. Values for stage 3 have been omitted from Table II. Values of base shear in Tables I and II appear to differ from those in Figs. 3 and 6.

The conclusions that elastic theory is reasonably applicable when deformations are very small (rigid base) but that deformations have to be considerable before fully plastic behaviour is found, emphasise the importance of the region between these two modes of behaviour, so graphically indicated by a big question mark in an earlier paper by Davis and Poulos (Ref. D3).

Finally, we have the scholarly paper by Davis and Booker to consider. This gives a clear historic account of the development of solutions to plane strain problems of surface bearing capacity, and gives a satisfying rigorous solution in terms of plasticity theory, in a manner more readily comprehensible to the engineer than most papers on plasticity. Your Reporter has neither the specialised knowledge, nor the temerity to offer criticism. He is left with the feeling, however, that Terzaghi, with his fairly rough 'engineering' approach, was remarkably close to the mark!

In conclusion, a note on jargon may not be out of place. We should, your Reporter feels, not continue to use everyday words in a misleading technical sense merely because they have become enshrined in the literature. Are not the terms "sloping" and "friction", as adjectives applied to piles, less liable to mislead the innocent than "battered" and "floating"?

References:

- D1. GIBBS and HOLTZ - Research on Determining the Density of Sands by Spoon Penetration Testing. Proc. Fourth Int. Conf. Soil Mechanics and Foundation Engrg., 1957, Vol. 1, p. 35.

- D2. SYMPOSIUM - Major Building Foundations in Auckland. Proc. Fifth A.-N.Z. Conf. Soil Mechanics and Foundation Engrg., 1967, p. 269.

- D3. DAVIS, E.H. and POULOS, H.G. - The Use of Elastic Theory for Settlement Prediction under Three-Dimensional Conditions. Geotechnique, Vol. 18, No. 1, 1968, p. 67.

Paper by S.B. BROMHAM and J.R. STYLES:

Discussion by L.K. WALKER:

The authors' paper raises several interesting questions related to the interpretation of pile load test data. Their Fig. 5 indicates that pile settlements under constant load can be divided into primary and secondary components. The primary component appears to be small in magnitude (if immediate settlement is not included), while the secondary component can be approximated by a semi-log straight line as in the conventional interpretation of consolidation test data. Secondary settlements shown in Fig. 5 are already a significant proportion of the total after only about 200 minutes.

The writer's specific queries are as follows:

- (a) Are the settlement-time data plotted considered to relate to drained or undrained deformation?
- (b) Do settlements for the long increment (19-hour) continue on a semi-log straight line as might be expected from a secondary compression phenomenon?

With respect to the above points the writer considers that pore pressure build-up around a pile under test load will be rapid due to the small zone of influence around the pile. Thus the settlement - time data should be representative of a drained loading increment. This would suggest that the secondary compression region shown in the authors' Fig. 5 should continue for several days. Data from the long-term increments would be instructive on this point.

One of the major concerns in pile load testing is to determine a suitable load increment duration. If a regular 24-hour increment time is used, there is little doubt that a "drained" load-deflection curve would result. The results from Fig. 5 suggest that an "undrained" load-deflection curve would be obtained if something less than half-hour load increments were used. It is assumed that some difference between the two curves would be anticipated, as is observed in laboratory shear tests. Similarly different ultimate loads should be obtained. Which of the two types of test is required would seem to depend on the type of load application to be expected in the field (i.e., the ratio of dead to live load). In general, the "undrained" test would appear to provide a more conservative result for ultimate load, but a lower estimate of likely settlement under load. Some judgement is obviously necessary in each particular case before a test load programme is defined.

The authors' data should lead to a more fundamental appraisal of test load procedures, and any further comment they can offer would be welcomed.

The Authors in Reply:

To the General Reporter:

1. The results of Index and Classification tests are shown in Table VII.

2. The Friction Ratio is defined as:

$$\text{Friction Ratio} = \frac{\text{Skin friction resistance (lb./in.}^2\text{)}}{\text{Cone resistance (lb./in.}^2\text{)}}$$

3. The correction to 'N' values to allow for vertical effective stress achieves predominance where estimates for density are required at relatively shallow depths. These corrections strictly apply to cohesionless soils, and thus for the very stiff clay it was not considered worthwhile to modify results which have doubtful application. Corrections to the S.P.T. values in the medium sand layer were not required because of the high value of effective overburden pressure.

4. Highly polished, lubricated brass end platens were used in all triaxial testing.

5. One of the factors which determines the shape of the secondary compression portion of the log. time versus deflection curve is the magnitude of the load increment ratio. Fig. 5 shows the deflection curve for the loading from 160,000 lb. to 180,000 lb. i.e. an effective load increment ratio of 1/8. The typical deflection curves associated with the Laboratory Consolidation Test do exhibit the reverse curvature mentioned by the General Reporter, but the load increment ratio is generally unity and the system is clearly one-dimensional in nature.

6. An error has occurred in Table V : the pile capacity from Loading Tests on Test No. 2 should be >300 tons.

7. Isolated direct shear tests, where shear was indicated on (a) a steel/soil interface, and (b) a soil/soil interface, indicated a c_a/c_u ratio of approximately 0.24. Whilst not conclusive it did reinforce the assumption of adopting the average value of $c_a/c_u = 0.25$ for very stiff clays.

To Mr. L.K. Walker:

1. The authors generally agree that the loading durations of 30 minutes or less probably represent "undrained" load-deflection states. The observed differences between the load-deflection curves for

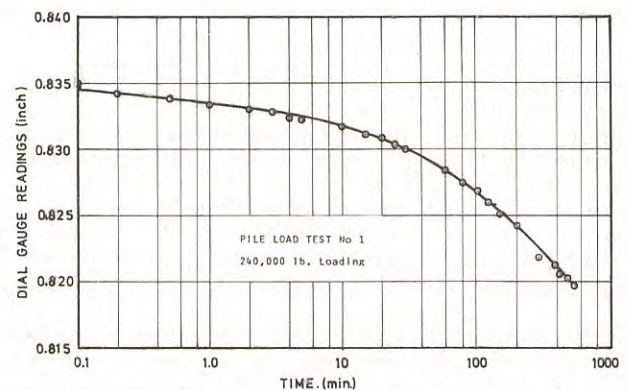


Fig. D1 - Deflection - Log. Time Curve for 19-hr. Load.

TABLE VII

RESULTS OF INDEX & CLASSIFICATION TESTS

Sample No.	Depth	LL %	PL %	PI %	% Sand	% Silt	% Clay	Classification
919092	4'6" - 6'	56	15	41	4	52	44	Silty Clay
093	9'6" - 11'							
094	14'6" - 15'8"	79	21	58	5	36	59	Clay
095	16' - 17'6"	69	14	55				
096	17'6" - 19'	76	15	61				
097	19' - 20'5"	74	20	54	7	36	57	Clay
098	20'6" - 22'	72	16	56				
099	22' - 23'4"	78	18	60				
100	23'6" - 24'11"	63	17	46	9	37	54	Clay
101	25' - 26'4"	71	18	53				
102	26'6" - 27'10"							
103	28' - 29'5"	65	17	48	4	41	55	Clay
104	29'6" - 30'10"	63	15	48				
105	40' - 41'1"	48	14	34	11	51	38	Silty Clay
106	50' - 50'10"	62	18	44	3	49	48	Silty Clay

the short and longer term loadings were small and indicate that for this type of material rapid loading techniques produce completely satisfactory results.

2. Fig. D1 shows complete deflection - log. time curve for the 240,000 lb. loading in test No. 1, which continued for 19 hours. The settlements do appear to conform to a pattern which is typical of secondary compressions.

Paper by N.S. MATTES and H.G. POULOS:

The Authors in Reply:

The General Reporter questions the accuracy of the small deformations measured in the pile tests. The authors feel that much of the possible inaccuracy arising from the use of dial gauges has been eliminated by the use of very rigid mountings, as shown in Fig. 1 of the paper. In addition, a dial gauge with a large face was used so that interpolation to a tenth of a division (i.e. 10^{-5} in.) could be carried out without difficulty.

The question of estimating the soil modulus for full-scale piles is very relevant. Attempts to measure modulus from ordinary laboratory tests do not give acceptable values, as found by Bromham and Styles in their paper. Apart from back-figuring the modulus from a pile load test, the authors consider that the best way of estimating modulus is to use empirical relationships based on previous published load test results. Such a correlation for piles in clay is shown in Fig. D2, where E_s is related to undrained cohesion c_u for both bored and driven piles. The effect of pile installation on E_s can be seen from this figure. It is also interesting to note that the data of Bromham and Styles fits the correlation for driven piles very well.

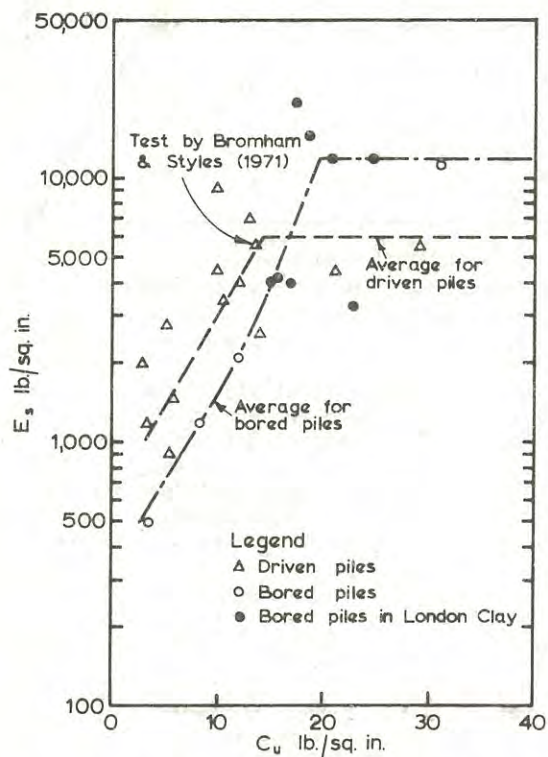


Fig. D2 - Backfigured Soil Modulus E_s for Piles in Clay.

Paper by H.G. POULOS and M.R. MADHAV:

The Authors in Reply:

In reply to the General Reporter regarding how "massive" a pile cap must be to be a fixed-head cap, there appears to be little data available on this question. Davisson (1970) suggests however that in practice, the degree of fixity that can be developed (i.e. the maximum moment in the cap) is about half the value corresponding to a truly fixed-head pile. If this is so, the assumption of a fixed-head pile would rarely be justified, and the assumption of a rigid pile cap which can rotate would be more realistic.

Reference:

DAVISSON, M.T. - Lateral Load Capacity of Piles. Highway Research Record No. 333, 1970, pp. 104-112.

Paper by R.J. MCKENZIE:

Discussion by E. GERMANIS:

The tests were obviously not designed for a full assessment of creep effects on footings under sustained long-term loads at their maximum working values. However, it would be of interest to know whether any change in the creep rates was, or could be, observed during the short time intervals of constant load, when the incremental loading method was used on slab footings.

A change of moisture content in the in-situ clay surrounding the piles and the footing excavations should have an effect on the test results for the adopted loading rates and on the uplift resistance generally. An artificial change of the in-situ moisture content would be desirable to study its effect. It is understood that it is intended that variations in this respect be estimated on the basis of laboratory test results.

For the "cone of earth" method some authors assume a variable angle (5° to 30°) between the vertical and the failure plane. The appropriate angle is to be determined by a test for each type of soil separately.

The advantage of the use of crushed rock in the backfill over slab footings has been clearly proved by the tests described in the paper.

The Author in Reply:

To the General Reporter:

In reply to the General Reporter's comment on introducing a safety factor in design in order to provide additional reserve in the uplift capacity of under-reamed piles under long-term loading, the author stated that the information on which to base such an increase is inadequate. For example, the creep effect under sustained loading is known only

for the short ten-minute periods when the load is held constant during the CIGRE method of test loading. This data cannot be applied to the long-term loading condition which is important in footings for strain towers. Other parameters to be considered in the choice of a load or safety factor would include the large differences in magnitude between the sustained conductor loads and the higher, but infrequent, wind gust loads. In addition, the variability of both the in-situ site conditions at each prototype tower and the footing as constructed must also be considered. As a consequence, footing design is based directly on the 90% value of the uplift capacities determined in the test series and no other factor is applied.

To Mr. E. Germanis:

In reply the author comments that changes in moisture content of the clay generally occurred in the top two or three feet, which results in cracking of the clay in dry periods, whereas the major part of the footing is located below the seasonal zone and moisture conditions, cohesion and tensile strengths of the clay remain virtually constant. A correction is made in the design for piles of shallow depth where the effect of seasonal cracking is significant by the reduction of the pile capacity by an amount equal to the capacity of a similar pile of depth equal to that of the seasonal effect.

Uplift displacements of the test footings due to creep during the 10-minute periods of constant load were measured, and it was noted that the creep rate was generally constant and increased in magnitude at the higher loading levels. In some cases, it was noted that the creep rate reduced during sustained load.

Paper by I.K. LEE and J.R. HERINGTON:

Discussion by Prof. D.H. TROLLOPE:

The results obtained by the authors are of considerable interest and value in that they provide through the medium of carefully conducted experiments an opportunity to compare differing theoretical predictions.

It is perhaps unfortunate that the authors err on p. 293 in quoting the maximum shear stress under no-arching conditions as $0.58 \gamma H$. This is, in fact, the full-arching value and the corresponding no-arching value is $0.15 \gamma H$. Earlier recognition of this might have avoided the unjustified criticism of the appropriate clastic solutions which are contained in the paper.

Fundamentally, clastic mechanics in its simplest two-dimensional form requires recognition of two parameters; the distribution angle (θ) and the arching factor (k).

The distribution angle θ can be calculated from "at rest" type earth pressure tests and is analogous to the Poisson ratio term in conventional elastic theory. Taking the simple four-unit systone (Trollope, 1969) it can be shown that

$$\frac{\sigma_3}{\sigma_1} = \tan^2 \theta$$

Using the authors' quoted value of Poisson's ratio as 0.4 (p. 295) the value of θ is calculated to be $32^\circ 18'$.

If the value of θ is known then the constitutive stress relationship depends only on the arching factor (k).

It is possible therefore to calculate appropriate values of k from the experimental results quoted in the paper.

Consider first the wedge sequence. Fig. D3 shows the calculated values of k and the associated linear stress distributions fitted to the maximum shear stresses in Fig. 3 of the paper. Perhaps the most encouraging feature of this analysis is that the movement of the peak shear stress away from the centre-line with increasing side slope as predicted by the clastic theory corresponds with the trend of experimental results. The fact that the calculated k values exceed 1 can be interpreted to mean either that the material can take tension or that some boundary restraint is present. In the present circumstances the latter explanation is preferred. Frictional restraint along the base, perhaps assisted by some wedging between the independent measuring supports could account for this. The appropriate theoretical elastic shear stress distribution is also plotted from data given in Table I of the paper.

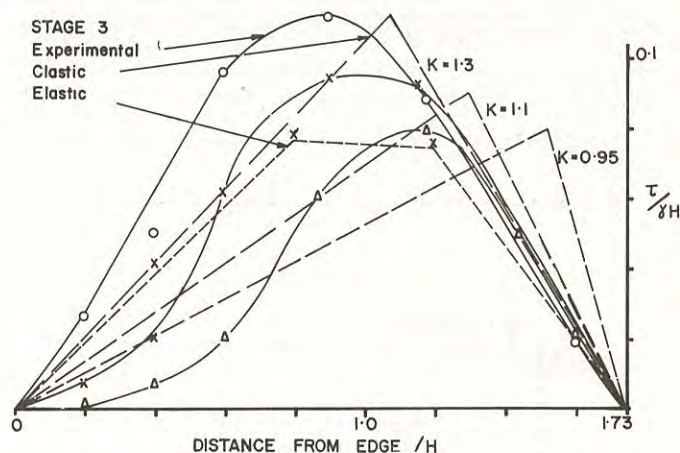


Fig. D3 - Comparison between Clastic, Elastic and Experimental Results - Shear Stress Distribution - Wedge Sequence.

Of even greater practical significance however are the results shown in Fig. D4. It will be seen that with only 0.1 in. central deflection upwards the measured shear stresses have altered to provide remarkable agreement with the no-arching solution. This suggests that the effect of boundary restraint can be nullified by trivial boundary displacements.

Analysis of the layered sequence also provides support for clastic theory. It has been pointed out previously (Trollope and Morgan, 1959) that introduction of the wide embankment profile introduces restrictions on the possible distributions of the arching factor k . The concept of zones of arching developed in this earlier paper has been adapted to the present situation. The results of the approximate k distributions are shown in Fig. D5 and D6. (It should be noted that when discontinuities in the k

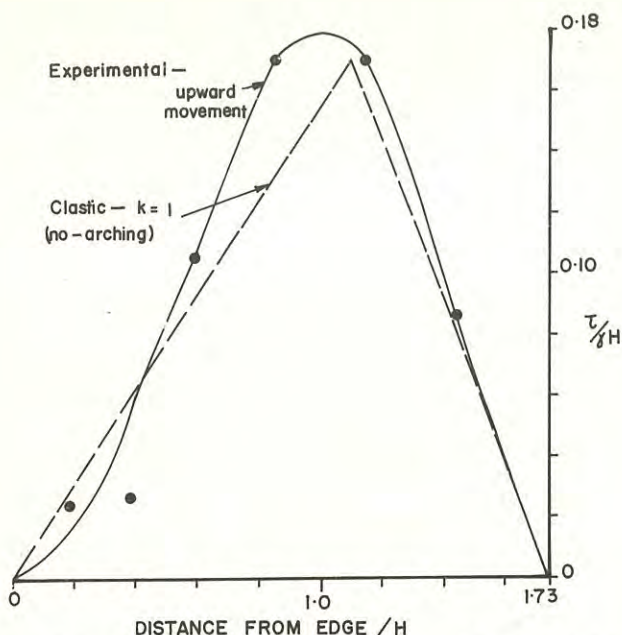


Fig. D4 - Shear Stress Distribution - with upwards movement of base.

zones are introduced, discontinuities are also implied in the stress distributions at the zone boundaries. The present results have been calculated on the assumption that the value of k in the left-hand zone operates on its right-hand boundary).

The only experimental points that do not fit well are those measured at a distance $0.4 H$ from the outer edge. Evidence in Fig. 6 of the paper suggests that these readings were consistently low and this might account for the disparity.

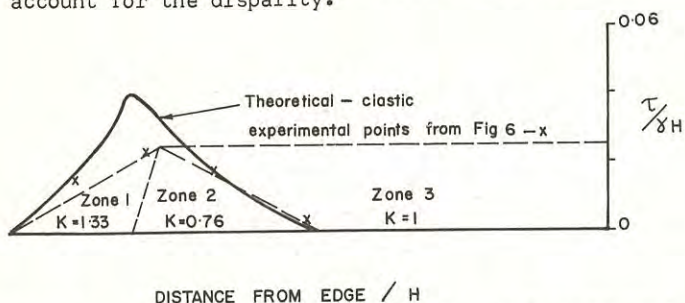


Fig. D5 - Comparison between Clastic and Experimental Results - Shear Stress Distribution, Layered Sequence Stage 1.

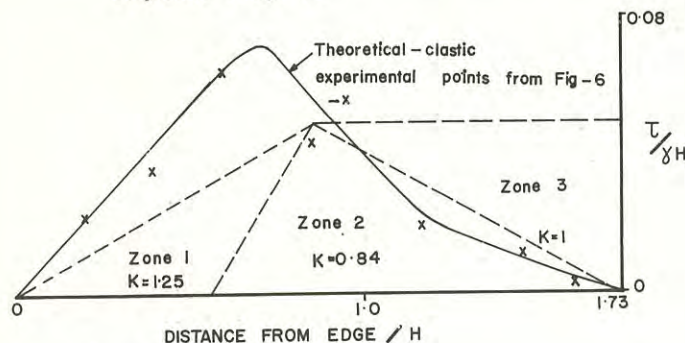


Fig. D6 - Comparison between Clastic and Experimental Results - Shear Stress Distribution, Layered Sequence Stage 2.

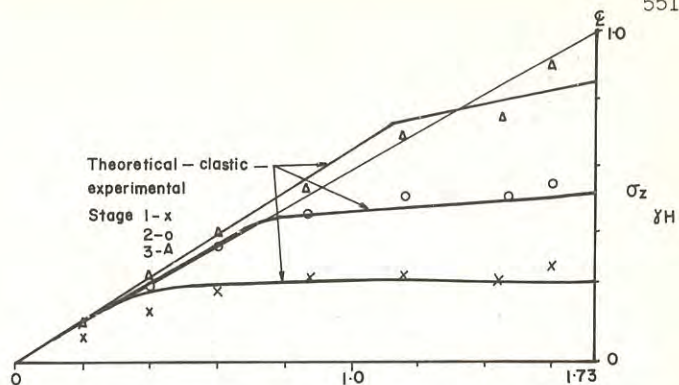


Fig. D7 - Comparison between Clastic and Experimental Results - Normal Stress Distribution, Layered Sequence.

To check further the predictions of clastic theory, the normal stress distribution derived from the k values produced in Figs. D5 and D6 were calculated and are shown in Fig. D7. Again, with the exception of the values at $0.4 H$ from the outer edge agreement with the measured stress is good.

The above analysis demonstrates the applicability of clastic theory to the behaviour of granular materials and particularly emphasises the versatility of the method in dealing with a wide range of geometrical and boundary restraint problems.

References:

- TROLLOPE, D.H. - The Mechanics of Discontinua or Clastic Mechanics in Rock Problems. Chapter 9 in Rock Mechanics in Engineering Practice, (Stagg and Zienkiewicz, Eds.). New York, Wiley, 1969.
- TROLLOPE, D.H. and MORGAN, J.R. - Stress Systems within Simple Slopes of Granular Materials. Civil Engg. Trans. I.E.Aust., Vol. CE1, No. 1, March, 1959, pp. 18-26.

Discussion by B.C. BURMAN:

The granular wedge under self weight loading is a configuration of fundamental interest in the field of practical geomechanics, representing in simplified form such engineering structures as earth dams, road embankments and mine waste tips. The authors have carried out probably the most carefully conducted set of granular wedge experiments yet reported and their results are a significant contribution to this particular field. However, physical models, per se, can deal only with one aspect of the whole problem and it is desirable to have a theoretical framework into which the results of physical experiments may be meshed. The purpose of this discussion is to introduce briefly a numerical model which is believed to provide a viable theoretical basis for such studies.

As part of a study of the mechanics of discontinua the writer has developed a numerical procedure, based on finite element techniques, which allows the modelling of block jointed systems (Burman, 1971). In essence, the behaviour of a jointed mass is described in terms of the translational and rotational displacements of individual block centroids as well as the contact forces between

adjacent blocks. Full account has been taken of the non-linear interaction between blocks with shear behaviour being defined by a general stress-strain relationship which may include work-softening and work-hardening as well as non-linear failure envelopes and dilation. The normal interaction between blocks allows for limited tensile strength of the joints in addition to locking in both compressional and rotational modes.

The granular wedge situation has been modelled numerically by a close-packed hexagonal arrangement of disc units consisting of 15 layers and with 30° side slopes. Vertical symmetry permits the modelling of only one-half of the wedge and boundary conditions restricting all but vertical displacements along the centreline and all three displacement components along the base. The shear stiffness of joints has been taken as a low value to represent shearless contacts and the tensile strength between discs taken as zero. The three stress components, σ_z , σ_x and τ_{xz} , throughout the jointed mass have been calculated as the appropriate average force per unit area in a manner similar to that described by Trollope (1969).

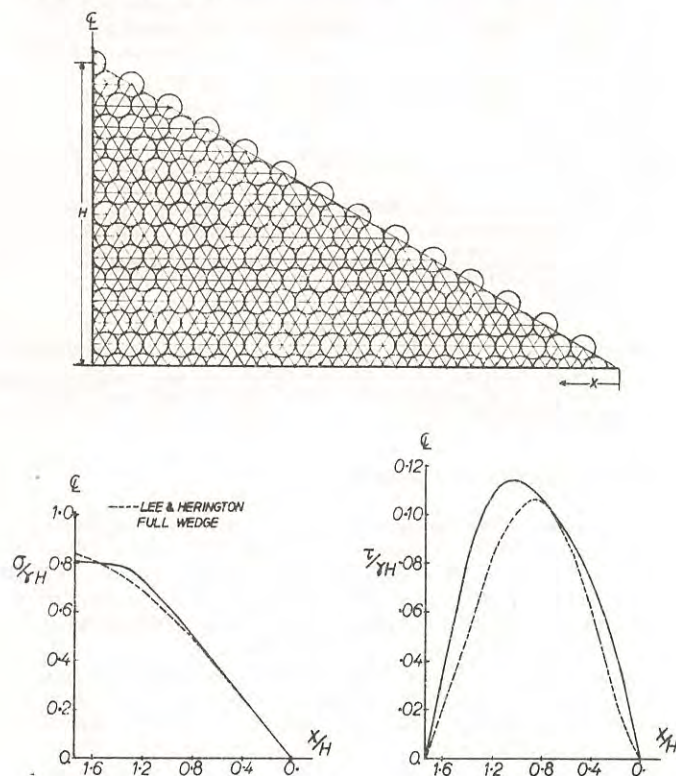


Fig. D8.- Comparison of Normal and Shear Stresses on Base of Numerical Wedge Model and Experimental Results for Sand Wedge (after Lee and Herington, 1971) for Full-Height 30° Wedge Condition with Rigid Base.

For these conditions, the results of three numerical experiments representing self weight loading of the full 30° wedge and close approximations to stage 2 of the authors' wedge and layered sequences are presented in Figs. D8, D9 and D10. The actual configuration of disc units for each analysis have been shown together with a comparison of normal and

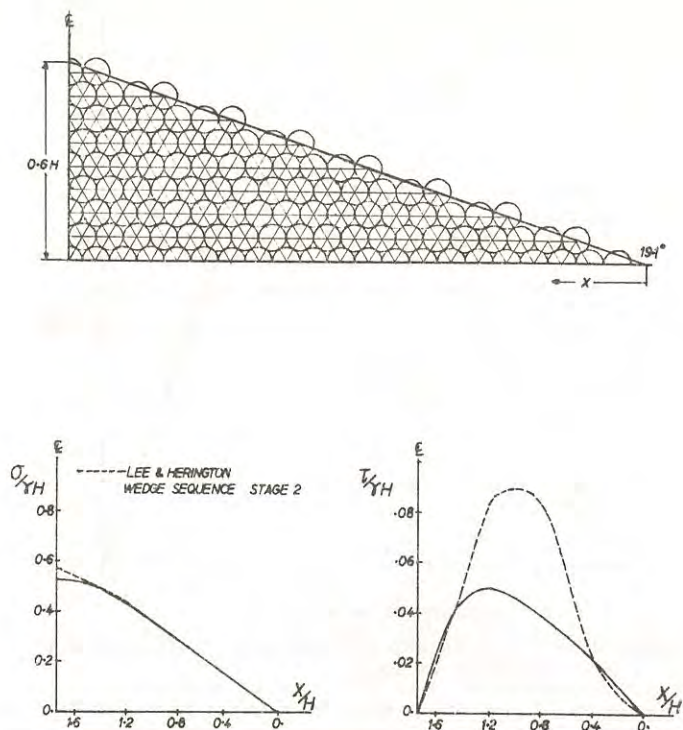


Fig. D9.- Comparison of Normal and Shear Stresses on Base of Numerical Wedge Model and Experimental Results for Sand Wedge (after Lee and Herington, 1971) for Stage 2 Wedge Sequence with Rigid Base.

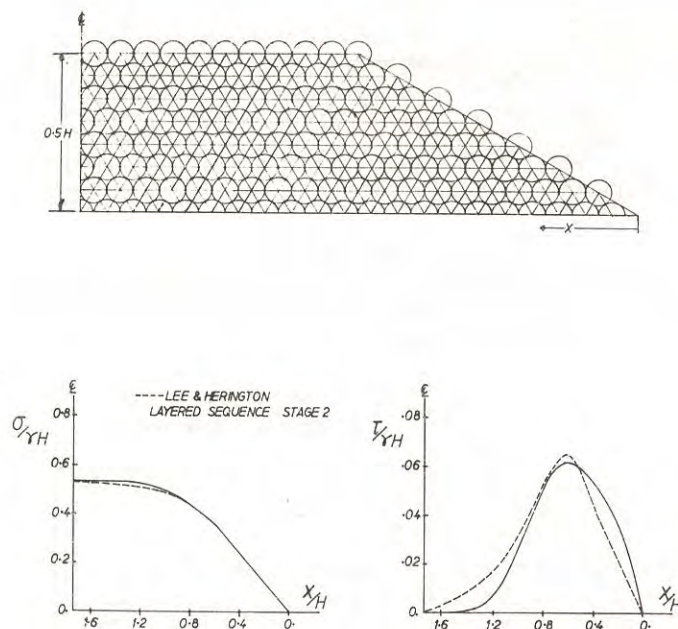


Fig. D10.- Comparison of Normal and Shear Stresses on Base of Numerical Model and Experimental Results for Sand Wedge (after Lee and Herington, 1971) for Stage 2 Layered Sequence with Rigid Base.

shear stresses along the base. It will be seen that there is excellent agreement between predicted and measured stress values in all three cases except for shear stresses in the stage 2 wedge sequence where the numerical model values are approximately 60% of those measured. This latter feature has led the writer to question the accuracy of measured stress components and although definitive conclusions cannot be reached in this regard some doubts remain. In particular it has been possible to verify that the vertical equilibrium requirement was satisfied generally throughout the wedge and in particular along the base for the numerical solutions, viz:

$$\frac{\partial \sigma_z}{\partial z} + \frac{\partial \tau_{xz}}{\partial x} + \gamma = 0$$

The fact that there is good agreement between measured and numerical values of the normal stress component along the base but some divergence between respective shear stress values raises doubts in the writer's mind as to accuracy of measured shear stresses. Although the authors will be unable to check the vertical equilibrium requirement in differential form they may obtain some indication of the reasonableness of measured stresses by considering the vertical equilibrium of free body sections. In this regard, it may be of interest to note that the numerical results reveal a marked degree of regularity in stress distributions along horizontal sections indicating that the differences between finite and infinite wedge configurations are slight.

For the deformed base condition, the numerical model shows a progressive transfer of normal stress from the central regions similar to that reported by the authors. However, the decrease in shear stresses reported by the authors for the concave base profile is in direct contrast to numerical predictions which indicate an increase from $0.12\gamma H$ to $0.22\gamma H$ for a central deflection of $0.02H$. In this regard there is at least qualitative agreement between the writer's finite wedge model and plasticity solutions for the infinite wedge reported by Booker (1969). In view of this correspondence it would be most enlightening to have the authors detail their basis for attributing the anomalous shear behaviour to the finite dimensions of their wedge models.

In conclusion it is most encouraging to note the authors' intention to extend this work to a wider range of base deformation states. Their demonstration of the inability of linear elastic theory to deal with stress distribution in granular materials is a point of immediate practical interest and experimentation along these lines is undoubtedly of considerable practical importance. In this respect, it is suggested that consideration be given to the measurement of additional stress or strain components, preferably horizontal stresses, so that the results may be even more soundly based than those here reported.

References:

- BURMAN, B.C. - Ph.D. Thesis, James Cook University of North Queensland, 1971.
- TROLLOPE, D.H. - The Mechanics of Discontinua or Elastic Mechanics in Rock Problems. Chapter 9 in Rock Mechanics in Engineering Practice, (Stagg and Zienkiewicz, Eds). New York, Wiley, 1969.

BOOKER, J.R. - "Application of Theories of Plasticity to Cohesive Frictional Soils", Ph.D. Thesis, School of Civil Engg., University of Sydney, 1969.

The Authors in Reply:

To the General Reporter:

The authors would like to thank the General Reporter and those who contributed to the discussion of the paper. There were many aspects which could not be detailed due to space limitations and some amplification is necessary to answer certain of the points raised in discussion.

As requested by the General Reporter, photographs of the model are now included. These are shown as Figs. D11 and D12 and show the general arrangement of the model and some details of the measuring strips. Referring to the apparent discrepancy between the shear stress values of the Figs. 3 and 6 and the Tables I and II, it should be made clear that the values of shear stress listed in the tables were divided by γh where h was the centric height of the embankment at any stage, whereas the values plotted in the figures were divided by γH , H being the final centric height.

Professor Taylor's comments that practising engineers may be comforted with the knowledge that the vertical stress beneath an embankment is within 15% of the weight of material directly above has to be qualified, since this only applies to the rigid base situation. There is a progressive decrease in centric vertical stress beneath the embankment as the foundation settlement develops. Earlier work referred to in the paper suggested that this vertical stress could diminish to a value much less than the minimum value of $0.85\gamma H$ shown in the paper since this latter value corresponded to a very small differential settlement.

A correction to the paper was that the value of the peak shear stress for the no-arching case should read $0.15\gamma H$ (p. 293). It was shown that the measured peak shear stress was in fact considerably less than the value predicted by the no-arching "solution", but, more importantly, that the distribution of shear stress was incompatible with this "solution" and, as stated in the paper, "the solution is inapplicable to the rigid base condition when there is no horizontal displacement of the foundation".

In modelling this problem particular attention was paid to the measurement of shear stresses along the base of the embankment as this provides data which is a very critical test of the applicability of any theoretical solution. The shear stress distributions published in the paper provide, to the authors' knowledge, the first opportunity to make such a critical comparison. Earlier work which concentrated on measurement of normal stress did not provide sufficient critical data since the normal stress distribution is rather insensitive to the analytical approach adopted.

To Prof. D.H. Trollope:

In contrast to Trollope's statical approach, which requires a pre-existing knowledge of the arching factor(s) for every new situation, the authors have attempted to obtain complete solutions, that is, solutions which satisfy statics and strain compatibility. There is little point in attempting to critically evaluate Trollope's discussion any further, and the reader is referred to the discussion by Davis and Taylor (1962) if he wishes to pursue the limitations of the arching theory as this discussion forms a succinct and relevant statement.

It is well known that the prime difficulty in analysing the behaviour of a soil mass is that the constitutive equations are complex, and therefore any simple mathematical model is necessarily an approximation to the real behaviour. At the present time the authors are examining various models by comparing predicted with measured values. The solutions for the linear elastic model were quoted in the paper to show that even this simplest (and obviously deficient) model led to interesting and revealing predictions. It was evident that the progressive build-up of the shear stress distribution was consistently predicted and even the quantitative correspondence exceeded expectations. This appears to justify the use of this model in a finite element analysis of the soil structure studied, and suggests that better agreement could be achieved by some relatively slight modifications to the model.

As an example of this, mention can be made of a special element based on the joint element developed by Goodman. From the results of direct shear test it is possible to obtain data for the normal and tangential stiffness of the elements as well as the residual tangential stiffness. The joint stiffness matrix can be developed and the joint stresses determined from the calculated displacements. The joint element cohesion, friction, and residual tangential stiffness are read in as data and the shear stress is calculated from the individual normal stress on each joint element. A Mohr-Coulomb criterion is applied if the joint shear stress exceeds the shear strength, that is, K_s is set equal to the K_s (residual) and the problem is recycled. Preliminary results on the embankment show that the normal stress distribution is accurately predicted but, as stated above, this is to be expected due to the insensitivity of the normal stresses to the model used in the analysis. The shear stress is closely predicted within the central half of the embankment but there are deficiencies towards the toe. This model is being further improved by taking into account

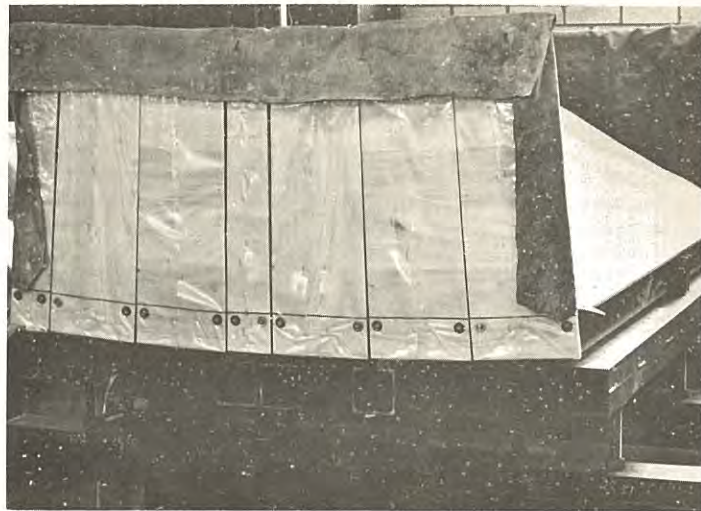


Fig. D11.

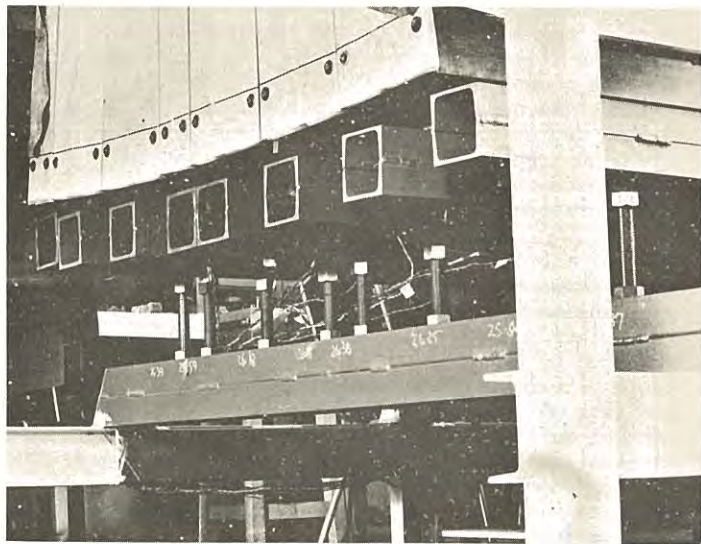


Fig. D12.

variations in models and Poisson's ratio with principal stress ratio.

To Mr. B.C. Burman:

Finally, it would appear that little comment need be made of Burman's discussion as his approach and philosophy appear to be identical with that of the authors.

TECHNICAL SESSION No. 7—SLOPE STABILITY

"Abutment Stability Studies for the Gordon Arch Dam", by D.T. Allen.

"Rock Slope Stability - How far away are reliable design methods?", by E. Hoek.

"A Statistical Method for the Design of Rock Slopes", by B.K. McMahon.

"Rock Mechanics Investigations for a Large Open Cut at Mount Isa", by K.J. Rosengren.

"Geotechnical Investigations for Slope Stability Studies in Brown Coal Open Cuts", by C.S. Gloe, J.P. James & C.M. Barton.

"The Practical Application of Thermal and Freezing Methods to Soil Stabilization", by T. Fujii.

"The Role of Progressive Failure in Clay Slopes", by P.M. James.

"Effects of Anisotropy and Sample Disturbance on the " $\Phi_u = 0$ " Stability Analysis", by G.R. Martin & T.J. Kayes.

GENERAL REPORTER - Mr. A.K. PARKIN:

For many of us, our earliest counsel in the matter of foundation engineering might have been derived from the New Testament, in which the wise man is portrayed as one who built his house upon the rock, whilst the foolish man built his upon the sand.* It was, of course, an analogy, based on a concept of rock as a material of unlimited strength.

The technology of slopes has probably contributed more than any other area of foundation engineering to our present appreciation of rock as a material having a finite load capacity which is usually a more significant function of structure than of intrinsic strength. Consequently, it is now accepted that thorough investigation and design is as necessary to the satisfactory performance of a rock slope as it is to an earth slope.

Whilst the subject of the session embraces both earth and rock slopes, there is no sharp distinction in behaviour. Some over-consolidated clays, for example, may fail in a manner resembling a rock slope (Esu, Ref. D1). Nevertheless, it would appear that rock slope behaviour is attracting the greater interest at the present time, and five of the eight papers could be considered to fall within this area. Considering that the economic feasibility of open pit mining projects is often dependent on the choice of slope, the economic pressure behind rock slope research is understandable.

The five papers concerned with rock slopes cover a wide spectrum, beginning with a probing state-of-the-art paper by our eminent guest, Professor Hoek. In the research area, McMahon's paper is a sound and sane approach to the situation where jointing is extensive and not closely defined in orientation: it is then scarcely possible to design on any other basis than a reasoned probability of success, and a conventional safety factor is misleading. We will hear much more of probabilistic design in the future, and should not forget that structural engineers are also researching in the field. Two papers, by Allen and Rosengren, report investigations for major structures in Queensland and Tasmania, which should be read with interest, as their authors represent organisations which have made a pioneering contribution to rock

mechanics in Australia. Finally, the paper by Gloe et al gives a report on field observation techniques in a major brown coal pit, and is interesting by virtue of the great depth of the installations, and the severe groundwater conditions that had to be overcome.

In relation to natural slopes of soil materials, the principal area of current interest would appear to lie in long-term or large-strain behaviour. This interest was largely evoked by the 1964 Rankine Lecture, and the ensuing nightmares to designers over the next few years. Fortunately, it has become apparent that the original concept of residual shear is excessively conservative in most cases, particularly where there are no pre-existing slips. Only one paper, however, has been submitted on this subject, although another by Cullen and Donald has been submitted to another session.

Of the two remaining papers, the one by Fujii is a somewhat unusual topic to include under Slope Stability, but is, nevertheless, a welcome exposition of two techniques of stabilization of which there is not a great deal of experience in this country. The last paper, by Martin and Kayes, is concerned with the relationship between field and laboratory behaviour of soils. This subject is also receiving a fair amount of attention around the world, but mainly in relation to normally consolidated clays.

I will continue with some specific comments about the papers.

The project described by Allen, the Gordon Dam, is surely one of the most challenging and spectacular dam projects to be undertaken in Australia. The abutment problem described is one frequently encountered in arch dams, and one which has already caused one major disaster in recent times. Not many years ago, serious difficulties were experienced with one abutment of the Kariba Dam on the Zambesi River, which required extensive grouting, and hence there is much to be learned from the way in which the present project has been tackled.

A "wave angle" concept is introduced to allow for the non-planar nature of the joint surfaces and the

* Matthew 7:24-27

consequent dilation during shear. It is, however, not new, and I feel it would be better to keep to the established concept of angle of roughness, as defined by Patton (Ref. D3). This would also apply to the angle δ as used by Rosenqren. I would also suggest that when a roughness parameter is used, cohesion should be neglected, since it arises basically from the shearing of asperities.

It is Hoek's conclusion that the state of knowledge on the behaviour of rock slopes is, at present, adequate, and that satisfactory analytical procedures are available. The problem is in defining the structural geometry of a rock mass. In his comments on limit equilibrium methods, he states, as other writers have, that the meaning of a factor of safety other than 1 is not clear, and there is room for fruitful discussion as to the significance that should be attached to a calculated value of the safety factor. A further comment states a problem that is not new to soil engineers who have for some years, recognised "short-term" and "long-term" design conditions.

There is some concern that groundwater flow through fissured rock masses is non-Darcian in character. Recent work, however, (Ref. D2) indicates that the usual assumption of porous media flow is unlikely to cause a serious underestimate of pore pressure and the author's presumption is therefore reasonable.

McMahon's contribution is aimed at the area already considered to be one of priority by Hoek. In a valuable study of the situation where jointing is not closely defined, a statistical model is fitted to the observed joint distribution, from which procedures are then available for estimating the probability of failure. He concludes by showing how the methods can be applied to economic slope design, by an approach similar to Shuk (Ref. D4).

Regarding the comments on limit equilibrium analysis, it is naturally every engineer's dream, but not often his good fortune, to have on-site information from a full-scale test. In general, therefore, a combination of laboratory testing and field experience will be necessary. In this case, it is possible that some of the required parameters might be more reliably determined from laboratory tests and others more reliably deduced from field data, and I will leave this thought for discussion.

Turning to the paper by Gloe et al on the brown coal open cut operation at Morwell, some points bear clarification. Firstly, could the authors give more detail of the nature of the subsidence described in IV. (e)? Does "a bearing capacity type failure" of the underlying strata refer to what is usually described as a circular slip and might the authors enlarge on their concern about lateral movement of these strata? It was not clear how the borehole survey detects rotation of the strata, or perhaps rotation requires definition.

In conclusion, the topic that appears to have been passed over in this session is that of the stability of earth and rock dams. Otherwise, the principal areas for discussion would appear to be the definition of the geometry of a rock mass, how to handle cohesion in relation to rock slopes, and what in fact should be the basis for examination of a long-term stability of earth slopes.

References:

- D1. ESU, F. - Short Term Stability of Slopes in Unweathered Jointed Clays. Geotechnique, Vol. 16, No. 4, 1966, pp. 321-328.
- D2. PARKIN, A.K. - Field Solutions for Turbulent Seepage Flow. Proc. A.S.C.E., Jour. Soil Mech. & Found., Vol. 97, No. SM1, 1971, pp. 209-218.
- D3. PATTON, F.D. - Multiple Modes of Shear Failure in Rock. Proc. First Congress Int. Soc. Rock Mech., 1966, Vol. 1, pp. 509-513.
- D4. SHUK, T. - Optimization of Slopes Designed in Rock. Proc. Second Congress Int. Soc. Rock Mech., 1970, Vol. 3, pp. 275-280.

Paper by D.T. ALLEN:

Discussion by L.G. ALEXANDER:

(i) The shear resistance of two joints acting together, the one tight, and the other soft-filled, was found in the paper by adding the peak shear resistances of the joints acting individually, using properties determined from test samples. As these tests show that the shear resistances depend on displacement, the correct procedure is to add the shear resistances of the two joints at each displacement, and to find the maximum from the resultant curve. The resulting peak resistance may tend either to:-

- (a) occur near peak strength of the tight joint at a displacement which is too small for appreciable shear resistance to be mobilised on the soft-filled joint (as judged from the rest curves given);
- (b) depend on the residual, and not peak strength of the tight joint, at displacements for which appreciable shear resistance develops on the soft-filled joint. For either of the above cases the computed safety factor would appear to be lower than that given in the paper.

(ii) Monitoring of displacement of the abutment block and the most probable displacements in practice were not mentioned in the paper. In practice the peak strength of the tight joint may be higher than the results given by the test samples, and the deformation of the rock measured across the joint during filling of the dam remain in the elastic range. It would appear desirable for the measuring method to be sensitive to elastic deformations, in order to confirm whether this occurs.

A reading sensitivity of a few ten-thousandths of an inch over base lengths of ten inches would appear adequate. This could be achieved by placing measuring pins to span the joint in an array to determine shear and normal components of deformation. The element of the array could comprise three pins at the corners of a 60° triangle between each pair of which a deformation reading is made. The array could be extended along the joint, and normal to the joint by the addition of further pins. By this means shear and normal displacements along and across the joint

could be compared with corresponding readings from points in the rock adjacent to the joint.

(iii) The orientation of the tensile crack was stated to be a critical factor in the safety factor calculation, but was not allowed for in determination of the 'worst-case' safety factor.

The Author in Reply:

To Mr. Alexander:

The points raised by Mr. Alexander emphasise the problems involved in applying the results of shear tests on rock joint samples to the determination of the stability of the potential failure block.

The shear resistance of the two failure surfaces acting together was determined by adding the mean shear resistance of the joint to the mean shear resistance of the fault. The mean shear resistance of each surface was determined from the plot of shear strength versus normal stress as described in the paper and each plot contained the points corresponding to the corrected values obtained from the shear tests on the samples from that surface. These plotted points represented the highest shear resistance obtained at shear displacements less than 0.1 inch. The "mean" shear resistance and the "minimum" shear resistance actually applied to each failure surface were adopted after examining the spread of points in the plot of shear strength versus normal stress, but in each case the adopted values were less than the highest measured values.

An important point that was not brought out in the paper was the shape of the typical graphs of horizontal load versus horizontal displacement. Neither the joint nor the fault exhibited a pronounced reduction in strength beyond the peak. In almost all cases the strength was 95% to 100% of the peak strength after the peak was reached. The graph given in Fig. 6 is an actual test curve and was selected for inclusion in the paper because it illustrated the meaning of peak and residual strength. However it was the only sample on fault 4 where a pronounced peak was observed. The remaining samples from fault 4 behaved as would be expected for a schistose material that had previously been severely sheared. What was more surprising was the lack of a pronounced peak in the tests on the rock joint samples. In part, this may have been due to the selective sampling used whereby the samples were obtained from the joint where the joint was in its worst condition. These locations were by no means representative of the overall condition of the joint as it was difficult to even find the joint in many other places. It may be worth noting that the peak resistance of the joint usually occurred close to the 0.1 inch limit and the peak for the fault usually occurred at about 0.2 inch. If the shape of the test curves had been such that a pronounced reduction had occurred after the peak then a procedure on the lines suggested by Mr. Alexander would be desirable.

The excavation of rock on the left abutment has been completed since the paper was written and this has provided new exposures of the rock at the predicted location of joint A1. It has been pleasing to observe that this joint is less continuous than the evidence

suggested at the time when the stability work was being carried out. The true overall shear strength of this joint would therefore greatly exceed the strength determined in the shear tests because shearing through some intact rock would be involved even if "stepping" across is considered.

Mr. Alexander's suggestions regarding the need for the displacement monitoring system to be sufficiently sensitive to detect displacements in the "elastic" range have been noted.

Regarding the effect of the orientation of the tensile crack, the computed factor of safety was 2.6 and not 1.6 as recorded in section X(d) of the paper (see Errata). Although the stability was checked for a crack direction other than the direction indicated by the finite element studies, the crack position rotated 10° downstream was considered to be an unlikely direction. A system of rock joints lay close to the crack direction indicated by the stress analyses and the crack would have tended to follow this joint direction. It was considered to be unreasonable to use an unlikely crack direction in the worst case calculation in combination with other improbably worst case assumptions. A comment on the significance of the crack direction was included in the paper mainly because at some future damsite the crack direction could in fact be critical.

To the General Reporter:

The General Reporter has raised two points which require comment:

Firstly he expresses his preference for Patton's angle of roughness rather than for the author's wave angle to allow for the non-planar nature of the joint surfaces. In Patton's approach the effects of shear strength and surface profile are lumped together. At the Gordon damsite the shear strength and the surface profile have been measured quite independently. The wave angle, which is the inclination of the major irregularities relative to the overall joint surface, was measured by surveying its geometry in adits and at the surface. The shear strength of the active faces of the irregular surface was measured by small-scale shear strength tests. In order to have applied Patton's method by including these irregularities in the shear test and obtaining one comprehensive "angle of roughness" the shear test at the damsite would have required an area of at least 1,000 square feet. As stated in the paper, the size of the irregularities was such that the direction of potential sliding was determined entirely by the orientation of the active faces of the wave angle surface. For this situation it follows that the stability analysis must be applicable to this new direction of sliding and that the shear strength parameters used should be those corresponding to the active faces of the wave angle surface.

The second point raised by the General Reporter concerned his own computation of reservoir filling time using the statement in the paper that the upper 200 ft. of the storage filled at a rate of approximately 2 ft./month. Although this point is outside the scope of this conference it is important that the record be set straight. The indicative figure of 2 ft./month was mentioned in the paper in order to make the point that the rate of increase of water loading on the abutments was sufficiently slow to provide ample opportunity for

measurements of abutment leakage, deformation and piezometric pressure. With no outflow the actual rate of rise in lake level is approximately 2.5 ft./month using the inflow from the Gordon river only. However the rate of rise can be as much as 4 ft./month when water is diverted to the storage from Lake Peddar. Under the combined inflows, power production can commence much earlier than the 10 years calculated by the General Reporter.

Paper by B.K. McMAHON:

Discussion by P. LUMB:

The author must be congratulated on developing his probabilistic approach to a stage where it can be used in practice quite easily, but the recommendation of graphical methods to obtain the probabilities rather than the more efficient vectorial method can be criticised.

Vectorial analysis yields an objective estimate of any parameter, and the precision of the estimate can be assessed in the usual well-known statistical way. The arithmetic can be fairly heavy but this is no great hardship if a simple desk calculator is available.

Graphical estimates are very subjective and inevitably are less precise than the data warrants; moreover the graphical estimates are difficult to incorporate into significance tests or confidence limits, often the principal aim of carrying out a statistical analysis in the first place.

Even with Dr. McMahon's scheme the elliptical probability contours of Fig. 4 would have to be calculated at some stage, and there is little real advantage in an analysis which is part graphical and part numerical. Furthermore, theoretical distribution functions which are more plausible than the planar bi-normal distribution can be inserted into a vectorial calculation quite easily.

The essential difficulty in the statistical analysis is the separation of over-lapping two-dimensional distributions, and unfortunately there is no satisfactory objective method for this. For the simplest case of a mixture of two one-dimensional normal distributions there are 5 unknowns to be sought, and the difficulties mount up rapidly with increasing number of components and dimensions. However, if the components are not too disperse an approximation to the modal co-ordinates can often be found graphically, which reduces the computational task considerably, and a trial-and-error procedure would soon converge onto more reliable values. There is obvious scope here for research into multi-modal spherical distributions.

The statement in Section V that the probability of failure of a slope can be interpreted as the percentage of the slope that can be expected to fail implies a somewhat unusual definition of probability. Geometrical probabilities are slippery things but not as slippery as all that, and some explanation seems called for.

Discussion by A.J. BOWLING:

Dr. McMahon has presented a valuable analysis of the statistical approach to the stability of rock slopes and he thereby conforms with the modern approach to engineering design in general, in which probability considerations are tending to replace the factor of safety concept. The statistical approach also emphasises the importance of obtaining as much information as possible about joint orientations so that the geometrical aspects of rock mechanics problems can be fully assessed.

However, although the graphical procedures described by the author are useful for illustrating the statistical approach, they do not seem to be practicable for application to actual design problems. Thus the plotting of the distorted elliptical contours over the joint orientation diagram (Fig. 4) and the graphic transformation of the probability contours and kinematic instability areas (from Fig. 6 to Fig. 7) must be quite difficult. Perhaps for routine design applications a computer program using vector methods would be preferable to the graphical procedures.

In applying statistical methods it is necessary to specify a maximum probability of failure as a design criterion. Theoretically the probability of failure will only be truly zero when the slope angle is zero although if the apparent angle of joint friction (ϕ_a) is not considered to be a statistical variable then zero probability of failure will be attained for slope angles less than the apparent angle of friction (as shown in Fig. 9 of Dr. McMahon's paper). In this latter case the curve of Probability of Failure against Slope Angle is not asymptotic to zero but actually reaches zero for a slope angle of ϕ_a which is thus the safest slope angle. However to be practical even this slope angle will have some finite, though small, probability of failure. It may thus be better to extend the analysis by incorporating the statistical variations in ϕ_a and to specify, as a design criterion, some maximum probability of failure which will depend on whether partial failure can be tolerated or whether absolute safety is the overriding consideration.

In dealing with the economic aspects of slope design an additional, though perhaps less significant item, which could be included, is the cost of geological investigations to provide the joint orientation data. This would introduce another statistical variable into the overall design problem since more comprehensive geological investigations would increase the total cost of the design but would also provide more reliable basic data.

The Author in Reply:

To Mr. Lumb:

I cannot share Mr. Lumb's view regarding the precision of geological orientation data. When it is considered that repetitive readings on a single joint may vary by several degrees in both strike and dip, the precision of plotting on a Schmidt Net (with-

in one degree) would seem to be sufficient, and the higher precision of vector methods a meaningless refinement.

I do not understand Mr. Lumb's comment that "there is little real advantage in an analysis that is in part graphical and in part numerical" when he points out in his following paragraph that the purely numerical analysis of multi-modal data is at present impossible. Indeed, my own experience with the application of multiple regression and factor analysis to geological problems leads me to suspect that the inherent lack of precision in much geological data would result in the degeneration of any numerical solution to a meaningless result in all but the most simple problems. Yet division of multi-modal data into individual populations is quite obvious on a graphic diagram (as shown in Figs. 2 and 4) and this alone must surely be justification for a graphic approach.

The two-dimensional Normal Distribution is a special case of Fisher's Distribution on a Sphere (Ref. D5) and was used because a graphic solution was readily available (Fig. 8) to provide approximate solutions within the precision justified by the data.

However, where a computer is available, the following approach whereby computer and graphic procedures are combined to analyse the rock defect data is preferable:

1. Orientation data (along with other characteristics of the defect) are key-punched onto computer cards and read by a program which then outputs scatter and Schmidt diagrams in graphic form.
2. The output is visually inspected and subdivided into sets. The approximate mean and planes of near-symmetry are selected for each set and input into the computer.
3. A program then computes the sample mean, Fisher's K and the angular standard deviation in the principal directions, fits a two-dimensional Fisher distribution (extended to the anisotropic case), computes the chi-square goodness-of-fit and 95-percent confidence intervals on the mean and angular standard deviation, and outputs a diagram showing each set contoured in multiples of the angular standard deviation.

The words "For very large slopes", which begin the sentence from which Mr. Lumb quotes "the probability of failure can be interpreted as the percentage of the slope that can be expected to fail", are the key to the proposition.

Let us assume that a large statistically homogeneous slope consists of N equal elements such that the probability that any one of the elements will fail is p. The probability that the element will remain stable is then $q = 1 - p$ and, from the properties of the well known Binomial Distribution, the probability that R elements will fail is given by the Rth term in the Binomial Expansion of $(p + q)^N$. For a large slope however, the expected number of elements failing will approach the mean value which, also from the properties of the Binomial Distribution, is Np . The percentage of the slope which can be expected to fail

is thus $100 Np/N$, which equals p expressed as a percentage.

Reference:

D5. FISHER, R.A. - Dispersion on a Sphere. Proc. Roy. Soc., Ser.A., Vol. 217, 1953, pp. 295-305.

To Mr. Bowling:

It is probable from the wording of his discussion that Mr. Bowling has not worked an example by the recommended procedures.

If he had, he may have found that once the concepts and procedures are understood they are not really as difficult or impractical as they appear to be. As discussed in my response to Mr. Lumb, a combination of computer and graphic procedures is probably the best approach when a computer is available. However, the practical advantages of a purely graphic procedure, which enables a preliminary slope design to be made prior to leaving a remote site so that the input data can be checked and refined if necessary, should be obvious.

The most tedious and time-consuming part of a manual analysis is the computation of standard deviations. With judgement this can be expedited by grouping the data. Alternatively, where the range of each joint set is obvious, a preliminary estimate of the standard deviation may also be obtained from a percentile estimate (Ref. D6) as follows:

$$s = 0.3388 (P_{93} - P_{07})$$

where:

s is an estimate of the sample standard deviation,

P_{93} is the 93rd Percentile,

P_{07} is the 7th Percentile.

Accurate plotting of the distorted elliptical contours from Eq. (1) requires only a knowledge of the properties of the Schmidt Projection and an elementary knowledge of Co-ordinate Geometry.

Graphic transformation of the two-dimensional normal distribution has been widely used in Ordnance studies (Ref. D6). When it is considered that each of the rectangular areas in Fig. 8 represents a probability of 0.1% and that with care, errors involved in the graphic transformation are likely to be compensating, the precision would appear to be adequate for most problems involving geological data.

The use of shear strength as a statistical variable rather than as a discrete value would be a useful and proper refinement.

When exploration costs are significant, they should indeed be included in the economic analysis. However, in many slope studies exploration costs are so small by comparison with the other costs involved that they would not even change the thickness of the line in the total cost versus slope angle curve.

Reference:

D6. CROW, E.L., DAVIS, F.A., and MAXFIELD, M.W. - Statistics Manual. New York, Dover, 1960.

Paper by C.S. GLOE, J.P. JAMES and C.M. BARTON:

The Authors in Reply:

The General Reporter sees our paper mainly as an exposition of field observation techniques and expedients, but the authors consider that the paper is more than this - recording as it does the multi-disciplinary attack and the broad spectrum of geotechnical investigations found necessary for the safe and economic development of a deep open cut in brown coal.

At Morwell it is intended that the northern batters will develop to a height of about 500 ft. of which about 450 ft. will be brown coal. As this height of coal batter is far in excess of that of any other brown coal open cut in the world, there is no previous experience of the long-term behaviour of such a height of coal in a permanent batter.

At the present time the horizontal component of movement at the top of certain batters has amounted to about 5 ft. and the vertical component or subsidence to more than 3 ft. As indicated in the paper, many factors contribute to these movements and investigations are necessary not only to determine the relative significance of the various factors but also to obtain data for aiding the very necessary prediction of future movements.

Field measurements are forming an essential part of these investigations and considerable ingenuity was required to produce extensometer, piezometer and inclinometer installations to depths in excess of 700 ft. in exceptionally difficult subsurface conditions involving high water temperatures (up to 60°C) and high pressures (up to 700 ft. head of water).

The General Reporter has asked for elaboration of a few points and these will be covered briefly in turn.

In Section IV(e) a component of horizontal movement due to differential subsidence is described.

This component simply arises from the tilting of overlying strata due to the differential consolidation of strata at depth, because there is more consolidation occurring in the immediate vicinity of the open cut than at points further removed from the cut. It appears that consolidation arising from dewatering of aquifers at Morwell may extend to depths up to about 1,000 ft. and calculations based upon the differential subsidence pattern in the vicinity of the open cut suggest that less than one foot of the present horizontal movement at the top of the northern batters, for example, could arise from this tilting mechanism.

A bearing capacity type failure is mentioned in Section IV (g). The coal seam being mined is underlain by a clay layer and the possibility of shear failure within this layer due to the weight of overlying material has had to be considered. The coal seam is strongly jointed with a pronounced system of vertical cracks; therefore, the development of a circular slip mechanism within the coal is unlikely. In fact, both analysis and field measurements show that the relevant mechanism is the sliding of blocks of coal on the underlying weaker clay layer.

Regarding the authors' concern about lateral movement of clay and sand beneath the toe of the batters, any movement beneath the toe must be regarded as being critical from the standpoint of stability.

Inclinometer surveys are made periodically in prepared boreholes at defined intervals of depth in order to determine any changes with time in the coordinates of points below the surface. Reference to Fig. 3, for example, shows that below the floor of the open cut there has been some continuous shearing of the M1 coal seam on the underlying strata. The general movements are of a very complex character, there being some tilting of strata and also rotation in the sense that the direction or bearing of movement of the M1 coal seam is different from that of the underlying strata.

TECHNICAL SESSION No. 8—MEASUREMENT TECHNIQUES AND INSTRUMENTATION

- "An Investigation of an Earth Pressure Problem using a Rod Model Analogue", by R. Butterfield & K.Z. Andrawes.
 "Instrumentation of Raft Foundations in Perth", by C.M. Gerrard, M. Kurzeme, D.C. Andrews & R. Topp.
 "An Infinitely Programmable Stiff Loading Frame", by O.G. Ingles & R.C. Neil.
 "Patterns of Strain in Strength Test Samples", by W.M. Kirkpatrick & J.S. Younger.
 "Simple Cantilever Beam Instrumentation for the Determination of Creep Behaviour in Rocks", by L.M.L. Klingmueller & M.J. Wallace.
 "Psychrometric Techniques for Field Measurement of Negative Pore Pressure in Soils", by B.G. Richards.
 "Laboratory Shear Testing of Weakness Planes in Diamond Drill Core", by E.P. Waghorne.

GENERAL REPORTER - Mr. J.M. MADDOX:

Introduction:

The title Measurement Techniques and Instrumentation casts a net over a large part of the science and art of Geomechanics. Virtually any study in geomechanics contains results of some measurements made somewhere, either in experimental testing procedures, or in model analyses of particular problems, or in investigation of the behaviour of actual structures or excavations. No doubt the criterion used by the Conference Committee in allocating papers to this session was whether the particular measurement technique discussed was itself the most important aspect of the paper. Improved measurement of relevant properties is certainly the theme of all seven papers in this session. There is some sense of regret that this subject did not draw a greater response and coverage, because one feels that the greatest possibilities for progress in geomechanics today are in improved and more economical measuring techniques.

It is appropriate to consider the economics of measurements and instrumentation, because the costs of investigations are often quite formidable. Two of the papers are directly concerned with more economical testing, while a third recognises the relatively high costs of a particular in-situ investigation. In general terms it is not sufficient to estimate the cost of an investigation and justify that cost simply as a percentage of the total for the project. A real value analysis should be required before any investigation is undertaken. While such a proposition might be rejected or disregarded by those engaged in research and development of techniques, these researchers are often more conscious of costs than those who are applying the techniques, or those who are requisitioning actual investigations. This is due primarily to lack of appreciation of the difficulty of making real and meaningful measurements.

Almost any measurement is more difficult than is generally supposed, and in geomechanics the problems are even greater than usual. This difficulty is of the greatest importance because making meaningful measurements of soil or rock parameters which are appropriate for any particular situation, and assessing the precision of those measurements in relation to the factors of safety to be applied in design is what practical geomechanics is. An interesting paradox in civil engineering is the low factor of safety frequently used for earth structures, where the

lack of precision in measuring true and representative parameters is notable, compared with those used for concrete or steel structures, for which the uncertainties are considerably less. The deceptively simple concepts of shearing resistance, deformability and permeability, when applied to geomechanics, provide many interesting problems in determining parameters which are even approximately correct for a given set of conditions. There are always uncertainties in any geomechanics analysis, and sometimes assumptions are so fundamental that the evaluation is at best only a rough approximation. It is the responsibility of those who have made, or have directed the measurement of physical properties to ensure that the limitations of the information available are fully appreciated and that the results are applied with proper caution.

There are, in fact, two problems: knowing what to measure, then knowing how to measure it with sufficient accuracy. The first of these is beyond the scope of this session, but one example will illustrate the point. In investigations of rock mechanics problems many engineers order unconfined compressive strength tests of the rock material because it is the most obvious property to measure. Yet in a large proportion of rock mechanics problems the compressive strength is of little value (Obert, 1971). However, there are difficulties enough for this discussion in the matter of making correct measurements with sufficient accuracy.

The first task in any investigation is usually identification and description of the material, and here the engineer deliberately does this in terms of form rather than substance. He is more concerned with physical state than mineral composition; plasticity of clay; particle size distribution of granular material; and spacing and nature of discontinuities in rock masses. Even at this early stage errors may arise. Particle size distribution (Turnbull, 1970) or plasticity tests if not performed on appropriately treated samples can be seriously in error. One of the papers in this session is concerned with the considerable difficulty of making relevant soil moisture measurements. Inadequate exploration of rock masses, and inadequate information about the structural geology, or misinterpretation of the information available often leads to poor identification.

After identification the problems of measurement are compounded, and each determination may further misrepresent the true state of affairs. For instance,

specific gravity values vary according to the testing procedure used, and density of granular materials is remarkable for inconsistency, so evaluation of voids ratio may be in error from both sources. Anyone who has attempted to determine maximum and minimum densities in the evaluation of relative density is aware that results vary with different operators and with different equipment. In-situ density is also difficult to reproduce, and the larger the maximum particle size the more widely the results vary.

So it is with considerable caution that one approaches the task of measuring shear strength, compressibility and permeability, and it is with equal caution that one views the results. The possible variation in results is well illustrated in the determination of the coefficient of friction between two rock surfaces. What is often thought of as a single, simple relationship between normal and shearing forces is actually a variable ratio depending upon the roughness and the unevenness of the particular specimens being tested, the presence of moisture, the presence of discrete particles between the surfaces, the history of previous movement, the intensity of pressure, and the manner of movement permitted in the test. For example, Horn and Deere (1962) showed that moisture can have a very large effect upon the frictional coefficients of very smooth rock surfaces. The static coefficient of friction for a massive-structured mineral (microcline feldspar) was 0.12 when dry, but 0.77 when wet, while the static coefficient for a layer-lattice mineral (chlorite) was 0.53 when oven dry, and 0.22 when saturated. Fortunately the effect of moisture is considerably less than this for natural surfaces, but, on the other hand, the roughness, unevenness, and lack of planarity of natural surfaces result in even greater differences in resistance to shear.

In summary one can say that no physical property in geomechanics has anything like precise values, and no measurement is exact. For this reason contributions to the science which improve the accuracy, relevance or meaning of measurements are particularly welcome.

Introduction of Papers:

Measurement techniques may be conveniently divided into three groups:

- (a) laboratory testing;
- (b) models; and
- (c) in-situ instrumentation of structures and foundations.

Each of these groups is represented in the seven papers presented.

Laboratory Testing:

Interest has been growing in recent years in equipment which can load specimens of brittle material with strain fully controlled. To allow strain to continue at the predetermined rate as rupture approaches, the machine used must be capable of shedding excess load very rapidly, and this excess includes the reaction energy stored in the machine. Ingles and Neil describe instrumentation in association with a standard loading frame and a small computer with sufficiently

rapid response to allow full control over strain at rupture of soft rocks and coal. The system is reported to be capable of off-loading stored energy represented by a frame deflection of 0.05 cm. at 30 tons load in 0.002 sec., which would seem to be a significant achievement. The authors claim that this system permits a greater range of control programme than commercially available servo-control loading frames.

A little more information concerning strain measurement would be of interest, particularly the frictionless band technique to give mean radial strain, and the proposed volumetric strain device. One assumes that all tests in which this equipment is used would be on specimens with "frictionless" platens to ensure uniform radial strain. An alternative method for measuring volumetric strain over the central section of specimens developed in the H.E.C. Tasmania laboratories is described in a discussion of this paper. Many engineers in the geomechanics field will appreciate the availability of such a sophisticated testing machine for controlled strain tests, as the cost of setting up a similar one would not be small.

Klingmueller and Wallace have devised a "simple, inexpensive, reliable and accurate system" for bending creep tests on specimens of rock material. The adjectives are appealing, as they must have been to the financial supporters of the study. The only matter which may affect the simplicity and cheapness of the apparatus is the need for humidity control, but perhaps close control is not required for most work and therefore easily obtained. The method measures the deflection of very small (6 cm. x 1.5 cm. x 0.3 cm.) cantilevered beams of rock. Increments of load are applied with lead shot and deflections are measured by micrometer with an electrical contact sensor.

The arrangement is attractive because, apart from its simplicity, the very small specimens would allow multiple tests on essentially the same material. The creep characteristics of each piece of material can then be amply studied at low cost and analysed statistically. With a high degree of confidence in the parameters of any one sample of rock, full attention can then be given to variations in the material itself. Having said this, one wonders whether there is really much to be gained from testing many specimens from one sample of rock, if the variability between a set of specimens is in fact small compared with the variability between different samples.

The most readily available samples of rock for testing are usually diamond drill cores, and are thus preferred, from the point of view of low cost, to other samples for laboratory testing. In any case drill coring is often the only means of sampling rock. Various arrangements have been made for measuring the frictional resistance of joints in rock core. Waghorne has described a direct shear apparatus for testing joints in rock core which form an angle with the core axis greater than 30°. The principal advantage of the apparatus is that it permits rapid setting up of core for test with the joint oriented in the plane of shearing and rotated to the required direction of shear. The alternative to this mechanical setting is to embed each specimen of jointed core at the required orientation in plaster or concrete - a slower and therefore more costly process.

Because most joints are not particularly plane,

drill core specimens are sometimes considered to be too small to allow a representative area of the joint to be tested. This is generally true, but then almost any area of joint which can reasonably be isolated for testing is too small to be representative. The best that can be done is to measure the frictional resistance of a limited area of joint, observing any movement normal to the plane which occurs, then to "interpret" the result with regard to whatever larger scale undulations and discontinuities that can be observed in the joint. This interpretation is frequently much more difficult than testing representative specimens for shearing resistance. Allen (1971) in a paper to this Conference describes such a study and Hoek (1971) expresses the same views about shear testing of joints.

However there are still difficulties with testing procedures in relation to the natural conditions. A testing apparatus such as that described allows shear in only one plane and one fixed direction, whereas on the actual joint some freedom in direction of movement, twisting and tilting, as well as a degree of progressive failure may be possible, leading to a reduced coefficient value. While the author may conclude "...that this machine has fulfilled the requirements of providing measurements of peak and residual frictional properties of joints", it is suggested that such measurements are only starting points in determining shearing resistance of joints.

Models:

Butterfield and Andrawes have revived and refined the rod model analogue for investigating earth pressure problems. The principal refinement is the use of stereo-photogrammetry to plot contours of displacement of the rods, a much less tedious process than measuring displacements by hand on the model or on photographs. Use of a survey plotting machine allows the contours to be drawn in quite a short time with much greater precision. Displacement measurements of the order of 0.01mm. are possible on the photographic plates used for plotting. The technique is described in detail in a previous paper by the authors (1970), but consists essentially of taking photographs of movement normal to the axis of the lens of a fixed camera and viewing as stereoscopic pairs, in the same way as conventional stereo-photogrammetry involving photographs of a stationary object taken by moving camera. The section of the model which has moved appears to be elevated, the elevation, which may be scaled, being proportional to the displacement. The particular model investigation described in deformation "at low stress levels behind a fairly smooth rigid model retaining wall rotating about its top into the fill". Comparison is made with results from a similar model using sand backfill. The retaining wall itself consisted of seven full-width load cells. The authors show that wall pressures developed in the rod model are similar to those in loose sand, but the kinematic behaviour is quite different from that in loose sand. Kinematic behaviour in the rod material more closely reproduces that in a densely compacted sand.

The General Reporter can only comment that a rod model analogue of this type was made by H.E.C. Tasmania some years ago, following communication between Mr. T. Farrant of Adelaide University and Mr. J.K. Wilkins, but was not pursued because of the difficulty of measuring displacements accurately. The authors' method

would seem to overcome this difficulty.

With a view to more accurate and more meaningful testing Kirkpatrick and Younger have been examining patterns of strain in strength test specimens. In an earlier paper (1970) they showed, working with single size, dry, medium density sand, that uniform axial strain is achieved in compression tests only when lubricated platens are used. Earlier still, Kirkpatrick and Belshaw (1968) showed how lateral strain distribution is improved with lubricated platens. This paper extends the work to include extension tests on cylindrical specimens and extension and compression tests on cubical specimens. All of these tests used single size Leighton Buzzard sand at medium density, and the displacements were measured from radiographs of the specimens which contained grids of lead shot. These papers have quantified what has been tacitly recognised for some time. The authors, perhaps cautiously, state in conclusion that "rough platens should be avoided where reliable stress - strain information is required", i.e., when estimation of strains from displacements measured at specimen boundaries are required.

The question arises as to whether standard procedures for compression tests should be changed, as those in use at present depart markedly from the ideal of uniform axial strain over a known volume of specimen. Should all strength tests be carried out with lubricated platens? Tests carried out in the H.E.C. laboratories on standard concrete specimens, of 2:1 ratio of height to diameter, have shown that removal of platen restraint reduces "strength" by 20-25%. The mode of failure is changed from shear to tension (splitting) and may be more appropriate for many applications.

In-Situ Instrumentation:

Instrumentation of soils and rocks in civil engineering works is quite common, particularly in earth and rockfill dams. However instrumentation of building foundations, beyond survey measurements, seems to be rare - and probably difficult to justify in most cases. Gerrard, Kurzeme, Andrews and Topp describe the instrumentation of soft foundations of three new bank buildings in Perth, which must surely be a record in comprehensive and detailed study of the behaviour of such foundations in Australia. How fortunate to find proprietors of three multi-storey building projects on neighbouring sites willing to contribute to the cost of such a study. Instruments were installed to measure

contact stresses under rafts,
pore pressures under rafts and at depth,
total settlement and deflected shape of rafts,
settlement at depth,
lateral movement at depth,
stresses and strains within rafts, and
column loads.

The authors note that "relative to usual investigation costs, field measurement projects are extremely expensive to undertake, and it is important to examine their probable economic returns." This would seem to be easier said than done, and some idea of the methods of estimating economic returns would be of interest. It is worth mentioning here some of the cost factors involved in instrumentation, because the total cost of an

installation can be severely under-estimated by neglecting some of the following:

capital cost of	sensing instruments,
"	" " reading instrument,
"	" " cables, tubes, connections, etc.,
"	" " calibration apparatus,
labour costs in	calibration of systems,
"	" " installations,
"	" " protection of instruments, cables, etc., during construction,
"	" " reading instruments,
"	" " analyses and interpretation of results.

It should be emphasised to anyone undertaking field instrumentation how important it is to protect the whole installation during construction activity, even though this may seem to be expensive at the time, and to read the instruments at frequent intervals to detect any malfunctions as early as possible. H.E.C. Tasmania experience shows that constant vigilance by one or two of the instrumentation staff on thin arch dam, concrete face rockfill dam and earth dam construction can result in nearly 100% successful operation. Attention only to instrument placing and reading may lead to anything up to complete loss of the whole system. The cost of the investment makes any appreciable loss of instruments intolerable.

Also to be emphasised is the authors' note about extreme care in calibration, installation and interpretation of results. If the difficulties of obtaining true and meaningful results in laboratory tests causes concern, as discussed above, then the possibilities of error in field measurements are so much greater that as many means of cross checking as possible are necessary to prove the validity of the results. Many engineers do not recognise this difficulty sufficiently because individual propositions appear to be so simple in concept that they tend to be thought of as "fool proof". Cross-checking different instrumentation system results or with design predictions is often chastening. Nevertheless the exercise is very necessary and worthwhile. Instrumentation can show that refinements in design have little meaning in reality, and true strain situations may only approximate those calculated. The paper indicates optimism in the expected degree of validation of prediction, method of modelling, and relevance of the material properties and initial conditions measured. The authors may be begging the question: it would be unusual if the behaviour of the materials were entirely predictable, and if the instruments themselves did not produce at least some anomalous results.

Richards' paper is an esoteric paper in that it will have value only to those who have studied the previous work on the subject of measuring negative pore water pressure. In an earlier paper (1968) Richards explains that apart from the difficulty of gravimetric water content not being a "physically continuous function" and that "extreme variations in moisture content occur over even microscopic dimensions in soil", little usefulness can be attached to moisture content results. There is no fundamental relationship between water content and soil engineering parameters. Pore pressure, on the other hand, can be regarded as a continuous mathematical variable, as it is related to the energy of retention of the

water in the soil and obeys hydrodynamic and thermodynamic laws. This applies also to negative pore pressure in partially saturated soils. However there are some difficulties here, because negative pore pressure, or soil suction, is not easily measured and is comprised of several components - capillary absorption, osmotic, gravitational and thermal. Some of this is explained more fully by Aitchison (1960). Total suction is the moisture stress measurable only in terms of vapour pressure. Matrix suction is the moisture stress measurable by tensiometer, pressure membrane or suction plate. In many soils the difference between matrix suction and total suction is not significant within the range of moisture stresses commonly considered, and Marshall (1959) suggests that matrix suction may be the only significant component of suction up to total suctions of about 220 atmospheres.

The present paper concerns total suction, which ranges up to 1500 lb./sq. in. in Australian conditions, and thus can only be measured in terms of vapour pressure. A wet and dry bulb thermometer is used, and from the second law of thermodynamics soil suction is obtained from the relative humidity in the pore space.

Some field measurements have been made with the thermocouple psychrometer probes. An adjustment of the calibration curve of suction vs. instrument output in microvolts has been necessary to bring suction values to levels which experience suggests to be appropriate. Further investigation is proceeding to improve probe design and calibration procedures.

Though perhaps still not widely recognised, the possible importance of negative pore pressures has been appreciated in Australia for some time. Trollope (1961) has suggested that if the development of suction is accepted few analyses of slope failures are valid because of the neglect of this factor, and that negative pore pressure may be a fortuitous compensating factor for inadequacies in assumed stress distributions in the standard analytical methods.

Concluding Remarks:

Most measurements in the field of geomechanics give values which are only approximations or representations of complex situations. The subjects of the seven papers presented in this session are diverse, but each paper is concerned with improving measurement or instrumentation techniques, whether to obtain information where little has so far been available, to increase their accuracy and reliability, or to reduce the costs. It is by such improvement in measurement, coupled with close observation of actual behaviour, that we increase our understanding of the problems.

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Paper by C.M. GERRARD, M. KURZEME, D.C. ANDREWS and R. TOPP:

The Authors in Reply:

The General Reporter's comments are very valuable in further highlighting the sources of possible error in large field instrumentation programmes. All these sources should be borne in mind when assessing the results of field observations. The scarcity of such observations presents a danger of over-valuing the worth of the available results.

Paper by O.G. INGLES and R.C. NEIL:

Discussion by A.J. BOWLING:

As part of the Commission's investigations into the properties of mass concrete for structures such as arch dams it has been necessary to determine the variation of volumetric strain with load in concrete test cylinders. A volumetric strain gauge has therefore been devised which consists of a combination of electric resistance strain gauges bonded to a concrete cylinder. This note describes the principle of the volumetric strain gauge and outlines how it has been applied in laboratory investigations.

Principle of the Volumetric Strain Gauge:

The volumetric strain e_v in a cylinder can be expressed in terms of the longitudinal strain e_l and circumferential strain e_c as follows:

$$e_v = 2e_c + e_l \dots\dots\dots(1)$$

Longitudinal and circumferential strains can conveniently be measured with electric resistance strain gauges. It is shown below that by connecting one circumferential strain gauge in series with two parallel connected longitudinal strain gauges a circuit is obtained which will indicate volumetric strain directly.

The total resistance R_t of the combination of three strain gauges is given by

$$R_t = R_c + \frac{1}{2}R_l$$

where R_c is the resistance of the circumferential strain gauge and R_l is the resistance of each longitudinal strain gauge. The change in total resistance, dR_t , is given by

$$dR_t = dR_c + \frac{1}{2}dR_l \dots\dots\dots(2)$$

The equation relating strain to resistance change in an electric resistance strain gauge is

$$dR/R = k \times e \dots\dots\dots(3)$$

where k is the gauge factor of the strain gauge wire.

Substituting in Eq. (2) for dR_c and dR_l ,

$$\frac{dR_t}{R_t} = k \times e_c \times \frac{R_c}{R_t} + \frac{1}{2}k \times e_l \times \frac{R_l}{R_t}$$

If $R_c = R_l = R_0$ then $R_t = 1\frac{1}{2}R_0$

$$\text{and } \frac{dR_t}{R_t} = \frac{1}{3}k (2e_c + e_l)$$

Substituting from Eq. (1) for e_v

$$\frac{dR_t}{R_t} = \frac{1}{3}k \times e_v$$

The combination of two longitudinal strain gauges and one circumferential strain gauge thus forms a volumetric strain gauge with a gauge factor of $\frac{1}{3}k$.

In the present investigation two volumetric strain gauges have been formed from four longitudinal strain gauges, spaced 90° apart around the cylinder, and two circumferential strain gauges, all six gauges being located near the centre of the cylinder to avoid end effects. The two volumetric strain gauges form the two opposite active arms of a Wheatstone bridge circuit, the other two arms of the bridge being formed

by temperature-compensating dummy strain gauges. This circuit is twice as sensitive to strain as a single volumetric strain gauge and the strain indications of the six individual strain gauges are automatically averaged. The out-of-balance voltage, V_d , across the bridge circuit, due to equal volumetric strains e_v affecting the two active arms, is given by,

$$V_d = \frac{1}{6} V k e_v$$

where V is the bridge voltage.

Application of the Volumetric Strain Gauge:

In order to have longitudinal and circumferential strain gauges with a length sufficient to average the strains across the concrete aggregate and matrix, the strain gauges are made in the laboratory by gluing resistance wire directly to the concrete. About 40 in. of rayon-covered copper-nickel resistance wire is used for each gauge giving a gauge resistance of about 80 ohms. For the longitudinal gauges a grid is formed with a length of about 5 in. and a width of about $\frac{1}{2}$ in. For the circumferential gauges the resistance wire is wound about twice around the cylinder.

Before the gauges are attached the cylinder is allowed to become surface dry. A thin coating of epoxy glue is then applied to the central section of the cylinder to fill in any small surface holes and to form a base for attaching the resistance wire. When the epoxy glue has set the two circumferential gauges are formed by gluing the resistance wire to the cylinder with a nitrocellulose glue. These two gauges are usually about 2 in. apart. The four longitudinal gauges are then formed, again using nitrocellulose glue, a jig being used to form the

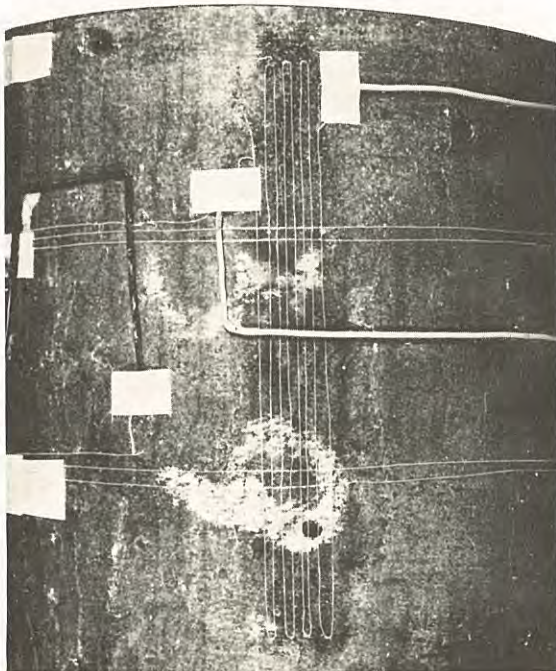


Fig. D1. - Electric Resistance Strain Gauges attached to a Concrete Cylinder.

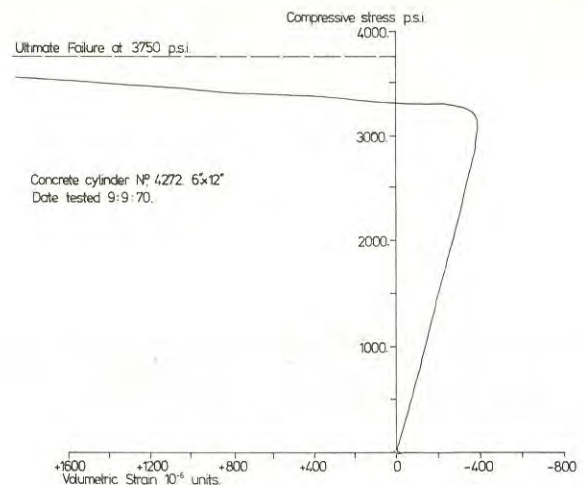


Fig. D2. - Typical X-Y Recorder Plot of Volumetric Strain against Compressive Stress.

wire grid before it is applied to the cylinder.

When the glue has hardened the resistance wires are trimmed so that all six strain gauges have the same resistance. The electrical connections are then made after which the cylinder is ready for testing (Fig. D1). For testing purposes the two dummy arms of the Wheatstone bridge circuit are formed by a similarly instrumented cylinder which remains unloaded during the tests. The bridge circuit is activated by a D.C. voltage of about 4V and the out-of-balance voltage, which is proportional to the volumetric strain in the test cylinder, is fed into an X-Y recorder. A typical plot of volumetric strain against load obtained from the X-Y recorder is shown in Fig. D2.

The Authors in Reply:

To the General Reporter:

The strain measurements shown have been taken by a Schaevitz model 100HR transformer having a linear range of +0.100 to -0.100 in. with an output sensitivity of 4.3 mV per 0.001 in. displacement per volt input at 2,000 Hz. All linear strains are measured between the platens, deformations in the total system other than in the specimen being automatically deducted using stored calibrations. The radial strain measurement by frictionless band is essentially an interim technique, in which one end of the band is anchored and the displacement of the other end measured by a linear displacement transducer. Although this can be made to give an accurate mean radial (circumferential) strain for the point of application of the band, multiple bands (or a continuous frictionless wire device) are required to ensure that variations in the whole vertical profile are measured. If this is done, the calculated volumetric changes can be compared with those taken by a direct volumetric strain measurement device such as a liquid displacement gauge operated at constant pressure and temperature. As observed by the Reporter, frictionless platens are highly desirable; but even if this condition is not achieved, any non-uniform deformations can be recorded by the present techniques.

To Mr. A.J. Bowling:

The technique of volumetric strain measurement described is ingenious and simple, and we are obliged to Mr. Bowling for its description. Where very low modulus materials are involved, the two axial gauges used to average vertical strain could introduce some error; this would be negligible for higher modulus materials such as rocks, concrete and the like.

Paper by W.M. KIRKPATRICK and J.S. YOUNGER:

The Authors in Reply:

The main point raised by the General Reporter on the subject matter of our paper refers to the question of whether all strength tests should be carried out using lubricated platens. Reference was also made in the spoken report to works in which statements have been made to the effect that measured stress-strain behaviour was independent of the end-restraint condition, whether rough or lubricated, imposed at the platens in the triaxial compression test.

Our interest in this problem stems from a desire to be able to make reliable measurements of stress-strain response and we have expended considerable effort in examining conditions within strength test samples of soil. As a result of this we can only state that any experience such as that described by the statement above is completely at variance with ours.

Regarding the matter of whether lubricated platens should be used as a standard in strength tests, there are certain well established facts with reference to the triaxial compression test which can allow judgements to be made:

(1) In sands, nominal peak strengths appear to be similar whether rough or lubricated platens are used.

(2) Also in sands the stress-strain response is different depending on the end condition. Only under lubricated end conditions does a homogeneous state of strain develop.

It can be said therefore that if only peak strengths are required it does not appear to matter but if reliable stress-strain data is needed lubricated platens are essential. Since end lubrication is easily provided there does not appear to be any reason to use another.

(3) Evidence of the benefits of using lubricated platens in the testing of clays is also available. In undrained tests it has been found that non-homogeneous pore pressure measurements - reflecting non-homogeneous strain conditions which are present with rough platens are largely removed by the use of lubricated platens.

(4) We have no personal experience with regard to the influence of end conditions in the testing of rocks or other brittle materials. The criterion for desirable behaviour is however the same - that is a homogeneous state of stress and strain should develop in the samples. We would suggest, on the basis of the behaviour of soils, that if effective means of end

lubrication can be found such should be used in the testing of rocks and similar materials. There appears to be a need however for research into the behaviour of test samples of these materials under different end conditions.

(5) There is less information regarding the distribution of stress within samples. Research at the University of West Virginia initiated by the senior author helps however to throw light on this matter in reference to soils. In this work reported by Seals, Newman and Kirkpatrick the distribution of normal (axial) pressure at the rigid platens of $9\frac{1}{2}$ min. dia. compression samples of Ottawa sand were measured using diaphragm gauges placed at various locations in the end platens. Tests were performed under lubricated and non-lubricated end conditions on loose and dense sands. The results were collected together to typify the conditions at three stages of the tests termed the "elastic" range at axial strains less than about 0.6%, the transition stage: $\Sigma \sigma_{ax} = 1.25\%$ to 3.75% and the ultimate range: $\Sigma \sigma_{ax} = 10\%$.

Fig. D3 shows boundary axial stress data for tests using non-lubricated platens on dense sand in the "elastic" range. In this the ratio of the measured stress at the gauge to the average axial stress (applied boundary load divided by the overall sample area) is plotted against the ratio of the radial position r of the gauge to the maximum radius R of the sample.

Appreciable scatter of the results is evident as would be expected in this type of measurement. Regression curves for the non-lubricated dense samples at the three stages of the test are shown in Fig. D4. These curves show a non-uniform distribution with the stresses at the centre being lower than the stresses at the periphery. There is a redistribution as the test continues to higher strains with the peripheral stresses becoming larger and the stresses at the centre becoming smaller.

This non-uniform stress distribution represents a non-homogeneous state of stress within the sample

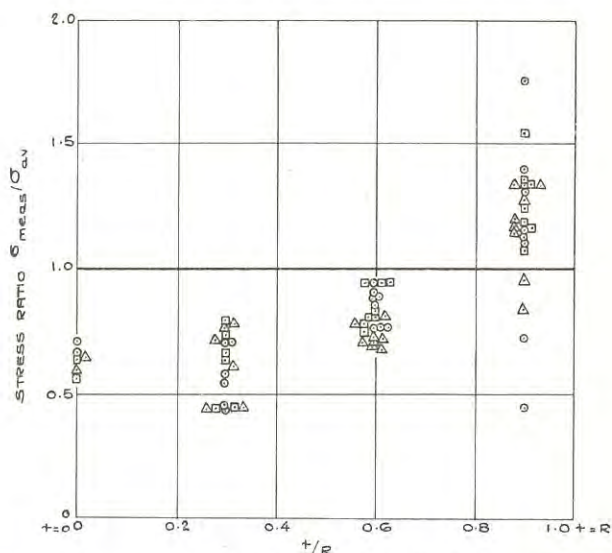


Fig. D3. - Rough Platen - Dense Samples "Elastic" Range.

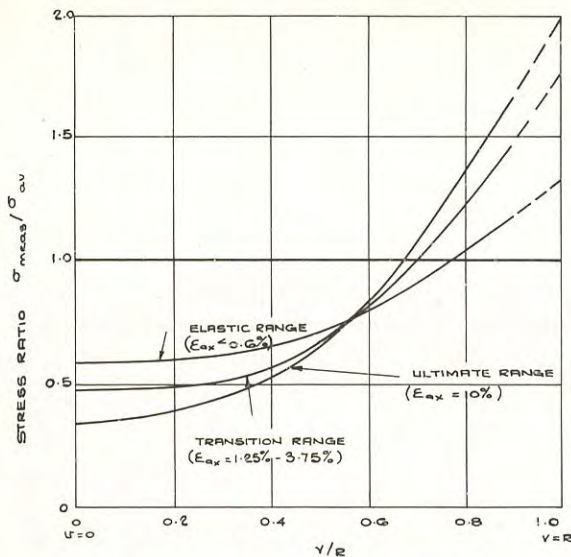


Fig. D4. - Stress Distributions over Range of Test Rough Platens - Dense Samples.

and is a reflection of the non-homogeneous strain states measured in compression tests with non-lubricated platens.

Similar stress distributions were found in loose samples tested with non-lubricated platens.

Fig. D5 shows typical data for tests using lubricated platens. Again a certain scatter exists but regression analyses show uniform or near-uniform stress distributions.

Although little can be said about the distribution of stress within the sample it can be presumed that any lack of uniformity is likely to be most severe at the rigid platen boundary. If this is the case it might be possible to assume for lubricated platens that a uniform distribution of axial stress exists throughout the sample. If it can also be assumed that platen lubrication reduces end friction to negligible proportions (Rowe and Barden quote very low values for μ) then the elastic solution to the stress distribution can be obtained by superposing the solution for the cylinder under purely uniform radial pressure on to the solution for the cylinder under purely uniform axial stress. This allows the inference to be drawn that the lubricated cylindrical

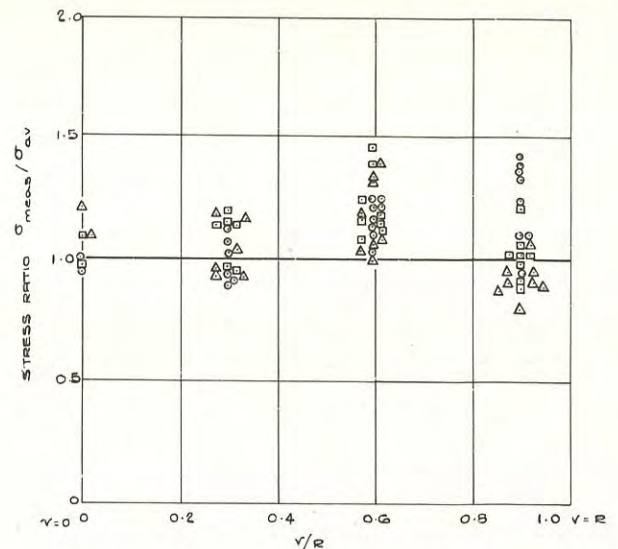


Fig. D5. - Lubricated Platen - Dense Sample - Transition Range.

triaxial sample supports a homogeneous state of stress with the condition that the radial and tangential stresses are equal. This refers to the elastic state but since there is no significant redistribution of axial stress nor any significant change in the strain distribution as the test advances it might be expected that the homogeneity extends throughout the entire range of the test.

It would appear from these observations of stress and strain that in sands at least platen lubrication produces a near-ideal state within the triaxial compression test and probably also in the other tests referred to in our paper.

These desirable features are absent in samples tested with non-lubricated platens. Tests involving such samples cannot be expected to yield reliable stress-strain data.

Reference;

SEALS, R.K., NEWMAN, B. and KIRKPATRICK, W.M. - Stress Distributions at the Platens of Compression Samples. In preparation.

TECHNICAL SESSION No. 9—ENGINEERING GEOLOGY

"Terrain Evaluation as a Basis for Engineering Geology", by K. Grant.

"Underground Investigation for Large Excavation at Victorian Arts Centre", by R.M. Johnson.

"Preliminary Geological and Foundation Investigations for a Nuclear Power Station at Jervis Bay, N.S.W.", by J.P. MacGregor.

"Some Properties of Weathered Greywacke", by M.J. Pender.

"Seismic Attenuation in Engineering Site Investigations", by E.J. Polak.

"Classification of Rock for Engineering Purposes", by L. Obert & C. Rich.

GENERAL REPORTER - Mr. D. STAPLEDON:

The prime function of engineering geology, whether carried out by an engineer, an engineering geologist, or a team is to contribute to the development of a geomechanical model, which has to be

project purposes, it can become almost an end in itself, because the objectives are so broad - to collect all geotechnical data significant for engineering purposes, regardless of the specific needs of the project.

The ability of the system to store and retrieve geotechnical data should in the long term prove to be its most valuable attribute. Certainly it would appear to work very well for data relating to engineering structures at or close to the ground surface, e.g. roads, airfields and small buildings.

One cannot, however, agree with the author's statement at the end of his third last paragraph on p. 403 and I quote, "thus all geotechnical information significant for engineering purposes can be tied to the terrain itself by the use of a system of terrain classification." On a site selection survey for an underground power station in the Snowy Mountains, a Swedish engineer wanted to avoid locating the building beneath a slope which showed leaning trees. In his experience leaning trees meant unstable ground - probably shallow soils creeping downslope over buried bedrock. In the particular case in the Snowy area, the leaning trees were in a 50-ft. deep surface layer of weathered granite, and the power station was to be 1,000 ft. below, in fresh granite.

Mr. Pender has produced an impressive array of test results, which should be of considerable value to site investigators in the Wellington district.

It is interesting to note that he found "Grade 3 was more in the gravel range and so the bulk of the discussion here is confined to the more weathered material". It appears that Mr. Pender appreciated that the grades IV to VI material were sufficiently homogeneous to be tested as soil substances, while the Grade 3 material was rock mass, the behaviour of which would be governed more by the properties of its contained defects (joints), than by the properties of the substance.

His tables show that there are characteristic ranges of properties for each of Grades IV to VI, but even with these grades the wide ranges of compression indices suggest that the sample behaviour was influenced partly by the presence of defects. The joint spacing being down to $\frac{1}{2}$ inch, apparently random, probably accounts for the reasonable groupings of results of the consolidation and

(1) sufficiently detailed to include all relevant geological features - but at the same time

(2) sufficiently simple to be tested, quantified and analysed using the methods currently available,

(3) described in terms which must be clearly defined, unambiguous, and as quantitative as they realistically can be.

Dr. Polak has observed some interesting variations in attenuation factors recorded during seismic refraction work. Unfortunately the surveys were not aimed primarily at research on attenuation changes, nor at engineering objectives.

Probably as a result of this, there is not enough detail available on the engineering properties of the rock masses investigated, for the meaning of the measured values (quoted at the top of p. 433) to be assessed fully. The geological names of rock substances convey very little in this context.

The relationships to porosity and water content - see VII(b) on p. 433, appear to have useful implications for engineering purposes. No doubt they explain the range of figures obtained for overburden and the increases observed in areas of faults and weathered dykes - Dr. Polak may wish to comment on this further. The greatest limitation on the value of attenuation factor measurements for engineering purposes appears to be that the factor for the overburden cannot be measured if the overburden-rock interface is horizontal.

Mr. Grant describes a classification system which is intended to be complementary to engineering geology. The system is certainly orderly and rational, but I wonder about the cost-benefit ratio, for many engineering purposes. The author may be able to tell us more about the costs involved.

It appears that there is a danger that if this particular terrain evaluation system is used for

triaxial tests. The author notes (p. 427, 2nd para.) that "only a very small number actually failed along one of these planes". It would be useful to know this number as a percentage, and to show these results by a different symbol on the graphs.

The author's note on preconsolidation pressures (p. 426, 3rd para.) is certainly agreed with. The lithology and engineering properties of the fresh greywacke substance are not described in the paper, but it is likely that it derives much of its strength and deformation properties from cementation of its grains. Can the author give more detail here?

The paper by Obert and Rich also discusses rock classifications. The authors are aiming to develop a general classification of rock masses for specific mining purposes. They are clearly very wise to collect the general data set out on Tables II and III, and apply logic to it, as on p. 437, but I wonder whether any more elaborate analysis of this data is warranted, considering its variable quality, and the variable descriptive terminology.

The major obstacle to Dr. Obert's research appears to be the lack of good quality cores and reliable test data from the mining companies, who, as in Australia, are often little concerned about core quality during their early exploratory drilling - proving orebodies is their prime objective.

I am surprised that the authors were not more critical of the RQD system. Although the logic in the 2nd and 3rd last paragraphs on p. 437 indicates partly the weakness of the RQD system, their inclusion of the Tunnel Support Table - Table I - suggests that the authors approve of this table. I hope Dr. Obert will give us his opinion of this table. We also look forward to further data on the seismic absorption coefficient, and on the microfractures approach, as these become available.

I particularly enjoyed the last paragraph of this paper, and the last sentence is worth quoting - again and again and again ----.

The papers by Johnson and MacGregor both describe the results of large site investigation programmes for major civil works. It is interesting to note the different approaches of the authors in their paper layouts. Mr. Johnson, an engineer, has described the proposed structures and defined the engineering objectives early in his paper, whereas Mr. MacGregor, an engineering geologist, barely mentions these until the fourth page. Allowing for the fact that the Arts Centre study was aimed at design, and the Power Station study essentially at site selection, this difference could still reflect a difference in the approach of the authors to their respective site investigations.

Mr. Johnson attempts to cover the results of a very large investigation in a short paper, and this gives the impression that the investigation was rather disjointed, which may not have been so.

Mr. Johnson appears to have collected a large amount of test data on a poorly defined geological model (Fig. 4 and "Silurian Rock", p. 412). Rather than fitting into the context of a geomechanical model for the site, the Geologist's description is put in a compartment and quoted exactly.

The author does not explain how he used the RQD figures!

Considering the water level indicated on Fig. 9, and wall depth criterion (iii), should seepage forces or hydrostatic forces on the underside of the failure wedge have been ignored? Is any system of drainage proposed here?

Paper by K. GRANT:

The Author in Reply:

The subject matter presented in the paper is essentially a summary giving a coverage of a large topic in precis form for the benefit of engineering geologists. Books could be written on what is known of this subject. Indeed, at the present time a manual, that will be a comprehensive treatise of the state of the art and how to do it, is in course of preparation. The restriction of the paper to seven pages coupled with the necessity for the presentation of the type of information gained at the terrain classification stage dictated brevity. Nevertheless, the Reporter has raised some important matters that provide the opportunity to expand on the subject.

The application of this system of terrain evaluation within the engineering sphere is not a costly procedure. The prime cost is in the terrain classification processes inherent in the procedure. This cost may, however, in the long term, be offset against savings in investigation costs and better performance of the finished structure, because each significant difference in relevant terrain factors has been evaluated, but duplication of effort has been avoided.

Regional classification of terrain at province, terrain pattern and terrain unit levels for feasibility studies and planning purposes has been shown to cost between \$0.25 and \$1.00 per square mile depending on the location of the particular area. For areas adjacent to cities the cost is low, but it increases with increasing distance therefrom. The highest cost is related to remote areas in which travelling and logistic costs are high. It would not be unrealistic to say that the whole of Australia could be classified at this level for a total cost of less than \$3,000,000.

The cost of detailed classification to terrain component level is more difficult to assess. This type of classification will usually be done only in the area that directly influences the particular project. The cost can be related to the relative complexity of the area and its location. Perhaps a figure of about \$25 per square mile would be a realistic average figure, but this may still vary somewhat with the location of the project.

Use of any system as an end in itself is, of course, pointless. Systems are always designed for application; unless they are applied, they die. Terrain evaluation, as here stated, is a system designed to assist engineering by rationalising the gathering and processing of engineering information. Terrain classification is but an incidental, yet necessary, part of the system. If this system is not

used for information gathering and processing or, in time is superseded by a better system for the same purpose, it too will die. What use is there in gathering and processing unused or unwanted information?

There is nothing new in the concept of terrain evaluation. Engineers and engineering geologists have been evaluating terrain for many years, but they have been doing this mentally, without recording it. What is offered in this paper is a standard system for recording the results of engineering investigation and for processing the information obtained. Information, for proper processing, must be tied to some invariant. In the short term, terrain can be regarded as invariant. The type of information gathered is about the terrain, so the invariant terrain becomes a logical choice for the base upon which information is processed; hence the principles of terrain evaluation as expounded in this paper.

Use of this system for terrain evaluation may require some small changes in the usually accepted procedures for gathering information. Information must be gathered in accordance with significant changes in the terrain as determined in the terrain classification stage of the evaluation process and not in terms of arbitrary measurements from fixed points as is now usually done. However, such a transformation should not be a difficult procedure.

An advantage of the rigidly defined terrain classes used in the proposed system is the facility for the extrapolation of information. Once it can be shown that any member of any class is sensibly homogeneous, irrespective of its occurrence, the properties gathered from any one location of that member can be extrapolated to all members. In these circumstances cost savings in site investigation can be considerable.

It is proposed that the system should operate as a two-stage process. Regional classification and evaluation of terrain at province, terrain pattern and terrain unit levels normally produce sufficient information for feasibility studies and for planning purposes. More detailed classification at terrain component level and more detailed evaluation at all relevant levels can be done later in the more limited area of interest defined by the planning process. In each case the set of information collected will be only that relevant to the purpose in hand. However when the terrain has been classified, that classification will remain valid for all future purposes in the same area.

The use of such a system allows consideration of regional as well as local factors in the planning stage. For instance, in planning the route of a new road in the open countryside, there is, initially, a large number of possible locations or, alternatively, for urban planning there is a large number of possible sites for each particular sphere of activity. The best location will depend upon the relative economics of building in particular areas with different types of topography, or different soils or from different natural constructional materials, etc. The use of regional terrain evaluation at a relatively coarse level supplies sufficient data for the assessment of all these factors in an integrated form. Such an assessment provides sufficient data to determine on a rational basis the most economical, general location.

When this general location has been determined, the detailed evaluation of the limited area that influences the exact location provides the data for the final design and construction of the project.

In proposing this system, it is envisaged that regional terrain classification for feasibility studies and planning purposes could be conducted economically on a continental basis, because such a procedure must cover all possible locations. However, it is not proposed that detailed classification for construction purposes should be carried out on such an extensive basis. It would, indeed, be uneconomic to do so because, in many areas, location would be chosen elsewhere and no evaluation would be required. It is proposed that this type of classification should be conducted only in the area that influences the particular project.

Whilst terrain evaluation allows and, what is more, is designed for the processing of all information relative to the engineering significance of terrain, it does not require the collection of all that information at any one time. Clearly it would be uneconomic and certainly futile to collect any information other than that which was required specifically for the purpose in hand. Indeed, who can specify what is all information of engineering significance?

Each engineering objective requires the collection of a set of information that is essentially different from the set required for any other objective, although some information may be common to two or more sets. For each objective, the information in the set relevant to it must be specified, then collected. Gathering of unspecified information can be simply a waste of time and effort, because it may never be used.

In any area, once the basic terrain classification has been done to the required level, the set of information collected for any one purpose can be stored in the system as can each succeeding set of information. Of course, if a set of required information contains some items that have already been collected in any other set, this information can be retrieved from the system and does not need to be collected again. Again it would be uneconomic and futile to do so.

Thus the information storage and retrieval system can be regarded as an incremental one. As each project requires the collection of a set of new information, an increment can be added to the system. At any time, any of the information in the system can be retrieved in any form.

Whilst the system is capable of handling all data of engineering significance, it will only contain all such data when all engineering works possible have been constructed. As engineering is a continuing process, it is likely that the information in the system will never be complete at any one time.

The Reporter also raises a question about the applicability of the system to engineering projects sited underground. He cites the case of an excavation in solid rock 1,000 ft. below the surface. The system is amenable to handling such a situation. If the excavation is, as the Reporter cites, in

solid rock, it is simply a matter of tying the information to the particular rock type concerned. An excavation in granite produces a set of information different from that in shale etc. This situation is provided for in the system by the concept of the province, which is defined simply in terms of geology. If the excavation is closer to the surface where soil or slope characteristics have an influence, the situation is provided for at lower levels, for instance, at terrain unit level, in the hierarchical terrain classification system. The word terrain embraces the earth, not simply the surface of the earth.

The paper offers a standard system of information collection, collation, storage and retrieval that can handle information in incremental form and can deal with that obtained at any level of detail from the finest to the broadest and from the surface to any depth underground. Processed information can be retrieved from the system in any form at any time. Costs are not high and certainly the system can be used only for the specific needs of any project and for engineering application; use of it as an end in itself is pointless.

Paper by R.M. JOHNSON:

The Author in Reply:

1. We consider that the geological model was adequate. This investigation was preceded by a series of 11 other boreholes evenly distributed over the site and allowed 6 cross-sections to be drawn. These boreholes are, in fact, shown on Fig. 1 as dots. The ground water level was approximately RL O.

2. The RQD figures were requested by registered tenderers for the excavation who wish to evaluate the difficulty of excavating in rock. The RQD figures were not used in evaluating the resistance to shear along a failure plane. For this purpose we studied the likely occurrence and properties of weathered rock.

3. No system of drainage is proposed beneath the failure wedge. Where the failure wedge became unstable it is to be held in position by rock anchors. An under-floor drainage system is proposed to collect water on the floor of the excavation.

4. A flow net with equi-potential lines and based on Darcy's Law was drawn for the ground water flowing beneath the perimeter wall. Seepage forces on the underside of the wedge should not be ignored and, in fact, have not been in subsequent analyses. At the time the paper was written, a simplifying approximation had been introduced into the analysis whereby seepage forces on the underside of the wedge and also on the back of the wall were ignored. Thus, both activating and resisting forces on the wedge were underestimated.

Paper by M.J. PENDER:

Discussion by P.LUMB:

This paper gives a very useful summary of the

properties of weathered greywacke, which fills one of the gaps in knowledge of the behaviour of residual soils.

The classification scheme of six grades I to VI, which originated from the Snowy Mountain Scheme, is often used in Hong Kong to characterise the degree of decomposition of the igneous rocks. The first four grades, I to IV, are really the only useful grades, referring to material which is still rock and can be cored. Grade V and VI are true soils in which all mineral grains have been separated, and from the descriptions in the paper it seems likely that the materials classed by Mr. Pender as Grade IV ought really to be classed as Grade V.

In Hong Kong the Grade VI material has quite different characteristics from the underlying Grade V, and it can be misleading to plot test results from both Grade V and VI material on the same graph. There is a discontinuity between V and VI, just as there is between IV and V, rather than a gradual change. The Grade VI results of the paper should have been analysed separately from the remainder, particularly in view of the doubt as to the origin of this material, since it is clear from the plasticity chart of Fig. 2 that there is a clear wide gap between two distinct groups, Grades IV and V and Grade VI.

In Section II referring to the Grade IV details the joint plane coatings are attributed to manganese dioxide. Is there any direct evidence of manganese other than the black colour? St. John et al (1969) showed that for residual soils in Georgia, U.S.A., the black coating contained appreciable iron oxides and organic matter but little manganese, and this may be generally true.

With regard to the apparent preconsolidation pressures of the Grade IV and V material, the two voids ratio/log. pressure curves of Fig. 4 are not very convincing evidence of a straight-line portion of the compression curve. Both test results show some curvature even in the range 20 to 100 T/ft. For granular materials such as these, the use of techniques developed for clays (such as Casagrande's method of estimating preconsolidation pressure) may produce a numerical value, but this value does not necessarily have any physical significance. The high pressures used in the tests must have caused some break-down of the sandstone particles, and the nature of the material may well be quite different at low and high pressures in the same test.

The strength test results of Figs. 7 and 8 show remarkably low angles of shearing resistance and high cohesion intercepts for some of the Grade V samples at a voids ratio of about 0.7. The results of Fig. 9 for the tests on samples saturated by back pressure are far more reasonable for this type of soil. If the samples of Figs. 7 and 8 were not fully saturated then the pore-pressures as measured would not necessarily give accurate estimates of the effective stresses unless very fine-grained porous plates were used in the tests. More details of the actual test procedure would be valuable.

Reference:

St. JOHN, B.J., SOWERS, G.F., and WEAVER, C.E.

"Slickensides in Residual Soils and their Engineering Significance". Proc. Seventh Int. Conf. on Soil Mechanics and Foundation Engg., Mexico City, 1969, Vol. 2, pp. 591-597.

The Author in Reply:

To Mr. P. Lumb:

Mr. Lumb has raised several interesting points. As was mentioned in the paper the information presented was gathered during the course of an investigation of a particular site. It is hoped that further investigations at other sites will provide additional information about the material found in the Wellington area and also provide an opportunity for more complete investigation of some of the conclusions given in the paper.

In applying the sixfold classification scheme of Fookes and Horswill to a particular weathered rock a decision has to be made on how the six grades are to be applied. This division is to some extent arbitrary and sometimes it is difficult to classify material that is near the dividing line between two adjacent grades. Nevertheless the sixfold division decided upon for the greywacke seems to provide a suitable means for visual description of this particular material. Apart from the differences in the values of the various properties grade IV is differentiated from grade V by being harder to crumble and by the presence of occasional much harder lumps, clearly the weathering process has not proceeded as far as in grade V. I do not agree with Mr. Lumb's comment that only the first four grades are useful. This seems to imply that the scheme can be applied only to material that is still recognisably rock whereas it is intended to describe the complete range of materials that can be derived by weathering from a given parent rock.

The test results for grades IV, V and VI were all plotted on common diagrams to illustrate one of the major conclusions of the paper. This is that the void ratio of the material is the most important factor in determining the values of the various properties. The fact that this conclusion seems to be valid over the full range of void ratios found, despite the discontinuity between grades V and VI shown on the plasticity chart of Fig. 2, is most significant. It means that the void ratio provides an independent basis for classification. It can be used as an objective check on the visual classification scheme, at least for the more weathered material, and also for narrowing down the likely range of values for various parameters in particular cases.

The presence of manganese dioxide along the joint planes had been confirmed by a chemical analysis and so the black deposit is of a different nature to that mentioned by St. John et al (1969). There seem to be other differences as well. In the greywacke the black seams are very much thinner than the typical thickness ranging between $\frac{1}{8}$ " and 1" reported by St. John et al and also in the greywacke they are local in nature, generally extending for inches rather than feet.

With regard to preconsolidation pressures the curves

for the grade IV and V material in Fig. 4 are linear in the 20 to 100 T/ft² range, the last three points on each curve lie on a straight line. Because of the reduced scale of this diagram it is not possible to determine the preconsolidation pressure accurately. All the values plotted in Fig. 6 were determined from larger-scale diagrams. It was explained in the paper that the values of the preconsolidation pressure obtained by Casagrande's construction do not imply that the physical explanation given by Casagrande for overconsolidated clays holds for the greywacke. However, regardless of the actual physical processes involved the "preconsolidation pressure" obtained does represent a pressure above which there are substantial irrecoverable deformations and below which most of the deformation is recoverable. In this sense it can be regarded as having physical significance.

As was explained in the paper the majority of the samples tested for strength and consolidation were not fully saturated. However the degree of saturation was in excess of 95% for almost all the samples. All testing was done with relatively coarse porous stones so that if there were substantial air pressures in the samples these and not the water pressure may have been measured. The significance of this incomplete saturation was checked in a limited number of samples by comparing the results obtained from samples saturated by the application of back pressure and those tested as sampled. The results are plotted in Fig. 9 of the paper; as concluded there seems to be no significant difference between the results of the tests on the saturated and as-sampled tests.

To the General Reporter:

The General Reporter comments about the likely effect of joints. The nature of the jointing was not discussed in detail in the paper simply because it is difficult to make general comments about jointing when examining cores which reveal such an infinitesimal proportion of the material present at the site. Since the existing buildings have not yet been demolished to make way for the proposed structure it has not been possible to examine the material in-situ by means of inspection pits or shafts. However, one can make some comments about the jointing observed in the greywacke around this part of Wellington which probably apply at the site.

Firstly, the joint spacing is very close ranging upwards from about $\frac{1}{2}$ inch. The joints are particularly tight and occupy only a few per cent of the total volume of the rock mass. To the naked eye no characteristic joint orientation seems evident, although, as far as I know, this has not been substantiated by a detailed joint survey. Also the joint geometry appears to be constant and independent of the extent of weathering.

Secondly, as the weathering process proceeds a characteristic deposit builds up along the joint surfaces. There is a gradual darkening of the joint surfaces through grades II and III with a black deposit becoming particularly evident in grades IV and V. Thus as the decomposition of the rock substance proceeds the amount of material deposited at the joints increases.

And thirdly, as well as these joints which are characteristic of the material, essentially local in nature, and more or less homogeneously distributed there are major discontinuities. These have spacings ranging upwards from tens of feet. Included are major joints, differentiated from the joints discussed above by extent and thickness, shear and crushed zones, and bands of argillite. These are major defects and must be described separately from the weathered greywacke between them. None of the information or comments in the paper are intended for application to such features.

In making the comment, "even with these grades the wide ranges of compression indices suggest that the sample behaviour was influenced partly by the presence of defects", the General Reporter has unfortunately overlooked the effect of void ratio emphasised in the paper. It is significant that there is a somewhat tighter grouping of results for the compression index v. void ratio and preconsolidation pressure v. void ratio plots and somewhat more scatter in the friction angle v. void ratio diagrams. The compression index and preconsolidation pressure are volumetric properties and thus are primarily influenced by the compressibility of the rock substance. Because of the very small proportion of the total volume occupied by the joints and because they are tight the joints will have only a secondary effect. This is consistent with the well-defined relationship between these properties and void ratio in Figs. 5 and 6 of the paper. From these diagrams it is clear that the major factor responsible for the range of values for the properties in each grade is the variation in void ratio. If the joint properties were the deciding factor the compression index and preconsolidation pressure for each grade would tend to be independent of void ratio.

On the other hand one would expect strength tests to reflect any influence of joint properties much more clearly because of the greater possible number of modes of sample deformation in the triaxial test. Once again it is seen that there is a reasonable relation between the friction angle and void ratio (Fig. 7 of the paper), although at some void ratios the range of friction angles is quite large. The strength tests did not reveal that the joints have a major effect on the strength parameters but there is a possibility that they may have had some sort of secondary effect. Less than 10% of all the samples tested failed along a joint plane.

No fresh greywacke was found at the site and so there was no discussion of the properties of this material. However petrological details of the Wellington greywacke have been discussed by Reed (1957).

Reference:

REED, J.J. - Petrology of the Lower Mesozoic Rocks of the Wellington District. N.Z. Geol. Survey Bulletin No. 57, 1957.

Paper by E.J. POLAK:

The Author in Reply:

In answer to the Reporter's questions I would like to stress that the attenuation data in my paper were collected on engineering geophysical surveys in which the depth to the bedrock was investigated. If there is a velocity layering in the overburden and the geophones are closely spaced, the attenuation in each layer except the top most can be measured.

The table on p. 433 indicates that the values of attenuation can be used for the identification of the bedrock. Further deductions of rock quality may also be made if many measurements are made in an area where the bedrock is of one type. In this instance the correlation between attenuation and any property of the rock is excellent. However any attempt to correlate the attenuation of several rock types with any of their properties will only give a general approximation (Fig. 4).

Further research in this field is being carried out both in Australia and overseas.

Paper by L. OBERT and C. RICH:

The Authors in Reply:

The thesis of the authors' paper is that there is very little purpose in classifying rock unless such classification(s) relates to specific engineering problems.

Table I was cited as an example to show that under qualified conditions RQD has been related to tunnel support.

The authors are collecting both operating data and rock property data (obtained from an inspection and tests on specimens from drill core) from a number of open stope and block caving mines. The data collected to date are given in Tables II and III. These tables tend to show which of these data are pertinent in relation to ground support (in terms of maximum unsupported span). However, this is a continuing investigation, and as these tables are supplemented a better classification of rock in relation to ground support can be made.

In regard to the significance of RQD the data in Tables II and III speak for themselves.