4th International Congress on Rock Mechanics, Montreux (Suisse) 1979, Vol.3, pp.311-312

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DISCUSSIONS / DISCUSSIONS / DISKUSSIONEN

V.F.B. DE MELLO
Brazil

I hereby submit a summarized general discussion on pronouncements heard at this Congress which have concurred towards an impression that Rock Mechanics may come to a dead-end, unless we reexamine separately its tasks and responsibilities connected with Engineering Science, and the engineer's tasks and responsibilities in solving a given problem, and therefore resorting either to Prescriptions or to Correlations, the latter hopefully statistical.

1. Single significant discontinuity.

Firstly I was happy to see the emphasis placed on the fact that statistical distributions of frequency of joint systems are an important tool of site investigation, but merely to serve as food for thought towards seeking any really significant shear or tension zone. The sheared band of significant movement (e.g. fault) is the real concern. The frequency of joints of itself may not be of significance, and in fact may be directly misleading, since in many a case a multitude of fine cracks may indeed have served as a relatively homogeneous release for energies, avoiding the creation of a single much bigger movement.

It stands to reason that in a relatively heterogeneous brittle strain-softening material, once the state of internal stresses has found a given plane for more significant movement, the tendency should concentrate on that plane and possibly broaden its width as movements increase. The ultimate development of a broad failure band when movements have been concentrated and big, may be inferred from limit equilibrium conditions (histogram of nominal FS values of several surfaces in the immediate vicinity of the first weakest one); also from the fact that the mere use of the strength envelope equations does not reflect the well-recognized fact that the nominal components of partial (c,ϕ) develop in quite a differentiated manner as strain increases, generally the cohesion intercept being absorbed first.

2. Limit equilibrium strength equation in routine use.

Whereas Soil Mechanics has long since recognized the need to investigate the influence of stress-strain-(time) paths closer to presumed in situ realities, Rock Mechanics continues to employ almost exclusively some routine tests prematurely established as valid. And, incidentally, how very rarely do we find published checks of prediction vs. performance in order to confirm the presumed validity! We should not illude ourselves with our oversimplified shear tests which are but nominal. In my experience, joints in sound rocks with highly stressed angular contacts can exhibit to a significant degree and effect of preconsolidation and OCRs similar to clays and compacted rockfills: it is in silts, sands, and well-rounded multitudinal-contact granular materials that such effects become imperceptible. Thus, for instance, under the upstream part of the base of a gravity dam the normal stress is high, for some time, before any shear stressing, and the stress path on reservoir filling includes a good OCR due to uplift as shear stresses increase. All our computations are nominal, and indeed will always be so: but we should retain a given nominal design and calculation procedure long enough to extract statistical correlations within a constant universe, the design being initially established by prescriptions.

Everybody recognizes that in the same way as in Soil Mechanics even the best possible undisturbed samples are far from capably representing the strain behavior of the would-be "intact" soil element, such a differentiation being worse in denser soils or saprolites and weak rocks, in Rock Mechanics the differences should be even greater because of the high E values, the discontinuities, and the internal stresses. No wonder that in some gravity dams on rock, despite extensive testing, finite element computation, and monitoring, the observed deformations have been of the order or 1/5 to 1/10 of the computed ones, suggesting the absolute need for statistically correlated coefficients of adjustment. In soils, capillarity (and minor cementation) establish a sample's limiting ability to retain in-situ stresses within our aim at $\Delta V \equiv 0$ (impossible): in rocks the minutest strains would be sufficient to cause the stress release, despite the assumed $\Delta V \equiv 0$.

3. Factors of Safety FS, Reliabilities and Probabilities.

The discussion on applicability of a FS concept (inexorably nominal), cannot progress with mere reference to mathematical manipulations, such as whether a resisting force should be subtracted from driving forces in the denominator, or added to resisting forces in the numerator. Any such consideration should firstly be oriented with relation to the physical changes that one anticipates would lead to the higher strains and final unacceptable straining of "failure": secondly, since it will always be nominal anyway, it should be related to some amplified indicator(s) that may provide early signal of the proximity to discomfort.

Inasfar as inexorably all data, parameters and correlations, must be understood statistically, and as soon as possible so expressed always, it cannot but follow that our degrees of confidence (FS etc..) in our decisions and solutions must concomitantly be probabilistic. Is it not deterministic to use rigid-plastic and elasto-plastic limit equilibrium statics? Is it not deterministic to adopt for definition of failure, even for probabilistic analyses, the condition of FS≡1,00? How will we ever check statistically such an idealized criterion?

That is why I contended in my Rankine Lecture (Geotechnique, 1977) that we should seek probabilistic formulations based on Satisfaction Indices to be extracted from really numerous observations from projects under non-failure operational conditions. Such observations should be of such a nature as to well distinguish between different degrees of satisfaction or discomfort. As regards dispersions, are we not illuding ourselves when we recognize the dispersions on material properties, loads, and even analyses, but forget that the greatest dispersion of all may well be on levels of acceptance, to be established by "experience". It is natural for Soil Mechanics of soft soils to move away from the deterministic FS≡1,00 hypothesis through monitoring deformations, because they tend to be significant (relative) and accelerating as FS nominal values approach failure. I would find it most uncomfortable however, to postulate FS acceptance criteria for shear in jointed rocks on the basis of deformations, since the smallest perceptible displacement may be too close to failure. Should we not urgently shift to other indices, such as, for instance, intensities of Micro-Accoustic Emissions?

4. Design Prescriptions and their improvement.

One cannot help but deplore the fact that the distinction between Design Prescriptions and Engineering Science Correlations seems so badly forgotten that such a tunneling settlement curve (Gaussian) as proposed by Peck (Mexico ISSMFE, 1969, .."although the use of this curve has no theoretical justification, it provides at least a temporary expedient..") continues to be used and even extended to a far different context. How can a crude Prescription stand when from every sector we have grown increasingly conscious of differentiated internal stresses, stress-strain behaviors etc.? Litviniszyn's stochastic formulation of displacements of solid loess bodies (1955) might almost seem self-evident as having implicit some mathematical hypotheses of homogeneity, and so on. Therefore, since the phenomenon causing the settlement trough is nothing but the creation of a horizontal cylindrical cavity in a pseudo-elastic medium, it is easy to see that the Gaussian curve postulation leads to crude empiricism and no future, whereas the analogous curve derivable from stress-release considerations has much greater Idea Fertility and potential for development and revision, because of being related to our tangible geomechanical parameters. There is nothing probabilistic in the transverse settlement trough in an idealized stress-release condition of a homogeneous isotropic semi-infinite body. The occurrence of greater or lesser loss of ground in a longitudinal profile should indeed be expressed statistically, and predicted probabilistically: but that is the supplementary settlement component which we try to minimize and even eliminate.

In short, a well intended and gratefully received temporary expedient of a Design Prescription becomes a stumbling block to theorizable progresses if it is not understood. Similarly, many a postulation should be carefully appraised regarding implicit concepts; for instance what hypotheses are implicit in Goodman's proposed 5 cent stress measuring device? What is implicit in physical models such as so ably employed by Barton: if one must go through the stage of abstraction of the mental model, why not stay with the higher degree of abstraction represented by mathematical computorizable models, rather than return to physical models?

Dinorwic Power Station

MOVEMENTS RECORDED AT COMPLETION OF CAVERN EXCAVATION

T.H. Douglas, Partner, James Williamson & Partners
W. Wilson, Senior Engineer, James Williamson & Partners

Introduction

The paper on Dinorwic in Volume 1 of the Proceedings presents an outline of the design work and some of the initial results of movement measurements. When the paper was prepared major cavern excavation work was underway but was far from complete.

Machine Hall excavation was completed towards the end of 1978 and the information which follows relates to the movements recorded up to August 1979.

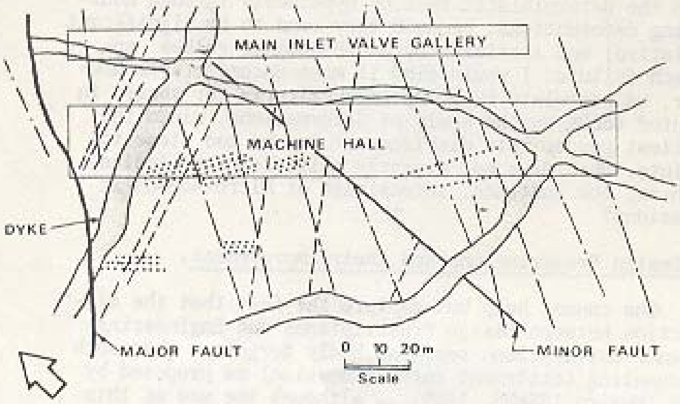

Fig. 14 Geological Plan

PREVALENT JOINT SETS

MAJOR FAULT	65/130
MINOR FAULT	75/090
- - - - - - - - - -	75/160
——·——·——	80/120
··················	70/050 or 70/130

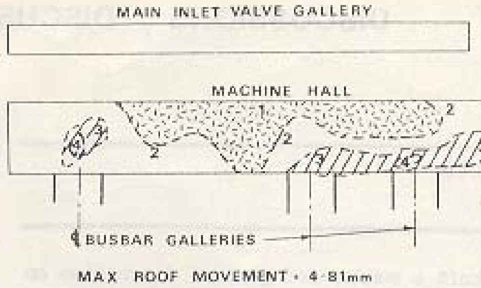

Fig. 15 Plan of Machine Hall Roof - Showing Movements (mm)

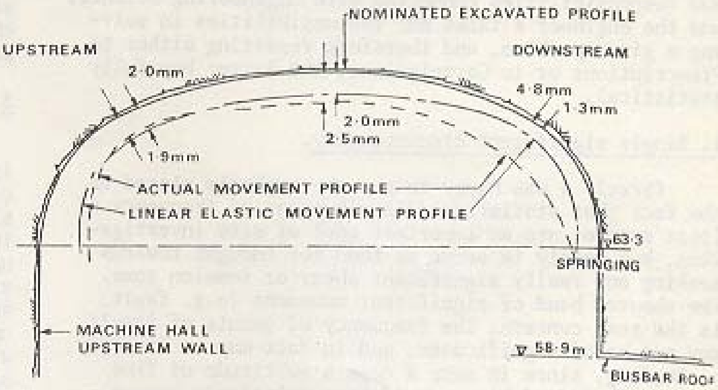

Fig. 16 Machine Hall Roof - Theoretical/Actual Movement Comparison

Roof Movements

The principal geological features surrounding the Machine Hall are shown in plan on Figure 14. The main rock mass is a competent slate interspersed with 6 joint sets (the sub-horizontal at 15/300 is not included on the Figure) many of which have traces or inclusions of chlorite. A complex of near vertical dolerite dykes have intruded into the slate mass and the margins have been severely altered. Figure 15 shows a contour plot of inward movements in millimetres and it will be seen that the maximum movements occur near the dyke. Figure 16 shows a comparison between theoretical and actual movements at a typical array. There is reasonably close agreement over much of the arch but the divergence on the right arises from the penetration into the wall below of a 15 m span Busbar Gallery.

Intersections

One of the principal problems encountered in constructing the Dinorwic caverns was the number and size of the intersections. Not including raise bored shafts or drillholes, there are a total of 78 intersections with the 9 caverns varying from 2 to 3 m wide personnel tunnels up to the 3 No. 15 m span Busbar Galleries which penetrate the walls of the Machine and Transformer Halls respectively. Figure 17 shows a view of one of the Busbar Galleries looking towards the Machine Hall.

Two different excavation sequences were used to form the Busbar Gallery intersections:

1. At the Transformer Hall, the Busbar crowns were fully excavated from a pilot heading and were supported prior to benching down the walls of the Transformer Hall.