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General Report - Stability of Slopes

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SUMMARY This paper presents a summary and discussion of the eight papers on the stability of soil slopes that have been submitted to this conference. Areas suitable for additional research are also discussed.

1. INTRODUCTION

This paper presents a summary of the eight papers accepted for this session of the conference on the stability of slopes. The report is limited to soil slopes with rock slopes being covered in a separate general report on open cut mining (Trudinger, 1988). In addition to a summary of each paper the author provides a commentary on the methodology and conclusions adopted by the authors of each paper. Finally, as requested by the conference organisers, the reporter presents his views on those areas in which future research on the stability of soil slopes could be usefully concentrated.

The papers accepted for this session do not cover a single coherent theme but span a diverse range of interests which are not readily reviewed as a single entity. I have therefore reviewed them individually: those dealing with analysis first; laboratory and field case studies second; and, finally, slope reinforcement.

The text of the papers that was available for this report is that which was sent to the reviewers, the papers may have been altered subsequently. I apologise for any inconsistencies that this may cause.

2. PREDICTION OF CRITICAL SLIP SURFACES - CHOWDHURY & ZHANG

2.1. Summary

It has become common practice in geotechnical engineering to use a set of critical slip surfaces to present the results of the analysis of a slope by the limiting equilibrium method (LEM). These critical slip surfaces are normally defined as those with minimum or near minimum factors of safety against shear failure. As pointed out by Tobbutt and Richards (1979), amongst others, such a critical surface may not be the surface with the greatest probability of failure.

This paper examines an alternate definition of the critical slip surface as the surface of minimum reliability. As indicated in equation 1 of the paper, the reliability can be thought of as the number of standard deviations that the safety margin is above zero (or the factor of safety above unity). If the statistical distribution of the factor of safety along a particular failure surface is known then the reliability is directly related to the probability of failure.

The reliability index in the paper is calculated by using either the simplified Bishop or simplified Janbu methods of slope analysis to determine the factor of safety of a particular slip surface. Then Rosenbleuth's method is used to calculate the mean and standard deviation of the factor of safety and, hence, the reliability of the subject slip surface. Finally the surface of minimum reliability is located using a simplex optimisation technique.

The authors then complete a parametric study using the proposed method. The study concluded that the critical slip surface defined by the minimum factor of safety for mean geotechnical parameters may be quite different to that defined by the minimum reliability index, incidentally confirming the statement of Tobbutt and Richards (1979).

2.2. General Comments

The author would like to make a number of observations which apply to this probabilistic model and, often, to other such models. Probabilistic methods usually require more computational effort than deterministic ones. Therefore those responsible for their development usually choose a simple limiting equilibrium method as the basis of their model - in this case the simplified Janbu method (Janbu, 1973) for failure surfaces of arbitary shape. As inferred by Janbu himself (1973), Mostyn and Small (1987) and Lumsdaine and Tang (1982), this method often results in a factor of safety that is significantly different from that obtained using the more rigorous methods and so is not really suitable for anything other than a quick hand calculation. It should also be noted in passing that Janbu's so-called rigorous method is not rigorous and, even when convergent, often produces quite inadmissible side force distributions (Fredlund, 1984; Ching and Fredlund, 1983).

A similar difference occurs when two dimensional methods are adopted for problems that are really three dimensional, although here the difference is always conservative (Cavounidis, 1987). This difference may be up to 30 percent of the factor of safety or reliability (Gens et al, 1988) but it can amount to an order of magnitude difference in the calculated probability of failure (Mostyn and Small, 1987). This may explain why observed probabilities of failure are often considerably less than those determined from probabilistic models.

Thus the development of any probabilistic method of slope analysis must acknowledge that the results are much more sensitive to any biases in the model than the related deterministic method.

The proposed model ignores spatial correlation (i.e. autocorrelation) of the model parameters. Earlier work by Chowdhury (1984) has taken some account of the effects of spatial variability. In section 2.1 on the theoretical basis of the model, the author states that analysis of the effect of spatial variability should proceed simultaneously with models such as the one currently under consideration which considers the soil as spatially "homogenous" (i.e. no spatial correlation). The reporter agrees with this provided that the non autocorrelated models are considered to provide only indices of stability and not actual slope reliabilities or probabilities of failure. Li and White (1987a; 1987b) present results that indicate that ignoring autocorrelation can produce probabilities that are several orders of magnitude too high. These and the other papers referenced by Li and White present a detailed model to derive slope reliability taking account of the spatial variability of the strength parameters. In comparison the method described in the subject paper is hardly "comprehensive" or "new".

There are some minor points in the paper that require comment:

* The chosen definition of reliability is that which is conventionally adopted. Unfortunately, as pointed out by Hasofer and Lind (1974), the value of reliability obtained depends on the specific format chosen; they propose an exact and invariant version. Li and White (1987a, 1987b) adopt this invariant reliability index in their work.

* Similarly models based on the use of the porewater pressure ratio, r_u, have enabled the presentation of simplified charts but have only a tenuous connection to a real slope where r_u is unlikely to be constant. The use of r_u should be discontinued.

* In section 2.2 the statement is made that "the slip surface with the highest probability of failure is obviously the surface with the lowest value of reliability index", this may not be true if the distributions of the reliability index are different for various modes of failure. Often these distributions are assumed to be normal and under this assumption the statement is true.

* The method of deriving the statistics of the factor of safety is Rosenblueth's method (1975 and 1981). This is a simple and effective approximate method: but the statement "A knowledge of the probability distribution of the individual geotechnical parameters is not required" is not true because the method, as developed for problems of more than one random variable, applies only for symmetrical distributions. Thus the method will not work with important classes of parameters. Transformations may help but introduce additional errors to an already approximate method. This fact has been overlooked too often in the literature.

* In section 4.3 it is stated that "the conventional critical slip surface (is) based on the mean values of geotechnical parameters". Obviously a mean level of the watertable is not appropriate for engineering design. More often than not the engineering analysis of slopes is completed using conservatively assessed estimates of the effective strength and location of the water table. The results are then compared with factors of safety that have proved suitable for such a selection of parameters and appropriate action taken (i.e. stabilise or abandon the slope). Thus rarely would critical slip surfaces for design be based on mean parameters. It should also be noted that the minimum factor of safety or reliability that is not the quantity of interest in the design of remedial works but the design is based on that set of failure surfaces that has less than a given level of safety. This highlights one of the problems with reliability or probability based slope design: that is, there is not a set of acceptable limits for either reliability index or probability of failure based on acceptable field performance. Thus even when the reliability of a particular failure surface is known the implications of this are not.

The aim of the paper was to investigate the relationship between the location of critical failure surfaces based on the minimum factor of safety and the minimum reliability. The paper indicates that these are not always coincident and that this and the variable nature of soil properties should always be kept in mind. The reporter strongly agrees with this conclusion but emphasises that the accuracy and realism of the model adopted must also be kept firmly in mind.

3. DETERMINATION OF CRITICAL SLIP SURFACES FOR SLOPES VIA STRESS STRAIN CALCULATIONS - GIAM & DONALD

3.1. Summary

This paper presents a search scheme to locate the critical failure surface. It uses as input a stress analysis of the slope. This input stress state can be determined in any manner but is most often arrived at by a finite element analysis (FEM). This provides the major advantage of the scheme in that the method does not require assumptions regarding side forces which are common to limiting equilibrium methods and, in contrast to such methods, always provides a physically admissible solution. The increasing power of microcomputers and availability of FEM packages should encourage more practising engineers to complete stress analyses as part of investigation and design.

Basically the method starts by using the stresses at a single point within a slope to derive a local factor of safety. The local factor of safety is then integrated along a path of minimum factors of safety extending in both directions to the boundaries of the slope. Several starting points are adopted to ensure that the minimum factor of safety and critical surface are obtained.

Two examples are analysed and the results compared with other methods of predicting the minimum factor of safety and the location of the critical slip surface. In both cases the results are in excellent agreement.

The method, as presented, does not consider pore water pressures in the slope: this is a serious limitation which, the author's indicate, is being removed.

3.2. Comments

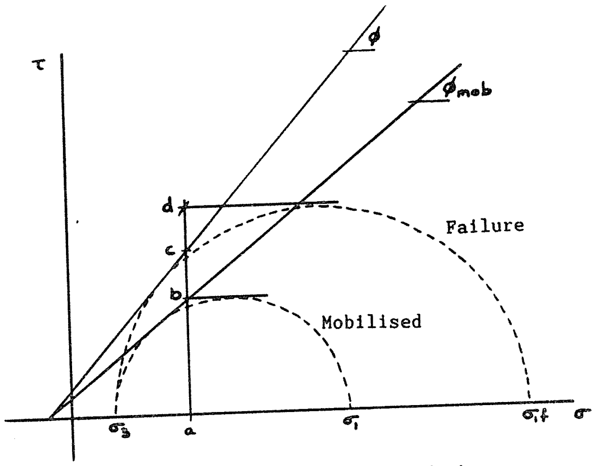
Several points need to be kept in mind. The factor of safety obtained will depend on the stress field adopted; in turn this will depend on the method of analysis and, for metastable slopes, on the constitutive model used. In the examples an elasto-plastic finite element model is used, this is likely to be suitable for many but not all slopes. In addition the stresses will often depend greatly on the initial (K_0) conditions which, unfortunately, are normally put into the "too hard basket" and rarely investigated. An investigation of the sensitivity to these factors would be interesting, especially as the authors quote Janbu (1973) as stating that "the location of the critical slip surface is primarily governed by the stress system in the soil". The reporter does not dispute this but finds it interesting that the authors validate their method by comparing its results with those from limiting equilibrium methods.

The definition of local factor of safety adopted (i.e. maximum shear stress at failure divided by mobilised maximum shear stress, herein called F_{FEM}) assumes that only the maximum principal effective stress changes between the mobilised (i.e. analysed) stress state and failure. That is the minor principal effective stress is considered constant. As this assumption is unlikely to be the true, the sensitivity to it should be investigated.

Further the above definition of the factor of safety is different to that adopted in limiting equilibrium methods (i.e. shear strength divided by mobilised shear stress, herein called F_{LEM}); these different definitions are equal only when the factor of safety, F, is one. The different definitions of the factor of safety are illustrated on figure 1 where the small Mohr's circle represents the mobilised stress state and the larger one the stress state at failure (i.e. for the example given the factor of safety is greater than 1). As can be seen F_{LEM} is less than F_{FEM} whenever the factor of safety is greater than one. Note that, strictly, this inequality applies to the local factor of safety but it must also apply to the global or average factor of safety unless substantial portions of the failure surface have local factors of safety less than one. It is interesting that the factors of safety reported in Table 1 of the paper for some of the limiting equilibrium methods do not satisfy this inequality and, thus, may not represent true minima for the proposed methods. It should be noted that this inequality does not result from the fact that limiting equilibrium methods provide theoretical upper bound solutions and finite element methods lower bound solutions: it results simply from the fact that different definitions of the factor of safety are adopted in each method.

Lastly the search scheme apparently is based on the premise that the critical slip surface must pass along a path of contingent local minimum factors of safety. It appears that a thin strong layer of limited lateral strength would force the search elsewhere even when the critical path would actually pass through this layer.

A few minor points to conclude. The statement in section 3 that the slip surface for the simple case should be circular or near circular is not necessarily correct. The slope strength is mainly derived from friction (as evidenced by the sensitivity analysis included in the following paper) but, even if it were devired from cohesion, Baker and Gerber (1978), in a controversial paper, indicate that the surfaces would still not be circular. A statement in section 5 infers that the critical slip surface is determined from the "known stress distribution". It should be kept in mind that the stresses are "known" only in as much as they are the result of mathematical analysis based on a vast number of simplifying assumptions and, often, poorly known material parameters.



$F_{LEM} = ac/ab \qquad F_{FEM} = ad/ab$

$F_{FEM} > F_{LEM}$ for $F > 1$

Figure 1 Comparison of different definitions of the factor of safety

In conclusion the paper presents a useful scheme for obtaining the critical slip surface and the factor of safety from a stress analysis of a slope: this is an issue that has needed attention for some time. Thus this paper provides a valuable alternative to limiting equilibrium methods.

4. APPLICATION OF THE NODAL DISPLACEMENT METHOD TO SLOPE STABILITY ANALYSIS - DONALD & GIAM

4.1. Summary

This paper is a companion to the paper presented by Giam and Donald; it presents the same examples analysed by a different method (i.e. NDM). In essence the method involves determining the factor of safety of a slope from a series of finite element analyses completed with factored strength parameters. The factor of safety is defined as that factor at which the displacement of selected nodes increases rapidly. The nodes are selected to lie within the expected failure zone or otherwise near the toe. This method requires a lot of computing time to complete many FEM analyses.

This method is similar to that given above in that it does not require the assumptions adopted in limiting equilibrium methods, thus the results are always physically admissible.

The authors include an analysis of the sensitivity of the factor of safety to the input strength parameters. This confirms that, in a soil with negligible cohesion, the coefficient of friction dominates the factor of safety. Similar sensitivity analyses can be completed by limiting equilibrium methods.

A major benefit of the proposed method of determining the factor of safety is that it automatically accounts for both overstressing and excess deformation of a slope; limiting equilibrium methods ignore deformation and often even the stress field.

The method currently takes no account of pore water pressures in the slope but can, the authors state, be extended to do so.

4.2. Comments

The authors state that the "sharpness of the definition of the factor of safety is dependent upon the choice of constitutive model for the soil, the node in the mesh for which the curve is ploted, the type of element and the mesh size used in the finite element formulation, the size of the increments in N and the method of plotting". This is emphasised by reference to figures 3 and 4 in the paper. If the analysis in figure 3 had been terminated at the same displacement as that in figure 4, or the last analysis not been completed in figure 4, then the factor of safety obtained would have been about 0.94 instead of the 1.05/1.06 actually obtained. Thus even the interpretation of the results can lead to significant differences in the factor of safety reported by this method.

The authors present their method in terms of total and incremental displacement. The far field displacements in figure 7 are unusual (they indicate tension parallel to the vertical boundary) and may be entirely related to fixed boundary conditions and not related at all to field stresses. The incremental displacements in figure 8 provide a much better picture of the likely field displacements and, thus, it appears that incremental displacements should be adopted.

The authors recommend that the displacements should be ploted at many nodes; this seems reasonable to ensure that an adequate section of the slope has actually failed and not just that a single element has yielded. The authors do not provide any advice on the factor of safety to adopt if the various nodes indicate significantly different factors of safety. Should the recommended factor of safety be based on the minimum which applies to only one node, or the average which involves inconsistent stresses from different finite element analyses. The former seems to be the only consistent and logical solution but results in a conservative local factor of safety compared to the global factor normally adopted.

The above does not pose a problem for the example shown on table 1 as here the minimum local factor of safety from the NDM is the same as the global factor of safety from the CRISS and LEM approaches. This implies that the factor of safety is constant along the failure surface which is true at failure (i.e. NDM method) but is it true at the working stresses implied by the other methods?

The authors comment in the introduction that part of the justification for avoiding LEM analyses is that they "could be unreliable and may not give a convincing result" for "a slope with complex, non-homogeneous and isotropic material where its physical and mechanical nature changes with direction and time". The reporter would like to suggest that most FEM analyses, especially those commercially available, would also poorly model such a slope.

5. CHANGE IN THE SHEAR STRENGTH OF SOIL DUE TO SEEPAGE AND THE SLOPE STABILITY - KUWANO, YOSHIDA & ISHIHARA

5.1. Summary

The authors use a total stress approach to model a shallow failure in a steep sand slope; the material parameters are derived from undrained tests on partially saturated sand.

The failure is 30m high, inclined at about 40 degrees to the horizontal and, from figure 2, appears to be about 0.5 to 1m deep. The sand is a uniform medium grain sized sand with a particle specific gravity of 2.7 which indicates that the grains are unlikely to be predominantly quartz.

A series of triaxial tests were completed on reconstituted samples of sand. Most tests followed isotropic consolidation to cell pressures varying from 49 to 98 kPa; although some tests were completed after anisotropic consolidation to 70% of the isotropic failure shear stress. Although pore pressure was measured it was not known whether it was the pore air or pore water pressure. The results were interpreted in terms of the total stresses and degree of saturation, S_r. This interpretation was selected because the total stresses are known within a slope and, the authors infer, the degree of saturation is easier to determine or predict than the pore pressures (especially when these may be negative). Thus a relationship is obtained between the total stress parameters, c and Ø, and S_r. A very significant drop in strength is apparent between 80 and 100% saturation with zero cohesion and an angle of friction, Ø, of less than 10 degrees for the latter. It should be noted that the authors adopt a different failure criterion for the saturated samples (i.e. maximum deviator stress) than for the unsaturated ones (an axial strain of 15% which is very near a maximum stress ratio criterion).

The slope analysis is completed using Janbu's method, it is not stated whether the simplified or so-called "rigorous" method. The analysis indicates that the slope would fail for a degree of saturation of about 80%.

The authors compare the strength parameters determined for samples that are isotropically consolidated with those for samples that are anisotropically consolidated. The latter have higher strengths.

The authors then present the same test results in terms of total stress minus measured excess pore pressure. The strengths of the the partially saturated samples fall neatly near one failure criterion: the authors conclude "it may suggest that the strength characteristics of unsaturated soils are described in terms of total stress minus excess pore pressure".

The authors overall conclusions are that strength decreases with increasing saturation; the slope failure can be illustrated with the increase in saturation; and total stress minus excess pore pressure explains failure shear strength.

5.2. Comments

There are three main areas that need comment in this paper, they are:

* The authors have not used the effective stress principle in most of their work.

* They adopt different failure criterion for partially and fully saturated soil.

* The total stress test programme follows stress paths that have no relevance to the field problem being modelled.

Comment on these and other matters is provided in the following sections.

5.3. The Effective Stress Principle

The authors refer to some of the literature on the effective stress within and shear strength of partially saturated soils. They do not apply this theory because it is "not simple to estimate the in-situ excess pore air and water pressures at failure". The method they propose in lieu of effective stress relies on estimating the degree of saturation of a sand slope during and after rain. They provide no details of how they have estimated or determined the degree of saturation in the slope, nor do they indicate how it might be done. In fact the degree of saturation is likely to be a poor choice of parameter in slope analysis because it changes during drained and undrained testing in a laboratory and with time, strain and depth in a slope (i.e. it is likely to be just as complex and less predictable than pore pressure). Thus they ignore the tradition of soil mechanics (i.e. effective stress principles) and substitute the degree of saturation in its place. Indeed if S_r was able to be predicted then in a sand such this the assumption that X equalled S_r would enable prediction of pore pressures (Lee and Donald, 1968).

The reporter would like to suggest that the effective stresses need to be determined for two situations: firstly, the laboratory testing and, secondly, the sliding soil mass.

A simple hypothesis to enable interpretation of the laboratory tests is to assume: (a) that the measured pore pressures are pore water pressures at high degrees of saturation; and (b) that the Bishop and Blight pore water pressure term, i.e. $X(u_a-u_w)$, is near zero at all but high degrees of saturation. These assumptions are considered to be reasonable for the uniform medium grain sized sand tested. Thus the total stress minus (measured) pore pressures shown on figures 9 and 10 are likely to approximate the actual effective stresses. These figures, now considered to be in terms of effective stress, result in a considerably simpler framework than figures 5, 6 and 8. In fact figure 9 shows that even the anisotropically consolidated results are reasonably modelled by the same failure envelope. Only one (effective) strength envelope needs to be established not many total strength envelopes. A good coverage of effective stress and partially saturated slopes is contained in Walker and Mohen (1987).

That the above is a reasonable interpretation is evidenced by the linearity of the data in figures 9 and 10 (the separate failure criterion for saturated soils is discussed later). The reporter has reasonably predicted the measured pore pressures, shown on figure 4, at 12, 40 and 60% saturation by assuming that they are pore air pressures, that the air phase obeys the universal gas law and that partial solution of air in water can be ignored. This supports the hypothesis that, at these saturations, the measured pore pressure is the pore air pressure. Lee and Donald (1968) state that this interpretation leads, at the worst, to an overestimate of the cohesion and underestimate of the angle of friction: but figure 10 shows the cohesion to be zero so it must not be overestimated but correctly estimated.

The authors claim in section 6 that the similar (measured) pore pressures but different shear strengths of the samples with S_r equal to 12% and 40% could be due to differences in suction. But as can be seen from figures 9 and 10 most of the difference in strength can be attributed to the measured (but small) difference in positive pore pressures and significant pore water suctions are not required.

It should also be noted that once the laboratory tests are interpreted in the above terms, then the effective stress law is that for a saturated soil using measured pore pressures. There is no need to predict or estimate either X of Bishop and Blight (1963) and others, or the tan $\emptyset^b$ of Fredlund (1985) and elsewhere. This removes any of the controversy regarding tan $\emptyset^b$ initiated by Escario and Saez (1986), who suggest reasonably that tan $\emptyset^b$ should be set to X tan $\emptyset'$ (i.e. that the suction component of strength should be a function of the relative area over which it acts). This produces the reasonable result that the apparent cohesion is zero for both very dry and saturated sands, with a maximum somewhere in between. The interpretation in the paper produces high cohesions for quite dry sand which seems contrary to observed field behaviour of clean uncemented sands.

As for the difficulty of using the effective stress law in slope analysis. Lee and Donald (1968) provide useful discussion of the assumption that $X = S_r$ and indicate that for sandy soils the errors are probably quite small. The reporter feels confident that such a model will explain the slope failures observing conventional soil mechanics principles. Alonso and Lloret (1983) have used effective stresses to model considerably more complex partially saturated slopes than the one here. In fact Alonso (1976) showed that pore pressures are the major contributing factor to slope stability: it seems that they should be included directly in slope analysis rather than indirectly via the mateial strength parameters.

5.4. Failure Criteria

The authors adopt the maximum deviator stress as the failure criterion for saturated samples. This occurs at small strains and is conservative and consistent with normal procedures for soft or loose soils but it is inconsistent with the criterion of 15% axial strain adopted for all partially saturated samples. This latter criterion appears to be close to or less than using the maximum principal stress ratio as a failure criterion. The two failure criteria are responsible for the rapid drop of strength as the soil approaches saturation (evidenced in figures 5, 6, 8 and 10). On the data presented this difference vanishes if the failure criterion is consistently defined as 15% strain or maximum principal stress ratio (which was not reached in most tests). That is the lower envelope in figure 10 is no longer required. It should be noted that the authors, themselves, have shown failure of the saturated samples in figure 9 using maximum stress ratio not deviator stress.

Two different failure criteria should only be adopted if the field behaviour is better modelled by such an appraoch. This is not discussed in the paper, yet the main conclusions depend the different failure criteria adopted in the test programme.

5.5. Stress Path in Laboratory and Field

The failure surface as shown on figure 2 appears to be at or less than about 1m depth and thus the total vertical stress on the failure plane will lie between 14 and 19 kPa depending on the average degree of saturation. The slope is very steep and the normal stress may be much lower. The authors adopt a total stress analysis but use 49 kPa as their lowest cell pressure. Thus the stress paths in the test bear no relationship to those likely to occur in the field. Total stress analyses should be based on similarity of stress paths. Escario and Saez (1986) recommend that the actual stress path and porewater history must be reproduced in the laboratory for tests on partially saturated soils. Note that curvature of the strength envelope with respect to suction that they observed in some clays is not likely to be a problem in this sand. The data plotted on figure 9 shows a fairly linear strength envelope over the stress range tested.

The test programme did not approximate the in-situ stress conditions at the commencment of shearing. Further the reporter feels that the path adopted in testing (which was not controlled) is unlikely to represent that occurring in a slope during saturation due to rain.

5.6. Other Comments

The authors adopt Janbu's method of analysis. If this is the simplified method then it is least reliable for large L/D ratios such as the long shallow slope analysed. Other comments on Janbu's method are included in section 2 of this paper. The slope geometry illustrated on figure 2 indicates that it may have been best and most simply modelled as an infinite slope.

If the total stress analysis adopted in the paper is to have any value then the degree of saturation in the slope has to be determined, there are no details or hints in the paper as to how this was done.

The authors third conclusion would apply to all their results if they had used a single failure criteria. In fact it would then be a restatement of a basic axiom of soil mechanics.

6. MONITORING AN ACTIVE LANDSLIDE AT HOWLETTS ROAD, YALLOURN NORTH - McKINLEY & RAISBECK

6.1. Summary

This paper describes the monitoring of an active landslide at Yallourn North. The landslide is 150m long, 75m wide and 9m deep with an overall slope angle of about 10 degrees and has moved 30m along the soil/rock interface since it initiated in 1984. The toe of the landslide is a 10m high road cut; the failure has closed Howletts Road. The clay is highly plastic and direct shear testing gave residual angles of friction between 10 and 13 degrees; these values were confirmed by back analysis. Initial monitoring after the landslide indicated velocities of 60 to 105 mm/day. Minor remedial work was completed to limit the ingress of water into the slope.

Several alternate methods of stabilising the landslide were considered, these were:

* Maintenance by continual removal of the failing toe.

* Removal of the landslide and installation of drainage to obtain a factor of safety of 1.5. This was estimated to cost up to $200,000.

* Construction across the toe of a large diameter bored pile retaining wall. Same cost as above but potentially unsafe working conditions.

* Constructing an embankment downslope of the slide to contain most of the potential failure material, this would protect the main road downhill of Howletts Road.

The embankment solution and a monitoring system were installed at a cost of approximately $20,000. It was expected that stable conditions would obtain in 5 years and Howletts Road could then be reopened.

Details of a simple monitoring system are given in the paper. The results show that the movement responds very quickly (i.e. within 12 hours) to heavy rainfall, this can be compared with results in the paper in these proceedings by Fell et al (1988). The average movement of the slide over the past thirty months indicates that the slide is, at present, gradually stabilising.

6.2. Comments

The solution adopted is certainly inexpensive and basically accepts that the slide will continue to move until some stable condition is obtained. Other options that may have been considered to actually stabilise the landslide could have included a toe berm, trench drains (although the slide plane is possibly too deep), horizontal borehole drains or micropiles (not yet used much in Australia). Details of these are given in Leventhal and Mostyn (1987) and the case studies included in the same volume.

The second option examined involving stabilisation would not normally be designed for a factor of safety of 1.5 as the strength parameters and geometry are quite well known. It is not unusual to design remedial works for a marginal increase in factor of safety of maybe only 0.1 to 0.3.

The reporter does not believe the statement in section 6 that two storms of 1 in 5 year intensity in any year reduce the likelihood of a similar event in the following 5 years. This is contrary to normal hydrological and probabilistic thinking.

Finally the fact that the rate of movement has decreased over 30 months does not necessarily indicate that the landslide is reaching equilibrium. Further analysis would be necessary to verify that extreme future hydrological events will not remobilise the slide.

This is an interesting case study but the details given are fairly general. It would be good to have access to publications giving more complete information on the investigation, analysis and monitoring of this or similar slides.

7. A LANDSLIDE IN A RESIDUAL SOIL CUT SLOPE-CRISPEL & MONT'ALVERNE

7.1. Summary

This paper describes a landslide that occurred during the excavation of a cut in residual soil. The initial topographical survey indicated that the slope would be about 7m high and thus the engineers considered that a 1 to 1 batter without any benches would be adequate. The survey was not very precise and the slope as initially constructed turned out to be 18m high. Figure 2 is interesting as it shows the expected topography, actual preslide topography and post slide topography - these are all quite different to each other. The soils are micaceous silts and clays derived from the deep weathering of a biotite gneiss.

The engineers commenced redesign of the slope but it failed at the end of the dry season before the redesign could be implimented. Due to economic pressures no additional investigation could be completed. Nevertheless the slope was back analysised using an assumed failure surface. The English text (presumably a translation) here is hard to follow but indicates that these parameters were adopted in a second (but really third) design. This design, like the back analysis, was completed using stability charts - which is probably in keeping with the "accuracy" of the design parameters. It is difficult to follow the derivation of these.

This second design did not entail removing all the failed material but consisted of laying the slope back to an approximate batter of 2 horizontal to 1 vertical. This slope failed at the completion of construction. The authors state that the reasons for this are clear but the reporter cannot understand their explanation. The movement of the toe started to fail the piles that had been driven for the high school building that was to have been constructed on the base of the cut. A trench was dug to relieve the pressures on these piles.

A third design was produced with no additional field or laboratory data. This involved cutting the toe back 10m to remove all the failed material. The slope started to fail (for the third time) as construction of this design was nearing completion.

A 3m high toe berm (the fourth design) was constructed. The slides so far had involved only about two thirds of the slope and these appeared stabilised. Unexpectedly the entire slope started to fail along a deeper surface.

A fifth design was completed and involved substantial earthworks. A change in government administration halted work on the schools and this design has not been implimented. Notwithstanding this the larger failure seems to have stabilised over the intervening year. The authors state "the government is resuming the services to finish the school and at that time will probably include a complete investigation of the materials".

The authors conclude:

* It is important to understand the geology and strength parameters of a slope.

* A traditional site investigation involving SPT tests would not have warned of the problems that occurred on this site.

* Geotechnical engineers should supervise earthworks to confirm the predictions made in the office.

7.2. Comments

The English is poor and the reporter is sure that some matters have been clouded in translation. This paper indicates that the authors have been on a particularly long learning curve. The reporter certainly agrees wholeheartedly with their conclusions and hope they are able to put them into practice. Although there may be economic and technological constraints on site investigations in Brazil the reporter suggests that SPT testing is not appropriate for residual clay soil.

8. GEOMETRY OF INTERNAL FAILURE OF REINFORCED EARTH WALLS - ARENICZ & CHOWDHURY

8.1. Summary

This paper presents the results of some small scale models of reinforced earth walls and some data on prototype behaviour from the literature; various equations are presented.

Two series of ten model tests each were completed: Series I consisted of short (190 mm) reinforcing strips and high friction (36 degrees) sand; Series II of longer strips (230 mm) and lower friction (31 degrees) sand. Reinforced earth walls were constructed in 50 mm lifts until failure. The resultant failure surfaces are curved; each series of failure surfaces is presented normalised in terms of the height at failure. A best fit equation is fitted to each set of failure surfaces, equations (1) and (2) in the paper.

The authors then assume that the failure height is a given function of an effective angle of friction for the reinforced sand composite material (ϕ'_r) and determine this angle for each individual model test. This angle varies from 2 degrees below to 5 degrees above the effective angle of friction of the sands alone. A general equation is presented for the individual normalised failure surfaces, equation (3) in the paper. The parameters for (3) are presented for ϕ'_r over the range 26 to 60 degrees.

The authors have obtained from the literature observations of the location of the failure surface in full size prototype walls; here the results are scaled in terms of the actual height of the wall not the failure height. The failure surfaces are actual rupture surfaces in the models but are taken to be the loci of maximum reinforcement tension in the prototypes. An equation is presented for a representative prototype failure surface.

The authors examine the information that they have presented and find that the model failure surfaces, prototype failure surfaces and so-called Coulomb failure plane do not coincide. They state that designs based on the Coulomb wedge could lead to gross errors in determining the volume of material enclosed by the failure plane.

They conclude that the failure surface in models depends on height of failure and the effective angle of friction of the backfill material and that the failure surface in prototypes is a function of the height of the wall.

8.2. Comments

The reporter has a number of comments regarding this paper.

One of the main points made by the authors is that both the Coulomb wedge and small models provide very poor estimates of the volume of material enclosed by the failure zone of full scale reinforced earth walls. They also state, but offer no support, that most design methods are based on one of these surfaces. Thus the reader is led to believe that existing design methods are inadequate. This reporter is not aware of any researchers or, more importantly, designers who adopt either of the flawed methods in design. In fact Schlosser's proposal appears to be most commonly adopted (see for example Boyd, 1987) and, on the evidence presented in this paper, fairly accurately and simply represents the failure surface in full scale RE walls. Further the authors in this paper and in Arenicz and Chowdhury (1987) cite references dating back to 1974 which support this view.

The reporter finds the lack of correspondence between the models and prototypes considerably more disturbing than the poor fit of the inappropriate

Coulomb wedge to either. This point receives only minor discussion in the paper and is presented, reasonably, in terms of the stress levels that exist in the prototypes and in the models. But the matter goes further than this. The reinforcing in the models described in this paper was deliberately chosen to pull out rather than fail in tension. In contrast the failure criterion adopted for the prototype is the location of maximum tension in the reinforcement. Thus the models fail by tie pull out and failure in the prototypes would be related to tie breakage. In fact Lee et al. (1973) found that even in models failure surfaces differed for these two modes of behaviour. In addition they found that the failure height of models failing due to tie breakage was independent of the soil density (and strength) thus mirroring the situation found in prototypes by Schlosser (1978) some time later.

It is difficult to understand why the authors scale their model behaviour in terms of the critical (i.e. failure) height and then interpret the results to produce an effective angle of friction for the soil and reinforcement composite material. They thus interpret their model in terms of two parameters that can not reasonably be determined for a design. They ascribe all the variation in failure height to variations in ϕ'_r. Thus the 20% variation in H becomes a 20% variation in ϕ'_r. The reporter would be inclined to attribute most of this variation to experimental effects because all 10 tests in each series were as close as possible to replicates of each other. This within group variation should not be ascribed to a parameter that cannot be independently determined in either the model or field. Proabably it would have been better to interpret the tests in terms of soil or soil-to-reinforcement friction, both of which can be determined in both models and prototypes. The determination of ϕ'_r is based on an untested hypothesis which is supposedly drawn from Lee et al (1973). In fact Lee et al do not use any concept like composite friction angle but only soil or soil-to-reinforcement friction.

The authors then determine an equation for the failure surface (equation (3)) in terms of two parameters, H and ϕ'_r, that can not be determined for anything but a model constructed to failure. They then extrapolate these from the range of their data (30 to 40 degrees) to the range of field occurrences (26 to 60 degrees). This is extrapolating well beyond the data, as shown on figure 2, but it does not matter because the failure surface in the prototypes (solid line in figure 2) is nothing like those in the models (shown as dashed lines). In fact the failure surfaces are independent of both parameters.

The reporter believes that the length of reinforcement is likely to have had some effect on the location of the failure surfaces in the models but all the variation is ascribed to a composite friction.

Those who wish to obtain some of the references cited in the paper should note that the paper by Schlosser is in French and the one by McKittrick is not included with the cited proceedings.

9. EXCAVATION CHARACTERISTICS OF A SLOPE CUTTING REINFORCED BY TENSILE INCLUSIONS - MATSUI & SAN

9.1. Summary

This paper presents some results of field instrumentation and model analysis of a reinforced

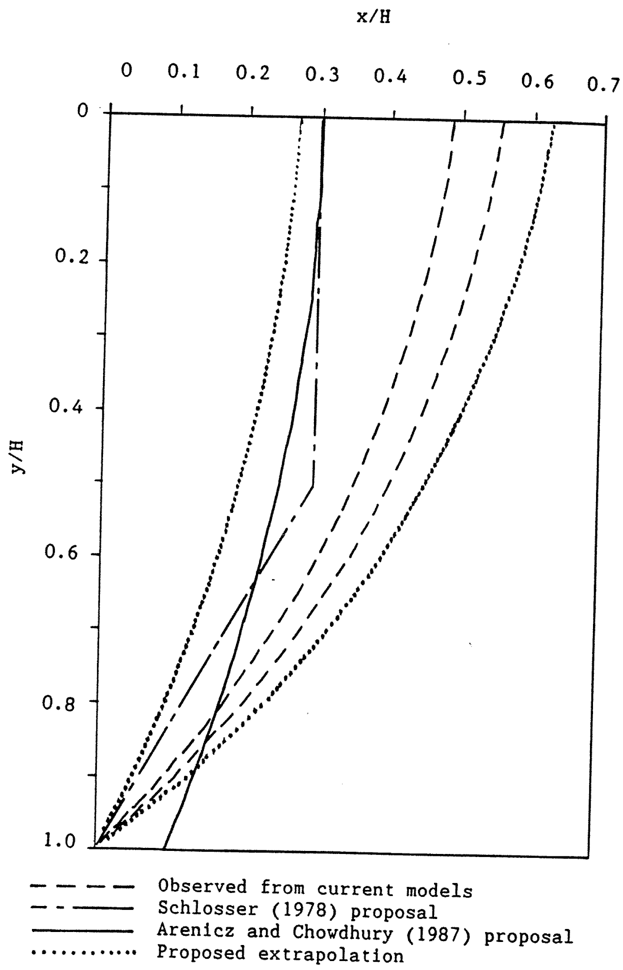


Figure 2 Proposed locations of failure surfaces in reinforced earth walls

slope. The 11m high slope consists of completely decomposed granite overlying bedrock.

Construction and instrumentation were as follows:

* The slope was excavated and reinforced to approximately 6m depth. The reinforcement is described in the paper as root piles which presumable means a fully grouted single reinforcing bar. Over portion of the slope these bars were instrumented with strain gauges, the slope face was sampled and then monitored for deformation. Settlement plates and a borehole with inclinometer were installed.

* The crest of the slope was then loaded with kentledge and the slope monitored and analysed; this part of the project is described in another paper not obtained by the general reporter.

* The kentledge was removed and the cut deepened by 4.5m. The analysis of this stage of the project is the subject of this paper.

A finite element analysis of the cut slope was completed. The soil was modelled by a hyperbolic stress strain relationship, the rock was considered linear elastic and one dimensional bar elements were used for the reinforcement. An elastoplastic joint element was used to model slippage between the reinforcement and soil/rock and between the soil and rock. The model adopted appears to be fairly comprehensive and more representative than those routinely available to designers. The material parameters adopted are given in Table 1 of the paper, no properties are given for the slip elements but these may be in the references cited by the authors.

The short term analysis of the cut slope was completed by simulating the actual surface unloading stresses. The analysis was also completed for 3, 5 and 10 times these unloading stresses. Figures 5, 6 and 7 of the paper show the predicted deformation of the slope. Most of the deformation is vertical. A comparison of the actual and predicted horizontal displacement at the borehole inclinometer is shown on figure 7 of the paper. The actual movement of the crest (1mm) is well modelled but the prediction is only 25% of actual movement further down the hole.

The measured and predicted forces in the reinforcement are given on figures 8, 9 and 10 of the paper.

A comparison of the predicted behaviour of unreinforced and reinforced cuts is given on figures 11, 12 and 13 of the paper. These indicate that the reinforcement significantly increases the shear strength by increasing the minor principal stress and greatly reduces the yield zone under factored loading.

Long term monitoring of the reinforcement stresses indicated that it took about two weeks for them to stabilise. A comparison of measured and actual axial force in the reinforcement is given on figure 15 of the paper.

Finally the authors compare the factors of safety obtained from their stress analyses of the unreinforced slope with those obtained from conventional method of slices analyses. The comparison is surprisingly good. In addition, the authors compare the factor of safety of the unreinforced and reinforced slopes; this would be difficult to do using the method of slices. The reinforcement is shown to significantly increase the factor of safety.

9.2. Comments

This is a useful paper and it is unfortunate that page limitations probably affected the detail presented. The following comments may be of interest.

There is little discussion of the role of pore pressures in either the field or analysis, many finite element analyses have difficulty including these. Section 2.4 of the paper implies that the soil is saturated and some discussion of pore pressures is warranted.

Further there is no discussion of the derivation of the material parameters shown in Table 1. The accuracy of a model depends greatly on these but many of the values given appear as though they may simply have been assumed (e.g. cohesion of 1.0 MPa, densities, Poisson's ratio, etc). Presumably the given parameters are short term, what about long term parameters?

Plotting the measured and analytical axial force distribution in the reinforcement on separate figures (8 & 9) does not allow ready comparison of them and no discussion is included in the text; also the information is too compressed. It would have been good to have a figure giving, at a reasonable scale, the measured and predicted forces in, say, reinforcement A. Nevertheless it may be said that the fit for the short term analysis is not too bad; as always reality is more erratic than the model.

The authors state in section 2.3.1 that "it is clear . . . the increment rate of the axial force is reduced due to the slippage of the reinforcement". It is not clear to the reporter that there should be a linear change in axial force in any particular reinforcing element as the slope is unloaded. In fact, as the model adopted is highly non-linear, a non-linear response may be expected even when slippage is not occurring.

There is no discussion of the results given in figure 15 for the long term situation. The correspondence between analysis and measurement is poor for the upper reinforcement and becomes progressively worse for the lower ones. In fact for anchor E the model predicts tension where there is compression and vice versa. Some discussion is warranted.

The scale on figures 11 and 12 must be incorrect as the reporter cannot believe that an unreinforced 11m high slope has stresses of the order of 10 MPa acting within it.

As mentioned in the discussion of the paper by Giam and Donald (section 3.2) and shown on figure 1 of this report, the definition of the factor of safety adopted in finite element analysis is different from that adopted for limiting equilibrium analysis. In fact for case 5 of the unreinforced slope (Table 2 of the paper) the reporter has calculated that if F_{FEM} equals to 0.62 then F_{LEM} should equal 0.71. Thus the reported 0.01 difference is suprising and possibly indicates that the failure planes considered for the two methods are not identical. In addition F_{LEM} is reported to be smaller than F_{FEM} for the other cases: this is contrary to expectations and some discussion would have been helpful.

No details are given on the method of slices adopted although the failure surfaces analysed are circular.

A final minor point is that the reinforcement acts almost exclusively in tension and is probably better described as fully grouted untensioned anchors rather than root piles which normally involve frame action - see Leventhal and Mostyn (1987).

10. FUTURE RESEARCH

As requested by the organising committee the reporter will now provide details of the areas of soil slope analysis where he feels that research could be usefully concentrated.

Some research could usefully be completed on predicting the pore pressures in embankment, cut and natural slopes. Particularly investigating the relationship between climatic conditions, time and pore pressures; the effect of seepage from underlying strata rather than along the slope; and negative pore water pressures due to either excavation or surface evaporation and dissipation of these pressures.

Well controlled and documented case studies of slope failures to enable the relevant material parameters for slope analysis to be determined by rigorous back analysis. Too often the data used in back analysis is so poorly known that the analysis is only really used to confirm "a priori" opinions regarding material strengths.

A thorough discussion of the requirements of the physical admissibility of various side force distributions in limiting equilibrium methods of analysis.

There are three papers in this section that present comparisons between limiting equilibrium and stress analysis of slopes. This work and the applicability of stress analyses of slopes should be continued, especially as reasonable computing power is available to most design engineers.

The analysis of progressive failure to include realistic modelling of strain softening materials, development of both finite element and limiting equilibrium methods of analysis for this problem and documentation of case studies.

Use of realistic models in probabilistic analysis of slopes, such that the probability of failure may be more useful than just a subjective index of stabilty. Investigation of the stochastic nature of real slope materials, this should include point estimates of the strength and pore pressures and determination of the spatial relationships between these. There is a need to develop efficient methods of simulating two and three dimensional general random fields.

Most probabilistic methods of slope analysis give an estimate of the probability of failure of the slope due to a given mode of failure or along a given failure surface. From a decision making viewpoint the quantity of interest is the total probability of failure of the slope due to all possible failure surfaces and modes of failure. The significance of this is illustrated in figure 3, where there are 10 failure planes and the probability of failure along each is 10%. The overall probability of failure, due to just this mode, would be 65% if the probability of failure along any one plane is independent of the probability of failure along all others. In contrast the overall probability of failure would be 10% if the probabilities of failure along the planes are perfectly correlated. Thus ignoring spatial dependence of material properties can lead to very large errors even for a simple problem such as that illustrated. Some recent papers by Chowdhury (1983 and 1986) and Li and White (1987 et al) document work relevant to this topic.

A fertile area for research is the grey area covering investigation, analysis and design of "soft" rock slopes, i.e. slopes intermediate between classic soil slopes and rigid body hard rock slopes. These materials are very poorly understood and current design procedures vary from very conservative to quite unconservative.

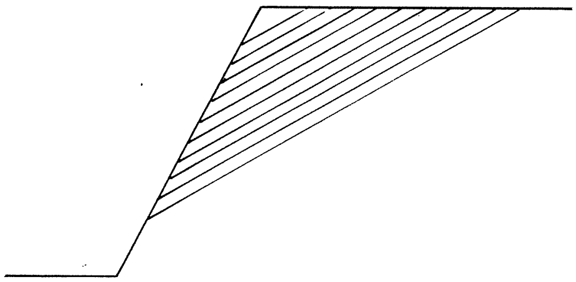


Figure 3 Simple model of slope failure

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