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Discussions and Replies

Multiple Authors

Discussion by Bhagwat V.K. Lavania, Professor, Earthquake Engineering Department, University of Roorkee, Roorkee, India, on "Grand Coulee Riverbank Stabilisation - Case History of the Design of Remedial Measures" by J. Lawrence Von Thun.

At the outset let me congratulate the author for putting forward the design of remedial measures for a common but ticklish problem of stability of slope under drawndown condition. The banks of the Columbia River below Grand Coulee Dam have history of instability because of presence of varved clay layer, of maximum about 80' thick, under Sand, Gravel, Cobble alluvium. The geological profile of typical riverbank cross section, with potential slide surfaces marked on it (larger slides not shown), is shown in Fig. 1 of the paper. From the back analysis of land slides, an effective residual shear strength of $\phi = 12°$ for the clay has been estimated. Assessment of excess pore water pressures on potential sliding surfaces under future draw-down conditions has been done on the basis of observations (Fig. 2a + 2b) on present (design time) fluctuations in pore water pressures with the fluctuations in river water level. However, no time lag between the fluctuations of pore water pressures and river water level has been shown. This is not expected in the clay. Author is requested to clarify this.

The remedial measures suggested are (i) easing out the river bank slope, (ii) provision of 4' thick riprap on the slope and, (iii) Subsurface drainage at vital spots (covering small areas) The flattening ofbank slope will certainly help in preventing the slides near slope surface but would be of no help in case of slides covering larger distances from the banks. The purpose of graded riprap isto prevent the migration of fines of the bank material rather than to add to the safety factor of the potential sliding surface. The subsurface drainage arrangement checks the development of pore water pressures in the area of influence of radial drains. But it does not act as a cut-off device to restrict the high pore water pressure zone towards the river side only. Moreover it involves pumping out of the seepage water and thereby high maintenance cost. With, of course, limited exposure to the problem, it is suggested that a major drain parallel to river bank with cross drains would have been a better alternative. The gradient of the major drain could be kept a little less than that of the river so that the seepage water could be put in the river at some suitable location. By providing such a drain, all along the length of the river, the area on the other side of the drain would have not been affected by fluctuations in river water level. The author is requested to throw light on the suggestions.

Discussion by Richard W. Stephenson Professor of Civil Engineering University of Missouri-Rolla on "Failure of Micaceous Waste Tailings Dam" by W. F. Brummond

The author is to be congratulated on presenting a concise discussion of both technical and managerial difficulties associated with the construction of a tailings dam. The work describes the procedures used for a slope stability analysis for a mica tailings dam, the resulting conclusions, the post analysis failure of the dam and a discussion of the cause of the failure. The description of the failure indicates two important facts: First, the slimes retained behind the tailings dam did not liquefy and flow as a "heavy liquid" as is often assumed. Secondly, the debris from the failure traveled only some 60 to 100 feet below the original toe of the embankment. This factor needs to be taken into account in setting criteria for downstream hazard analysis for tailings facilities. It is obvious from this case and other reported cases of failures of tailings dams, that breaching of the embankment will not release a wall of slimes to travel down the valley. Rather, the slimes have some internal shear strength and will resist flow to some extent thereby limiting the downstream affected area.

Mr. Brummond reaches two important, non-technical conclusions in his paper as well. First, it is important that access to the toe of a dam structure be assured. Secondly, accurate records of all transactions regarding geotechnical engineering work is both mandatory and prudent in these days of excess and eager litigation.

Discussion by J. Lawrence Von Thun
Senior Technical Specialist
Bureau of Reclamation
Denver, Colorado, USA
On "The Failure of a Soil Blanket Lining Caused by the Action of Bacteria" by G. W. Plant and P. B. B. Vosloo

An interesting soil blanket failure hypothesis involving alteration of material permeability post-placement is presented by the authors. Examination of the expected and observed conditions as presented in the paper, however, indicate the failure mechanism suggested by the authors may not have occurred.

Although the exact dimensions and water levels were not provided in the paper, a rough estimate of an overall blanket permeability at "failure" can be made from the average water loss of 7.5 L/s (.0075 M^3/s) recorded under controlled conditions. Taking the average head (h) as .66 x 7M = 4.6M, the blanket thickness as (l) = .5M, and the area of the floor in one compartment as ≈ 50,000 M^2 the observed permeability may be computed as

$$k = \frac{Q}{iA} = \frac{Ql}{hA} = \frac{.0075 \times .5}{4.6 \times 50,000} = 1.63\times10^{-8}\ \frac{M}{sec}$$

This value is very close to the measured permeabilities of the block samples taken during construction. If a general change in material permeability had occurred as suggested by the authors' hypothesis, then an average water loss one order of magnitude higher would have been observed (≈ 75.0 L/s).

The failure described (wet areas around the perimeter of the embankment) could have been caused by local breaks in the blanket or local zones of high permeability. Such zones would not seem to be unusual for a thin blanket such as this especially considering the sensitivity of permeability to density and water content as shown by the authors. The increase in flow in the blanket drain outlet pipes during the progressive filling in April would seem to be an expected condition as would the flow direction determinations made along the embankment perimeter.

The permeability tests on two samples taken post construction are not statistically significantly different than the two samples taken from the floor during construction. The authors' note that a maximum of 5.8 L of nitrogen gas would be developed per 1 M^2 of floor or per .5 M^3 of soil blanket. This amounts to 1 percent by volume of the soil which may not be significant with respect to altering the physical characteristics of the soil as suggested by the authors. Verification of the hypothesis given in the paper would require controlled mixing of the sewage water and pure water with soil specimens followed by permeability testing at intervals which allowed bacterial action to take place.

Given the information and evidence in the paper, a general change in blanket permeability leading to excessive seepage does not seem likely. The problems experienced upon filling appear to be like those that would be expected due to normal variations in blanket placement.

Discussion by M.K. Yegian,
Associate Professor in Civil Engineering,
Northeastern University, Boston, MA
on "Reconstruction of Failure Initiating Mechanisms for Teton Dam" by G.A. Leonards and L.W. Davidson

The authors are congradulated for their pre sentation of an excellent summary, of the cronological explanations of the plausible failure mechanisms of the Teton dam. The comprehensive review provides valuable insight to the state-of-the-art investigations of failure of a major geotechnical structure

The authors also propose an interesting and an alternate failure mechanism which may explain as to why 1) failure of the dam occurred in the right abutment and not in the lef and, 2) failure occurred in a short period of time. The laboratory test results presen ed by the authors provide evidence of the te dency of Teton dam fill compacted dry of opt mum, to collapse and to undergo volume reduc tion upon wetting. This information was use by the authors to support their proposed fai lure mechanism for the Teton dam, namely, co lapse of a "wet seam" in the key trench on t right abutment followed by hydraulic fractur ing. The field compaction results shown in Fig. 5 indicate the possible presence of a "wet seam" compacted dry of optimum. Howeve the % dev. OMC for all the 10 data points ra ge between 0.9 and 2.2 which is lower than t laboratory values used in Fig. 4 to determin the % volume reduction of the fill. Based o this range of moisture content and the resul shown in Fig 4, the % volume reduction would be less than 2%. This raises the question: Would less than 2% volume reduction lead to cracking in the zone 1 fill? Also, the dry densities of the 10 observation points withi the potential collapse zone are greater than 95 pcf. When plotted on Fig. 3, these data points indicate that the identified zone has a relatively low coefficient of permeability similar to that of "non-wet seams".

Although, there will always remain some uncertainty about the presence of such a wet seam and its tendency for large volume reduc tion upon saturation, the authors' proposed failure mechanism has significant merits sin it attempts to explain the two observations the failure of the dam stated earlier. Exte sive geophysical investigations could have provided additional field observations which would now be useful to substantiate the proposed failure mechanism. Scaled model tests using Teton dam fill compacted dry of optimu may be useful to demonstrate the proposed fa lure mechanism and the rate of occurrence of failure.

Once again, the authors are congradulated fo their paper. Such an extensive investigatio of the failure of a geotechnical facility indeed enhances the state-of-the-art.

Discussion by J. Lawrence Von Thun
Senior Technical Specialist
Bureau of Reclamation
Denver, Colorado, USA
on "The Delayed Failure of a Large Cutting in Hong Kong"
by R. R. Hudson and S. R. Hencher

The authors present an interesting case of slope failure in highly decomposed granite which by coincidence of time must have been related to an intense rainstorm five days earlier. The delay of five days in failure and the noticebly "dry" manner of failure remain, in the authors' opinion, to be satisfactorily explained. Because of the lack of quantitative information on slide deformation history, pore water pressures, and stress strain relationships of the failure surface material it is unrealistic to expect definitive resolution of the questions surrounding failure. However, because this case history presents a rather open invitation to provide an explanation of the behavior, this discussion will concentrate on that endeavor.

After considering the mechanism of progressive failure, the required quantity of water involved, and the size of the mass relative to the required triggering force, the delayed, "dry" failure appears reasonable under either one of two variations on the authors' second (ii) hypothesis.

The occurrence of the rainstorm and subsequent infiltration would have very likely created ground water conditons close to those suggested by the authors. As the water pressure rose at the head of the slide effective stresses would have decreased thus creating greater shear stress along the potential slide surface. The absence of observed water indicates one or more of the following: (1) that the rise in water level was small, (2) that a low permeability barrier (perhaps due to weathering) existed within the mass parallel to the slope, and (3) an abrupt drop in the groundwater occurred over the top of the dike without exiting at the surface. A small rise in water level is consistent with a scenario of delayed (progressive) failure. A small amount of additional stress on the rather steep potential slide surface could have initiated a progressive failure. If the materials in the slope were stressed to or beyond peak strength and displayed a strain softening behavior, the increased strain in the upper portion of the slide would result in a loss of shear stress capacity. The loss in load would be transferred to the area just below where additional load would create additional strain and resulting stress transfer until the toe of the slope was reached and sliding would result.

The limited size of the failure mass would certainly seem to permit the existence of changes in geology, material properties and loadings due to water pressures which although subtle or small could be great enough to trigger movement of the mass.

The "dry" appearance of the mass upon failure may be attributed to the ground water differential in the hill created by the dike. Considering that the dike may become more permeable near the ground surface it may be reasonable to expect that the water table existed in the slide mass only for a short distance, flowing over the dike like a water fall, seeking the level of the lower ground water table. The ponded water could represent that water which was within the slide mass at the moment of failure. A gradual development of such a water table condition reaching its apex in 5 days (at which time stresses necessary to cause failure result) is a reasonable alternative to the progressive failure hypothesis for delayed failure described above.

Discussion by J. Lawrence Von Thun
Senior Technical Specialist
Bureau of Reclamation
Denver, Colorado, USA
on "The Failure of a Cut Slope on the Tuen Mun Road in Hong Kong"
by S. R. Hencher and R. P. Martin

The evaluation of the geologic and hydogeologic aspects of the problem by the authors was well done and the use of parametric back analysis to study the relationships of strength and pore pressure was most appropriate. Considering the space limitation and scope of the paper little more could be expected of the authors; however, in the interest of providing some discussion the following questions are raised.

(1) Was the permeability of the dolerite and granite measured in-situ or in the laboratory (i.e., effect of jointing)?

(2) Were drainage holes above the dolerite dikes considered as a remedial measure?

(3) Questions relative to shear strength determination:

- explanation, purpose, and use of multistage testing
- explanation of how the C' value was obtained when corrected data passes through the origin
- relation of dilatant-compressive behavior terminology to the common "i" angle correction

(4) What was the orientation of the "critical jointing" in the small scarp and what does "highly jointed" signify quantitatively?

(5) Did the original design criteria consider the testing and analysis performed on the materials and how was the original mapping and drilling, assembled for use?

Discussion by John P. Sully, Principal Geotechnical Engineer, INTEVEP, S.A., Venezuela, on 'An Embankment on Soft Clays' by A. Cancelli and A. Cividini.

The authors have presented a very interesting paper on the analysis of the effect of sand drains on deformation properties of a lacustrine deposit on construction of an earth embankment.

The design criteria presented suggest that the embankment had to be constructed in stages in order to avoid overstressing of the foundation and instability. Sand drains were required for increasing the settlement rate. The analysis of stability was carried out neglecting the shear strength of the 2,5m thick dessicated crust, which would appear overly conservative in view of sand drain installation. Consideration of the surface layer permits construction of a 7,6m high embankment (FS=2; unit weight of fill assumed to be 20 KN/m3) which contrasts considerably with the 4,5m calculated ignoring the strength of the upper layer.

Cancellini and Cividini are asked for their comments on the following points:

- the sand drain spacing of 3,5m-3,8m would appear to be uneconomical in light of previous studies. Since only a limited site specific study was carried out, would it not have been more economic and beneficial to use a closer drain spacing in view of the uncertainties in the design.
- the advantages of in-situ testing for providing parameters for drain design and assessment of small-scale structure within the foundation soils.
- it would appear that consolidation rates are up to 6 times higher than those suggested in Table I. This fact is also supported by pore pressure measurements given in Fig. 5. It is unfortunate that the pore-water pressures could not be analysed due to water table fluctuations. However, it would appear that induced excess porewater pressures were very low suggesting either higher than measured permeability values or significant layering of more permeable material.
- how the results of ID consolidation tests, where the specimens contained internal (vertical?) drains, were assessed for use in the analysis.
- the ratios of soil moduli from backcalculation (assumed in-situ value) and laboratory tests for soil layers A and C are 2,4 and 3.1 respectively. This is in agreement with previous studies of stiff clay behaviour.
- the installation of settlement measuring devices at depth, especially in layer B, would have permitted a more exhaustive analysis of settlement distribution. This would improve the accuracy of backcalculated parameters since the design requires almost total consolidation of layers A and B in the 2-year period.

On the basis of the results obtained, how would the authors modify their initial design, if at all?

Disucussion by John P. Sully, Principal Geotechnical Engineer, INTEVEP, S.A., Venezuela on 'Performance of an Embankment on Peat' by R.E. Olson.

The author has presented some interesting results fron the application of sand drains for an embankment on peat; an application which is contrary to present prac ce since previous experience has shown that sand drair are ineffective in peat soils. Since the results of tr majority of performed consolidation tests were in accc dance with the above, the author's comments as to the reasons for employing sand drains would be of interest

In addition the following points are raised for commer

- the more conventional 1-D consolidation results ob tained for the peat from the quick-loading flow te may be misleading since it has been shown that the gradient of secondary compression increases with l increment and rate of loading.
- Figures 5,6 and 7 of the paper show that consolida tion of the peat occurred within the construction period with little or no secondary compression. Se lement data in Fig. 6 suggest that consolidation i complete by day 200. However, in Fig. 7, at the sa time approximately 50% of the excess head of pore pressure still exists and does not reduce to its b value until about day 400. This apparently occurs with negligible settlement. This could suggest tha the foundation behaviour is being controlled by th drains acting as piles with the peat undergoing a load reduction by redistribution of surface stress
- the adjustments made to obtain correlation between measured and computed settlements would appear to l excessive. If, as the author suggests, peat can be considered as behaving in a Terzaghian manner, and that it is normally consolidated, the the void rat log-effective stress plot should be linear. Hence constant relationship between load increment and settlement can be assumed. This gives the result that under the first 4 ft of fill, a settlement o 2,5 ft would occurs. This value could possibly be increased when penetration of the fill and soil dis turbance are taken into consideration. Hence the reduction of measured settlement by 4ft seems unrea sonable.
- the author considers that disturbance of the peat probably occurred during installation fo the drains by jetting. This is evidenced by an increase in wat head of approximately 0.91m (3ft) as shown in Fig. Considering the low submerged weights of the peats, it may have been possible that jetting permitted ir trusion of the sand into the peat between sand drai thus forming a more rapidly draining system. This could be one reason to account for the faster than predicted rate of settlement.
- the high ratio of horizontal to vertical consolidation coefficient for peat would appear reasonable especially for the highly structured fibrous peat.

Written discussion by E.Zaharescu, dr.,
Hydraulic Engineering Research Institute,
Bucharest, Romania:
"TWO FAILURE CASES OF EARTH DAMS"

Two failure cases of homogeneous embankment dams in Romania have emphasized the consequences that could result by neglecting apparently insignificant geotechnical and hydro-geological conditions in the first case, and by not rigorously observing the project provisions in the second one. The two dams were located on secondary tributaries of the Prut river, in the Northern part of the Moldavian plateau. The hillsides consist of an almost horizontal Sarmatian formation, in which marly clays, sandy clays, sands and sandstones are alternating. They have gentle slopes and frequently present springs and landslides in deluvium.

Mileanca dam, 10.5 m heigh, with a crest length of 442 m, is based on clayey alluvium, 3 to 6 m thick, with thin clayey sand layers. The bedrock is Sarmatian. 20 to 30 m downstream the dam location, near the left hillside toe, there was a marsh, with low bearing capacity, that had been dewatered by pumping. In the summer of 1974, following a high flood, the water level in the reservoir became 1 m higher than the normal retention level. In the marshy zona mentioned above, seeping water has been noticed, with increasing flow, leading to the washout of the foundation material by piping. In a couple of days, three huge diffuse boils emerged, having some meters in diameter, the carried material rising 0.8 to 1.0 m above the natural surface of the soil. After the drawdown of water in the reservoir, below the normal operation level, the piping stopped by leakage continued. Subsequently analyses showed that instability was effective even before the retention achieving (quicksand brought by inflow from hillside), but was made worse by superposition of seepage flow nets from the reservoir and from the hillside. The special interest in this case consists in the fact that, although the instability phenomenon was uncommonly spectacular, the diffuse feature and the slow evolution have prevented the begining of a gradually increasing development, with catastrophic effects, before drawdown in the reservoir. To remedy, a surcharge platform of local material has been built, with draining blanket at the bottom, covering the boils and the surrounding area on some thousands square meters. The efficiency of this remedial measure is controlled by discharge measurements and by piezometers installed in the draining layer.

The second case is that of the Plopi dam on the Gurguiata river, 11.5 m high, 340 m in length. The dam was located 150 m upstream the dyke of a very old piscicultural pond. The designer decided to found the dam on the poorly consolidated, muddy alluvium of the old pond, 2 to 5 m in depth, stipulating gentle slopes of 6:1 - 5:1, a large bench upstream and a draining blanket downstream, as well as a very slow construction rate. In the first year, 1975, only 2.5 m fill has been completed and 1.5 more in the following year, without execution difficulties or any unexpected phenomena to be encountered. At the end of June 1976, administrative considerations imposed the increase of the execution rate, without designer's notice, so that by August 19, that is in 1.5 months only, 6.5 m fill was performed, up to 1 m from the final designed level. As the bottom outlet was not yet completed, a section of about 100 m near the right hillside was at its beginning.

On August 19, cracks at the surface of the upstream slope and heave of foundation soil near the toe of the upstream bench have been noticed; in a couple of days, cracking extended on the crest and the downstream slope of the dam, opening of cracks reaching some tens cm and dam body dividing into fragments on about 2/3 of its length. Unevenness near cracks reached 2 m. Subsequent studies and measurements pointed out a very slow consolidation rate (in 4 years the consolidation degree did not exceed 25%). Immediately after the failure, the piezometric head in the muddy layer was 3 m higher than the surface of the executed fill. The dam was rebuilt after partial demolition and hydraulic filling of cracks with silty sand, redimensioning of the cross section with 9:1 upstream slope and 5:1 - 6:1 downstream slope, providing a clayey cut-off screen on the upstream face, as well as upstream and downstream benches, 40 m and 30 m respectively. The works have been completed in 1978; settlement gauges and piezometers have been placed. The measurements showed a slow decrease of the pore pressure and the fact that 85% of the total settlement, reaching about 35 cm in 4 years of operation (with water level restriction), had been consumed in the fractured part of the dam body. It is very interesting to go on with studying the dam behaviour, with respect to the consolidation of weak foundation soil, as well as from the point of view of consequences of the progressive closing of holes and cracks at the dam bottom, under its own weight, and the influence of precipitations and variation of water level in the reservoir.

Discussion by A Marsland, Principal Scientific Officer, Geotechnics Division, Building Research Station, Garston Watford, UK on 'Behaviour of Ramganga Dams' by B V Lavania.

The dam described in this paper is constructed on an interesting site and is built entirely of weak rocks. Stress meters installed in the core, which was formed of processed clay shale, indicated vertical pressures less than half the overburden pressures. This suggets that the softer core is being partially supported by the sides of the cut-off trench and the shoulders of the dam. This could lead to eventual cracking and errosion through the core such as occurred at Balderhead Dam in the UK (Vaughan P R, Kluth D J, Leonard M W, and Pradoura H H M 1970 'Cracking and erosion of the roller clay core of Balderhead Dam and the remedial works adopted for its repiar; proceedings 10th International Congress on Large Dams, Montreal, Vol I pages 73-93). Apart from mentioning this aspect the paper gives very little information about the behaviour of dams. The paper would have been improved if more details and results of instrumentation had been given.

Discussion by A Marsland, Principal Scientific Officer
Geotechnics Division
Building Research Station,
Garston Watford UK on
'Deep Seated Base Failure and Reconstruction Work'
by M Fukuoka.

The case history given in this paper describes a failure of a 4 m high 30 m wide embankment 10 days after it was completed. The large movements and the rapidity with which the slip developed confirm that the clay is sensitive but the author gives no idea of how sensitive, perhaps he could give some typical stress strain curves and values of sensitivity. It is not clear whether the factor of safety of 1.27 was obtained from effective or total stress analysis. Perhaps the author would like to enlarge on this aspect giving details of soil parameters c', Ø and pore water pressures or the undrained strengths (c_u) which have been used. It would also be useful if they could explain values of c and Ø in Fig 1 and provide values of L.L, PL, clay content, organic content, and photographs of the soil fabric. The observations of the movement and the pore pressures developed during installation of the sand compaction piles are interesting especially when compared with the small horizontal movements (50 mm) during reconstruction of a 2.6 m high bank. I would also like the author to explain his adoption of $Ø_c = 10^o$ and c = 34 kPa.

Is $Ø_c$ the effective peak, post peak, or residual effective angle of shearing resistance? I am also surprised at the low factor of safety (1.11) which the author states as showing that a 4 m high bank could have been built. In my experience a factor of safety of this magnitude is no guarantee that failure will not occur. I would also like to ask the authors to give the length of the original bank, whether they can explain why failure occurs at this particular location, and the present use of the embankment.

Discussion by A Marsland, Principal Scientific Officer,
Building Research Station Garston Watford UK on
'Failure of a Dredged Slope in a Sensitive Clay'
by D P Lagatta and S L Whiteside.

This paper describes an interesting failure which occurred due to inadequate evaluation of relevant soil parameters and potential problems. In this type of construction the most important factors to be considered are the effects of unloading, whether or not the sensitive clay will result in progressive failure, and the excess pore water pressures created by pile driving. Total stress analyses incorporating undrained strengths are inappropriate for investigating unloading conditions since negative excess pore water pressures which developed during unloading decrease with time and this leads to a decrease in strength. The presence of silty and layers and lenses accelerates this process. In order to obtain a more realistic indication of the stability it is necessary to use effective stress analysis. This requires a reasonable assessment of effective stress strength parameters of the soil and the most critical pore water pressure distributions which are likely to arise. Both these factors are affected by the extent to which progressive failure can develop and the effects of pile driving operations. Another aspect which should have been considered is a possibility of lateral sprea of excess pore water pressures produced by the additional load of the fill and the proposed surcharge loa ing. All these factors are difficult to determine. How ever, uncertainties could have been reduced by preconstruction trials to determine the pore water pressures and changes in soil strength due to piling oper tions and observations of movements and pore water pressures in a dredged slope.

Perhaps the authors would comment on the above aspects and if possible provide additional data on:

1. What size and type of sampler was used to obtain t samples.
2. The size of the test specimens and the rate of tes ing together with any other relevant test details.
3. The location of the critical slip surfaces obtaine from the stability analyses.
4. An approximate idea of the overall movement of the slope.
5. Is the 600 ft wharf fully operational? Have there been any further problems since construction and what remedial measures were required?
6. The percentage additional cost arising from the problems.

Discussion by M.K. Yegian,
Associate Professor of Civil Engineering,
Northeastern University, Boston, MA,
on "Earth Dams at Nuclear Power Plants"
by R. Pichumani, D.C. Gupta and L.W. Heller

The authors present a brief summary of en gineering analyses and investigations made f an earth dam constructed to impound the emer gency cooling water for a nuclear power plan However, the paper lacks details of these an lyses and specific relevant technical inform tion and data which would have significantly enhanced their paper. The analysis procedur adopted for the investigation of the seismic response of the dam are consistent with the state-of-the-art practice. Professor Seed a his colleagues have demonstrated that analys of permenant deformations of a dam construct of granular soils, susceptible to liquefacti and subjected to seismic excitations, is mor reliable than the evaluation of the seismic response of the dam using psuedo-static slop stability analysis. The inclusion of the de tails of these analyses and the results in this paper would have provided valuable case history information.

iscussion by S.R. Hencher,
enior Geotechnical Engineer,
eotechnical Control Office,
ong Kong Government on
Efficacy of Grout Curtain at
amganga Dam" by
.C. Goel and B.N. Sharma.

oel & Sharma have presented an extremely nteresting case study which has led them to onclude that an impervious blanket is referable to a grout curtain for reducing ore pressures downstream of a dam, other actors being the same.

'he authors are to be congratulated for queezing so much useful information into a short paper but I would be grateful if they ould expand further on their important onclusion.

'irstly I wonder whether the fact that the two am sites were dissimilar hydrologically prior o treatment, as evidenced by the site nvestigation, might have contributed to the omparatively poor performance of the grout urtain. Do the authors consider it likely hat, had a blanket been used at the main dam site rather than a grout curtain, the pore pressures would have been much reduced?

eading from this question, impervious blankets re generally cheaper, presumably, and easier o construct in a controlled manner than grout curtains, and I would be interested to know whether the authors would advocate their use n all cases or if other factors or particular hydrogeological conditions might lead them to support the use of grout in some circumstances?

'inally with reference to Figure 4, it is pparent that the observed pore pressures were n all cases outside the predicted range of pore pressures for any conditions from a fully effective grout curtain to no cut off at all. This suggests that the theory being used to predict pressures is not very accurate and I would be grateful if the authors could comment.

Discussion by S.R. Hencher,
Senior Geotechnical Engineer,
Geotechnical Control Office,
Hong Kong Government on
"Earth Dams at Nuclear Power Plants" by
R. Pichumani, D.C. Gupta and L.W. Heller.

The authors are to be congratulated on presenting a reassuring picture regarding the design, construction and maintenance of earth dams at nuclear power sites.

Clearly the most difficult part of such work concerns stability under the extreme earthquake loading conditions for which safe shutdown is required and this is indicated by the subject matter of all the papers referenced.

I would like to ask the authors, based on cases where designs have actually been tested under such severe conditions, how confident are they in the analytical methods now available?

Secondly, concerning the requirements for filters for drainage systems, I wonder whether geotextiles might be approved for use in such structures where safety is of paramount importance. If geotextiles have been used for this purpose, what provisions are made regarding long term clogging and deterioration?

Discussion by John P. Sully, Principal
Geotechnical Engineer, INTEVEP, S.A.,
Venezuela on 'Deep-Seated Failure and
Reconstruction Work' by M. Fukuoka.

The author has presented a very detailed case history for a deep-seated failure caused by embankment construction. Subsequent stabilizing measures for reinstatement of the failure are also discussed.

The analysis of embankment stability was performed using results from drained strength tests presumably using measured preconstruction pore pressures and calculated increments. For a deep circular failure surface, a factor of safety of 1,27 was obtained. The author's comments on the agreement between calculated and measured excess pore pressures due to embankment construction would be welcome. It would also seem that the analysis would have better been carried out using undrained parameters as this would appear to be the most critical stage i.e. during or immediately after construction. This is emphasized by the author's statement that a reduction in unconfined compressive strength occurred under sustained load. Given the strain-dependent undrained strength of the soil, it is possible that settlement of the fill could have caused a reduction in shear strength initiating a progressive type of failure. If the behaviour of the clay is similar to that of the Scandinavian quick clays, a large strength reduction could occur with residual strength on the failure plane being very low. This would account for the large movement observed.

The stress-strain behaviour of the clay would be of interest in relation to the strain required for strength reduction to residual compared to the strain levels recorded in the field.

The author's comments on the possible triggering effect of the small seismic event which occurred several days prior to failure would be of interest. Examination of the time-settlement curve for point Q before collapse shows that several days prior to failure the settlement rate increased.

In addition, the author's comments on the strength parameters shown in Fig. 1 would be informative especially in relation to the low drained friction angles recorded. Also, the dependence of drained cohesion on water content, as is the case for quick clays, would not appear to apply here as suggested by the values of unit weight.

Discussion by Sh. A.C. Khazanchi, Sr. Astt. Director, Regional Research Laboratory, Bhopal, M.P(India) on clay shale Foundation slide at Waco Dam Texas by William R. Stroman.

At the outset let me congratulate the author for his excellent attempt to focus the importance of detailed site investigation to avoid failures of slopes on Foundation soils.

The author has also demonstrated that a combination of high pore pressure and low residual strength is indeed a dangerous situation in precipitating slope/Foundation failures.

I would like the author to throw some light on the following queries:-

1. Would the author please project some estimates about the expenditure involved in (a) Soil investigation undertaken before the Design and execution of the Project. (b) Subsequent detailed soil investigation undertaken after the failure (c) Expenditure of the repairs undertaken.
2. Kindly give some suggestions how to make soil investigation binding for all the foundation/soil Projects.
3. Any special treatment of soil which could be adopted to reduce the development of high pore-pressure and improve the low shear strength of soil.
4. It is also not clear how the preliminary investigations carried out could miss the soil strata responsible for failure of slope which happen to be directly under the Dam.

Discussion by J. Lawrence Von Thun
Senior Technical Specialist
Denver, Colorado, USA
on "Failure of an Embankment on Soft Clay"
by M. K. Yegian and Hugo Perez La Salvia Teproy

The authors present an ideal case history for examination of the behavior of a compacted embankment on a weak foundation loaded beyond its capacity. The dike slide described appears to have the characteristics of a progressive failure in the sense of an erosion of material strength properties with time as well as in the possible sequential development of the slide mass. The triggering mechanism for failure was suggested by the authors as large undrained deformations in the foundation materials. The nature and pattern of these deformations are not discussed; however, it is presumed that the authors consider them to be horizontal or subhorizontal shear deformations. The effects of su deformations were considered by the authors to be cracking or spreading of the core. The cracking or spreading of the core would significantly change the stability of the postulated critical slip surface si it would eliminate the resistance of the high streng unsaturated, "in-tact" core material.

Response of the saturated foundation materials an undrained mode would include pore pressure increa as a result of (1) increased overburden weight and (an increase in the shear stress. This pore pressure increase would result in lowering the shear capacity which would result in additional strain. Volume cha of foundation materials was not considered to occur under the undrained loading assumption; however, it be noted that any consolidation deformation which ma have taken place as a result of the drainage under t shell and at the toe of the fill would tend to creat tension at the crest and compression at the base of core. Another possible deformation response of the system would be in the nature of a fluid displacemen flow around the more rigid embankment. For the case presented the actual existence, magnitude, distribut and history of the deformations are not known. The failure scenario presented by the authors is most li the appropriate triggering mechanism; however, the f and development of the slide suggest an extension an slight variation of the failure hypothesis which inc porates the possibility of progressive failure. Thi possibility appears to be supportable by the concept strength loss with respect to strain in clays and by final form of the failed mass. Further, this expand explanation leads to a design rationale for the geom of embankments on weak foundation deposits.

A critical failure surface for the dike-foundati system would be one exiting just beyond the head of t dike slope. This potential sliding mass would put ma mum shear stress on foundation materials. Pore pres increases in the already weak material would likely result in this material being stressed at or beyond peak. As the material strain softened, load would b released to embankment materials until such time that the embankment could no longer provide the support required to hold up the critical failure mass. To s degree the horizontal shear displacement of the fail mass would also decrease resistance along the slidin plane. Once the initial mass failed, the remaining oversteepened slope would rapidly slide or slump progressively until the overall failed slope was fla enough to be supported by the foundation at its resi strength. The post failure form of the slide illust tes this type of behavior.

It is reasonable to postulate that flatter dike slopes (say midway between the design and the post failure slope) would not have created shear stresses excess of the strength of the foundation thus avoidi the large shear strains and associated load transfer. The design goal in problems of this nature would be t keep the slope geometry flat enough such that a gradu continuous strain (creep) does not take place. An actual factor of safety on shear strength to load of foundation material of 1.25 or greater may be require to ensure such a condition.

In summary, the primary question of concern in explaining the failure is why and how did the initial foundation deformations take place? Were they a resu of the mass of the dike inducing a major circular typ failure or were they brought on as a function of slop geometry?

Discussion by James L. Sherard, Consulting Engineer, San Diego, California, on "Peculiar Behaviour of the Manicouagan 3 Dam's Core", by Oscar Dascal.

The author is congratulated for presenting a valuable record of dam behavior. The writer believes that this paper is an important fundamental contribution to the embankment dam technical literature and will receive much attention from specialists over the next few years.

Since about 1970 evidence has been accumulating, both from experience with dam behavior and the insights given by new calculation methods, to support the conclusion that small concentrated leaks occasionally develop through embankment dam cores. The writer has concluded that this evidence is now strong enough to make it prudent for the designer to assume that such small concentrated leaks may develop even in well-designed and constructed embankment dams, in spite of the normal precautions taken to avoid them (measures to minimize differential settlement, etc.). Acceptance of this assumption leads the designer to place more reliance on downstream filters for control of concentrated leaks, and to spend less money and place less reliance on design measures to reduce the likelihood that leaks will develop.

The Manicouagan 3 Dam behavior shows unequivocally that small concentrated leaks developed on near horizontal planes through the earth core, carrying nearly full reservoir pressure to the downstream edge of the core, and that the resulting condition has been constant for nearly ten years and is completely safe. The piezometers located in four instrumented sections, located 200 feet apart over the central 600-foot length of the dam, all showed about the same pattern of measurements, so that the situation is not isolated, but a general condition. (Through professional courtesy of the Hydro-Quebec staff the writer had the opportunity to study the entire set of instrument measurements and details of the three exploratory borings made in the core in 1977-78 to investigate the condition.)

The author concludes that the concentrated leaks were travelling in "a set of cracks (filled with loose muddy material), or decompressed layers distributed erratically...". He also concludes that these "... can be attributed to arching ..." caused by greater compression of the central earth core than adjacent granular shells. The writer agrees completely that all the evidence supports these conclusions, and that there is essentially no other tenable hypothesis to explain the conditions which developed. Also, the finite element method calculations show that such arching can easily occur in central core dams of typical embankment materials (2).

Since they could not be seen or sampled in the borings, the physical nature of the leakage paths is not reliably known. The three exploratory borings in the core, made without water or mud for drilling fluid as described by the author, showed the following interesting results:

1. In most of the boring lengths in the core, no measurable quantity of water entered the unsupported sections of hole drilled 1.5 to 3 meters below the casing bottom during the 1 to 2 hour waiting period. This result occurred in spite of the fact that the pore water pressure in the impervious core material penetrated by the boring was near full reservoir pressure. While the core material is nearly cohesionless, it was compacted to a sufficiently dense state to allow it to arch around the unsupported bore hole without collapsing and the embankment was sufficiently impervious to limit the seepage inflow to a negligible quantity in 1 to 2 hours.

2. When the borings encountered the "cracks", water entered the 1.5 to 3.0 meter length of boring below the casing in a small concentrated stream. For example, in the first of these borings (TFD 14), drilled from the dam crest, no water entered the advancing hole until it reached a depth of about 64 feet, at which time water rose in the casing about 35 feet in the first 60 minutes of the waiting period.

Figure 6 shows calculations of "equivalent" coefficients of permeability from these records, showing about $2x10^{-4}$cm/sec for the 35-foot water rise in 60 minutes at depth 64 feet. These calculations are based on the assumption that the leakage water was seeping through a meter or two of embankment thickness, the entire length of hole drilled below the casing where the leak entered the boring. If the calculations were made assuming a thickness of one or two centimeters instead of meters, the calculated permeability would be about two orders of magnitude higher.

Most of the piezometers in the upper half of the dam core are located at either Elevation 550 (75 feet below maximum reservoir level) or at Elevation 625, Figure 3. As seen in Figure 8, piezometers at the downstream face of the core at Elevation 550 followed the first reservoir filling during the last part of 1975 with measured pressure head of about 8 to 10 feet lower than the reservoir level. (For piezometers at Elevation 550, there had been essentially zero measured pressure before the reservoir was raised above Elevation 550.) When the reservoir stopped rising (became full) in December 1975, the measured pressures in the downstream piezometers also stopped increasing, with apparently zero time lag. Then for four years starting in January 1976 the reservoir was kept nearly full, with level generally between Elevation 672 and 675, Figure 8. During this four year period the pressure head in the downstream piezometers at Elevation 550 remained nearly constant, varying only to follow the small variations in the reservoir level. During this four year period of full reservoir the downstream piezometers at Elevation 550 measured pressure heads of about 8 to 11 feet below the reservoir level, and upstream piezometers at Elevation 550 measured average pressures only a foot or two higher than the downstream pressures.

The average pressure head distribution as measured by the piezometers located at Elevation 550, with full reservoir at Elevation 675, is about as follows:

Location	Head Measured
Piezometers at Upstream Edge of Impervious Core	Elev. 668
Piezometers at the Downstream Edge of the Impervious Core	Elev. 665

This shows that there is only about 3 feet of head loss due to seepage across the main part of the core, between the upstream and downstream piezometers (about 90 feet).

The measured head loss in the relatively short length of the core remaining downstream of the downstream piezometer was about 668 -558 = 138 feet. We can only speculate about the distribution of the pressure head in the thin strip of impervious core at its downstream face, downstream of the downstream piezometer. The writer believes that it is quite likely that if piezometers had been located in the core at Elevation

550 very near the core filter boundary, such as one foot from the filter, the measured average pressure had would be about Elev. 668; i.e., the upstream face of the filter is probably sealed by eroded core material carried by the concentrated leak, and the sealed upstream face of the filter is probably acting as the "hydraulic control", with essentially all the head loss (138 feet) occurring through a few centimeters of choked filter at the upstream face of the filter. This is the action commonly seen in laboratory filter tests in which high water pressures are used and an initial concentrated leak through the impervious base specimen (4).

While admittedly speculative, the writer believes that it is probable that the following are generally valid:

1. From arching and internal stress transfer during construction of the dam, and continuing as the lower elevations of the reservoir filled, the average total stress on some horizontal planes through the upper half of the core (Elev. 550 and above) was reduced to nearly zero, probably to value equivalent to about 5 to 10 feet (1.5 to 3.0 meters) of water head. This low total stress acting on horizontal planes was the minimum principal stress.

2. When the reservoir level rose to about Elev. 560, the water pressure acting on the upstream face of the core at Elev. 550 exceeded the total pressure acting in the core on the horizontal plane at Elev. 550. At this time water from the reservoir was able to enter the core in a thin concentrated leakage channel (crack) perhaps with an initial width of the order of 0.1mm or less, on a near horizontal plane. At the time of the initial leak, the water pressure head in the concentrated leakage channel (about Elev. 560) created a neutral stress which was only slightly greater than the previous total stress on the horizontal plane, so there was no significant change in the total stress conditions. The width of the initial leak is so small that the velocity is too low to cause erosion. The fact that the water pressure in the concentrated leak is higher than the pore water pressure in the immediately adjacent embankment creates seepage from the crack into the embankment, acting to hold the crack open.

3. As the reservoir continued to rise in 1979, the pressure in the concentrated leak rose at the same rate, increasing the total stress on the horizontal plane at Elev. 550, causing some strains, resulting in an increase in the width of the initial, thin water-filled crack. The water pressure in the crack is subsequently always equal to the total embankment stress on the horizontal plane and the effective stress is nearly zero. As the width of the initially small crack increases, the velocity of the concentrated leak increases to a point at which some eroded base material is carried to the filter where the eroded debris is caught and the upstream face of the filter is choked, creating a relatively impervious skin and reducing the velocity of the concentrated leak to near zero. After this time the concentrated leak extending across the core on a near horizontal plane, open to the reservoir at its upstream end, is blocked at the downstream end, and the water pressure in the leak is nearly equal to the reservoir pressure for its full length, and nearly the entire head loss occurs through a few centimeters of choked filter face.

4. Because the embankment material is nearly cohesionless, and the effective stress adjacent to the concentrated leak is near zero, the embankment material forming the roof of the concentrated leakage channel softens and falls into the space, creating a "wet seam" of soft embankment material of relatively high water content, several percent above normal saturated water content of the compacted embankment material.

The writer believes that action similar to that described above is the only reasonable hypothesis available to explain the observed behavior of the Man couagan 3 Dam. But the writer also had the opportuni 20 years ago (1965) to inspect the interior of a glac till dam core after the first few months of operation and found strong evidence for the above hypothesis. These observations were presented as part of the desc tion of the "peculiar behavior" of the Yard's Creek U Reservoir Dam, New Jersey, a dam with rockfill shells thin, vertical earth core of glacial till (3). A num of test pits were put down through the core to bedroc in parts of the dam where there had been no visible seepage emerging at the toe. In the walls of these p it was clearly seen that there were thin horizontal " seams", which were much softer than could be explaine by the normal action of water seeping through the cor and displacing the air in the pore voids. The embank material below and above the "wet seams" was stiff an hard. The origin of these "wet seams" was not unders at the time (1965), but the writer now believes that is overwhelmingly probable that they were caused by t action described above.

The downstream filter is a gravelly sand with av age D_{15} size in the approximate range from 0.2 to 0.4 As determined by recent research (4) such a filter is very conservative for a sandy silt core of this type. In laboratory tests with glacial till of the type of Manicouagan 3 Dam core, filters with D_{15}=0.4mm will s immediately with no significant erosion in a test wit concentrated leak caused to act through the base spec men. Hence, current understanding of the fundamental action of filters gives confidence that it was imposs for concentrated leaks through the dam core to have created a serious condition.

There has been another recent similar experience the El Guapo Dam in Venezuela, a 65-meter high centra earth core dam with gravel shells (1). In El Guapo D some piezometers at the downstream edge of the core a about mid-height of the dam measured nearly full rese voir pressure and borings made to study the condition (also without water or mud drilling fluid) encounter concentrated leaks in the impervious clay core which caused the water level in the casing to rise abruptly.

Summary of Main Points

1. The experience supports the conclusion that is prudent for dam designers to assume that concentra leaks may occur through dam cores in dams designed an constructed according to good modern practice.

2. This can occur even in dams with cores of cohesionless sandy silt.

3. It is overwhelmingly probable that the concen trated leaks in the core were caused by arching and stress transfer caused by the greater compressibility the core than the shells.

4. The experience supports the conclusion that downstream filters in central core dams should be capa of sealing concentrated leaks and should be stable in laboratory filter tests using high pressures (high res voir pressure).

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Discussion by Bhagwat V.K. Lavania, Professor, Earthquake Engineering Department, University of Roorkee, Roorkee, India, on "Peculiar Behaviour of the Manicouagan 3 Dam's Core" by O. Dascal.

At the outset let me congratulate the author for the lucid analysis of the probable causes of the weakness zones in the relatively wide core of Manic 3 dam which led to high pressure development in the downstream part of it. In the brief summary of the design principles for the dam, it has been mentioned that 'in view of the nature of the available impervious till, i.e. low plasticity and deficiency of Coarser particles and considering the particularities of the dam's foundation, a wide and nearly symmetrical Core, having a width to hydraulic head ratio of about 0.8 was designed as a precaution against Cracking'. But while searching-out for the cause of existence of weakness zones the author has aid that 'these soft zones may have started, probably during construction, as horizontal cracks due to arching of the core between the much less compressible filtertransition zones and shells'. The glacial till (morain) has low plasticity and low compressibility and therefore the difference in compressibility characteristics of core, transition and shall materials is not expected to give rise to arching that may result in cracking of the core. The author is requested to give compressibility characteristics of the three materials.

It has been also mentioned that the rather flat slope were adopted in view of the liquefaction potential of the fine loose sand in the foundation when subjected eventually to earthquake loading. It is requested that the author may throw more light as how the flattening of the slope provides safety against damage due to foundation liquefaction.

To explain the development of high pore pressures in the downstream part of the core, the three hypotheses have been considered, viz, (i) anisotropy of the core, (2) relative imperviousness of downstream transition and shell, (3) cracking of the core due to arching. The dam is situated in a very narrow valley, The left abutment slope is about 0.75:1 (H : V) and lower part of right abutment slopes at 0.5:1 and upper part at about 1.5 : 1. Therefore, the consideration of effect of valley shape on the distribution of stresses and strains in the dam body is important. It is not known why this aspect has not been considered as one of the hypotheses for cracking of the core. It has been well established that in the dams situated in narrow valleys, tension zones develop at the top near the abutment. Covarrubias (1969-HSMS 82) Carried out analyses of Gepatsch, Infiernillo, and Hyttejuvet dams and showed that the locations of the tension zones as indicated by the analysis were very similar to that suspected at these dams. The discussor also studied the effect of valley shape on stress distribution (study report, Norwegian Geotechnical Institute, 1975) and observed that tension zones or low compressive stress zones (depending on the steepness of abutments and elastic properties of fill materials) occur near abutments. In case of Manic 3 dam, it is expected that tension zone should occur near the left abutment and also in the area where the extension of the right abutment lower slope meets the crest.

Discussion by Bhagwat V.K. Lavania, Professor, Earthquake Engineering Department, University of Roorkee, Roorkee, India, on "The Santa Helena Dam on Compressible Foundation" by V.K. Garg, A.V. Rocha and H.G. Ramos.

At the outset let me congratulate the authors for boldly revealing that the estimation of settlements, in the silty clay layer in the foundation of the dam, was out by about 100%. But the details and the procedure of estimation of the settlements have not been given in the paper and therefore it is not possible to search for the lacuna, for future guidence. Authors have attributed the difference to (1) over estimation of C_c Value, (2) reduction in mass compressibility due to provision of large diameter sand drains, (3) arching due to sand drains and, (4) variability of the fluvial deposit (clay). Authors are requested to give details of settlement estimation and mention the method of computing total stress be-neath the embankment.

This is well known that sand drains are provided in saturated clay stratum to accelerate consolidation with the construction of an embankment on it. Therefore, there is nothing new as far as the technique is concerned. The important part is the design of the sand drains, i.e., their diameter and spacing as related to soil type, the rate of loading and development of pore water pressures. It has been mentioned that when the fill elevation was about 10 m, the placement was suspended temporarily due to high piezometric recordings. It would have been of interest to know if the development of pore pressures was according to, (1) one dimensional, (2) three dimensional elastic, (3) Skemption's equation or, (4) Henkel equation. The authors may throw some light on it if such exercise has been done.

Transverse cracks developed in the embankment firstly when its elevation was 10 m and secondly when it reached to 18 m due to local failure of soil and differential settlements respectively. Now with this experience, authors may tell if it is possible to avoid such cracking at other similar sites in future by controlling the rate of fill placement and (if needed) by providing staggered transverse joints in the embankment.

Fig. 2 of the paper shows that a sigr finant area of the foundation consists of sil fine sand (loose also) which is suspectable i liquefaction during earthquake. Authors are requested to mention if some safe-guards agai this hazard have been provided.

Discussion by Sh. A.C. Khazanchi, Sr. Astt. Director, Regional Research Laboratory, Bhopal (M.P), India on Power House slope Behaviour Fort peck Dam Montana by J.V. Hamel and G.S. Spencer.

At the outset let me congratulate the author for presenting an excellent and wel documented history of slope movements of the site from 1933 - 1983. Such records are indeed of paramount importance for all the sites and can be a very useful too for the planners and designers for future programme of works.

I would like to ask the authors a few clarifications on the following aspects on slope movements which have occured on the site:-

1. Would the author please tell us if the soil excavated from the building site was dumped on the slopes. If so how this was assessed. Whether the dumped soil mass was responsible for further soil movements?

2. Since the shear strength of the soil was low, what methods were adopted to improve the strength to avoid slope failures/movements.

3. What is the explanation for significantly higher field residual strength than the laboratory?

Reply by M. Fukuoka, Professor of Civil Engineering, Science University of Tokyo, Noda City, Japan, to the discussion by John P. Sally on "Deep Seated Base Failure and Reconstruction Work by M Fukuoka".

No evidence of failure had been found before the rupture took place. Therefore, no detailed investigation was performed at that stage. The soil investigation was conducted following the method commonly used in Japan. Unconfined compression tests and unconsolidated undrained triaxial compression tests were made with undisturbed samples. Thus, unconfined compressive strength q_u, effective cohesion intercept c' and effective angle of internal friction ϕ' were obtained. The design shear resistances were calculated as plotted in Fig. 1. Usually shear resistance increments are neglected assuming rapid construction work.

I am afraid that the discussor could not understand fully the intention of the author. The author wanted to explain that this deep seated base failure with sustained shear stress along the bottom of the clay layer started by the unbalanced earth pressures on assumed vertical wall I and II. The discussor pointed out that the progressive failure might take place. The author agree with him. But it should be noticed that the incipient failure took place at the bottom of the clay layer and not at a point on the slip circle with minimum factor safety. Shear resistance along the actual sliding surface was not static but kinetic. The failure was dynamic and progressive one but not the static one. Therefore, the residual strength has no direct connection with resistance during rapid movement. The author's idea is stated in the following paper. "Kinetic friction in Landslides, IX ICSMFE Vol. 2, p.71 by M. Fukuoka". "Oral discussion, X ICSMFE, Vol. , p. 585.

The earthquake might cause increase of pore water pressure and result in increasing shear stress and reducing shear resistance, but it is not very clear. Time settlement curve could have not been used for predicting rupture. Figure 2. shows time settlement curves at the Stations 46(collapsed) and 50 (safe). No evident increase of settlement was observed before the slide. No cracks were observed on the surface of the embankment.

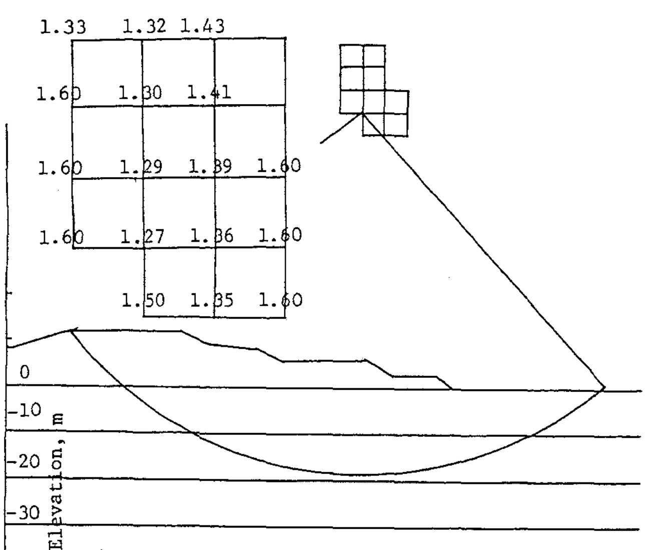
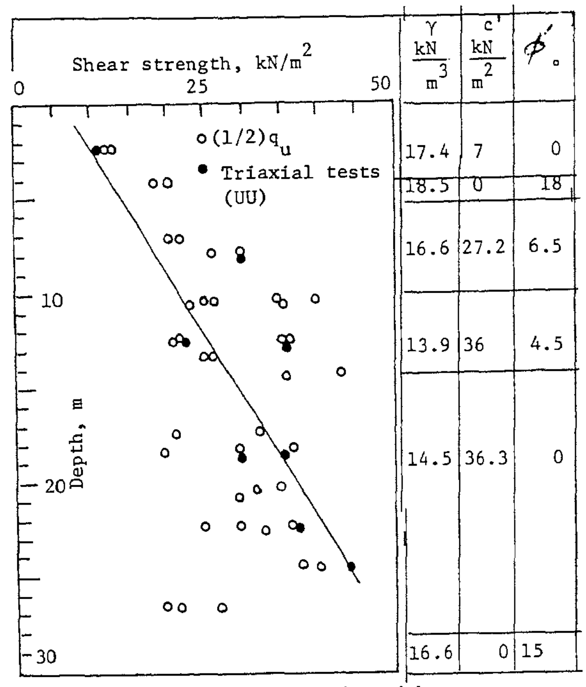


Fig. 3 Factor of safety with slip circle.



Note: Design parameters, unit weight γ, cohesion c', angle of internal friction φ'.

Fig. 1 Soil parameters for design.

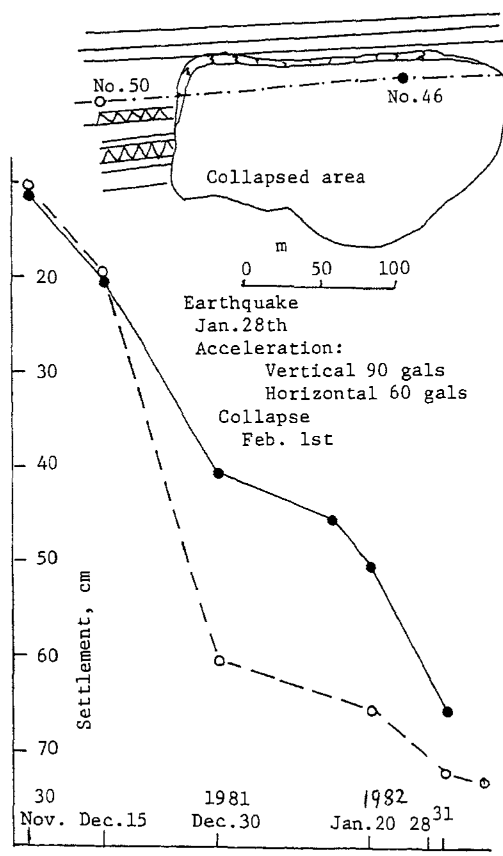


Fig. 2 Time-settlement curves.

Reply by O.Dascal, Eng. Hydro-Quebec, Montreal Quebec, Canada to discussions on "Peculiar Behaviour of the Manicouagan-3 Dam's Core"

The author would like to thank prof. B.V.K. Lavania and J. Sherard for their interest in the paper and for their valuable comments.

Unfortunately, only data on the compression characteristics of the till are available. Laboratory test results on the Manicouagan-3 till, published by Loiselle and Hurtubise (1976) indicate that the material placed at the lower part of the core (with high water content and at low degree of compaction) exhibits an average coefficient of compressibility (expressed as) of 2.0-2.5X10^{-1}(KPa^{-1}). These values correspond to an initial void ratio of about 0.33 - 0.35 with a consolidation pressure range of 500-1 000 KPa. Under these conditions, a block of till of only 10m (30 ft) thickness can undergo a compression of 10-20cm. At the same time the well graded sand and gravel filter-transition zone, compacted at the lower part of the dam, to a relative density of over 95% (dry unit weight over 2100 kg/m^3), can be considered practically incompressible (values of E=(1.5-2.0)x10^{-3}- Jakobson 1965). Thus the difference in compressibility of the two materials (till and sand/gravel) facilitate the development of arching.

The development of an arching phenomenon in the dam's embankment was also confirmed by finite elements analysis (fig.9) carried out by Lefebvre and St-Arnaud (1975).

"The entire horizontal plan is fine sand and half of it's liquefied during an earthquake. If one makes the very severe assumptions that during an earthquake along a potential sliding surface one half of the volume of fine sand in the foundation is liquefied, i.e. the shear strength reduced to zero, and if the remaining shear strength along the sliding surface would still ensure a safety factor of 1.5, the dam could be considered to be amply safe". These criteria impose a maximum slope of IV:3H for the required safety factor of FS=1.5.

The effect of the valley shape on the stress distribution and deformation of the embankment was analyzed (Lefebvre et al. 1977) and monitored by a set of instruments (Dascal 1973). It should be mentioned that one of the factors considered in adopting a curved embankment, was the closure of eventual tension cracks by the deformation (flattening) of the dam's curvature. Neither the finite element analysis nor the instruments results have indicated the development of tension zones in the embankment. As mentioned in the paper, the weakness zones (cracks, fissures, etc) seem to have a horizontal or subhorizontal position, and are situated at a depth of more than 30 m (100 ft) below the crest. These weakness zones are distributed along the entire length of the embankment. Their characteristics are inconsistent with those of tension cracks, which develop at the top and near the abutments of a dam and have in general a vertical or quasi-vertical orientation.

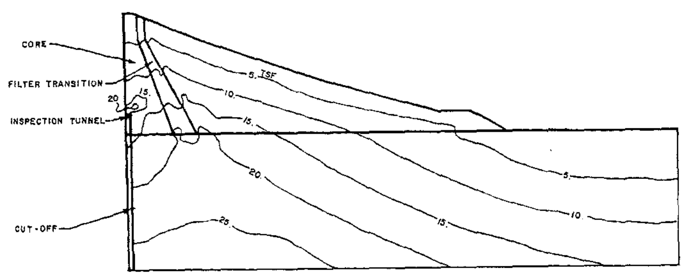


Fig.9:Major principal stresses in the embankment and its foundation

The Manicouagan-3 dam site is located in a relatively low seismic activity area. The Seismology Division of the Earth Physic Branch Ottawa evaluated in 1970 that the site could experience a seismically induced peak ground acceleration from about 1% g with a probability of exceedence of 0.01 per year to 40%g for a probability of exceedence of 0.001 per year. However the presence of a loose, fine sand formation in the foundation of the dam required consideration of its liquefaction possibilities. Without developing a more elaborate analysis, the following design criteria were adopted (recommended by prof. A. Casagrande - Chairman of the Counsulting Board).

The author would like to thank particularly Mr. J. Sherard for the valuable supplementary explanations.To emphasize his conclusions fig. 10 is presented, illustrating the pore pressures recorded at elevation 550 for the last 10 years. As can be seen since the reservoir's impounding the pore pressures have remained practically constant (small variations due to reservoir level variations) with only a few feet head loss betwen the two piezometers. The adequate filter material by preventing the migration of the core's fine particles, allowed the buildup of a thin impervious zone at the core-filter interface, which remained stable for such a long time period in spite of being submitted to a very high hydraulic gradient.

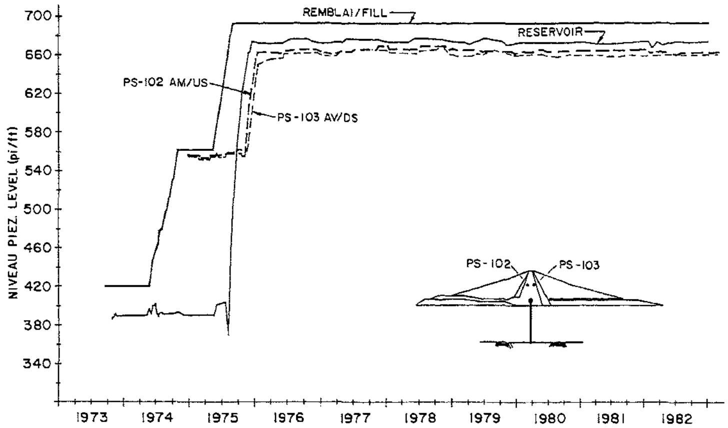


Fig.10:Pore pressure at the upstream and downstream zone of the core (elevation 550')

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Closure by W.R. Stroman, R.R.W. Beene, and A.M. Hull on "Clay Shale Foundation Slide at Waco Dam, Texas."

The authors very much appreciate the review provided by Sh. A.C. Khazanchi, and for this opportunity to provide answers to the questions brought forth by the reviewer. In the following paragraphs, the questions will be addressed in the same order as the discusser's questions.

1. a. The cost of geotechnical investigations performed prior to the slide is estimated to be $0.4 M.
b. The cost of investigations made after the slide is estimated to be $1.7 M.
c. Embankment repair and modification is estimated to have cost $3.7 M and modification to the spillway structure to have cost $3.0 M.

The figures given above are in U.S. dollars spent at the time the investigation and repairs were made. Using Engineering News Record factors, the mid-1984 cost of items 1.b. and 1.c. is estimated to be $38.9 million.

2. The discusser asks for suggestions on how to make soil investigations binding. We assume that by the word "binding", he means, How can all potential subsurface problems be identified prior to construction? This can not be done positively or economically. A reasonable approach involves (a) review of literature of the geologic conditions of the project area (if any is available), (b) surface mapping and reconnaissance, including aerial photography, (c) initial drilling and sampling, (d) expansion of the drilling and sampling to provide "adequate" design information, including groundwater conditions, (e) applictions of the information obtained to laboratory testing programs and design studies, (f) instrumentation and observation during construction to determine if conditions encountered conform reasonably with those that were inferred. The degree of "adequacy" of the investigations and of the construction depends on the judgment of the engineers and geologists, which in turn, depends on their experience.

3. Theoretically, the strength of the clay shale might be improved by chemical and electroosmotic stabilization, separate or combined, but the cost would be prohibitive, and the feasibility of stabilizing such a large mass of clay shale is doubtful. Relief of pore pressures by drainage was considered, but a test installation was ineffective, and the extremely slow dissipation during the twenty years since completion of the dam shows that drainage during construction could not be relied upon to increase the degree of stability.

4. The foundation conditions responsible for the slide were not recognized during design and initial construction due to a combination of circumstances:

a. The foundation clay shales were incorrectly classified as being the Eagle Ford formation, instead of Eagle Ford, Pepper, and Del Rio. A dam similar in size to Waco Dam had been completed in 1954 in northern Texas on a foundation composed of Eagle Ford shale. There were no signs of instability in that dam, and this contributed to the impression that the foundation at Waco would be stable.

b. A few quick shear (Q) tests were performed on the shales found at Waco during original design. The measured strengths were larger than required for stability. Therefore, even if the geologic formations had been correctly identified, the embankment design would not have been different.

c. At the time of design and construction of W Dam, the state of the art did not include recognitio the potential for development of positive pore press in clay shales. When the first measurements of high pore pressure in the foundation at Waco were present to Prof. Karl Terzaghi in late 1961, he immediately responded "Nonsense! You don't get pore pressures i shales." The existence of both the high pore pressu and the low residual strength remaining after tecton distortion was required to cause the slide. Since neither of the circumstances was recognizable within state of knowledge at the time, the slide was not predictable.

Author's reply by G.W. Plant and P.B.B. Vosloo, "The Failure of a Soil Blanket Reservoir Lining Caused by the Action of Bacteria," to discussion by J. Lawrence Von Thun

The authors would like to thank Mr. Thun for taking the time to comment on our paper. He is quite right to put the word failure in quotation marks as we agree that the increase in flow over the design value was not substantial but was, nevertheless, higher than intended.

The increase in permeability of block samples taken after emptying compared to the soil as originally compacted is "statistically significant" in our view.

Regarding the nitrogen generated, we believe that it is not necessarily the volume which is important but rather the "leavening" effect as the gas was released, thereby altering the structure of the soil.

We agree with the need for controlled verification tests but, unfortunately, funds were not available. However, the presence of nitrite and nitrate and the observed nitrogen is important evidence.

Closure by A.Cancelli and A.Cividini

The authors would like to thank Mr. Sully for his interest in the paper. He raised some comments and questions related to the disegn criteria and some aspects of the measurement program.

The height of the first-stage embankment was determined neglecting the shearing resistance of the thin upper layer because of the following reasons:

a) the presence of cracks in the surficial dessiccated crust, mainly due to the water table fluctuations;
b) the opening of subvertical failure surfaces induced in the crust by differential settlements at the early stage of construction;
c) the failures occured in the same area in similar works, for which the shearing strength of the surficial crust was taken into account.

Economical consideration suggested to adopt bored piles filled with sand. The ratio between spacing and nominal diameter was about 8-9, close to the lowest values commonly adopted (Hansbo, 1960; Navdocks, 1962; Johnson, 1970). A closer spacing (leading to a faster consolidation rate) would have been useless, inasmuch two years were required for the tunneling work necessary to complete the railway branch.

At the time of the site investigation (1976-1977) the assessment of the small-scale structure within the foundation soil (for instance by means of the 'piezocone') was not possible. Of course, a better knowledge of the structure would have provided useful information on the sand layers within the clay deposits. This information, and that deriving from 1-D consolidation tests on specimens containing an axial sand drain, could have been used in the determination of the in-situ permeability coefficients.
Note that in the backcalculation by means of the Bayesian approach the initial values of the permeability coefficients were assessed on the basis of the laboratory results and of the presence of sand drains, and that the corresponding initial variances accounted for the large uncertainties in the initial choice.

The authors agree with Mr. Sully's remark that a more exhaustive picture of settlement distribution can be obtained by means of measuring devices installed in the depth, unfortunately this implies also a non negligible increase of costs.

Summing up, the information and data obtained during the geotechnical investigation do not suggest major modifications of the initial design philosophy. Of course, the improvement of geotechnical instrumentation and computer facilities attained in the most recent years would nowadays permit a more refined design, leading perhaps to shorter loading steps or, alternately, to a larger spacing.

The back analysis presented in the paper leads to results having different ranges of validity. In particular, the backcalculated deformation and permeability coefficients can be used only for further design in the same area and under similar geological conditions. The conclusions on the effectiveness of the field measurement program and the suggestions for the "design" of the same have a more general validity and can be applied to different geotechnical problems.

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Reply by Vinod K. Garga on discussion by Bhagvat V.K. Lavania on "The Santa Helena Dam on Compressible Foundation".

It should be remembered that the soft clay at the site is a soft heterogeneous deposit with wide variation of compression index C_c (see example Figure 8). The settlements were estimated using laboratory determined values of C_c at the different borehole profiles. Complex computational methods are deemed to be of litle relevance in such heterogeneous deposits.

The high pore pressures recorded when the fill was approximately 10 metres high were undoubtedly due to rapid rate of construction. The development of high pore pressures are predicted by Skempton's equation. The high pore pressures were expected and field charts defining dangerous, high and satisfactory limits of pore pressures with fill height were prepared in advance for guidance of field staff. It is interesting to note that while the first cracking at fill height of 10 metres was caused by shear deformations, the second cracking at fill height was entirely due to differential settlements. No significant increase in pore pressure was observed when the fill height reached 18 metres (see Figure 9). The discussor inquires whether staggered transverse joints may be used in embankments to avoid cracking. This is dangerous and unsound course of action to which the author is totally opposed. If adequate construction time is available then it is advisable to place the fill in advance over compressible foundation in orderto reduce subsequent differential settlements. Unfortunately in the case of Santa Helena project, time to preload the compressible area of the foundation was not available.

This area of Brasil is essentially a nonseismic zone and it is deemed to be improbable that liquefaction in the foundation would be triggered by seismic activity.

Reply by S.R. Hencher now of Department of Earth Sciences, Leeds University, England and R.P. Martin of GCO, Hong Kong to discussion by J. Lawrence Von Thun on "The Failure of a Cut Slope on the Tuen Mun Road in Hong Kong".

The authors thank Mr. Von Thun for his comments regarding their paper and for his carefully considered questions which they will attempt to answer in the order that they were given.

(1) Regarding the permeabilities of the dolerite and granite, no field testing was carried out and the statement regarding the relative magnitudes was based on values calculated from triaxial test data. In-situ, the mass permeabilities were probably even more contrasting with the highly decomposed, sandy, silty granite being more intensely jointed and the completely, decomposed dolerite being relatively fine grained and homogeneous.

(2) Drainage holes above the dolerite were not considered for emergency remedial measures partly because the designed works were based necessarily on a very rapid appraisal of the failure during which the significance of the dykes was not realised. It was however recognised that infiltration had probably caused the failure and therefore a concrete buttress used to support the granite tor in the rear scarp of the main failure was constructed using no-fines concrete to prevent damming of water. The trimmed scars were protected with a concrete/ plaster mix to reduce infiltration.

(3) With respect to the shear testing, multi-stage testing is preferred because it provides more information than single-stage testing for the same number of samples and with little extra effort. A comparison of strengths in second and third stages with the envelope defined by first stages is useful for interpreting the origins of strength. This does not imply that the authors advocate the use of multistage tests for determining 'in-situ' strength parameters, but provided that the multistage tests are overlapped so that first stage results can be used alone, as in the example given, then the method provides important additional information.

It is considered significant for example that the data plotted in Figure 5 of the paper reveal little loss of strength on second and third stages. If bonding due to remnant rock texture were contributing to strength then one might expect a loss of 'cohesion' in the second and third stages. On the contrary, the strength envelope for the multistage direct shear test on decomposed granite of dry density 1.78 Mg/m^3 falls neatly into a family of envelopes distinguished by dry density and defined by first stage tests only. In this series of tests, shear strength can be attributed to a basic frictional resistance supplemented by strength due to dilational or compressional effects which in turn are dependant upon original dry density and normal stress level (Figures 6 and 7). There is no evidence of cohesion due to shearing through original bonding. Clearly, in the test cited above, th effect of close packing causing overriding was not lost during the first stage, noting of course that increasing the normal stress would in turn have altered the structure.

Concerning the other questions related to shea strength, the authors confirm that the correction for dilatent - compressive behaviou is indeed an 'i' angle correction although it should be emphasised that here the correction is carried out at peak strength rather than averaged throughout the test as is sometimes done. The corrections were carried out to investigate the wide scatter in original data points for decomposed granite. By doing so, it was established clearly that the scatter was not random but was sample dependant and could be explained in terms of a basic frictional resistance plus or minus some additional strength according to density and normal stress.

The basic frictional resistance indicated by the line in Figure 7 could not be expected to represent the strength of most material in-situ which might compress or dilate during failure depending upon its density and the applied stresses. The granite in the major failure was mostly of low density (1.5 Mg/m^3) and at the low stresses on the failure scar (0-100 kPa) the material might be expected to be slightly dilatent (becoming less so with increasing normal stress) therefore having a shear strength marginally higher than the basic frictional strength (see Figure 6). The strength envelope defined by this behaviour can be expressed for convenience using parameters of $c' = 5$ kPa and $\phi' = 36°$ over the stress range of interest. These parameters will provide the same shear strengths for analysis as would using a variable (ϕ + i) parameter which decreases with increasing normal stress although they represent the soil behaviour envisaged less realistically.

(4) The granite in the minor failure scar, described as "highly jointed" in the paper, had numerous joints with spacing generally less than 500 mm (very poor quality according to Hoek & Brown (1980) and ranging from extremely closely spaced to medium spaced according to BSI (1981)). In the minor failure, six joint sets were recorded and lines of intersection at 32/134, 32/180 and 32/128 daylighted in the slope (roughly 47/155). Despite their apparent kinematic ability to cause failure however, the joints were recorded as impersistent (less than 1 m) and that, together with the lack of exposed joint surfaces along the failure scar, suggested that the failure had not been simply jointed controlled. Later back analysis showed however that the factor of safety of the slope was greater than 1.0 even for the worst water conditions and lowest intact material strength imaginable and it was concluded that the joints must have had some weakening effect.

(5) Concerning the original design of the slope, geotechnical aspects of the highway design as a whole are discussed by Slinn, Greig (and Butler)(1976). Slopes through decomposed rock were cut according to "traditional" criteria at 5 on 3 individual slopes with 1.5 m berms at 7.5 m vertically rather than on a slope specific basis. Modifications to cope with local conditions were made as the slopes were cut. Boreholes were put down at an average of 1 per 100 m length of road which clearly would not have allowed detailed geotechnical appraisal of each cutting.

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Authors' response to discussions by S. R. Hencher, Senior Geotechnical Engineer, Hong Kong Government and M. K. Yegian, Associate Professor of Civil Engineering, Northeastern University, Boston, USA on "Earth Dams at Nuclar Power Plants" by R. Pichumani, D. C. Gupta, and L. W. Heller, U.S. Nuclear Regulatory Commission, Washington, D.C.

The authors thank the discussors for their comments on the paper. Hencher has asked how confident the authors are in the analytical methods now available for the earthquake analysis of earth dams in light of actual cases. Our confidence in the prediction of the behavior of dams due to earthquake excitation is about the same as our confidence in other geotechnical analyses such as predicting dam crest settlement and avoiding tension cracks in compacted embankments. Dams that have sustained strong earthquake shaking generally exhibit surface cracking and slight settlement, as was the case with the Franklin Falls Dam in New Hampshire; current analytical methods lead to an expectation of some permanent deformation.

Hencher has also asked about the approval of geotextiles in filter applications. The authors are aware of two such applications where the filters could be easily replaced, if necessary. No particular provisions were made in these two cases to alleviate clogging or deterioration. However, approval of geotextiles for use in other situations in safety related earth structures would be based on the demonstrated performance of geotextiles under similar loading and environmental conditions.

Professor Yegian has suggested that the authors' paper include details of engineering analyses and investigations, specific relevant technical information and data. This information may be found in a publication entitled "NRC Inventory of Dams" (NUREG-0965) available from the National Technical Information Service, Springfield, Va. 22161 and in the NRC Public Docket Room, 1717 H. Street, N.W. Washington, D.C.