J21 0194/84 Fourth Australia - New Zealand Conference on Geomechanics, Perth, 14-18 May 1984

Concrete Gravity Dam Foundations: An Open Case of Geomechanical Interaction, Structure-Foundation and Theory-Practice

OFERECE VICTOR F. B. DE MELLO

V.F.B. de MELLO

Professor, Consulting Engineer, President International Society for Soil Mechanics and Foundation Engineering, Brazil

SUMMARY A discussion of gravity dam foundations on jointed rock is undertaken for the purpose of broadening perspective on the geomechanical interaction problems with civil engineering. There are, among others, the interactions in: geology-geomechanics; soil-rock mechanics; the routine load-settlement, and also the important shear-displacement; test-prototype; seepage-uplift-drainage; theory-practice; and also instrumentation-observation-acceptability indices; and finally, the interaction of subsequent recycling of design and computational models for iteratively improved quantifiable criteria for avoiding misbehavior.

1 INTRODUCTION

The theme of the Conference has been very aptly chosen, at a time when, over most of the world, civil and geotechnical engineers have been forced into a period of more reflection, to compensate for the frustrating lull in activities. Moreover it strikes me as suggestive that we are gathered in one of the principal countries in evidence for the implementation of big civil engineering development projects under the most updated international practices; and also that this theme is taken up in one of the regions that has had the wisdom and vision to congregate all efforts in a single all-embracing Geomechanical Society.

It has been oft emphasized that civil engineering develops principally through the creative visualization, generally presumed intuitive, of physical models that superabundantly satisfy the desired function. Failures open the window to enlightenment on the physical model of ultimate total misbehavior, to be guarded against. Thereupon ensues the phase of a triple-pronged pincer movement: on the one hand the codifiers attempt to crystallize the technological gains for a comforting assurance of repetitive guarantee against failure; on the other hand, reality and Society's ever-growing needs force us to recognize each case as singular, notwithstanding the general tenets of theory, and force us forward to test the frontiers of impunity; and, finally, squeezed between these two advancing flanks, the conscientious column tries to advance through quantifiable criteria, iteratively revised as testing-computational-observational abilities progress and are cross-examined with regard to quantifiable degrees of partial misbehaviors.

Whenever foundation-structure interaction is mentioned it has been almost automatic to associate the interaction with redistributions of settlement deformations. In tribute to the special conditions above mentioned, under which I had the honour to be invited to offer my message, I accept the responsibility of emphasizing that multiple are the conditions of structure-foundation interaction and theory-practice interaction to which we must devote our enthusiasms. As a single example, all-important though frequently stigmatized as routine, I shall resort to the problems of gravity dams on rock foundations. Among interactions involving deformabilities and settlements, shear deformations and sliding failure, impervious treatments plus drainages and consequent uplift pressures, and some others of secondary concern, I shall but cursorily consider the first, and concentrate on the other two significantly discussed.

2 SETTLEMENT DEFORMATIONS IN JOINTED ROCK: SOME PREDICTION VS. PERFORMANCE EXPERIENCE, AND SUGGESTED REVISION OF HYPOTHESES.

In the problems of soil mechanics and foundations, the importance of structure-foundation interaction with regard to settlements was justifiably one of preeminence. At the start, interest was drawn to large consolidation settlements in soft clays, and to damages inflicted to buildings, structures with which every human being is in some way concerned. As buildings became higher and more rigid they also became more sophisticated, with finishing materials more brittle and expensive: thus the interest in soil-structure interaction, and theory-practice interactive adjustment, has not ceased, even though settlements and differential settlements have been reduced to the scale of but a couple of centimeters. As regards limits of tolerable vs. unacceptable differential settlements the first attempt at quantifying quite naturally concentrated on the presumed "initial cracking". That is, the first symptom that is subjectively taken as striking. I have noted that observations close to zero are very erratic and inconclusive, and that progress in establishing quantified criteria will increase when we shift attention to plotting and interpreting in a more tangible range, the variations of behavior cause-effect, partial differentials $\partial C/\partial \Delta s$, where C = crack, Δs = differential settlement, while all other parameters are maintained constant.

In the case of gravity dams founded on rock, the concepts and practices started from the other extreme. Rock foundations were generally sought and assumed, and rock was initially taken as so good as to dispense with quantifications on deformations. Concomitantly the dam's behavior has been routinely analysed under a rigid-block hypothesis. It was in connection with the statically indeterminate arch dams, their higher stress levels, and foundation deformations of consequence, that the problems of structure-foundation interaction on rock deformabilities came to the fore.

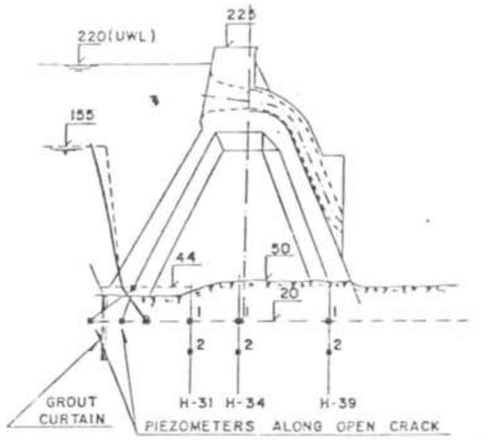
Of course, the conventional hypothesis for stress distribution to the foundation is recognized as nominal, applicable if the body of the dam continued with its geometrically limited crossection into the rock. For tangible ratios of concrete to rock moduli E, and for the block on a semi-infinite rock foundation medium, revised stress distributions have been computed by finite element analyses (Rocha, 1978), and there have been sundry attempts to determine in situ parameters and predicted settlements, as well as prototype observations for comparisons of observed vs. predicted. Although there is but little concern with settlements (up to a few centimeters, such as would tear waterstops between blocks, or impair operation of mechanical equipment eventually affected) the basic fact is that one seeks to quantify predicted and observed settlements in order to investigate the thresholds of impunity for the cases, more probably in the offing when less competent foundations may have to be accepted.

Many are the projects of my experience in which the best available international (European and North American) testing techniques were employed for determining in situ parameters for foundation rocks and rock discontinuities, and subsequent settlement predictions were carried out with every sophistication of finite element analyses and parametric variations. Special reference may be made to the São Simão, Água Vermelha, Nova Avanhandava, and Itaipu Dams (the latter an alleviated gravity type) all of them reported in many publications. Figs. 1, 2 limit themselves to reproducing indications of observed vs. predicted settlements of the lowest and highest dams, Nova Avanhandava (35m) and Itaipu (190m). The gist of the comparison is that the prototype behavior has been invariably very much better than predicted.

Foundation deformability has been postulated on the basis of conventional parameters E,μ of the rock masses (subhorizontal) varied basalt horizons and K_n and K_t (normal and tangential rigidities of discontinuities, apud Goodman and Christian, 1977). All values of E, K_n and K_t for computed prediction have been extracted from very many meticulous field and laboratory tests; the exception refers to μ, for which the routine has been to adopt 0.2 (based on a few published laboratory tests, on hard rock specimens, a questionable routine that prevails, for absolute lack of data).

The lack of compatibility between computed and observed values is so great that it becomes difficult to choose how to express it, whether as a difference (mm) or as a ratio, or ratio of difference: such ratios in some points go to infinity or even to absurdity, when an unexpected +ve value, settlement, registers as a -ve value, due either to observational errors or to erroneous geomechanical or computational models. Such vicissitudes are, to begin with, inevitable, in the range of the very small values at play, but, moreover, in such a range should be recognized as meaningless and purposeless.

A recent conclusion (Souza Lima, 1983) would be that (for the Itaipu dam) much better correlations of observed vs. predicted deformations were obtained when the conventional assumption was abandoned of a vertical crack into the rock along the heel (upstream)of the dam. One must guard carefully against any unsupported conclusions that might in a given case arise by fortuitous compensating errors; the likelihood of such a vertical crack is discussed below, and postulated as a debatable design decision.



SETTLEMENTS (mm)

		ANCHOR 1		ANCHOR 2	
		OBSERVED	PREDICTED	OBSERVED	PREDICTED
H : 31	AC	0,63	11,7	0,45	8,6
	AF	0,70	10,0	0,50	7,4
H : 34	AC	3,15	11,5	2,35	9,2
	AF	3,65	10,9	2,80	8,0
H : 39	AC	1,05	10,0	0,75	7,4
	AF	1,70	15,0	1,35	11,0

AC = AFTER CONSTRUCTION
AF = AFTER FILLING

SHEAR DISPLACEMENTS (mm)
(UWL = 220 m)

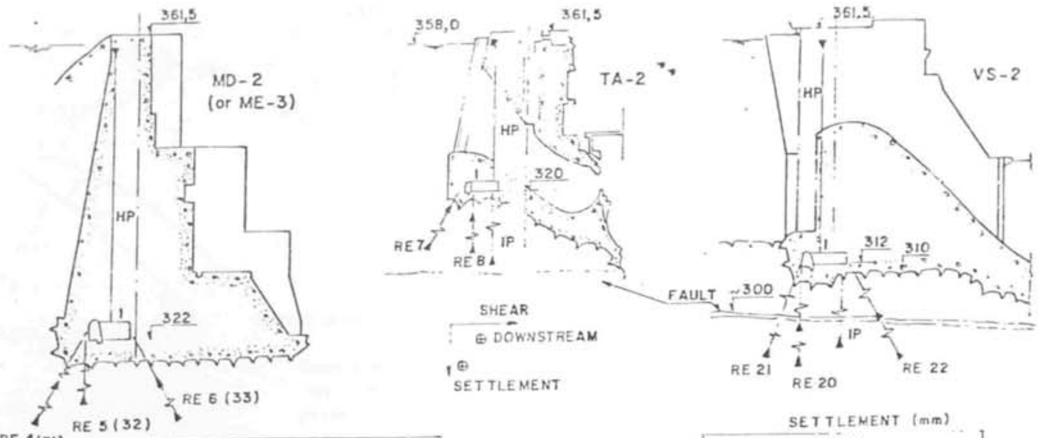
BLOCK	HIGH (m)	OBSERVED	PREDICTED
E 6	85	1,0	5
F 1/2	102	1,0	20
F 5/6	130	1,6	17
F 13/14	185	1,4	36
F 19/20	185	2,3	36
F 27/28	135	1,6	16
F 35/36	110	1,3	11

Fig. 1 Itaipu Dam foundation deformations, observed vs. predicted. (Souza Lima, 1983, de Barros and Barbi 1983)

The basic question recognized as blatantly significant is the interference of in situ stresses in rock. Geomechanical computations in rock mechanics took over from early soil mechanics the hypothesis of a known uniform geostatic overburden stress, and stress variation with depth.

The question merits reappreciation at depth, since not only settlements, but also shear displacements along discontinuities, seem to be invariably overestimated (Figs. 1,2), at least in igneous and metamorphic rocks. I daringly submit that we face interesting problems of geology-geomechanics interaction in theory, much more promising in significance than random adjustments in sophistication of finite element computations.

To begin with, in competent rocks the dispersions are very much bigger than in soils, since a rock mass may have very high strengths (internal stresses) but along planes of discontinuity can have zero strength (precisely because the strong elements carry the burden). We must, therefore, reason on the basis of histograms. Secondly, when dealing with a broad histogram and hysteresis (ine-



		SHEAR DISPLACEMENT (mm)			
		BLOCK			
INSTRUMENT		MD 2	TA 2	VS 2	ME 3
HP	AC	1,3	0,7	–	0.9
	AF	1,0	–	–	–
	P	5,7	–	–	–
IP	AC	–	-0,4	–	
	AF	–	1,1	1,7	–
	P	–	6,3	7,0	–
I	AC	0,1	0,2	-0,3	0,2
	AF	-0,1	0,1	-0,1	0,2
	P	6,4	4,6	5,1	5,5
RE		RE 4/5	RE 7/8	RE 21/20	RE 31/32
	AC	-0,4	0,5	-0,4	-0,8
	AF	-0,4	0,7	0,1	0,5
	P	7,7	6,3	7,0	7,7

		SETTLEMENT (mm)			
		BLOCK			
INSTRUMENT		MD 2	TA 2	VS 2	ME 3
	–	RE 5	RE 8	RE 20	RE 32
RE	AC	-0,1	0,4	0,7	2,0
	AF	0,1	0,4	0,8	2,0
	P	16,6	20,5	20,7	22,8

AC = AFTER CONSTRUCTION
AF = AFTER FILLING
P = PREDICTED
HP = HANGING PENDULUM
IP = INVERTED PENDULUM
I = INCLINOMETER
RE = ROD EXTENSOMETERS

Fig. 2 Nova Avanhandava Dam, shear displacements observed vs. predicted (Miyaji et al, 1983)

xorable in Nature) we must attempt to interpret from which end we approach the histogram (which rejection criterion, upper or lower), as schematically shown in Fig. 3. In an analogous manner it was shown (de Mello, 1980a) that whereas uniform sands were reported (in early simplified concepts) as associated with a single stable repose slope, in the case of rockfills the excavated slope is much steeper (≅55°) than the slope of the end-dumped stockpile agrowing (≅35°).

In short, I would postulate that in a sound brittle rock it is rigid rock elements that carry the stresses (and absorb the consequent deformations), much as I postulated for the behavior of saprolites (de Mello, 1972): the PRINCIPLE OF EQUIVALENT DEFORMATIONS, whereby initial locked-in stresses under overburden conditions are different in contiguous rock elements of different rigidities, and whereby for small stress increments the deformations will be equivalent, and for somewhat higher stress increments, the incremental stresses will be distributed in direct proportion to the rigidities, in an attempt to maintain deformations equivalent.

Under such a postulation the geologist must be intensely consulted regarding formative and degenerative processes of a given rock and site. May we or should we assume that if a subhorizontal crack weathers, the areas exposed to weathering do not carry more than a small fraction of the imposed stresses, and meanwhile it is on the hard (sounder ?) contacts in limited areas that the stresses

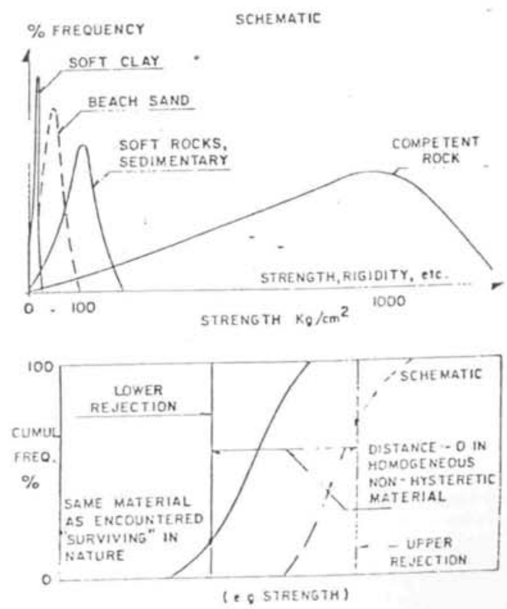


Fig. 3 Comparative behaviors of materials under "natural selection"

proportionally higher) are effective ? How much longer can we permit ourselves the uneconomic play at geomechanical structure-foundation interaction, without introducing the geology-geomechanics interaction ? Are we not conscious that in situ stresses need not be zero at the surface, neither do geostatic variations with depth (in the small scale of our routine projects and the relatively rigid materials) follow the flexible behavior postulates of sediments ?

We might, indeed, profitably shy away from the intense dispersions of point measurements of in situ stresses, to concentrate attention on the behaviors of ∂(stresses) vs. ∂(strain), in higher stress and strain ranges, of more tangible interest. Knowledge is especially elusive in matters of lesser relevance.

3. SHEAR DISPLACEMENT INTERACTIONS. SAFETY AGAINST FAILURE

3.1 Discontinuities Really Critical as Regards Shear

One of the lessons that in my experience made a noticeable impression in my mind is that our intuitions, generally visual, regarding a discontinuity presumed critical with respect to shear resistance, are frequently illusory. a) In the 1930's and 1940's many shear tests were carried out on the concrete-rock contact plane, presumed to be critical: no test ever corroborated this fear, the failure having always occurred along rock-rock planes a trifle lower. b) When I inspected the failures of the concrete dams of Malpasset (arch, 1959) and Anel de Dom Marco (gravity, 1973), it was noticeable that all the concrete foundation blocks (those of Malpasset transported hundreds of meters downstream) always occurred with some foundation rock adhering. Even in the argillite of Anel de Dom Marco, the cementing effects of the concrete's slurry penetrated to some depth (a few to a dozen centimeters): the weakest plane ceases to be coincident with that of the descriptive or apparent discontinuity (concrete-rock). c) In the investigation galleries of the São Simão dam (1973) the flat planar discontinuity of the black dense basalt on top of the yellow intertrap sandstone (as if "ironed" by the basalt flow) was so salient that many tests were run to investigate it. Once again, the basalt had "cooked" the sandstone down to a few centimeters below the contact, and all the shear failures occurred along sandstone-sandstone.

3.2 Current in Situ Shear Tests in Rocks: Soil-Rock Mechanics Interaction

In a manner similar to what had occurred in Soil Mechanics, the direct shear test in Rock Mechanics started out in utmost simplicity, seeking to maintain σ_n constant and increasing τ to failure. Let us exclude ab limine the cases of unfortunate ignorance in which the test equipment or set-up did not respect applying the horizontal shear force tangent to the plane (or inclined passing through the center of the area to be sheared). The most frequent convenient routine applied an inclined transverse load (at about 20°) ipso facto increasing τ. To minimize numbers of test set-ups and problems of interpretation of series of tests affected by heterogeneities, one routinely adopted the multiple stage testing technique (developed in soil mechanics around 1948, Taylor et al.) of special interest for establishing $\phi = d\tau_f/d\sigma$ along

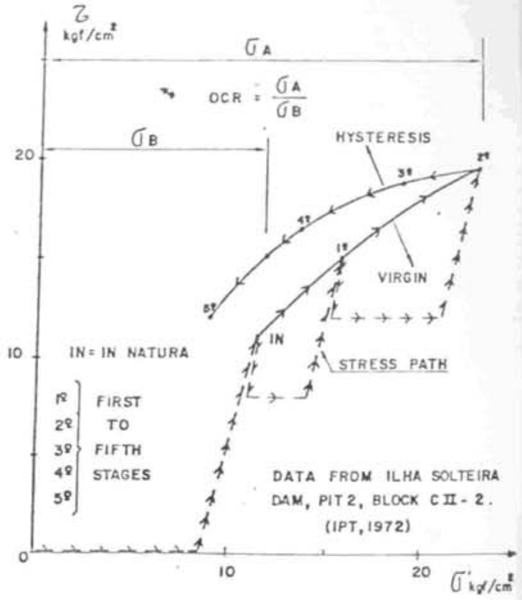


Fig. 4 In situ rock test, OCR effect

the selfsame joint.

Further consideration will be given below to the difference between strain-controlled tests (more frequent in soil mechanics) and the stress-controlled tests (soft load) of more frequent use in rock mechanics because of justified search for equipment simplification if and when facing bigger forces.

Of the many possible links between soil and rock mechanics experimentation the following two are selected for mention.

3.2.1 Precompression effects, observed both in sound rock-rock contacts and in joints weathered into clay.

An early observation in the great number of tests on shear and tension joints in the basalt flows arose when by chance one tested "precompressed conditions" (that is, with overconsolidation ratios OCR > 1.0): the shear resistance reflected a definite increase in comparison with virgin pressure conditions (Fig. 4). This was observed since the earliest series of tests, 1964-73, and orally communicated, being thereupon published by Nieble et al. (1974). Interest in such a fact would be modest were it not for its consistent nature: and this, despite the most frequent strain-softening type of stress-strain curve, which significantly attenuates the direct appearance of the effect. Theory-practice interaction fully supports the observation through association of such strength increment with the hysteresis of compression-decompression. In sound angular rockfills the crushing of point contacts causes a marked hysteresis and the consequent apparent cohesions under OCR > 1. Thus, at the two extremes, that of clean sound cracked rock-to-rock angular contacts, and that of joints weathered into clay (de Mello, 1980a), if the stress path incorporates a precompression OCR > 1, we must be favoured with a resistance higher than that of the routine test which imposes a distinctly unfavourable stress path in comparison with most of the base of the dam, principally upstream.

This effect is not so small as to merit being summarily set aside in design practice; at any rate it must be investigated under scientific-technological curiosity. Fig. 5 indicates the increment of area of shear resistance reaching up to 20% based on the test data, and assuming OCR reaching 2. It is of interest that while increasing uplift pressures affect stability unfavourably, ipso facto the increasing OCR would contribute with a compensating effect.

Any such hypothesis should serve principally to orient immediate testing for confirmation or rebuttal. Some forthcoming in situ tests IN NATURA should be run imitating more realistic stress trajectories of dam prototypes, including an increase of τ simultaneous with the decrease of σ after a certain time of rest under end-of-construction σ. In soil mechanics tests there have been repeated demonstrations of the importance of the time of accomodation under "secondary compression" in increasing the "cohesion component". Fig. 6 shows that the same type of tendency has been proven in rock, which is as one should expect, since the effects of a "break of structure" and crushing are analogous, dependent on time of accomodation.*

In appraisal of resistances to be attributed to natural joints the concern, intuitively comprehensible, has been for investigating as most critical the joints weathered to a clayey condition. Setting aside temporarily the question as to which materials are really effective, we recall that the advantages of OCR in clays are well established. Thus, since at both the extremes, of sound fractured rock and of a clayeyfied joint, we expect perceptible "cohesions", it may be that the cases less benefited by the real stress path in comparison with the routine test be represented by moderately weathered joints (sandy-silty conditions) corresponding to greater contact areas face-to-face, and correspondingly smaller contact stresses.

The multiple-stage shear test has found frequent use, under the sound principle of better defining a $\phi = \partial\tau f/\partial\sigma$ on the self-same specimen and shear surface, and also for minimizing costly specimen and equipment installations for each test. However, there should be some correction applied to the test results of the 2nd, 3rd and 4th stages, in consonance with the stress-strain curves. Fig. 7 illustrates how the subject has been often treated in soil mechanics when one possesses strain-controlled tests (cf. 3.3 below), and it can be seen that under strain-softening conditions one should be benefited by an incremental resistance shematically indicated in Fig. 7c.

3.3 More Realistic Stress Paths for Foundations of Gravity Dams.

Fig. 10 summarizes routine indications under the nominal hypothesis of the statics of a rigid body.

* With due prudence I postulate that the secondary compression or creep time effects may work either favouring or deteriorating (semilog linear) a shear resistance, and the limit for differentiating such tendencies does not seem to have been defined by research to date: possibly under conditions of FS > 1.5 the time effect should be beneficial, whereas for FS < 1.5 the analogous effect could be deteriorating.

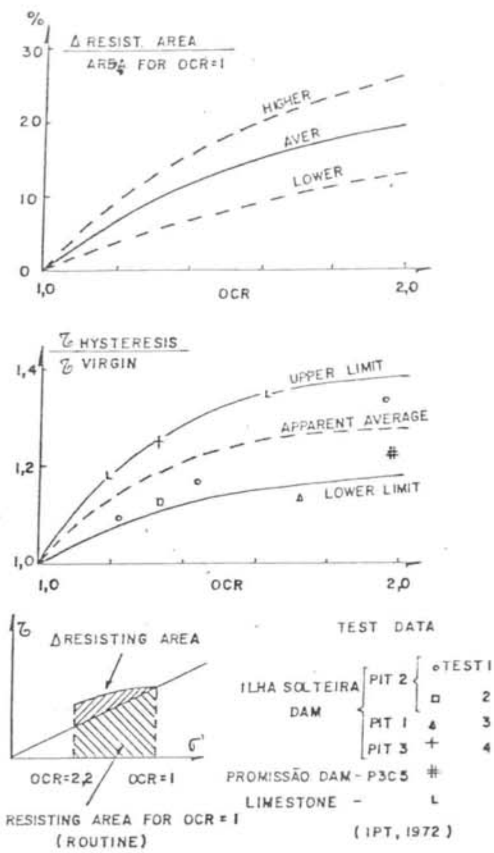


Fig. 5 Some Data on % effect of OCR hysteresis

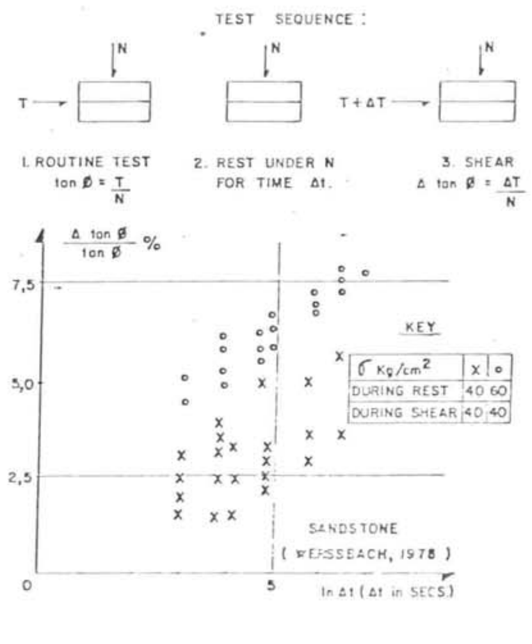


Fig. 6 Time and OCR influences on strength

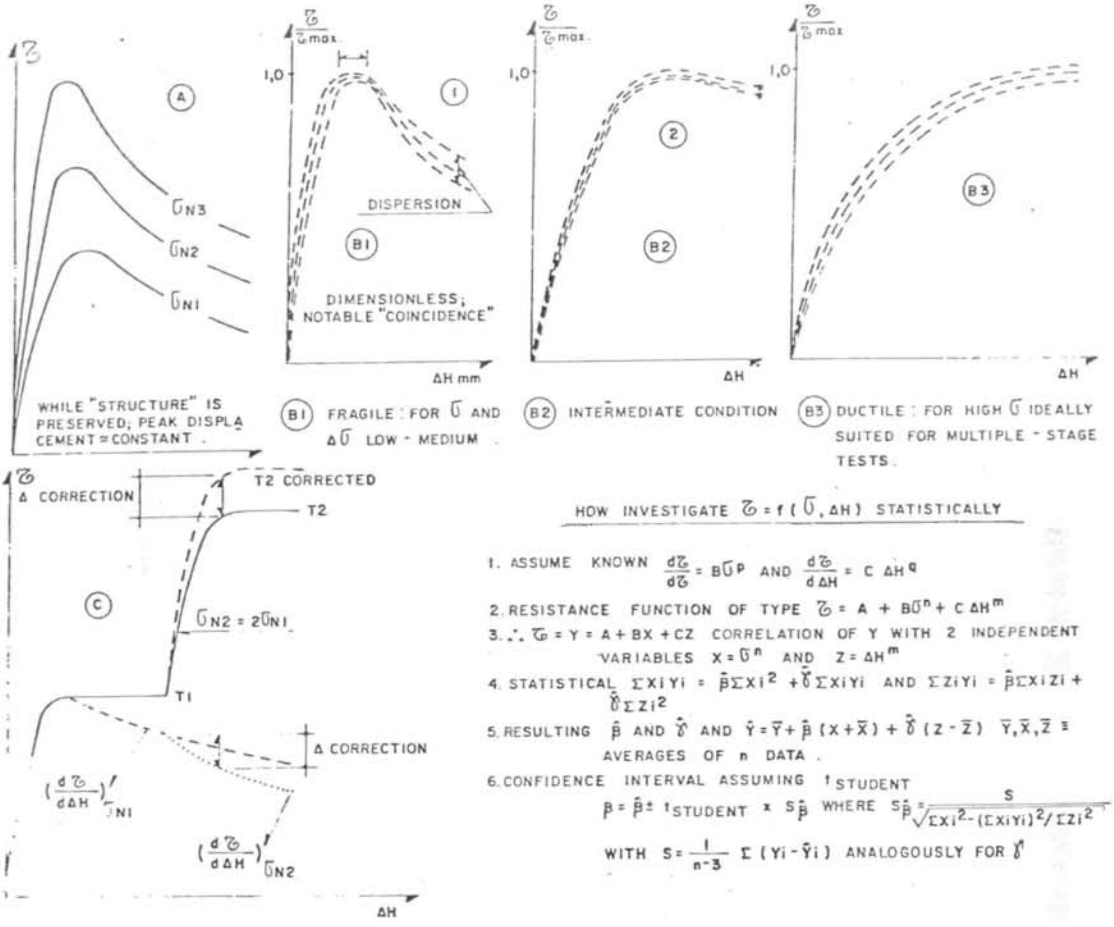


Fig. 7 Schematic corrections on multiple-stage tests

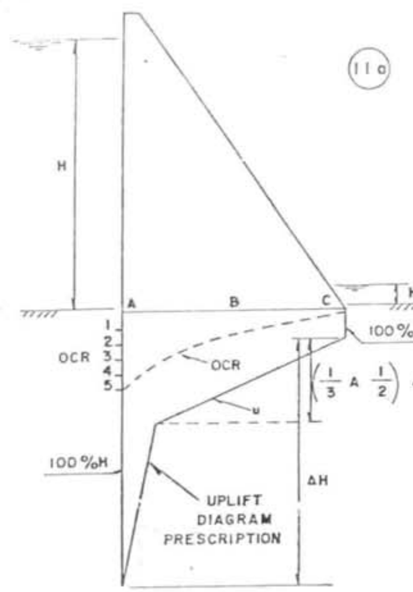
Since the concrete rigid body is much more impervious than the foundation rock, and the latter generally occurs as saturated whereas the concrete does not, then, as the reservoir fills and applies the shear τ the uplift pressures on the base develop simultaneously generating some value of OCR under part of the base. Only in some area under the toe, if we rely on the good drainage generally imposed, we face the unfavourable stress paths without OCR such as pertain to the routine rock mechanics test. Further refinements may be introduced regarding changing seepage and drainage conditions with time, and somewhat different stress distributions within the gravity block and at its base under more realistic conditions of elastic bodies. (Fig. 11)

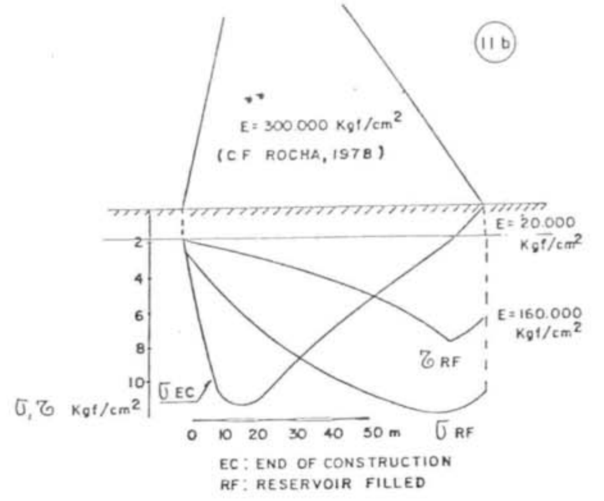
3.4 Stress-control vs. Strain-control Behavior: Test vs. Prototype Comparisons

It is accepted that for the better investigation of materials behaviour the strain-control test is more appropriate, permitting characterization os stress--strain curves as one approaches failure (e.g. FS < 1.5): all the more so, when one intends to control and investigate post-failure conditions, especially in brittle materials (strongly strain-softening). Fig. 8 summarizes the classic information on the topic, including the influence of equipment rigidity on the stress-strain shape.

The tendency is for the stress-control (T) equipment to record a peak value slightly smaller (which may be attenuated by progressing by very small stress increments as one approaches the peak); it also fosters an illusion as to the existence of a "plateau" after failure (non-brittle), simply because the deformation accrues very rapidly, and one cannot record with ease the concomitant drop in stress. Meanwhile the excessive deformation cannot but subject the shear plane to some crushing and polishing, and thereupon the strength differential $\partial\tau_f/\partial\sigma$ for the subsequent test stage suffers a reduction in comparison with a multiple-stage strain--control test (D) in which one is able to stop right after each peak. Yet, even the strain-control stage test itself cannot but evince some loss of strength if compared with a series of identical specimens IN NATURA (IN) under different σ values.

Understandably, practical and economic factors led to Rock Mechanics' in situ testing as stress-control, and with simultaneously increasing σ with τ. Upon such recognition, however, it would have been of interest to alternate the great numbers of in situ tests with some laboratory research testing to decrease the ignorance on applicable weighted correction factors for the routine in situ test results. The question affects not merely the peak





NOTES

1. RIGID BODY STATICS ASSUMED
2. CONSIDERING FLOWNET IN CONCRETE INCREASES OCR DOWNSTREAM.

POINT	σ' END OF CONSTRUCTION	σ' RESERVOIR FILLED (SHORT-TERM)	OCR
A	141.6 tf/m²	27.6 tf/m²	5.1
B	71.1 tf/m²	53.9 tf/m²	1.3
C	0.7 tf/m²	80.1 tf/m²	N.C

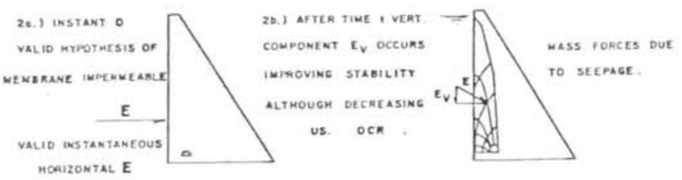


Fig. 11 (a) Estimates of OCR for different loading phases
(b) Example of better adjusted stress distributions on base

The application and applicability of the correction to the friction angle ϕ as a function of the asperities of inclination i (Newland and Allely 1957, Patton 1966, etc.) has been one of the most discussed points of practical rock mechanics as applied to the stability analysis of gravity blocks as compared with slopes etc.

One does not question herein the interference of the average inclination (dip α) of the hypothetical plane (polished) to be analysed under the rigid block routine of statics (Fig. 10). The discussion centers on the asperities of angle i with the average shear plane, presumed polished: in accordance with Patton and subsequent school, the inclination i should be used for correcting the experimental results to obtain the true ϕ; thereafter, in the stability analysis of the prototype one should apply a judicious composition of the ϕ value with the i value judged to be applicable.

Having run a shear test it is easy to open (in tension) the two faces of the sheared plane, and to measure angles i if the asperities have not been too much impaired by crushing; and, assuming a uniform i it is easy to apply the proposed correction to the ϕ apparent obtaining $\phi = \phi_{app}{}^{-i}$. The thesis is crystalline in "engineering science", and applicable when assuming uniform asperities (in inclinations and crushing resistances). However, I submit that in practice this geometric-physical idealization set aside earlier dilatancy/compressibility corrections by the work equation (Taylor 1948, Rowe 1964, etc.), and led to a dead--end alley. One must distinguish between what is useful and valid for academic conditions and a-posteriori analyses, and what the engineer can apply with reasonable confidence as a basis for design, prior to failure (since such failure is what must be averted ab limine).

I submit thus that in my experience the orientation of the i correction turned out to be unfortunate for professional practice. The following are some of the principal doubts to which we have been subjected:

a) In the observed values of ϕ_{ap} we absolutely do not know what partial contributions to assign to cohesions, whether cementitious, whether pertaining to secondary compression of millenial interlocking. The references generally considered non-cohesive contacts.

Setting aside the scientific comprehension of phenomena, what benefit is there to engineering in resorting to solutions based on intrinsic, internal behavior, only determinable a posteriori, after failure, in comparison with an approach based on observing the composite effects of dilatancy//compressibility as exteriorated (since the very beginning of the shear stressing), and on applying the corresponding correction in function of the data as observable in an analogous manner both in the pre-design tests and in the project monitor-

IN resistance values and the strength equation to be adopted, but also the use of more judicious FS values, which depend greatly on the nature of the stress-strain curve (brittle vs. ductile etc.).

Setting aside for the present the "corrections" of the resistance (observed) through the work equation, in the search for "intrinsic" shear strengths expurgated of effects of compressibilities and dilatancies, one recognizes that τf must be defined as a function at least of σ (normal stress) and ΔH (shear displacement), $\tau f = f(\sigma, \Delta H)$, through two partial differentials $\partial \tau f/\partial \sigma$ with ΔH constant, and $\partial \tau f/\partial H$ with σ constant, in the manner suggested in Fig. 7 (for statistical regression).

3.5 Corrections for "Asperities" (roughness) in Accordance with Patton 1966 et al. (many others)

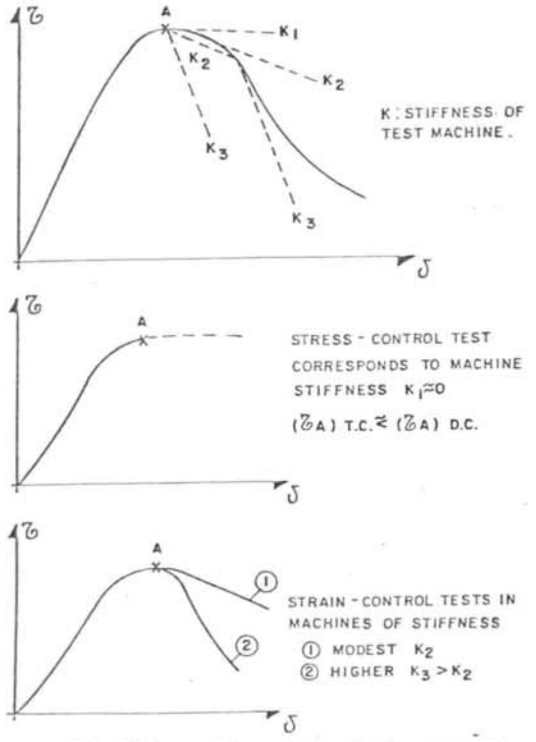


Fig. 8 Comparative stress-strain curves, stress vs. strain control

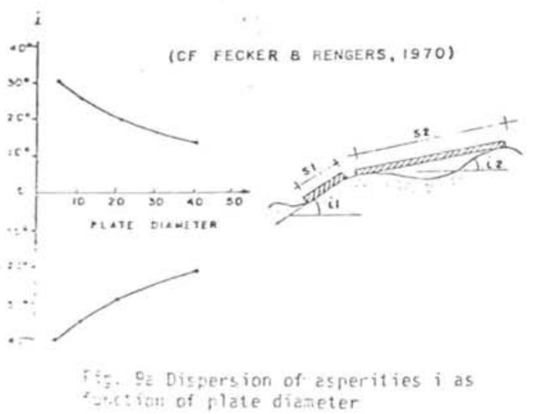


Fig. 9a Dispersion of asperities i as function of plate diameter

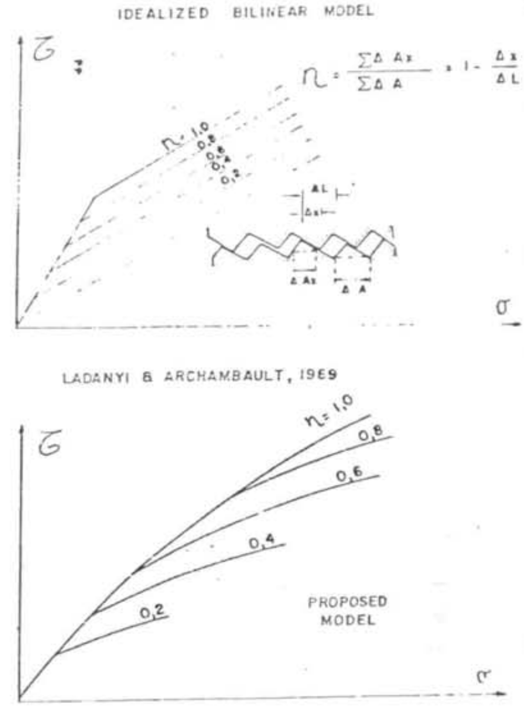


Fig. 9b Idealized influence of interlocking on strength envelope

CASE	(a)	(b)			(c)		
			α=10°	α=20°		α=10°	α=20°
NORMAL	V	Vbn+Hbn	1,13V	1,29V	Vcn-Hcn	0,95V	0,87V
SHEAR	H	Hbt-Vbt	0,87H	0,73H	Hct+Vct	1,36H	1,73H

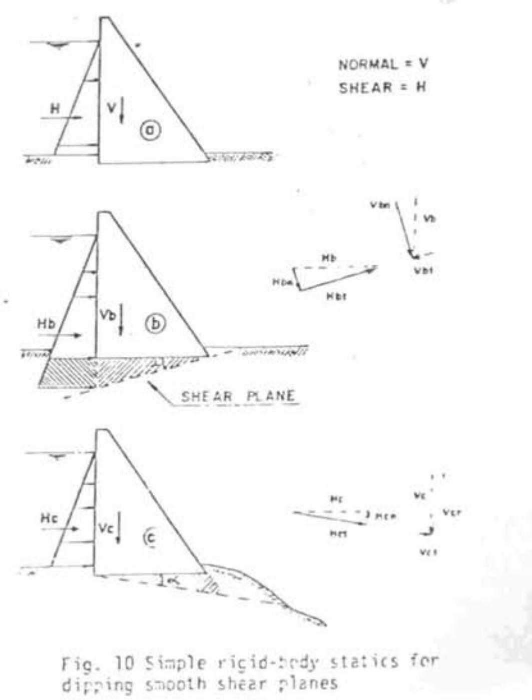


Fig. 10 Simple rigid-body statics for dipping smooth shear planes

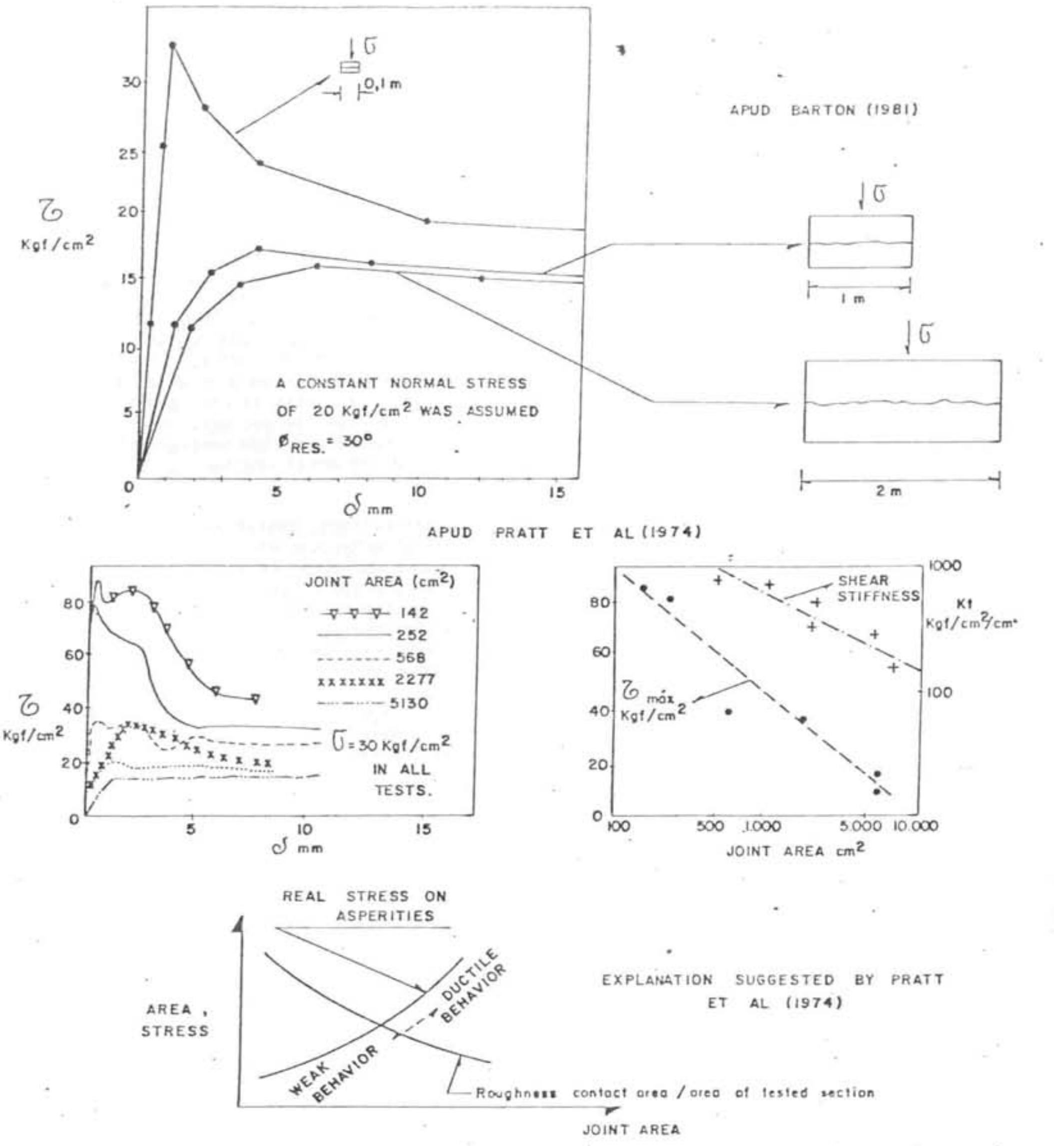


Fig. 12 Test data on influence of area on peak resistance

ing? There may well result an unfavourable influence if a misunderstood academic attitude be presumed to substitute, and not to complement, the approach more amenable to professional practice.

b) In the shear test itself analysed a posteriori, there is a full histogram of i values of asperities, another histogram of degree of contact face-to-face of the different asperities, another one of conditions of riding-over, another one of resistances to crushing, and so on: which should then be the values of i to be considered as effective? Obviously they should vary with σ: yet in each test subjected to multiple failures they have been taken as nominal constants of the specimen. There are cases when one observes a dilatation (external) but (erroneously) continue to subtract the i of asperities visually measured for σ = 0 after the specimen has been opened.

Many are the problems in reality in comparison with the idealization (Fig. 9a).

Theorization based on uniform asperities i, perfectly snug simultaneous contacts across faces, saw peaks of identical shape and unbreakable, etc., inevitably demonstrates the equivalence of the correction through energy balance with the geometric $(E = \infty)$ method of Patton et al (1966) (Fig. 9b).

c) The application of the correction (either by work equation, or by geometry) lowers the resistance up to the peak, but assuming a drop of stress-strain and a compression, after transposing the peak the τ_{app} should be corrected upwards (for the τ_{ult} and τ_{res}). However in the routine test the interferences of $\Delta\sigma$ introduce errors. One method of correction sometimes adopted has been by calculating nominal i values based on concomitant vertical ΔV and horizontal ΔH deformations: thus $i = \tan^{-1} \Delta V/\Delta H$ would be calculated and either subtracted from ϕapp when $\Delta V = +$ ve, dilatant, or added to it when $\Delta V =$ ve, compressive. Needless to mention, in order to correct for the interference of $\Delta\sigma$, one should have obtained some data of ΔV vs. $\Delta\sigma$ (compressibility under normal stress) in order to subtract it from observed ΔV and obtain the remaining ΔV related to shear. Such corrections based a corrected nominal i are clearly more acceptable than the geometric ones based on profilometering.

In short, independently of the results it seems that in professional practice it proves preferable to apply corrections based on the work equation in consonance with meticulous observations of deformations during shear, applicable analogously in tests and in prototypes. The academic (synthetic) approach is useful for estimating when no tests can be sought, but geologic and geomechanical estimates of asperities are made available.

One might yet speculate on the context behind the proposition by Deere, Patton, Barton etc., in comparison with conditions that obtain in gravity dams. Unquestionably one needs to reach a ϕvalue expurgated of i when it might be necessary to substitute one value of i by another. In the cases of very big masses (slopes) subject to large displacements and deformations (meters to several meters) there are situations in which one should subtract the effect of asperities of second order (millimetric) and include, in substitution, those of first order (decimeters or more). It seems that at the origin of the propositions of the authors mentioned the problem of stability of slopes was generally implicit. In the case of gravity dams the displacements predicted and "accepted" are of the same order of magnitude (submillimetric to mms) as those of the in situ shear tests. The asperities to be considered in any situation bear relation to the shear displacement, and not to the dimensions of the shear plane: influences of dimensions of planes (Fig. 12) and model-prototype scales only have to do with probabilities of occurrence of significant and/or conditioning extreme vs. average-quality asperities.

3.6 Frequent Inapplicability of ϕ Residual (or even less) on Clayey Joints: Clayeyfied Joints vs. Clay Infillings.

A concept that has frequently led to the adoption of strength parameters excessively lowered arose from laboratory test-confirmation (Kanji 1974, apud Deere etc.) that clay in-fillings (remolded, plastic) consolidated between two rigid faces (of rocks, polished or with synthetic roughnesses) give a low ϕ, analogous to ϕres. or even lower, presumably because the platy clay particles are flattened parallel to the shear plane. Set aside the concomitant indication that such behavior is a function of Ip, obviously to be reappraised in function of at least the pair of indices (W_L, Ip).

There is nothing more difficult in civil engineering professional practice than to face in prototypes, of serious responsibility, a pseudo-truth generated in the laboratory; nothing supports us but the "mental tests" which suggest the inapplicability of the results in reality. Valid in their own specific conditions such results "only" impose more conservative (and expensive) project solutions: the eventual lack of SAFETY is a factor still difficult to quantify and confirm, but has a justified impact of absolute taboo.

The mental test refers to two aspects: a) the postulation of the geologic history of the joint and its weathering; b) the visualization of which contact areas do effectively work and will so continue under the incremental stresses.

I repeat that it seems to me inexorable that in the conditions of very broad histograms of stresses and rigidities really effective, and the really small strains at play, what should prevail is the principle of equivalent strains, strains made uniform by stress redistributions.

On a subhorizontal joint in rock, if certain areas were permitted to weather into a soft clayey condition, we must reason that on such areas the effective stress must have a very low value, whereupon, for the static equilibrium of the overlying weight, the rock-rock contacts remain under high contact stresses. The Soil Mechanics assertion that a soft clay can only be associated with a low effective stress is undisputable: what remains to be affirmed, by geology, is that a sound rock "bridge" under high intergranular stresses cannot suffer chemical weathering to the softened clayey state.

What matters, therefore, is to investigate geomechanical behaviors of such point contacts under pressures and pressure increments very much higher than the average values computed from overburden weight. The clayey area will only be brought into action under the proviso that stresses distribute to the rigid and compressible areas respecting the equivalence of deformations (de Mello, 1972).

Big projects of responsibility cannot adopt such intuitive reasonings without the supporting information and confirmation. The intuition must be enunciated, however, in order to orient purposeful programs of testing and instrumentation-observation. It is in the concrete works of hydroelectric projects that lie the biggest costs and biggest probabilities of savings. In all cases (especially the Água Vermelha Dam with a wide-open subhorizontal crack of several mms even to cms) observed deformations (of compression and shear) were insignificant (Fig. 1, 2).

The weathered-in-situ clay that results in a joint (mineralogic expansions etc..) can only have a "clay structure" of well "flocculated" particles, using terminologies of Lambe 1958 somewhat revised more recently by Mitchell. Moreover, because of the differentiated mineralogies in the parent rock components, the residual clay cannot possibly occur in the homogeneous plastic condition of a clay-mineral, laminated and "dispersed" (apud Lambe). Recognizedly however if the dam's shear displacements reach some mms (thousands times the clay particles) the clay platelets should become aligned, due to the enormously different moduli of the rock faces and the sandwiched clay.

Thus, if we do deal with a clayey thickness participating in the shear, the decision on the applicability of lowered ϕ' because of clay alignment (indicated by the strain-softening curve) it is indispensable to rely on expert advice from a geologist, and from micro-structural analyses of the clayey material, in order to establish the probable history of shear displacements (Fig. 13). On the other hand, regarding the job itself it is important that the civil engineer predict some range of shear displacements to be considered (cf. Figs. 1, 2). This important parameter is not a fixed behavioral property, but depends on structure-joint interaction taking into account best

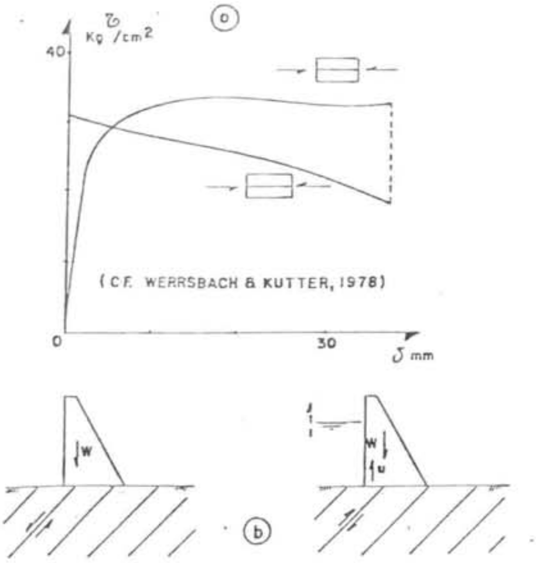


Fig. 13 a) Inversion of shear direction.
b) Schematic example for dam.

estimates of geology-joint interaction.

I mention in passing the weight carried by terminology in conveying implicit concepts. It is unfortunate that the term "clayey in-filling" has been applied to all joints: "implicitly one envisions a fixed container, and a filling by entry and depositon. There might be some such joints: the Kanji 1974 tests might well adapt to such cases. However, most joints would merit being called clayeyfied weathered joint" or "siltified weathered joint". Their contents might even be removed, even by the dreaded term "piping", but obviously with no engineering consequence at all.

4. SHEAR STRENGTH EQUATION TO BE USED

Some problems have been intensely discussed over the past 20 years, without any tangible learnings (cf. INSTRUMENTATION below). Briefly considered:

1) applicability of a strength equation realistically closer to the maxima, to averages, or to the minima (within the statistical dispersions);

2) resistances at peaks, or at given deformations (e.g. 5mm or 10mm) or even corresponding to the conventional "ultimate" or "residual" values;

3) influence of comparative areas, test vs. prototype, and comparisons of "asperities" at the scales of test vs. gravity-dam base (already discussed).

[illegible] lack of knowledge on the topic has been systematically camouflaged under discussions on the [illegible]itant interference (obvious) of Factors of Safety. For minimizing confusion on the topic, we [illegible]arily set aside the question of "judicious [illegible] of Factors of Safety (and of Igno-[illegible]

[illegible] that depending on different prevailing [illegible] it may be more logical to employ either [illegible] above the statistical average, or the averages, or even those closer to the lower bound of confidence bands. This should be principally dependent on the history of development of the rock and its joint, and on the relative rigidity of the gravity-block vs. the shear deformability of the joint. It is, once again, a crucial structure--foundation interaction problem.

For employing statistics and probabilities in such topics we cannot exclude assessing first the physical model implicit in the statistical universe. The first question refers to the influence of areas and/or dimensions (of the bodies and/or the dis-placements).

Fig. 12 reproduces some research results on the topic. At first sight such data conclude in favour of undisputable reduction of the resistances (peak), concomitant with an increase of displacements to reach the peak. The autors explain that the sheared surfaces opened-up a after failure revealed contacts only on 10-20% of the area, and the very high contact stresses would be responsible (by a shift in the position of the Mohr envelope of $\partial\tau f/\partial\sigma = A\Delta H^n$). Thus they affirm "There would probably be no size effect if the contact areas of 'large' and 'small' specimens were the sam". I agree, and submit that this important problem hinges immensely on judicious judgment, associated with three index behaviors related to "plastification", that is, the change from brittle to ductile behavior:- a) curvature of the shear strength envelope; b) increase of deformation to the peak with increasing σ; c) the differente between the peak vs. residual strengths (Bishop's brittleness index) or as proposed to be hopefully better, the differences in slopes $\partial\tau/\partial\Delta H$ before vs. after the peak in stress-strain curves (de Mello, 1977).

Idealized theorization of mathematical-probabilistic safety of structures Ferry Borges,(1954, 1971) suggests for the "ruin of groups" a distinction between types of groups: brittle, strong, fibrous, ductile; the ones of closer interest to ourselves are the brittle and ductile. Further, in the theory of similitude one must compare similitude conditions for brittle and ductile failures, and also for "failure by deformation". In summary we transcribe: a) in failure by deformation (which would hardly apply to the majority of dams under discussion) the average values stay constant irrespective of scale variations; b) in brittle failure, average values decrease as dimensions increase; c) in ductile failure, if there is a single sliding surface, the average value for the prototype will be slightly smaller than for the model: however, if there are several shear sufaces, the average strength for the prototype is higher (a hypothesis that although admissible is generally excluded by prudence).

In case of brittle failure a considerable number of tests are needed to arrive at averages despite the dispersions: averages are the only behavior applicable, imposed by the "rigid block" hypothesized, relative to the joint sufficiently weak to be suspect. What is, indeed, the possibility of obtaining and maintaining brittle failure, except in a great number of relatively sound contacts, that is, under moderate contact stresses ?

Generally we thus conclude both for cases b) and c) that the applicable values should be "slightly smaller" in the prototype. However, when facing submillimetric deformations, a base dimension of 1m would already be sufficiently analogous (relatively speaking) to those of prototypes, bases of dams.

In summary, for all three questions 1, 2, 3, I propose that:-

A) Regarding applicability of some resistance of the residual type, the starting appraisal must be based on the microstructure, postulated geologic history of deformations both old and relatively recent. As frequently occurs in Brazil relatively old millimetric deformations are encountered fully healed by limonitic cementations: thus a microstructural geologic interpretation cannot preclude a concomitant interpretation of the stress-strain curve.

B) My personal experience has not yet encountered the case suggesting the need to abandon the "peak resistances" of IN tests, in favour of ultimate or residual types of resistances.

C)I would hardly question applying the "averages" of various tests for the strength equations. The very tests generally tend to be run on samples somewhat more suspect, since seldom or never does one have the possibility of as many tests as would represent the full histogram.

D) It would not seem valid to apply a tangible reduction because of model-prototype scales, for direct shear on single weak planes.

E) It should be important to recheck the "nominal stability", with a reduced Factor of Safety (e.g. 20% or 30% lower) assuming eventual development of displacements of 5mm and 10mm. The special purpose is to verify the increment of behavior $\partial\tau/\partial\Delta H$ after the initial stability has been computed as guaranteed.

The concepts on Factors of Safety and incremental behavior checks are further discussed below.

5. UPLIFT PRESSURES. CRITERIA AND EFFECTIVE AREAS

5.1 Concrete-rock Base

At present (since 1952 ±) the concrete-rock base is undisputably recognized as "impervious" (ratio of permeabilities between about 10^{-10} cm/sec for concrete and about 10^{-4}cm/sec for the top of jointed rock), and therefore it cannot but receive the uplift pressure on the total area, I = 1.00. It is a direct consequence of pervious-impervious interaction. Therefore when one resorts to "design criteria" of earlier projects (e.g. Criteria of USBR, TVA, U.S. Corps of Engineers etc..) one should necessarily adjust for the fact that some considered hypothetical lower I values, as is expatiated in high level discussions through the period 1898--1940 (cf. Serafim, 1954). In considering other planes of weakness, rock-rock, the situation is not analogous (cf. item 5.2).

Uplift diagrams to be applied have been very much discussed. It is a very significant problem of structure-foundation interaction. In our gravity dams it has been automatic to incorporate an upstream gallery, a line of groutholes upstream and some appropriate drainage downstream. In almost all projects, irrespective of specific local geology there has been the row of subvertical drainage holes from the downstream face of the gallery, and the same conventional diagram of two straight lines (Fig. 11a) has been used, associated to the hypothetical vertical tension crack along the dam's upstream face (an absurd hypothesis except in cases of special geology and dams of significant height relative to the foundation rock quality). Although the top of rock is often much damaged by blasting, the cleanup by high-pressure air-jetting guarantees that the insitu initial horizontal stresses across vertical faces of rock blocks are significantly compressive (or cemented): thus the accepted generation of an increment of tensile stress does not necessarily lead the joint to a cracked condition (lateral stress reduced to zero, over increasing depth).

The absurdity is that a profession that insists on the fact that each case is different, to be examined on its distinct conditions, and wherein the use of simplified routines can only be damaging professionally and commercially, one should have to denounce how routinely a conventional uplift diagram becomes a dogma, generally leading to systematically conservative uneconomic designs. And the curious fact is that by merely applying oneself to a more detained analysis of the very references claimed to support the would-be dogma, one finds all of them agreeing in basic principles.

Phraseology is not homogeneous but consensus is:
a) in most of our jointed rocks, much more jointed near surface, the use of a single-line (eventually confined, locally, by lateral lines) of grouting upstream represents an exploratory treatment of correction, self-testing-and-confirmatory, to be considered a starting step; its principal result is to exclude wide-open cracks (conditions of statistics of extremes, de Mello 1977), by fully sealing them (as well shown by the two piezometers in Itaipu Dam, Fig. 1). It is sufficient to permit applying to the treated rock band (of highly irregular widths) the nominal flownets of head losses. In the downstream foundation rock any wide-open cracks are of no concern, but rather beneficial, as natural drains.

b) the appropriate drainage (designed) downstream represents the indispensable factor for guarantee of stability; control of uplift cannot be by modest reduction (to about one-tenth) of average permeability of grout buffer, since the concrete-rock comparison is of about 10^7; it must be by drainage (points wherein hydraulic head is reduced to zero or, at most, to the velocity head $v^2/2g$ if outflow is significant, reducible by complementary drainage and/or grouting.

c) the grouted buffer zone (avoiding the misnomer "curtains" that generated erroneous implicit concepts) principally homogenizes permeability by fully clogging the wider open cracks in which grout travels farther (tens of meters) and impermeabilization is absolute. The exploratory grout holes permit eliminating erratic conditions of geology, to some extent uninvestigatable but accepted as existing: design principles that one can only design for problems definable and quantifiable oblige us to exclude the erratically wide cracks. It is the drainage (based on flownets) that guarantees the design, but it is the (single-line) grougning that establishes conditions of statistics of averages validating Darcy-Laplace flownets. The very important design tool of grouting is a treatment-foundation interaction problem.

In a separate paper I have discussed grouting criteria routinely applied, and demonstrated that requirements and acceptance criteria have been overly stringent. The use of benefit/cost evaluations reveals that after completion of a good first line, the incremental benefits vs. incremental costs on grouting treatments drop vertically.

Once it has been accepted that (after grouting) the foundation seepage conditions are definable by flownets, the question to be put is why not design (calculate) the uplift drainage features (downstream) for a given residual value of hydrodynamic pressures, as a design feature meriting technical-economic optimization just as much as any other project feature ? Why find oneself bound to design PRESCRIPTIONS (belonging to the 1930's and, by the way, questionably digested) in this one feature that is the fundamental weapon, effective and trustworthy, of design for stability-safety ?

Some major projects in which downstream heads were almost as high as the upstream ones, situations not covered by the Prescriptions, but which have become frequent because hydro power optimization is presently recognized to occur not with maximized differential head but when there is considerable downstream submergence, forced us to reconsider the entire question and to use galleries for grouting and drainage both upstream and downstream. Thereupon it became quite clear that the "recommended uplift diagrams" merely derived from the basic principle that for any and every condition the need is to draw an appropriate presumed flownet, and definitely not the adoption of a conventional uplift diagram.

Curiously one did nought but return to the essence of recomendations of all, Casagrande, USBR, TVA, Corps of Engineers, Serafim, etc.

The persistent barriers to such a crystalline design principle have been: 1) fear of setting aside the dogmatized criteria, and especially that of so-called "inoperative drains"; 2) lack of confidence in the practical design weapon represented by the drains, although they can be easily complemented as a function of the very observations of behavior; 3) alleged lack of confidence in the durability of drains (TIME INTERACTION), and so also in the competence and sense of responsibility of operation and maintenance technical staff (SHAMEFUL LACK OF INTERACTION BETWEEN COLLEAGUES WITHIN A PROFESSION); 4) doubtless also, a lack of intimacy of some engineering geologists and structural engineers intervening in the gravity dam, with the simplicity of drawing flownets.

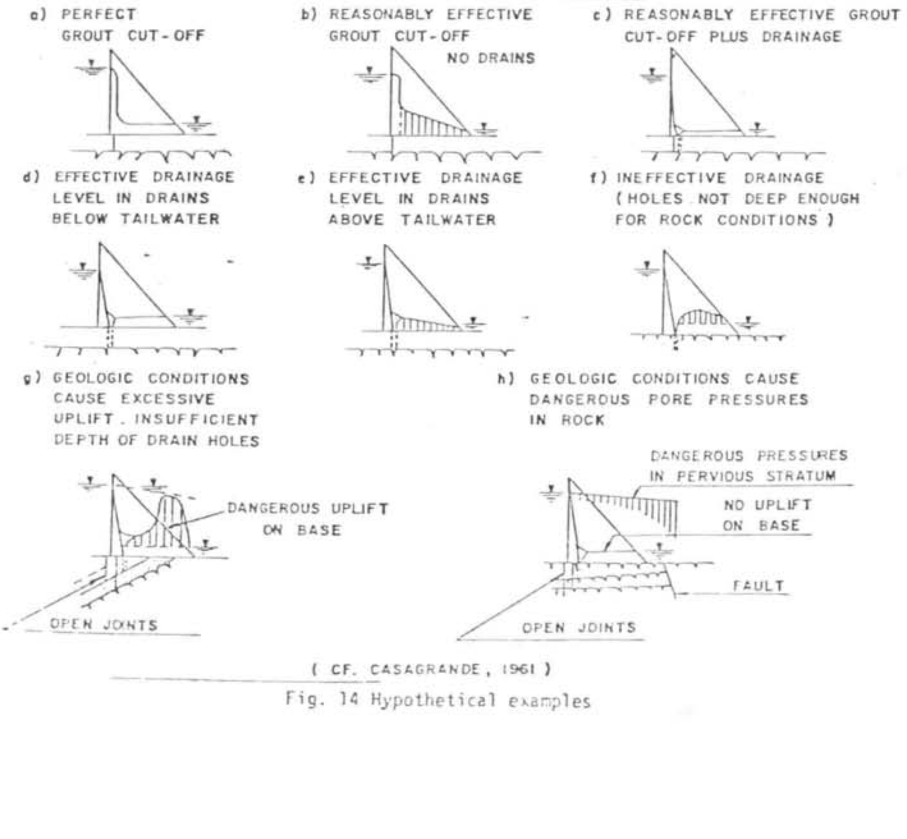
Some detailed examination of the few basic references is offered for obviating unwarrantedly uneconomic misunderstood design criteria.

5.1. Drainage taken to signify subvertical holes

Casagrande's Rankine Lecture 1961 is judged to have exercised great influence. It seems, however, that under a quirk of misinterpretation it may have caused "appropriate drainage" to be taken to signify a row of subvertical (deep) holes, completely setting aside subhorizontal drainage near the concrete-rock surface. Such a stereotype flagrantly contradicts the concept of adjustment to greatly varied geology, eloquently expressed in the shcematic figures (Fig. 14).

A single phrase was found in which he foregoes mentioning the "line of drainage wells" whenever he refers to drainage: (p.164) "because replacement of deep penetrating wells b y a surface drain, and assumption that the rock is of uniform permeability are excessive deviations fromthhe usual conditions .." one should not expect close agreement with Brahtz's (1936) theoretical solutions. Incidentally, one fully agrees with the statement, especially considering that the only horizontal contact drain vonsidered by Brahtz was parallel to the dam axis.

But in the field of gravity dams the use of horizontal contact drains (ahlf-rounds) was mentioned since decades before. In Wegmann, 1918, we find reference to Vyrnwy Dam (41m) in Wales in which a system of drains is constructed in the foundation to relieve the base of the dam from the upward pressure....a complete system of drains....lie on the rock near such places where leakage is



(CF. CASAGRANDE, 1961)

Fig. 14 Hypothetical examples

apt to occur....". Books by civil engineers who did not claim any specialization, geotechnical or other, but were design and project engineers of big hydraulic projects, have repeatedly mentioned surface drains set directly on the rock surface to control uplift. It is significant that in the lighter structures (spillway slabs, stilling basins, etc.) where failure due to uplift has occurred more often than suspected, any lack of half-round drains (in design drawings or in construction) is promptly corrected as a serious mistake. The ASCE Subcommittee on Uplift in Mansory Dams (1952) starts by listing reasons why "many existing structures, designed without any allowance for uplift - perhaps without any conception of it - have been for many years and are still, functioning satisfactorily", and the main one is the natural drainage by the jointed rock. The Subcommittee analysed 42 dams (8 USPR, 5 TVA, 20 US Corps of Engineers) and mentions "In addition to the grouted cutoff an additional provision in many American dams is a system of drainage holes". Referring to Swedish and Scottish experience of "a wide buttress-type dam ...in this type drains can readily be installed between the buttresses and a very complete drainage of the entire foundation can be secured. The same result can be obtained by a system of galleries transverse to the axis of solid dams with drains drilled in the floor thereof. Subsurface conditions may make such drainage advisable". Some references to the objections of Swedes and Scots to the drains would lead to the presumption that they might be due to the danger of sealing of the drains by freezing.

One might conjecture why Casagrande on the basis of the USBR and TVA references would have implicitly excluded horizontal surface drains. Meanwhile some dam engineers have been conditioned to obviating such horizontal drains under a reasoning that the slurry infiltration from fresh concrete renders impervious an upper lining of cracked rock (accepted as proven): thereupon the surface drains would be wasted.

Well, one should remind oneself that the critical plane is neither at the concrete-rock infinitesimal discontinuity (cf. 3.1) nor necessarily some decimeters below, along a rock-rock weakness plane just beyond such cementing improvement. The critical plane depends on the composite effect of uplift pressures, shear strengths, and presumed statics: it is, as most cases are, a multiple-interaction problem. In analogy we cite the pile adhesion problem, wherein extracted piles come out with some adhering soil thickness determined by appropriate minimization of the composite statics.

At any rate, I repeat that Design is Decision, and Decision is conditioned by Desire. To guarantee the desired control of uplift, the decision should be to eliminate the problem where it would occur most significantly. One must remember especially the meticulous geologic mapping that is carried out at the surface in preparation of concreting. For the flownet there is no difference in setting the "impervious" boundary 0.5 or 2m lower than the concrete-rock surface. The desire to drain the rock below the slurry-infiltrated lining can be easily met by drilling lines of 3-4m percussion holes under the half-round drains.

Incidentally, most holes installed for uplift measurements were precisely such shallow percussion holes, and since/if they were installed at points of surface-evidenced suspicion, should be reflecting the pessimistic, conservative part of the histogram of uplift conditions prevailing under the concrete.

In short, in "homogeneous" geologic conditions everybody should favour subhorizontal surface drains because of the concrete-rock discontinuity of permeabilities. A designer does not agree with the attitude of dispensing with something conceptually important, merely under the negative premisse that construction defects would impair the feature's efficiency: the further design obligation would be to exclude or attenuate the construction defect, or to compensate for it.

Serafim, Casagrande, and all, emphasize the design of foundation treatments by use of flownets. Fig. 14 (Casagrande 1961) should be obvious but is ever needed for emphatic reminding that flownets are conditioned dominantly by boundary conditions and the geologic features contained within the flownet boundaries, such conditions being specific to each site. How could there be any logic, under such irrefutable reasonings, to establishing and discussing "generalized" uplift diagrams except for pre-feasibility estimates ? How shocking that a "criterion" claimed to be extreme,conservative, of "drains inoperative" (cf. 5.1.4) be represented by uplift linearly decreasing from heel to toe, when such a geologic condition as that of Fig. 14g would be extraordinarily worse ? Recognition of this extreme case goes back to the 1890's since Kiel and later Lieckfeldt (1898) warned of it (cf. Serafim, 1954).

If we refer to the Malpasset dam failure and one of the explanations of it, we should emphasize that in some rocks the sutructure-foundation interaction can somewhat "seal" the downstream zone due to increased compressions. For such cases the upstream subvertical drainage holes would be a good first line of drainage, and the complement could be sought through deepening the percussion holes under the half-round subhorizontal drains.

5.1.2 Hypothesis of a vertical crack at the dam heel.

A simplifying hypothesis (for the mathematical analysis of relief wells, Muskat 1937) of a vertical face down through the rock subject to full upstream head was stated by Casagrande 1961 to be reasonable and has in many parts been taken as a dogmatic criterion.

Casagrande merely intended to limit the scope of his analyses, intending to compare "grout curtains" (vertical) vs. vertical drainage holes. One must evaluate such a hypothesis case by case, and probably exclude it in most cases. It is often mentioned that the rock surface would find itself fully cracked, therefore unable to resist tensile stresses. However, such surfaces are thoroughly cleaned by high pressure (water and) air jets: in general (despite considerable variation from job to job) a rock that resists displacement by such air jetting has a high enough horizontal stress across the subhorizontal joint right from the top. We have been misled to reason as in soil mechanics as if horizontal stresses across the subvertical joint are zero close to the surface. For a crude estimate of the horizontal compressive stress I submit my visual observation that such air jets have been found capable of rapidly disintegrating a massive hard clay or argillite (with no joints) of unconfined compression strengths of the order of 10-20 kg/cm^2. The dam's loading produces a tensile increment, but the final stress might often yet remain compressive. Once again, it is an important structure-foundation interaction which under insufficient information has been assumed over-conservatively.

5.1.3 Uplift diagrams suggested by USBR, TVA, etc.

Such diagrams could only refer to the interface, the boundary condition for the flownet: therefore, in concept they could never presume to impose a standardization, since it is the prerogative of the Engineer, design and construction, to impose the boundary conditions desired for his specific case. Moreover, in some of the uplift diagrams suggested although there was full upstream head accepted at the heel, this was associated with I<1.00(2/3 or 1/2 for instance): therefore upon correcting to I=1.00 (as one must) one must recognize that the equivalent head would have been lower.

The observations at the time were not many, and appear to have been often chosen to be on the conservative side. The USBR Tech. Mem 636, 1948, refers to the practice then current "Prior to construction of a dam, the uplift pipe system is laid out ..... at locations where high pressures might be expected.... by a study of the geological characteristics." Keener, 1951, emphasizes "For instance, hole H, Fig. 2 was located within a large fault area" (Hungry Horse Dam, USBR) explaining "Naturally, the general purpose in locating uplift pressure holes is to cover ...the areas...of possible geologic weakness where the uplift pressures would be expected to be greatest". A total number of 360 holes (up to 1950) in 15 dams, deterministically located (good prudence) in critical points: under what logic could one extract "average conclusions" (or confidence bands thereof) for design prescriptions ?

For drawing flownets the difficulty has been recognized of establishing boundary conditions (e.g. indications of "creep ratio" of Lane etc.. for concrete or masonry dams supported on soils): therefore the recourse to indications from monitored observations. It is indispensable, however, to delve into the analysis and recognition of what was really being observed.

The principal criteria used in most areas of the developing world (South America included) derived from the cited USBR and TVA writings in spite of the moderate confusion of concepts that still prevailed into the 1950-54 period. The USBR (1951) Design Criteria suggest two methods, presumably for preliminary design and final design. For the first the simplest triangular uplift diagram is given but "A further assumption is made that these pressures are applied over a fraction of the area ...Design values of 1/2 or 2/3 are generally considered as satisfactory fractions of horizontal area subjected to uplift pressures, depending on the type and structure of the foundation material". For the final design, "By the second method, it is assumed ...the dams as impervious and ...steady state of flow exists. Due account is taken of grout curtains, or other cutoff walls, and any drains that may exist": thus one concludes that I=1.00 and seepage flownet uplifts are duly analysed. Meanwhile in the Engineering Monograph nº 19, Kirn 1953, for preliminary design the diagram is already given crediting the line of drains with reducing 2/3 the upstream-to-downstream head differential, and I=1.00 is recognized. Meanwhile for final design "the pressure intensity at the line of drains should be based on electrical analogy or other comparable method of analysis, assuming that the drains are operative, that the grout curtain does not affect.. ...significantly, and that the pressures act over 100% of the area".

In analysing the TVA, 1952, prescriptions we should recall the often very difficult geologic conditions, including karsts etc. "For the low heads... The uplift...will vary from that of headwater at the upstream face...to that of tailwater at the drainage gallery or line of drains. From this point downstream, uplifts will have the intensity of tailwater pressure. All pressures....effective on ....two-thirds of the area of the base..." On the higher dams ...a somewhat more conservative assumption...The...uplift...assumed to vary from headwater pressure at the face of the dam to one-half of the difference between tailwater and headwater at the line of drainage, and then to vary to tailwater pressure at the downstream face ...The same effective area of two-thirds of the gross area was retained. The validity of this assumption as to intensity has been demonstrated by observation at four of the dams in question, Hiwassee, Cherokee, Douglas and Fontana".

The erratic and inconsistent points of the above basic references, blindly followed without due cross-examination, are merely cited with the hope that discussion be enlightening and constructive. How many of the dams built in the U.S.A. in the 1926-52 period above mentioned would have been recalculated under the varying criteria as the subject, of extreme importance, advanced in concerned comprehension ?

Under the assumption of simple geologic conditions I have rapidly sketched the flownets of Figs. 15, 16 exemplifying that there is no mystique to justify foregoing the right and obligation to use flownets at the final design stage. Uplift diagrams can be preliminary suggestions for the phase of feasibility studies.

5.1.4 Drains fully inoperative.

Such a hypothesis has been very frequently imposed, causing considerable worry and increased costs. Many a publication of observed uplift diagrams shows how strikingly far the reality falls below such an absurd prescription.

The first objection should be on principle. The civil engineer uses a specific and purposeful basic weapon for a given design: how then to impose on him the obligation to recalculate under the hypothesis that the weapon used did not exist at all ? Analogous counterparts would be: I design a steel structure, subsequently should I recalcu-

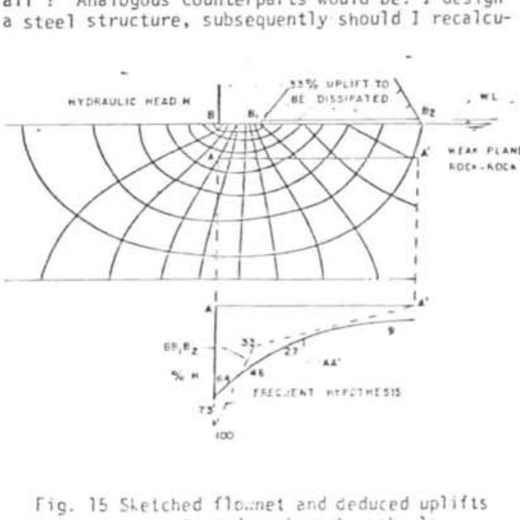


Fig. 15 Sketched flownet and deduced uplifts

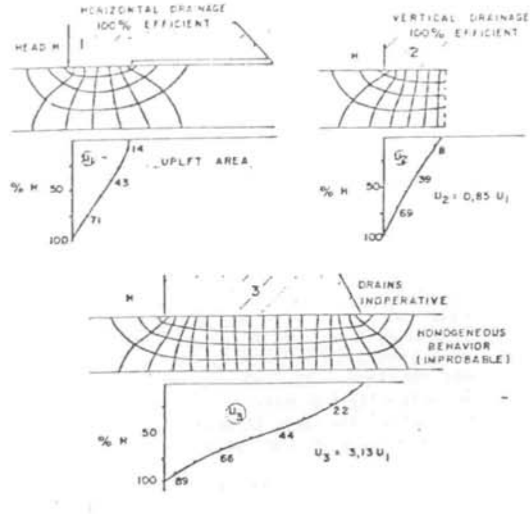


Fig. 16 Sketched flownets and comparative uplifts

late the structure as if the steel did not behave as such ? A reinforced concrete beam depends on the steel to resist tension; do I recalculate the beam design under the hypothesis that the steel reinforcing does not exist at all ? Everybody emphasizes flownets as the true basis, and these are dominantly influenced by drainage features; how then to reconsider, assuming drainage inoperative ?

The spurious hypothesis appears only in the USBR 1953 publication. The ASCE Subcommittee, 1952, declares "Those who would install drains but take no credit for their presence can still logically assume an intensity factor materially less than 1.0 if they use the grouted cutoff". The TVA does not even consider it: "Having provided the drainage system, it appeared logical to make allowance for uplift pressure accordingly in the assumptions for design of structures".

The absurdity becomes more striking when one recognizes that the extremely unfavourable hypothesis of drains inoperative is much worse (cf. Fig. 14g) than upon assuming the linear head loss from upstream to downstream face.

To set aside the absurd "drains inoperative" hypothesis one resorts to another argument, practical but pregnant with concept. One can never admit that an entire line of drains (e.g. every 3m, i.e. several per gravity block) could clog suddently and simultaneously. A gradual loss of efficiency of drains should be prudently assumed, possibly through an x% increase in the optimized design uplift diagram. A procedure of calculating the incremental unstabilizing action as a function of decreasing drainage efficiency has considerable relevance, to orient the responsible dam monitoring with regard to the need for additional drains and/or cleanout of partially clogged ones. It is important to respect the routine checking of the efficiency of each drain within the group, by alternate closing and opening of each drain to check the influence on pressures and flows of contiguous ones. One cannot overemphasize the importance of checking the gravity dam stability under extreme hypotheses, because of the reservoir soft load and the eventual brittle failure, and especially because of the incalculably catastrophic consequences of any failure. But one does [illegible] he goal by attempting absurd compe[illegible]atic[illegible] e USBR 1953 paper attenuates the inoperative- [illegible] prescription by proposing that whereas for operative conditions the sliding FS > 4.0 be required, for the "extreme" postulation it would be sufficient to guarantee an FS "sufficient to insure stability". Would that be FS > 1.3 ? Who knows and how ? It should greatly depend on the stress-strain behavior and their influence on rate of change of FS conditions.

I strongly suggest a priori rejection of the criterion of "drains" fully inoperative" substituting it by judicious proportions of inefficiency, and analyses of rate of change of effects with rate of change of cause.

5.2 Weak Shear Plane Rock-Rock, at Depth.

There cannot be any doubt as to the application, direct and judicious, of the pore pressures derived from flownets when one deals with a weak plane, rock-rock interface, within the rock foundation mass. Curiously we find that in professional practice the same uplift diagrams above mentioned, eventually suggested for the special boundary condition (of marked $k_{conc} \ll k_{rock}$, and $I = 1.00$) have been literally transplanted to the rock-rock weakness planes along which no such interface conditions would apply.

Fig. 15 exemplifies for a hypothetical boundary condition how we would readily derive the "uplifts" along the assumed plane AA'. The shear strength discontinuity is taken as not accompanied by a significant difference of permeabilities as compared with the rocks sandwiching it. If a permeability differentiation needs to be incorporated, it would alter the flownet correspondingly.

The open question concerns the probable reality of $I < 1.0$ for the effective uplift area in rock-rock contact planes. In granites, basalts, and like rocks of extremely low porosity there are (as per my visual observations) crossectional areas that can be considered effectively impervious. One recalls the highly variable and erratic piezometric heads in rock masses at short distances between points of observation. The extensive bibliography on the classic research on the question (cf. Serafim, 1954) indicates the need for further elucidation, most experimental results having determined I values at near-failure or tensile failure conditions. It is suggested as plausible that values of $I \approx 0.8$ may be frequent.

Once again, there is an interaction problem, of test conditions and conclusions, vs. prototype conditions.

6. INSTRUMENTATION AND MONITORING

As I have often observed, the types of instrumentation developed and also the planning used for their installation-observation-monitoring, obviously presuppose some theorization, i.e. certain "laws of behavior", which, in order to become "laws", represent averages of repetitive behaviors. Terzaghi is widely credited with spreading, through geotechnique, into civil engineering, the concept and practice of instrumenting and monitoring. Setting aside the instrumentation on the technology of mass-concrete, what is it that we have monitored, why, what for, and with what success ?

Firstly there is the frequent observation of deformations (settlements and tilting) as has already been explained and discussed in item 2. These have been the dominant structure-foundation interaction problems considered. Most of the instrumenting may hint at expenditures of essentially no return for professional practice, and even of limited academic interest because of timidity at reevaluating basic hypotheses. In the references to hundreds of important dams, among the thousands built in the last 80-100 years, one gains the impression that such movements rarely merited interest, partly because of no real need. Problems would develop with movements of some centimeters, such as would tear waterstops and cause big leakages; and even then, what of a big leakage ?

The instrumenting and monitoring for shear displacements merits special mention. It now seems that the effort may have laboured under a conceptual error, which besides being innocuous, could eventually reach the point of being dangerous. One should never rely on observations of types in which the differentiation between a satisfactory vs. eventually catastrophic condition would be very small, and, a fortiori, when one does not possess any indication of what magnitudes would serve as the "green, yellow and red" (traffic) signals regarding the problem faced. Brittle failure under a totally soft loading is a dangerous condition: there may be all the difference in the world in shifting from 2mm to 3mm; and how do we know, how will we know ?

For analogous situations in rock (much less dangerous because the loading would be "hard", though the failure brittle) the sounding of alarms on underground excavations (since c. 1950) has been based on development of techniques of registering microacoustic emissions; this index discriminates very much better between the safe elastic deformations and those of incipient plastification. The techniques have been succesfully extended into other areas, such as landslides etc. In Fig. 17 I merely submit as a reminder some typical data of the great increase of frequency of microacoustic emissions as failure is approached. Such monitoring might well furnish more objective data connected with shearing and proximity to decreased Factors of Safety. The very widely used observations on pore pressures and uplifts have been discussed above leding to the proposal of much less exaggegerated design hypotheses. The subject is at a standstill because of insufficient interaction between geology-geomechanics-structures, and a possible sterilizing interaction between authority/fear and the routines of practice.

Regarding pore pressure observations in rocks there has been a lack of an aim somewhat more stimulating than merely proving the prevalence of some flownet condition. I have long since insisted that piezometer installations should be carefully planned and executed, with the help of water loss tests in boreholes which would have detected and investigated specific important cracks. Thereby one should advance knowledge on a) indices I < 1.0, by using batteries of piezometers closely spaced, on the same plane; b) the efficiency achieved by grouting and/or drainage, traditionally applied, when restricted to specific cracks. Fig. 1 shows that at Itaipu a significant crack specifically monitored proved the grouting to have rendered it partially impervious.

Regarding the monitoring for the safety of the Itaipu dam the above discussions have shown that most important data pertain to the drains, and the reaction of the drains belonging to a group, to programmed provocations. If the closing of one

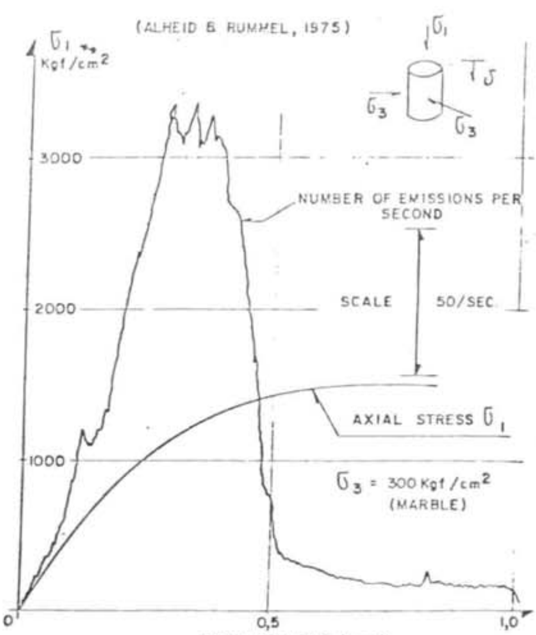


Fig. 17 Acoustic emissions resulting from shear displacement

(or more) hole(s) reflects perceptibly in the observations of the adjacent ones, it indicates that the group of drains do not have sufficient margin of safety. Project specifications require such check testing, and its results, statistically evaluated would furnish quantifiable criteria for designs of drainage. Curiously one does not recall having read papers furnishing the data of such routine check investigations, of utmost importance for recycling experience.

The great importance of laboratory research in civil engineering is that it furnishes directly the partial differentials of cause-effect for each variable, while "all" other variables are maintained constant: $X = f(a,b,c,d.....\alpha)$ and we obtain separately $\partial X/\partial a$, then $\partial X/\partial b$, and so forth. In seeking purposeful monitoring of prototypes we should bear in mind the similar desire, of partial differentials of cause-effect behaviors of significance, which is the big challenge because of the complex interactive conditions that obtain in any job.

The second need is to obtain indications of thresholds of acceptance-tolerance-unacceptability on the critical parametric variations.

7. FACTORS OF SAFETY WITH RESPECT TO SLIDING

The matter does not seem to have been specifically debated. Presumably the automatic tendency was to adopt the average Mohr-Coulomb equation (statistical average if at all possible) taken directly from shear test results as obtained. Previous criteria applied FS = 4 (USBR) on the sum total of resisting forces. Rocha 1978 (among others) proposed the reduction of strength parameters c and tan ϕ by differentiated factors of safety:
FS_c = 3 to 5 on "cohesion" because of greater dis-

tensions, and $FS_{\tan\phi}$ = 1.5 because of much more [illegible] variability in friction values. This recommendation is in wide use since it represents a definite advance, under the strictly nominal working hypotheses prevalent.

The entire subject still seems to be under very precarious formulations. This may be one reason for the implicit fears dominating present design hypotheses. It would be desirable to approach the topic through statistical confidence bands on parameters: for instance, for the shear strength the dispersions should recognize the separate contributions from $\partial\sigma$ and from $\partial\Delta H$ (Fig. 7). In the face of the absolutely inadmissible catastrophich consequences of failure, we must guarantee working with a statistical universe that should physically preclude the hypothesis of shear failure. The analyses of nominal FS numbers lose any meaning.

Greatest interest lies in parametric analyses of the dispersions of intervening parameters as affecting results. There is essentially no dispersion on loading (weight and hydrostatic): therefore attention concentrates on uplifts and resistances. For each of these parameters the decision must depend on would-be predictable consequences if unfavourable changes of 10%, 20%, 30% etc. are postulated to occur. Such curves of variation must determine the FS to employ, to obviate, at the very root, the start of any tendency to progressive plastification. With some effort on judicous research and observation, we should not be long in reaching the necessary information. We recall the repeatedly proven fact that for several tests under different σ, the developments of C_{eff} and ϕ_{eff} components at a given compatible deformation follow trends quite separate from the C', ϕ' relationships at the failure (peak) condition.

8. SATISFACTION INDICES ON VARIOUS INTERACTION PROBLEMS OF SIGNIFICANCE

ig civil engineering projects of responsibility nvolve multiple aspects of interactions because of he all-embracing nature of civil engineering. Furher, within our closer interests with problems of nteractions with geologic-geomechanical behaviors, here is still a very broad spectrum of interaction roblems.

first-stage solution tends to illude, by setting side most or many such problems, and yet apparenty meeting with success. In concept, however, henever we disregard a parameter of interaction, e really set it in a grossly exaggerated condiion: mathematically either as zero or as infinite; tatistically as beyond the histogram of even remote probabilities of interfering. The inexorable onclusion is that we cannot be optimizing, in chnical aptitude simultaneous with economy. gineering without heed to economy is a poisonous ed that spreads imperceptibly: engineering with sufficient safety makes itself all too evident headlines.

us, even after we have managed to bring into our stograms of observable (quantifiable) experiences e parameters of interaction hitherto consciously unconsciously unnoticed the histogram will be evitably assymetric, because our Bayesian quisition of knowledge and culture (cultivation ereof) is much more affected by fears than by sires (fear of failure vs. desire of economy).

e intent and hope of this message was but minute. will have been amply rewarded if, in the classic and conventional problems and solutions, some enthusiasms may have been generated towards introducing many routinely accepted practices into the realm of establishing purposeful data: such data to be treated as histograms of quantified design decisions as causes, vs. quantifiable behavior indices ("SATISFACTION INDICES") as effects.

9. REFERENCES

ACEVEDO, P. et al (1982). "Resistencia al Corte de Discontinuidades Rellenas con Arcilla", Primer Congreso Chileno de Ingenieria Geotecnica, Vol.II, p. 521.

ALHEID, H. & RUMMEL, F. (1975). "Acoustic Emission During Frictional Sliding Along Shear Planes in Rock", 1st Conf. on Acoustic Emission, pp. 149-154, USA.

ANDRADE, R.M. (1982). "A Drenagem nas Fundações das Estruturas Hidráulicas", Engevix, Brasil.

de BARROS, F.P. & BARBI, A.L. (1983). "Analise do Comportamento das Fundações das Estruturas de Concreto de Itaipu", XV National Seminar on Large Dams, Rio de Janeiro, Nov. 1983, p.287.

BARTON, N. (1981). "Shear Strength Investigations for Surface Mining". 3rd Int. Conference on Stability in Surface Mining, p.171, Vancouver.

BRAHTZ, J.H. (1936). "Pressures due to Percolating Water and their Influence Upon Stresses in Hydraulic Structures", Trans. 2nd Congr. Large Dams, Washington.

CASAGRANDE, A. (1961). "Control of Seepage Through Foundations and Abutments of Dams", 1st Rankine Lecture, Geotechnique, Vol. XI, p.159.

FECKER, E. & RENGERS, N. (1970). "Measurement of Large Scale Roughness of Rock Planes by Means of Profilograph and Geological Compass", Symposium on Rock Fracture, 1-18, Nancy.

FERRY BORGES, S. (1954). "O Dimensionamento de Estruturas". Publicação nº 54 do LNEC, Portugal.

FERRY BORGES, S. & CASTANHETA, M. (1971). "Structural Safety". Curso nº 101 do LNEC, Portugal.

GOODMAN, R.E. & CHRISTIAN, J. (1977). "Finite element analysis for discontinuous rock", Numerical methods in Geotechnical engineering, Desai, C.S. and CHRISTIAN, J.T., McGraw-Hill Book Co, New York.

IPT (1972). "Resistência ao Cisalhamento e Deformabilidade de Feições Geológicas Descontinuas em Maciços Basálticos", Relatório IPT nº 6700 de 09/11/72.

KANJI, M. (1974). "Unconventional Laboratory Tests for the Determination of the Shear Strength of Soil Contacts", 3rd Congress of the Int. Soc. for Rock Mechanics, Denver, Vol. IIA, pp.241-591.

KEENER, K. (1951). "Uplift Pressures in Concrete Dams", Transactions ASCE, Vol. 116, pp.1218-1264.

KIRN, F. (1953). "Design Criteria for Concrete Gravity and Arch Dams". USBR Eng. Monograph nº 19.

KRSMANOVIC, D. & POPOVIC, M. (1966). "Large Scale Field Tests of the Shear Strength of Limestone", 1st Congress Int. Soc. for Rock Mechanics, Vol.1, p. 373, Lisbon.

LADANYI, B. & ARCHAMBAULT, G. (1969). "Simulation of Shear Behavior of a Jointed Rock Mass", 11th Symposium on Rock Mechanics, New York, p.105.

de MELLO, V.F.B. (1972). "Thoughts on Soil Engineering Applicable to Residual Soils", 3rd Southeast Asian Conference on Soil Engineering, pp.5-34, Hong Kong.

de MELLO, V.F.B. (1977). "Reflections on Design Decisions of Practical Significance to Embankment Dams", 17th Rankine Lecture, Geotechnique, Vol.27, p.280.

de MELLO, V.F.B. (1980). "Comparative behaviors of similar compacted earthrock dams in basalt geology in Brazil", Symposium on Problems and Practice of Dam Engineering, Elsevier, 1982, pp.61-79, Bangkok.

de MELLO, V.F.B. (1980). "Some problems and revisions regarding slope stability assessment in embankment dams", Symposium on Problems of Dam Engineering, Elsevier, 1982, pp.81-98, Bangkok.

de MELLO, V.F.B. (1980). "Practice, Precedents, Principles, Problems and Prudence in Embankment Dam Engineering", Symposium on Problems of Dam Engineering, Elsevier, 1982, pp.3-17, Bangkok.

MITCHELL, J. (1976). "Fundamentals of Soil Behavior", J.Wiley & Sons, New York.

MIYAJI, Y. et al. (1983). "Observação de deslocamentos nas estruturas de concreto da Usina Nova Avanhandava", XV National Seminar on Large Dams, Rio de Janeiro, Nov. 1983, p.111.

MUSKAT, M. (1937). "The Flow of Homogeneous Fluids Through Porous Bodies", McGraw-Hill, New York.

NEWLAND, P. & ALLELY, B. (1957). "Volume Changes in Drained Triaxial Tests on Granular Materials", Geotechnique, Vol. 7, p.7.

NIEBLE, C. et al (1974). "Shear Strength of Typical Feature of Basaltic Rock Masses - Paranã Basin - Brazil", 3rd Congress of the Int. Soc. for Rock Mechanics, Denver, Vol.IIA, pp.294-301.

PATTON, F. (1966). "Multiple Modes of Shear Failure in Rock", 1st Int. Congress of Rock Mechanics, Lisbon, Vol.I, pp.509-513.

PRATT, H. et al (1974). "Friction and Deformation of Jointed Quartz Diorite". 3rd Congress of the Int. Society for Rock Mechanics, Denver, Vol.IIA, pp.306-310.

ROCHA, M. (1978). "Analysis of the Foundations of Concrete Dams", Gen. Report Int. Symposium on Rock Mechanics Related to Dam Foundations, Rio de Janeiro, Vol. 2, p.III.1-III.70.

ROWE, P. et al (1964). "Energy Components During th Triaxial Cell and Direct Shear Tests", Geotechnique Vol. 14, pp.247-261.

RUMMEL, F. & FAIRHUST, C. (1970). "Determination of the Post-Failure Behavior of Brittle Rock Using a Servo-Controlled Testing Machine", Journal of the International Society for Rock Mechanics, Vol. 2, nº 4, pp.189-204.

SERAFIM, J.L. (1954). "A subpressão nas barragens", Publicação nº 55, LNEC, Portugal.

SOUZA LIMA, V.M. et al. (1982). "Rock Foundations with marked discontinuities: criteria and assumption for stability analysis", 14th ICOLD, Vol.1, p.1155.

SOUZA LIMA, V.M. et al (1983). "Nota sobre os deslocamentos horizontais na barragem principal de Itaipu", (Note on horizontal displacements of the Itaipu main Dam), XVth National Seminar on High Dams, Rio de Janeiro, Vol.1, p.155.

Subcommittee on Uplift in Mansory Dams (1951). "Uplift in Mansory Dams", Transactions ASCE, Vol. 117, pp. 1218-1252.

TAYLOR, D. (1948). "Fundamentals of Soil Mechanics", J. Wiley & Sons, New York.

T.V.A. (1952). "Civil and Structural Design", Report nº 24.

USBR (1948). "Summary of Uplift Pressures at the Bureau of Reclamation Dams", Tech. Mem. 636.

USBR (1951). "Treatise on Dams", Ch.4, Design Supplement nº 2, Vol.10.

WEGMANN, E. (1918). "The Design and Construction of Dams Including Masonry, Earth, Rockfill, Timber and Steel Structures", J. Wiley & Sons, New York.

WERSSBACH, G. & KUTTER, H. (1978). "The Influence of Stress and Strain History on the Shear Strength of Rock Joints". 3rd Congress of the Int. Ass. of Engineering Geology, Madrid, Vol. II, p.88.