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Some problems and revisions regarding slope stability assessment in embankment dams

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SYNOPSIS. In assessing slope stability of embankment dams, current practice of limit equilibrium calculations may be accepted as established, temporarily. Such factors of safety FS are nominal. The need is to establish histograms of FS values vs. varying behaviour, setting aside the right-wrong dichotomy at 1.00 < FS < 1.00, and to search for meaningful acceptability criteria. A distinction is recognized between Factors of Safety FS and Factors of Guarantee FG. The conventional stability calculations of construction period, full reservoir, rapid drawdown, and of dumped and compacted rockfills are discussed, exposing the needs and procedures for significant revisions in present practices.

INTRODUCTION

In some of my latter papers (including one of the accompanying ones herein presented), I have attempted to emphasize that one begins by sorting out carefully which are the types of failures that one should visualize physically associatable with a major civil engineering project such as a dam, and should thereupon carefully distinguish between viable design philosophies for the different cases. Much depends on the statistics of truly repetitive conditions from which we derive our "laws" and the implicit or explicit histograms of probable behavior.

In the present summary paper I shall concentrate on the problems of slope instability and sliding failure analyses.

1. DISTINCTIONS BETWEEN NOMINAL FACTORS OF SAFETY

It is more than comprehensible that engineers should concentrate attention initially on failures. Human cognizance of the continuum comes from perception of when it ceases, the recognition of the discontinuity: one does not feel health, does not notice one's members except when they begin to hurt or to fail in the continuous functions of the silent majority. Slopes slide, some of them inexorably, in their allotted geological time. But how do we distinguish between a slow sliding movement that merely causes cracks and acceptable damages, and the more rapid movement that "endangers" lives and "totally disrupts" property? The cognizance of slope sliding started being associated with the latter rapid and major movement. Understandably the analysis of slope slides, both rapid and of major volumes, and the analysis of Factors of Safety FS against such sliding absorbed a major proportion of the efforts of the geotechnical engineer and of dam engineering.

Let us herein accept that the analytical methods of limit equilibrium, and of corresponding computations, are reasonably established as working tools of the geotechnical engineer. Even if we wish to discard such working hypotheses and vindicate more modern working methods of stress-strain distribution analyses, let us assume that they also have been distilled into a comfortably established routine engineering tool. It is the purpose herein to emphasize, however, that no matter how good our methods of investigation-testing-computation, all our FS are but "nominal", and the problems of DECISION (yes-no, acceptable-unacceptable) continue to be an arbitrary discontinuity within the continuum of reality. At some point in the histogram of Percent Probabilities PP% "Failures" (?), or better, PP% of Satisfaction Indices SI (de Mello, 1977) vs. varying nominal FS values, a designer must sever with the yes-no guillotine.

The problem would become one of discussing acceptable FS values. The behavior of a sliding mass should be reasonably conditioned by statistics of averages within the big volumes and surfaces at play. Why then is it that we are yet totally lacking in histograms of "behavior indices" of slope movements vs. nominal FS? The fault is surely not in Nature, but in our own mental model of rigid-plastic limit equilibrium, that would transform the problem into one of adjusting our analyses of failed slopes to the hypothetical condition of FS $\lesssim$ 1.00, having always stumbled on the presumption of dealing with "true" FS values, as if engineering were science, and both were deterministic. Moreover failures have always been analyzed a posteriori, under all the psychological and technical conditioning that this implies, with no real observations from the plane of failure at the time of failure movements.

In this paper I shall restrict my comments to the discussion of: (a) the nature of Facture of Safety FS vs. Factor of Guarantee FG (de Mello, 1979); (b) the conventional stability analyses of soil mechanics as applied to earth and earth-rock dam slopes, and the need to adjust them to reality in order to accumulate observational experience on the histograms desired of SI vs. FS or FG; (b_1) the need to adjust tests; (b_2) the need to adjust some of the analyses. The discussion of concepts regarding dam failure and embankment dam slope failure must be faced independently of any common slope sliding failure because of the disproportionate risks at stake. Practising dam engineers may be right in their disagreement with theoreticians when they insist that "a dam cannot fail" (i.e. cannot be permitted to visualize a risk of catastrophic sliding failure). Definitely the downstream slope of a reservoir should never be permitted to fail rapidly. Probabilistic calculations are an illusion. We should definitely resort to a physical change of statistical universe, so that the probability of the event should be guaranteed to be zero (de Mello, 1977).

2. FUNDAMENTAL PRINCIPLES OF SLOPE DESIGN AND OF STATISTICAL DEFINITION OF ACCEPTABLE INDICES OF BEHAVIOR

2.1 In the companion paper I expatiate on what I propose as the most fundamental principles of good theorizable design (de Mello, 1977), particularly relevant in embankment dam engineering calculations. Principle of the pretest, wherein one ensures that construction conditions should be more critical than the operative ones (FG vs. FS); further, the principle of humility in changes of conditions, wherein one avoids rapid changes of conditions disproportionate with experience and/or the status quo; thirdly, the all-important aim that foreseeable changes with time be in the favourable trend, however minute. The fundamental consequence is that in good design often a stability computation under critical operational conditions may well have been turned quite unnecessary: that is when wisdom supercedes knowledge. However, recognizedly we still lack statistical knowledge of acceptable indices of slope behavior, and of indices of acceptability of such behavior.

2.2 By convention we define FS as the ratio of:

$$\frac{\text{Predicted Resistances } R\ (\pm\ e_r)}{\text{Predicted Stresses } S\ (\pm\ e_s)}$$

$\pm$ e = dispersion

If during construction we have established satisfactory stable (elastic?) behavior up to a given stress level S_c (c = construction), we know deterministically that $R \gtrsim (FS)R_c$. Now, if we have enforced that $S_c \gtrsim S_o$ (o = operational-time) and reasonably anticipate that $R_{ot} \gtrsim R_c$, then we cannot continue to use the ratio $\frac{R_{ot}\ (\pm\ e_r)}{S_{ot}\ (\pm\ e_s)}$ as a value FS. I have proposed to call such a different nominal factor of safety as FG = Factor of Guarantee. Obviously one may accept FG $\ll$ FS without risk of dissatisfaction: e.g. FG = 1.1 might well prove satisfactory in a material and condition that would require FS = 1.5.

2.3 In Fig. 1 I attempt to demonstrate that whereas Soil Engineering has generally considered only one definition of Factor of Safety, FS, it can be important to recognize three distinct Factors (considering only the differentiation of statistical dispersions around Resistances, R, without any further delving into the histograms of acting stresses S). Factor of Safety FS is a routine definition. I have chosen to call Factor of Guarantee FG the situation wherein by some lower rejection criterion I have assured myself that the histogram of Resistances can only be higher than some value already pretested or guaranteed. Obviously a value FG = 1.5 constitutes a much greater assurance of success than

FS = 1.5. In order to clarify the concepts it may be convenient to exemplify with regard to piles, with which familiarity is greatest, and softground tunnels, in which execution effects are of greatest moment.

A pile jacked down under 60 tons to absolute stoppage of penetration/settlement has FG = 2 if used for a working load of 30 tons: if the estimated resistance is 60 tons it has the conventional FS = 2. Setting aside the discussions on dynamic vs. static resistances of piles and cases of sensitive clays, driven piles checked by "refusal" observations can well be said to imply factors FG. In contrast, a bored pile would suffer from two disadvantages in its load-settlement behavior. Firstly, it would never have been pretested, and therefore one might conclude that it is affected by the FS (poorer than FG). Secondly, upon closer examination we should reason that it is even worse than that. All efforts of advancement of Soil Mechanics are towards minimizing sampling and testing disturbances and better representing in situ soil parameters (intact soil elements). In reality the assessed intact parameters would establish an upper rejection criterion, since the soil affecting bored-pile behavior represents a histogram of resistances always lower, to varying degrees, truncated at the upper value. A situation diametrically opposite to that of FG, with the lower rejection criterion. One could denomite the new ratio of averages (Resistances/ /Stresses) a Factor of Insurance FI: insurance is against something essentially inevitable, that should be attenuated.

The basic fact is that FI < FS < FG and depending on the dispersions of the histograms the differences may be very significant. If projects continue to be designed generally for (nominal) FS = 1.5 without recognition of this significant difference, all structures in which FI is at stake will record a much greater degree of troubles, while structures in which FG is at stake will incorporate an unnecessarily higher degree of safety. Tunnels and bored piles involve execution effects that only deteriorate in situ parameters (resistance, deformation) and therefore involve FI conditions. In the case of dams, if we allow flownets upon first filling to alter significantly the stability conditions of the downstream zone, we may be inviting FS conditions rather than the desirable FG situation of pretested behavior. More over, if the long-term flownets generate uplifts on tensile stresses, which can only deteriorate the strengths achieved as ascertained, we may be inviting most unfavourably the conditions associated with FI.

There is yet another important point to emphasize: the distinction between conditions which permit applying averages (as above), and those that involve localized situations corresponding to somewhere along the ends of histograms. That is, confidence limits and factors of safety might be related to "individual events on the histogram" (or fractiles) rather than on the median. Such is, for instance, the situation of instability of localized pockets along a bentonite-stabilized bored pile before concreting: after the concreting, the rigidity of the concrete guarantees applicability of the average over the profile. Similarly in a tunneling open face, during excavation localized instability may well be at play, corresponding to much more unfavourable conditions: behavior behind a steel face-plate of shield tunneling, or around a lining, can well be accepted to be averaged, which implies an inevitable benefit in comparison with localized worse conditions. A steel face- -plate of shield tunneling, or around a lining, can well be accepted to be averaged, which implies an inevitable benefit in comparison with localized worse conditions. In the case of dams it has been argued (de Mello, 1977) that sliding mass instability can be treated on the basis of averages but locally generated piping failures could be characteristically associated with extreme-value statistics. In this paper I shall concentrate on problems associated with averages.

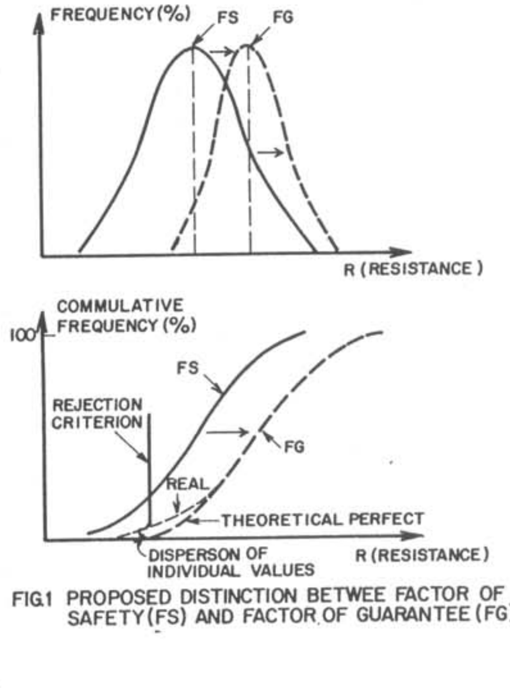


FIG.1 PROPOSED DISTINCTION BETWEE FACTOR OF SAFETY (FS) AND FACTOR OF GUARANTEE (FG)

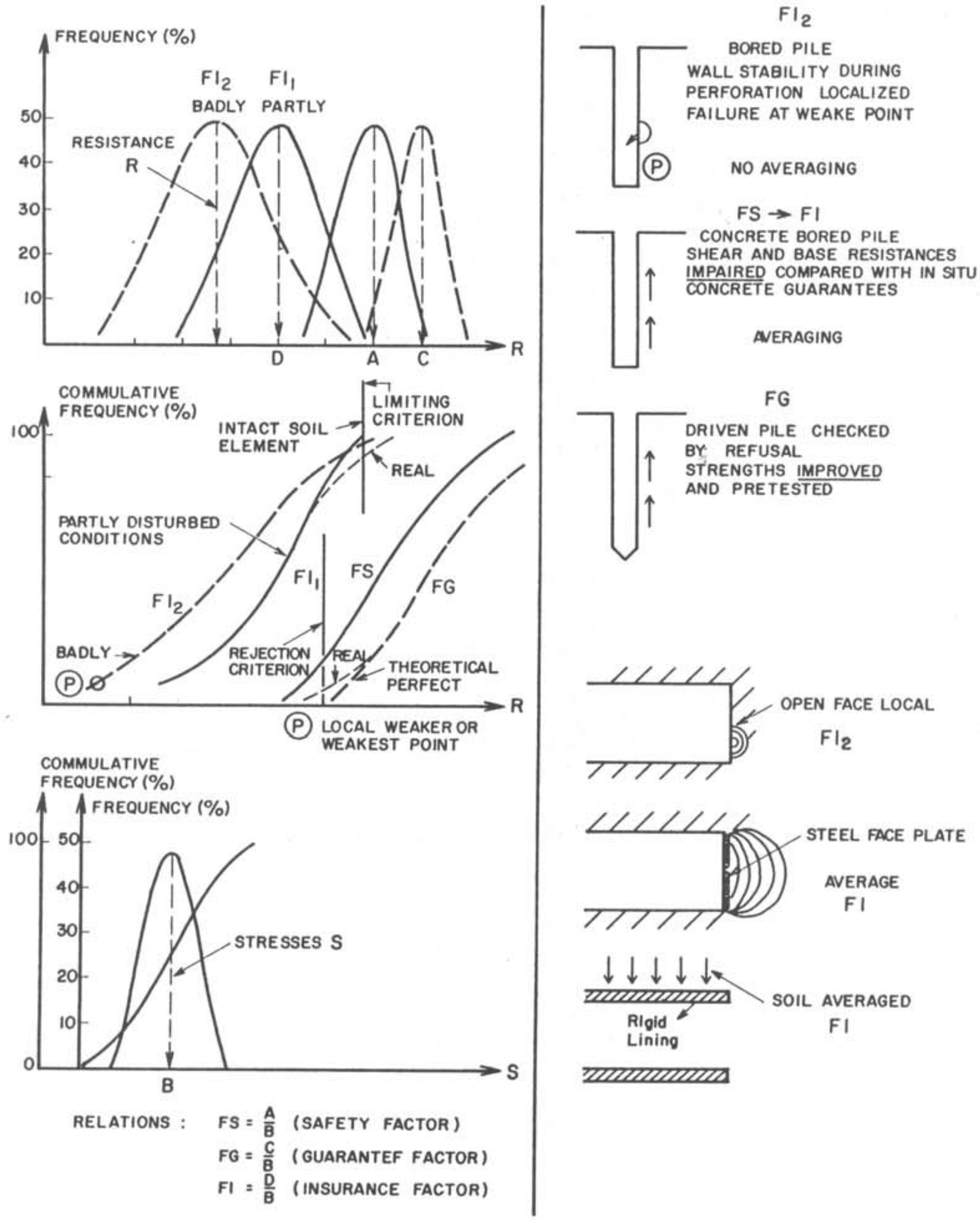
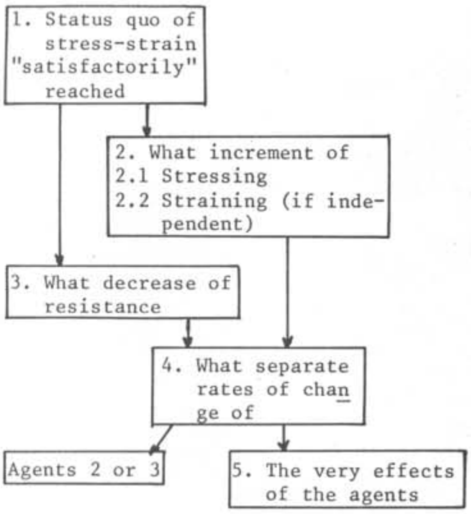


FIG. 2 SUGGESTED DISTINCTIONS IN "FACTOR OF SAFETY"

2.4 In "complete" reasonable stress-strain-time path trajectory (Taylor) reasoning and testing, we must not neglect to consider



Note that internal stresses (1) are most frequently different from the simply adopted geostatic assumptions of early soil mechanics. Moreover, straining (2.2) is sometimes quite independent of load-stressing (e.g. collapse of structure). Further, the onset of failures can be due to any of the agents (2) and (3). Finally, what matters much is not merely the rate of onset (4) of the agents (2.3), but also, the rate of onset of the effect (5) of the agent, since it is well-known (e.g. "viscous" and other complex rheologies) that the rates of causes and rates of effects are not similar.

Thus in estimating pore pressure development along a potential sliding plane due to a change of flownet pore pressures, it is not sufficient to consider, for the effective stress analysis, the u value as that corresponding to the new flownet. One should consider the u value as composed of two parts: the first is the hydrodynamic flownet pore pressure; the second is an incremental excess (positive or negative) pore pressure due to tendencies to variation of volume (ΔV) that would accompany the incremental straining (normal and shearing). This $\Delta u = f(\Delta V)s$ depends of course on an estimate of the incremental stresses anticipated (always assumed on the pessimistic side), but it depends also on an estimate of the anticipated rate of change of stresses, and principally rate of change of strains. The latter important consideration is why liquefaction slides and mud flows are foreboded when the stress-strain curves show a sharp post-peak drop, and the incremental shearing is highly contractive.

In recognizing the above we merely emphasize the recognition of how nominal are our procedures of sampling-testing-computing (stability). They will always continue to be so in our engineering endeavours.

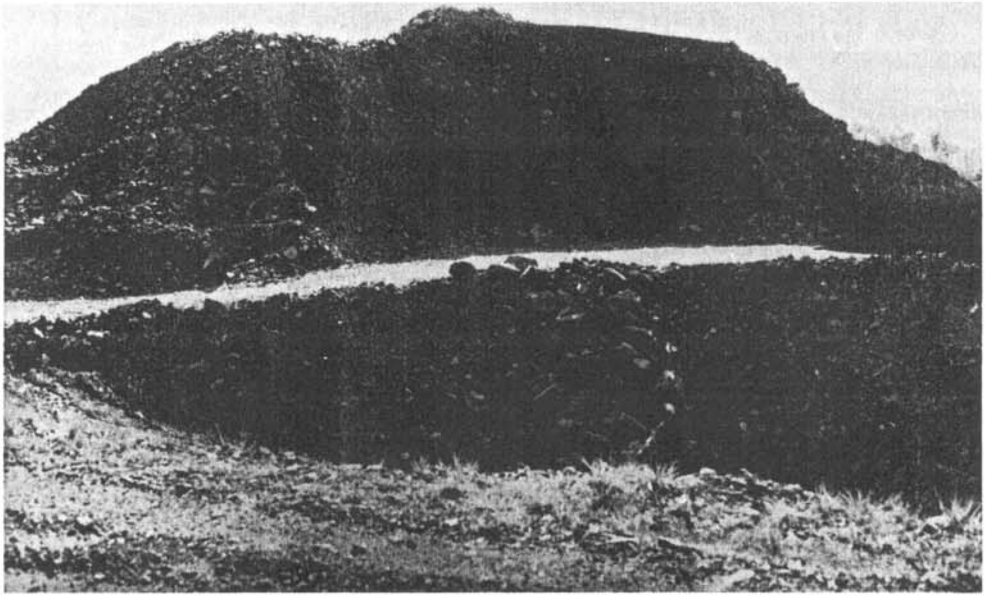
2.5 That is why I proposed (de Mello, 1977) that we should establish an operational Satisfaction Index SI for assessing the behaviors of slopes associated with different FS (and/or FG values). The importance is to use in each same statistical universe (same embankment) several different slopes, to accumulate theorizable observational data (e.g. on "plastic incremental movements" compared with pseudo-elastic "stable" reference values). The importance is to collect hundreds, thousands of such pairs of data (SI vs. FG) so as to establish the necessary histograms. Then we will be in a position to apply our acceptance - DECISION truncations, rationally and economically.

Statisticians conversant with its mathematics will kindly develop the relationships between the conventional FS and the newly proposed additional factors FG and FI in function of the histogram truncations. In good dam design, if consequences of risk are high we want to be dealing with FG conditions: thus, in much of the following text I shall limit myself to mention of FG. However one must emphasize that FS conditions may well be at play in most cases of conventional designs, and if so the corresponding computed value for satisfaction must be decidedly higher than if FG conditions were guaranteed (under pretested situations).

3. SLOPE STABILITY IN DUMPED AND COMPACTED ROCK-FILL

In my Rankine Lecture the subject was somewhat discussed, to emphasize that: (a) infinite slope analysis is an extremely conservative lower bound and could well accept a FG $\approx$ 1.00 +; (b) "stability is automatically self-tested as the fill rises at its constant slope" (i.e. we are dealing with FG and not FS); (c) there are advantages of deterministic u = 0 to permit vely low FG; (d) there should be advantages of locked-in prestress (in crushed angular contacts) whereby we should count on greater stability than implied by conventional computations.

In furthering the subject herein the following facts are emphasized, summarizing



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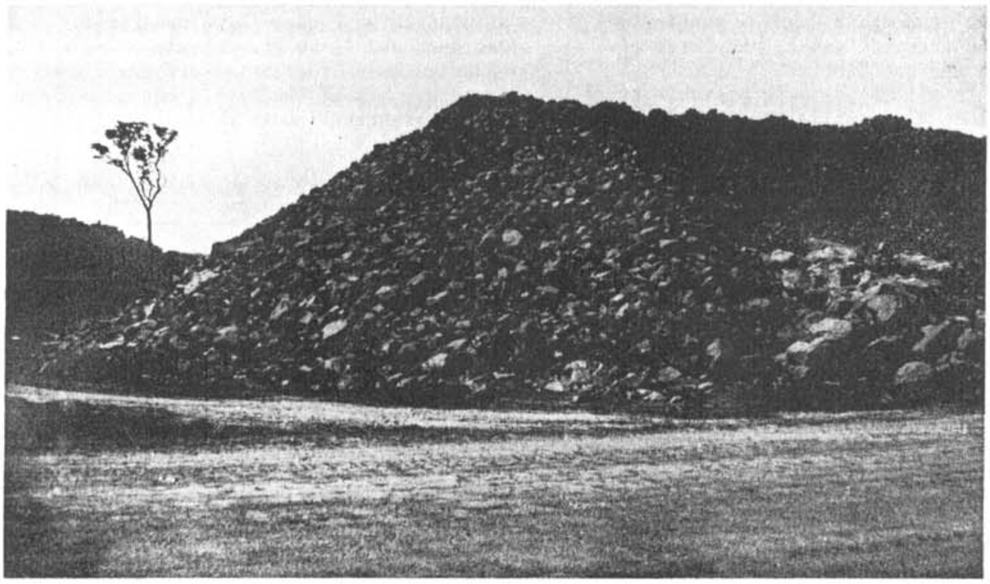
a vast number of observations on rock-fill and corresponding aggregate stockpiles (heights 30-45m) of very big projects. For interesting comparisons, specific data are presented on the sound dense angular basalt quarried in the Salto Santiago and Foz do Areia Projects.

The following photos (1 to 7) represent (a) comparative end-dumped vs. bottom--excavated repose-slopes of basalt rock stockpiles on which preliminary statistical data on face angles were carefully surveyed; (b) the 48m high 1:1 compacted rockfill slope of the upstream cofferdam



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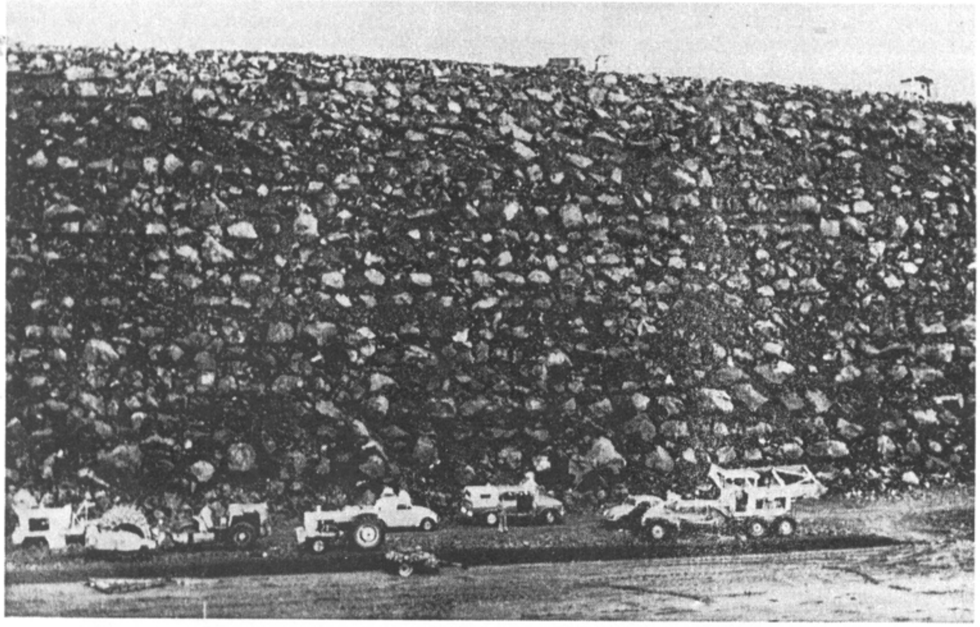

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incorporated into the 78m high earth-rock dam of Salto Santiago; (c) the final 1:1.4 downstream slope of the Salto Santiago dam; (d) the finished 1:1.25 downstream slope of the 160m compacted concrete-face rockfill dam of Foz do Areia.

(1) The stable slopes (angles of repose) were surveyed in detail in minimum stretches involving more than about 15 big-size rocks. The histogram for the end--pushed loose rock may be considered conditioned by the most unstable surface rock having to stop from a moving position. (Fig. 3)

(2) In comparison, the excavated slopes show two distinct trends, a steep stretch

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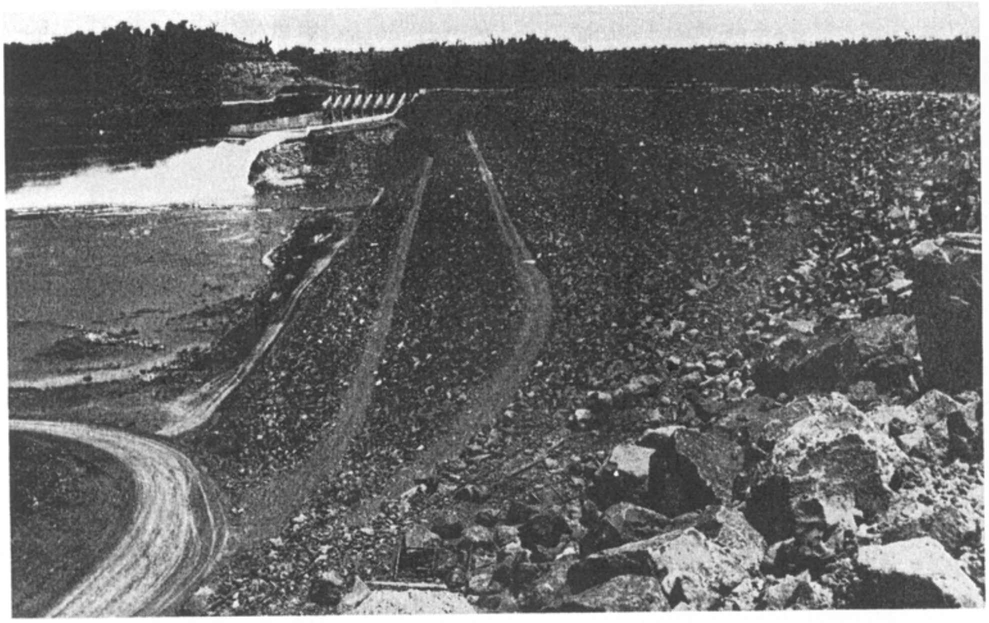

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(even partly subvertical), dominated by the more stable rocks having to be moved out of their interlocked rest ("static" vs. dynamic friction?), and the lower stretch comprising mixed excavation-slope and rolled--slope material.

(3) In comparison with smaller granular material, we deal with a histogram that is not so tight as in "uniform sand laboratory tests".

(4) However, even in loose end-dumped angular rock stockpiles there is a definite strength gain from prestress.

(5) The rhetorical question posed is, which ϕ' aver should prevail in nominal stability analyses, that of slopes a-fill-

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ing, or that of slopes excavated from the bottom, after benefit of prestress?

(6) Considering the very significant prestress contributed by compaction of rockfill in lifts, how much steeper can we go without any risk of unsatisfactory behavior?

(7) In consonance with the Rankine Lecture suggestion, the vertical and horizontal movements of points on the downstream slope were carefully recorded as the compacted rockfill dam rose by its final increment of height. (Fig. 4). Can we say that the tendencies of movements are such as upon progressing would model a slope sliding failure?

(8) Note that this rockfill was relatively uniform and compressible, not suggestive of the most stable interlocking rigid blocks. Face instability is satisfactorily tended by arranging bigger blocks as markers for offset and lift thickness.

4. CONSTRUCTION PERIOD STABILITY, CLAYEY MATERIALS

In the companion paper it is emphasized that under modern heavy earthwork equipment the conditioning factor is trafficability, and that in any well-designed dam having a chimney filter (cf. Rankine Lecture) it should be difficult to conceive of compactable conditions facing end-of-construction instability. There is inexorably an effective stress preconsolidation cohesion even if only short-term) for the short--term condition considered. Moreover, there is the initial negative pore pressure, and not the presumed u_c vs. γz (c = construction) diagrams insinuated by the early (and most recent but faulty) laboratory tests and field observations (USBR). Finally, the constant r_u or $\bar{B}$ coefficient assumed for simplifying computations is quite unnecessary, and misleading regarding greater instability for shallower circles (really benefited by suction and cohesion). (Fig. 5)

In the zone downstream DS of the chimney, affecting the all-important DS stability, we should prefer generating some u_c in order to assure ourselves of the pre-test principle and satisfactory FG, and especially its inexorable improvement with u_c dissipation with time (cf. Rankine Lecture). Should any u_c develop higher than desired, we may use at will the intermittent u_c - ADJUSTERS comprising dry layers (functioning as "blotting-paper" non-exiting filters), without fear of layering permeabilities. The same expedient may be used to advantage also in the upstream US zone of the dam, excluding what would be equivalent to a "core": in the "core zone" it is necessary to avoid unfavourable permeability $k_h >> k_V$ effects on flownet, and is desirable to have a high u_c, perferably close to the u_{fn} (fn = flownet, full reservoir) so as to minimize the change of conditions in the

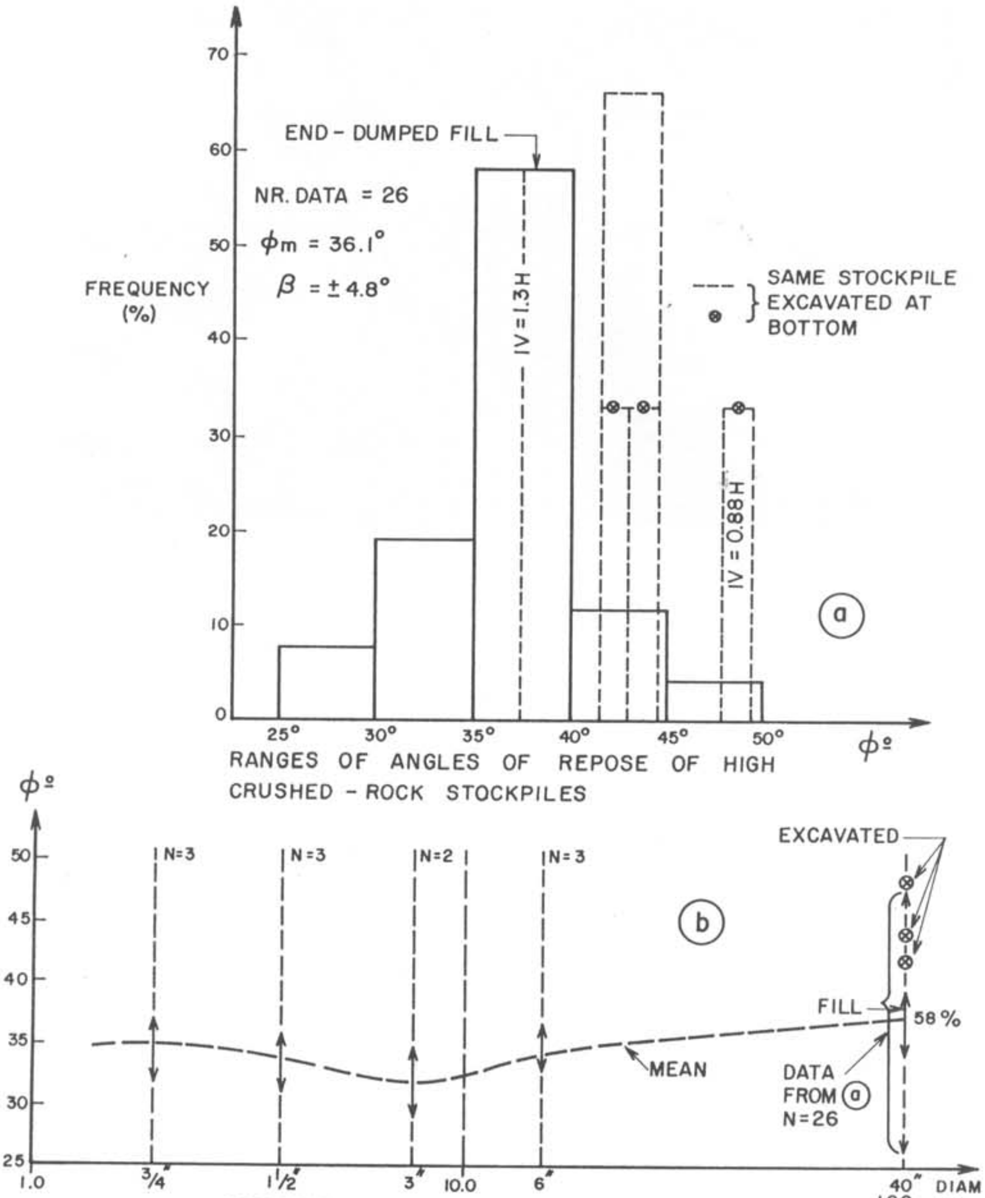


FIG.3 BEHAVIOUR OF ANGULAR ROCK SLOPES AT F.S. = 1.00

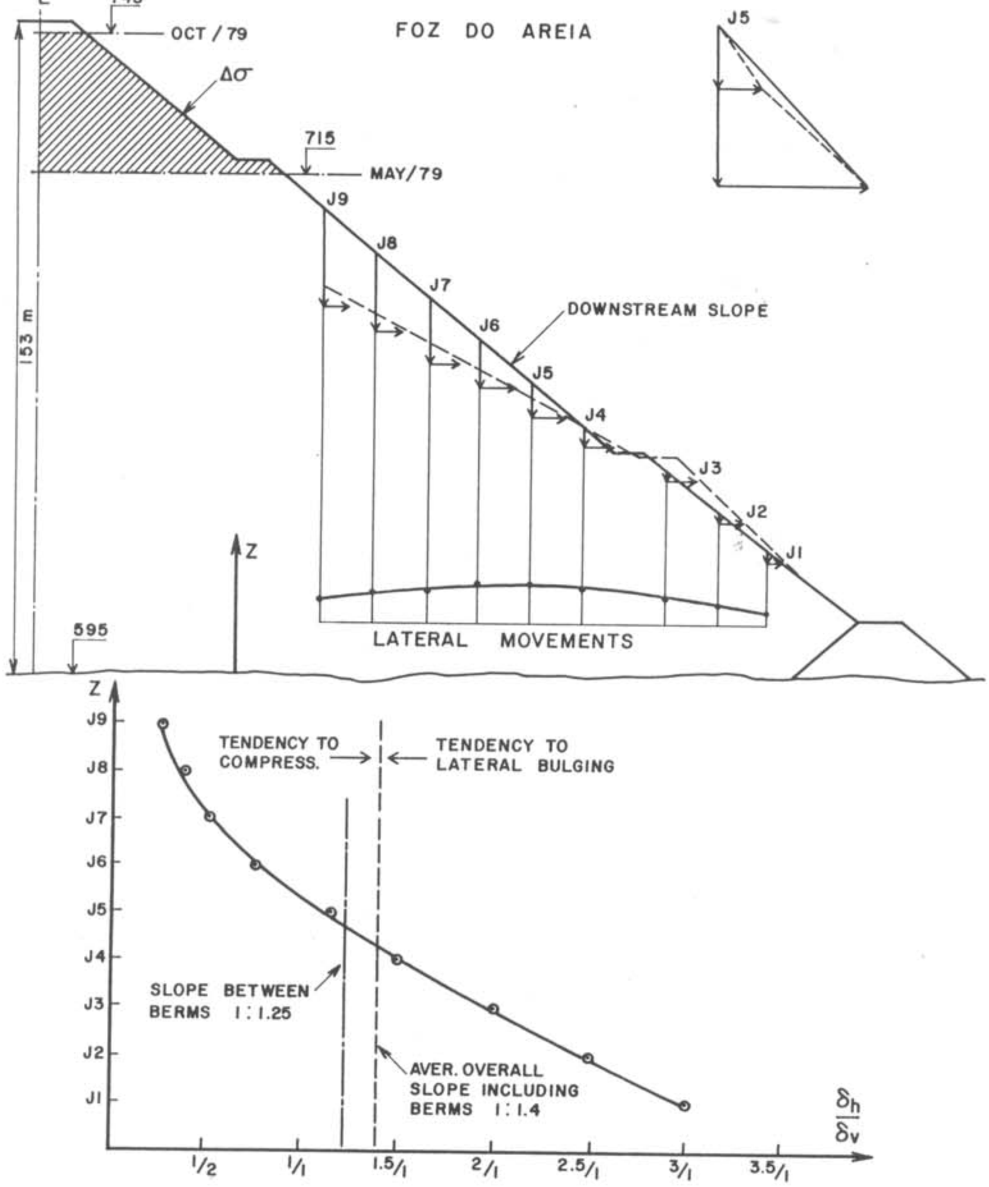


FIG. 4 OBSERVATIONS ON PRE-FAILURE SLOPE MOVEMENTS FOR "SATISFACTION INDICS" (cf. RANKINE LECTURE) vs. NORMAL F.S (160 m ROCKFILL)

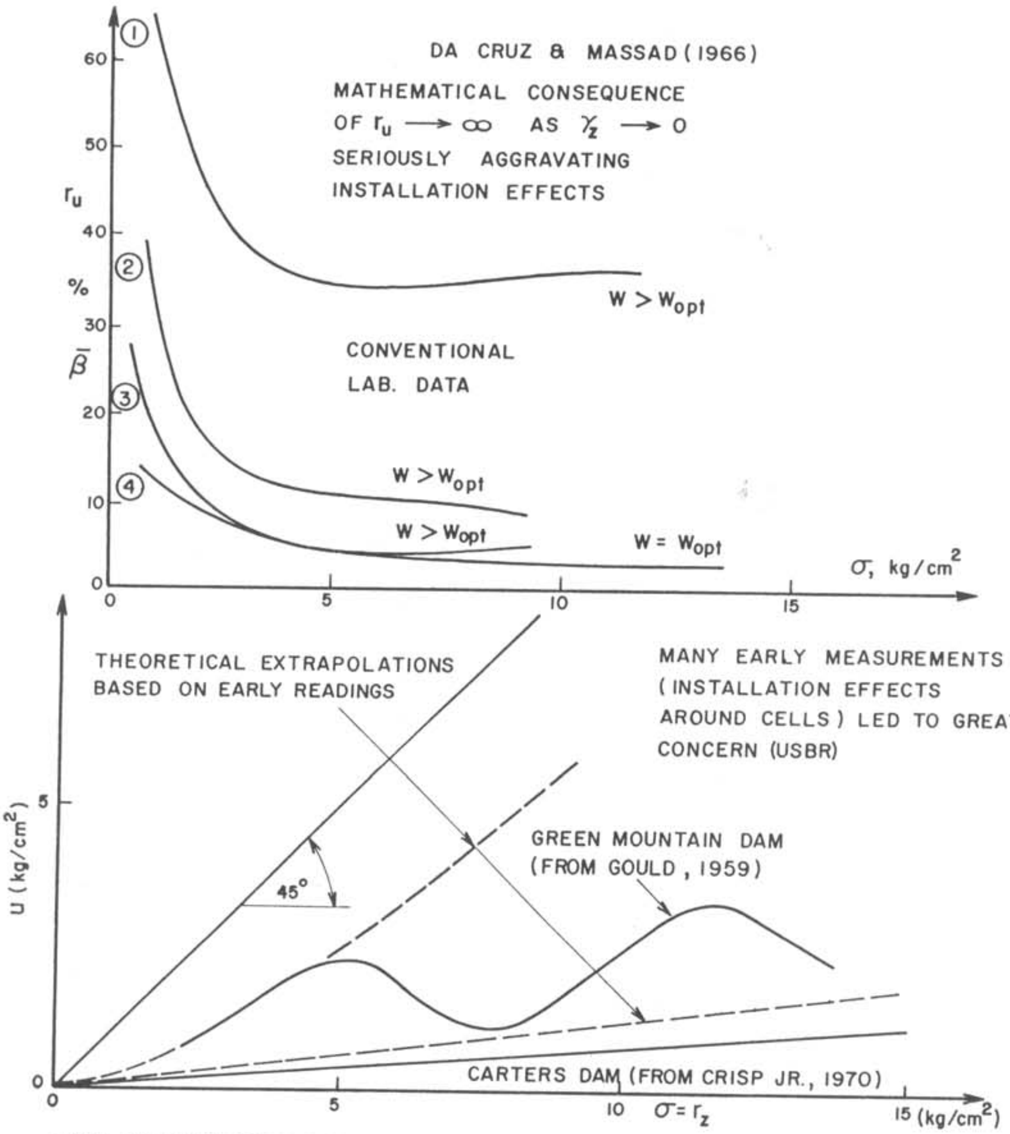


FIG.5 CONSTRUCTION PORE PRESSURE MISCONCEPTIONS, LAB. AND FIELD

core on first filling.

As is easily proven and well known, a core may have quite high construction pressures without any impairment of end-of--construction US slope stability. If necessary, stability analyses can well be run, using appropriate estimates of internal stresses, cohesion, negative and positive u_c, and the effective stress envelope but the acceptable (and even desirable design aim) FS should be close to 1.0 for adequate "pre-testing". The analyses are merely to facilitate Bayesian insertion of succesive u_c observations for continually improved assessment of FS, while deformation measurements furnish indications on SI.

Since one cannot anticipate the coincidence of achieving $u_c \approx U_{fn}$ it is of interest to discuss in which direction the tolerance should be more favorable, $u_c > u_{fn}$ or $u_c < u_{fn}$. The ideal situation would be to have had u_c developed to values higher than u_{fn} but dissipated to a value slightly lower than the u_{fn}. Thereupon the soil will have been pretested to values of u higher than necessary, and will be behaving within the precompressed or preconsolidated range wherein changes of void ratio and of behaviors with change of stress are small.

5. DOWNSTREAM STABILITY, FULL RESERVOIR

5.1 First filling

The interesting problems of rapid vs. slow filling will not be discussed herein. We shall firstly assume a traditional critical "permanent flownet".

One would imagine that finally this will be the all-important case in which a conventional stability analysis is indispensable. Truly, however, quite to the contrary. Indeed, the hypothesis of such a failure is so unthinkable that one can only accept the wisdom of a design wherein the establishment of critical full reservoir conditions will not cause u values, u_{res} (res = reservoir), higher than the u_c already satisfactorily borne (cf. Rankine Lecture). This is fundamental. And with appropriate design of position of chimney, controlling u_{fn}, and appropriate control of compaction parameters, controlling u_c, it is quite simple to achieve this wise design situation that dispenses such additional stability calculation. With $\Delta u = u_{res} - u_c$ = negative (modestly) the change of stability from end-of-construction to first filling can only be an increase (ΔFG = positive). || Note. There are other conditions that may similarly be reasoned to be satisfactory ||.

Many a dam has behaved satisfactorily without any inkling of such principles. However, absence of evidence is not evidence of absence. The only way to guarantee zero probability of downstream sliding failure is to have pretested the $FG > 1^{++}$, and to have ΔFG $^+$ve. Any number of design sections and conditions may be rapidly sketched showing how to compare u_c vs. maximum possible u_{res}: it is a simple exercise, and requires no qualms of decision.

What is the most unfavourable u_{res} possible? The flownet hypothesis u_{fn}? Under which hypotheses (cf. Rankine Lecture), all higly dependent themselves on other hypotheses? Would it then be right to assert that the maximum maximorum attainable would be the full u_{hyd} (hyd = hydrostatic)? Most especialists might still claim so. I dare emphasize that even this claim is, in principle, wrong, because it would not incorporate the influence of rates of change and of possible rheological consequences, and rates of consequence. If a rapid reservoir filling, and unfortunate upper limit, leads to a positive $\Delta u = u_{fn} - u_c$, or $u_{res} - u_c$, and thus there is a rapid drop in stability, there can be a rapid strain and consequent ΔV due to shear around the sliding surface. If we guarantee a dilatant rheology, we would be secure, but if there were a tendency to compression, we would have an increase Δu_s (incremental excess pore pressure due to contractive tendencies as already discussed) such that the real maximum u at play in the sliding stability could be $u_{res} + \Delta u_s$. It is imperative to seek the right geomechanical and rheological statistical universe, that may dispense the full reservoir stability analysis as such, by assurance of much better known truncated histogram analyses of changes of conditions.

5.2 Long term stability

For a long-term DS slope stability once again the only appropriate analysis is by mentally postulating what changes of loadings and resistances can tend to occur to affect the already established first-filling FG. It is really absurd to think of a slope analysis "from scratch", because of the much much greater probabilistic imprecision than by Bayesian analysis of posterior probabilities as superposed on the prior (cf. Rankine Lecture). There is no difference in principle in using the probability changes of u from u_{res} ff (ff = first filling) to u_{res} lt (lt = long-term), in manner similar to the roughly suggested progressive adjustment of u_c as a quantification of the Observational Method.

Would u_{res} tend to increase with time? Depends on how consolidation, secondary compressions, tensile cracking, etc... would change relative permeabilities. We must arrange for strength to increase with time, and u to decrease with time: both trends are associated with tendency towards compression (desirably modest). Strength may further gain from favourable cementing and thixotropic effects. It isn't at all difficult to design to guarantee such compressions, such that after FG_{res} ff $> 1^{++}$ we guarantee a ΔFG + ve.

It would seem that the fear of long-term instability in natural slopes has unduly influenced dam design: whereas long-term instability is inexorably a problem in a natural slope at FS $\approx$ 1.00, in a good dam design of FG > 1.0^{++} and + ve ΔFG, there should be scant probability of such a problem. Note, however, that much depends on the shear strains and brittle stress-strain, and strain rates: repeating concisely what has been emphasized, it is not valid to be content with using effective stress envelope and merely u_{res}, when there may well be a $\Delta u = f(\Delta \text{ strain})$.

One loading condition that would seem to thwart the postulated simplicity would be the seismic one. I cannot herein advance into this additional case. Suffice is to emphasize, however, that even the probabilities of a catastrophic seism should not be considered independently of the probabilities of occurrences of smaller seisms. Except for the extreme event of the very first seism being that of maximum maximorum probable intensity, the occurrences of smaller events could and should be considered with regard to cumulative improvement of conditions by successive compressions (cyclic). It would be ideal to aim at a slightly dilative instantaneous behavior for intensities higher than some moderately rare recurrence level.

6. UPSTREAM SLOPE, INSTANTANEOUS DRAWDOWN

This is, indeed, a topic in which both the theorizing, and the conventional practices, are blatantly wrong. And, as a result, it would appear that upstream US earth slopes are significantly overdesigned. There have been cases such as the emptying of the Tarbela reservoir ($\approx$ 4m per day) on the 10 hr emptying of the 58m deep Euclides da Cunha reservoir (Jan 77) after overtopping failure, in which nothing budged. The following photos (8,9) of the rapid and subvertical erosion scar clearly indicate the sliding that can be said to have been generated on this steep face by the instantaneous drawdown condition to which it was exposed: one must note, however, that along this face the condition is much more exacting than merely that of a rapid drawdown, because besides instantaneous removal of the water pressure diagram there is the removal of the earth-pressure diagram also.

Thus, as regards slope stability analysis and design of upstream slopes subject to rapid drawdown it is not merely a case of permitting lowering FS to 1.1 (as presently often applied), and has little to do with the fact that drawdown is never quite "instantaneous" and that some drainage lowering of the phreatic might be included. It is a case in which the very mental model appears fraught with inconsistencies from the start (cf. Rankine Lecture): if one does rightly lower FS but does it on a wrong mental model, there can well occur some undesirable surprises.

In consonance with routines of examination of limiting critical hypotheses, I shall herein limit myself to considering the hypothetical absolutely saturated embankment. We well know how high are the backpressures necessary to saturate triaxial specimens (6 to 12 kg/cm^2), especially if the air micropores are first reduced in diameter by the soil consolidation under confining stresses. Thus it is obvious that in modest dams and/or shallow sliding circles, the compacted material would not be saturated. The principles below summarized restrict themselves to considering tendencies to change of σ' (assuming saturated incompressible pore fluid): in a generalized extension, we will have to consider tendencies to change both of σ' and of u (besides, of course, the incremental shear stress and strain rate Δu_s, and any sophistications such as rotations of principal stresses, etc...).

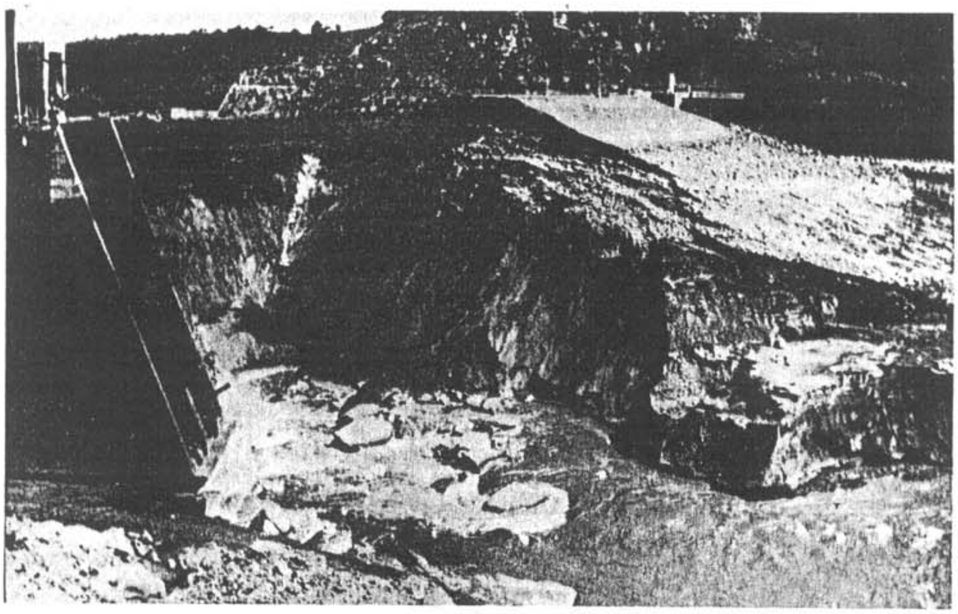
The fundamental errors of concept may be summarized as the following:

(a) The dichotomy could not possibly be between:

"FREELY-DRAINING FILLS" (Terzaghi-Peck,1948, accepting flownets, rapid drawdown RDD)	vs.	"COMPRESSIBLE FILLS" (Pore pressures generated due to $\Delta^{\sigma}t$ on dam face and r_u concept, $r_u \approx 1.0$)

Obviously draining vs. non-draining has no obligation to any assumption on compressibilities. Likewise, incompressible vs. compressible has no obligation of direct deterministic association with drainability or rates of drainage.

(b) Except in extremely differentiated materials, there is no black-white distinction of intrinsic qualities of draining vs. undraining, compressible vs. incompressible, as regards materials and conditions thereof. The Portuguese Spanish languages (and others?) have a conceptually important distinction in the verbs ESTAR (to be temporarily, to behave as being) and SER (to be in essence, to be permanently). Firstly no material possesses a homogeneous tendency (to compress, to drain, or whatever) over the entire upstream body of a dam. Secondly, compressibility (etc...) is a temporary behavior problem (ESTAR): a material will not be have as compressible (even if it is generally or frequently



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compressible) if it is subjected to a stress release. A soil element in one part of the critical circle may be subjected to stress release, and want to behave as dilatant, while another exactly similar soil element in another part of the same circle may be subjected to a stress increment, and want to compress.

As a curious extreme example to emphasize the conceptual point we could say that in a material that is (SER) homogeneous and pervious functioning under a flownet, each flowline (surface) behaves as (ESTAR) impervious, since no iota of water from one side or other of the surface crosses it (it is immaterial that it does not "wish"

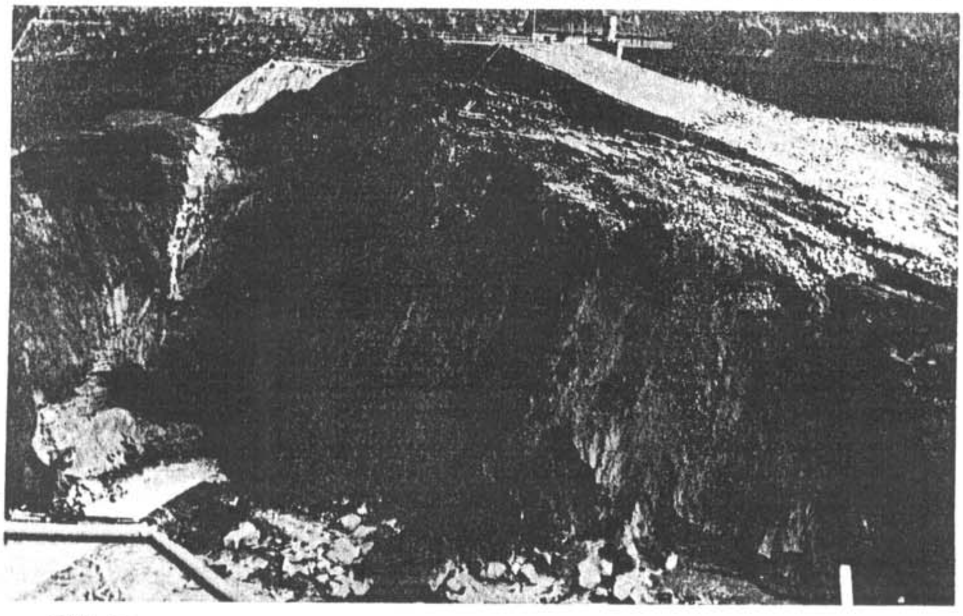

PHOTO - 9

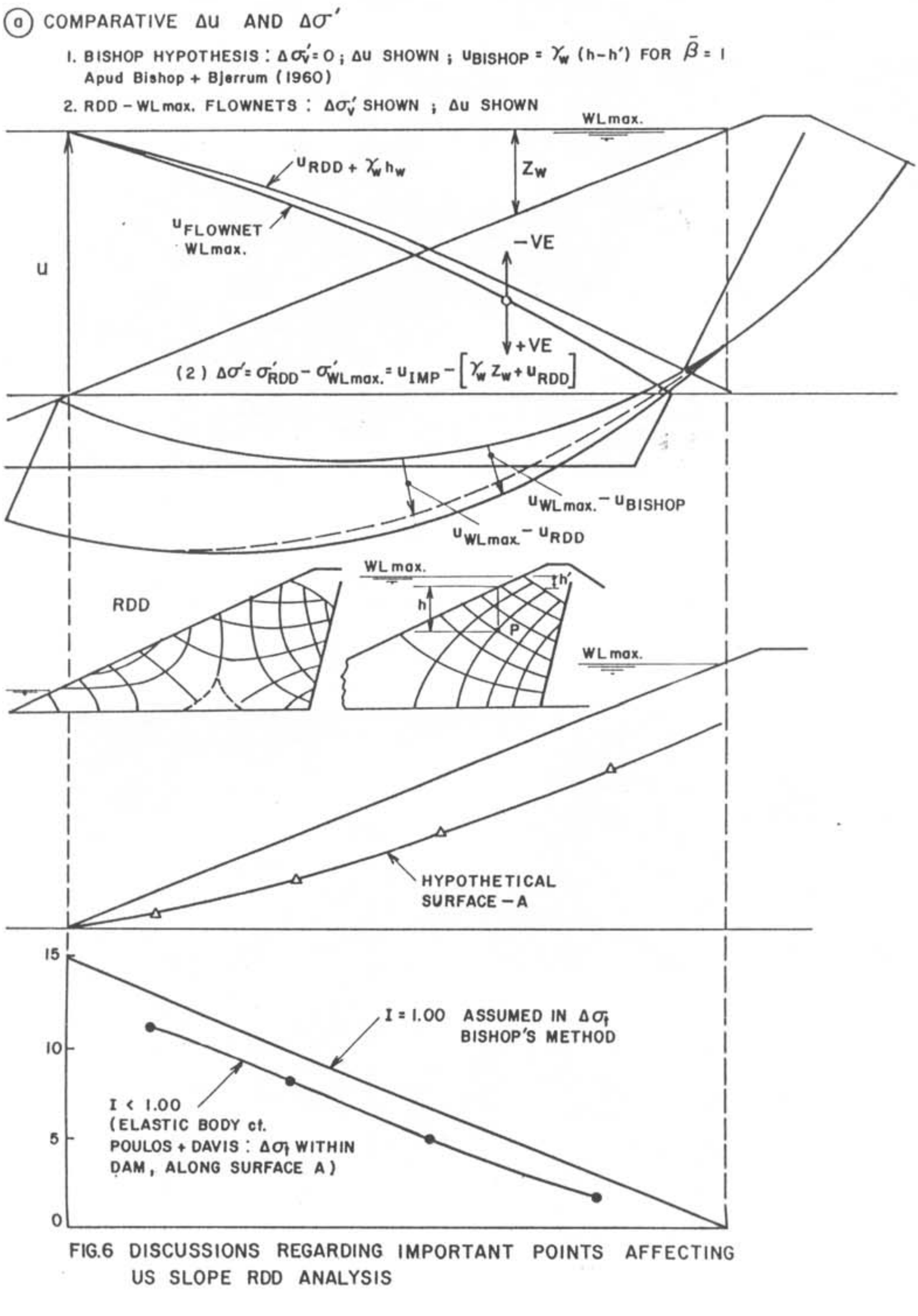

FIG.6 DISCUSSIONS REGARDING IMPORTANT POINTS AFFECTING US SLOPE RDD ANALYSIS

to cross the line). All materials are compressible, pervious, etc..; it is a question firstly of algebra (tendency to expand vs. tendency to compress) and secondly of degree (compress more, or less).

(c) Thirdly, there are some over-simplifications in the σ_t and r_u or $\bar{B}$ procedure that in no way permit incorporating the advantages (or disadvantages) of the most fundamental design weapon of a good dam design, which is the chimney filter and the drainage (u - controlling) features (be they exiting or non-exiting drains, cf. Rankine Lecture). The degree of significance of such consequences varies considerably in different design crossections.

The principle proposed is simple, and quite consistent with the hypothesis of perfectly saturated (incompressible) pore fluid. Tendency to change of flownet is "instantaneous", and what matters are instantaneous pore pressures changes (compare with rheological model of Terzaghi consolidation theory). Therefore we can draw the two flownets, WL_{max} and RDD; the positions of the drainage features are duly incorporated. Once these internal flownet pore pressure conditions are established, based on the rapidly changed boundary conditions, it is quite simple to check what would be the tendencies to change $\Delta\sigma'v$ that determine whether or not the soil elements would wish to behave as compressible. If there is a tendency to compression, obviously there should be a corresponding increased transient pore pressure (due to ΔV). The stability analysis should be based on the RDD flownet (valid for incompressible pores) complemented (at worst) by the Δu due to the compressive $\Delta\sigma'$ (and any consequent shearing ΔV). In other words, the principle is that routine flownets presume incompressible pores and incompressible fluid: therefore, one checks first what would be the tendency, to compress or to expand, in an instant aneously incompressible assumption. Thereupon one immediately concludes whether the material will behave as compressible or behave as dilatant, but in any case superposed on the background of the saturated instantaneous RDD flownet. Fig. 6 exemplifies this principle of stability analysis that is quite generally applicable.

Once again instability conditions are best analysed with regard to changes of conditions superposed on the earlier proven slope obliquities of stress under WL_{max}. Moreover, the occurrence of frequent partial pool drawdowns can be introduced with the appropriate analysis of whether or not the trend is toward gradual small tendencies to compress (and increase strength). The ideal design of crosssection and compacted material would aim at slight compressions under the frequent smaller drawdown episodes, and, hopefully, a tendency to dilate under the most critical drawdown.

However, once again, if construction u_c had been satisfactorily high in comparison with the new $u_{RDD} + \Delta u$, and US stability well established, we should be dealing with a pre-tested slope stability and a perfectly satisfactory FG $\approx 1^+$.

There is considerable evidence, direct and indirect, that US slope instability due to rapid drawdown is much overestimated by presently current analyses, and that any tendency to sliding would tend to be of shallow scoop circles.

7. CONCLUSION

Recommendations are made that stability analyses be used as furnishing nominal FS (or preferably FG) values and that the acceptance levels of such index values be established by great number of non-failure situations, treated statistically. For more critical conditions the only satisfactory approach for guarantee of design is to consider changes of conditions, starting from pretested conditions, preferably more critical than the operational ones.

In a manner similar to the evolution of foundation design as it moved from discussing FS with regard to bearing capacity formulae, to the more fruitful approach of aiming at limiting deformations for avoiding minor damages, the field of dam slope design will only move significantly forward when such countless statistical data are collected, correlating FS or FG vs. Satisfaction Indices SI.

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