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Riesgo en Obras Geotecnicas: Sugerencias Conceptuales y Prácticas

Risk in Geotechnical Works: Conceptual and Practical Suggestions

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SYNOPSIS

Risks in engineering were roughly associated with Factors of Safety FS, for rigid-plastic-instantaneous statics (and dynamics as successive d(static) differentials integrated). The FS nominal index is recognized as an inexorable first step, of naked simplicity, but ipso facto suggesting a mathematical dilemma. Such dilemmas arising from over-simplified hypotheses can be reasoned to wither under physical and geotechnical scrutiny. Case histories analysed under such early hypotheses call for reanalysis for establishing improved statistical data-banks for probabilistic estimations: probabilistic formulations are proliferating, but cannot profitably rest on questionable statistics for closing the cycle of experience. Nature responds to natural selection around FS ≃ 1.00, randomness being linked with temporary, varying, degrees of ignorance. Using routine cases one perceives the special importance, to geotechnical engineering, of working with cause-effect changes ΔFS of nominal factors of safety, a more sensitive and reliable index, under general concept of avoiding the increased dispersions in any empirical graph close to the origin of coordinates. Refinements of analytical solutions of equilibria are of little profit, in comparison with incorporation of stress-strain-time changes of conditions, and corresponding strength equations and limiting deformations. Bayesian advances in the experience-cycle and ulterior decisions, intuitive or probabilistic, are inexorably inductive, with a significant asymmetric probability density function PDF conditioned by differentiated risk--aversion vs. risk-seeking. Emphasis falls on greater attention to geomechanically interpreted physical scrutiny, on physical choices of designs that achieve zero risk by precluding the statistical universe of the failure-types feared; and on humility in improving and tempering the statistically-based probabilistic risk estimations.

1. INTRODUCTION

Most Engineering Decisions and materializing Actions involve some degree of Risk, either of some degree of Unsatisfactory Behavior, or, at the limit, of Failure. Design, Construction, and performance Monitoring have aimed at two fundamental goals of engineering: that is; (1) primarily, averting any catastrophic failure, understood as a major disfunction, uncontrollable and irreversible, occurring under unforeseen and inappropriate conditions and timing, and causing damages so far out of proportion with the project's costs and benefits, that any accepted routines of economic evaluations lose significance; (2) secondarily, within a most fundamental tenet of all engineering endeavours, the optimizing of benefit-cost ratios on the investment. An important additional tenet that I would emphasize in the face of modern recognition of the acceleratingly changed environmental impacts, is (3) the choice of engineering structures which can be foreseen, at least within the prevailing and anticipated surrounding conditions, to increase their safety, and adequate behavior, with time: structures that would naturally tend to deteriorate (and, mind it, most of them do) should be regarded as undesirable as design decisions, since the ever most difficult problem is to predict which is the last straw that would break the camel's back.

One cannot, and is therefore obviously not obliged to, guard against time changes, that may either naturally or by provocation, swerve off of the known and predictable conditions at the time of design, construction and initial commissioning: the recognized dynamics of all things has been well emphasized to impose the periodic reassessment of a given project under the new conditions. Against any Society's guilt complex and/or presumption of guarding against "all" future possibilities of changes of conditions, we might merely quote that "sufficient unto the day is the evil thereof", and that the priority principle, of any Society or being, is survival against the challenges of the present and immediate future, in order to be available to face the ulterior challenges as they might come.

As would be inevitable in line with the progresses of scientific and technological evolution, engineering designs first evolved by pragmatically intuitive creative ideas of physical solutions that "worked", functioned unquestionably. Subsequently, as the uses of the specific structure multiplied, and subconsciously advanced towards the frontiers of impunity, some misbehaviors and failures forced recognition of dominant physical failure-modes, and, for each, engineering-science endeavours concentrated on finding nominal methods of quantification of the failure condition ($FS \equiv 1.00$) and the degrees of safety ($FS > 1$). Accompanying the trends of statistical and probabilistic thinking, during the past 20 years the quantifications have developed progressively along these lines, formulating probabilistic risks in presumed substitution for the nominal Factors of Safety FS. One of the objects of this presentation is to recall that routine treatments of mathematical statistics apply to a constant universe, and not to a rapidly changing one as has been forced upon geotechnique. All technologies begin from enthusiastic analyses of the reasonably homogeneous, simple, idealized, repetitive cases, and gradually go on to facing more complex ones: in analogy, God's infinite wisdom guarantees that children are not called to ponder on Einstein's theory, and common man pragmatizes that "rather than complain of the darkness, we should at least light a match or a candle".

The changes of "universe" in geotechnical engineering have advanced along various fronts, the most successful of which is that of construction methods and ground treatments. However, this presentation's purpose imposes restricting consideration to testing-interpretation-analyses by which the problem-universe has been defined: most of our colleague engineers harbour the delusion that test-characterizations and theoretical analyses have had an appropriate, constant, stand (since the 1950's). Thereupon, mathematical statistics has been the central concern of risk estimations via probability theory. I expose some blatant questions in typical problems, to prove the delusion emphasized. Thenceforth, propose two avenues for rapid and efficient recovery of the backlog, and for optimized forthcoming use: (1) firstly, to analyze principally the changes of Factors of Safety, ΔFS, and correspondingly the changes of risk probabilities rather than the much more uncertain and nominal status-quo conditions; (2) secondly, to apply such methods in backanalyses and reanalyses of existing cases, fitting-in estimated data and parameters wherever necessary. It is hoped that a significant collective effort of theses may be conducted to reassess the multitudes of published cases, for updated statistical universes.

As a sequel the presentation discusses Failures as "accidents", therefore statistically "extreme-value" cases, difficult to handle by frequency-distribution quantifications, but effectively averted by physical changes of the designed structure; and, it is argued that by deterministic changes of statistical universe (Project Design Decisions) one can, indeed, achieve zero risk, without transgressing the mathematical problem of some functions that never reach absolute zero probability.

Regarding misbehaviors, comprising multitudes of cases of (for instance) greater-than-desired deformations, the postulation is that in our development of Bayesian experience criteria we should not be lured by oversimplied mathematical statistics, but should genuinely recognize the dominant interference of INDUCTIVE PROBABILITIES and their inexorable subjective components of risk-aversion and risk-seeking, with consequent asymmetric distribution functions. The principal problem is exposed as the significant (and increasing) separation between the frequency distribution functions (FDF) of analyses of "facts and events", and those FDFs of design decision facing risk, especially in a field that lies naturally close to "failure", and has no freedom of unbiased experimentation, but must draw experience from the very decision-dominated asymmetric universe of PRECEDENTS on earlier projects.

2. EARLY STATICS. REASSESSMENT OF IMPLICIT HYPOTHESES.

In Fig. 1a I reproduce the most classic starting definition of sliding equilibrium of a solid body, employed as a basis of all stability analyses in soil and rock mechanics. The Factor of Safety FS was automatically defined as the RATIO of resisting over mobilizing "FORCES". The problem recently drew the concerned attention of Habib, 1979 (1)*, in his ISRM Presidential address at the Montreux International Conference, wherein he referred to several eminent authors (e.g. Fellenius, Londe, Kovari and Fritz (3), (4), Leichnitz and Natau (2), et al. without any attempt at wider coverage) who had expatiated on the problem. In summary, Habib incorporates the interference of tendencies for deformation in the two opposite directions, demonstrates that "theoretically" there would be two limiting equilibria, and emphasizes that "The notion of Factor of Safety cannot be understood except in association with the method of calculation corresponding to the mechanism chosen or the loading chosen" (cf., Taylor 1948, "... no such thing as the factor of safety .... . When a factor of safety is used, its meaning should be clearly defined"). A frustrating extreme is the contention of Leichnitz and Nasau (2) that "The definition of factor of safety is a question of opinion, and the consequences of the definition chosen must be examined with care". In contrast to the deterministic certainty that prevailed in the 1940-70 period, of a simple and definite criterion of Safety (since FS 1.00 defined as limiting equilibrium of STATICS), the growing dissatisfaction (> 1970) encountered a conceptually BROADENED DEFINITION (e.g. Leonards, Terzaghi Lecture, 1982 "... unacceptable difference between expected and observed behavior") which, however true in recognition of deformation as the culprit ("de-forming" the form or shape functionally chosen), would dismally humble attempts at quantifying experiences. Engineering cannot survive without numbers.

In my opinion, a radical reassessment would seem necessary, and should prove profitable. Firstly one should question, and probably reject, the direct use of the Ratio: not every index (e.g. FS) intuitively defined on first perception of a problem really merits perpetuation without diminishing returns. A ratio is well known to lead to wide dispersions and meaningless values and abstractions, as numbers become small, and denominator tends to zero: the concept of DIFFERENCE

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* (1) References as numbered at the end of the text.

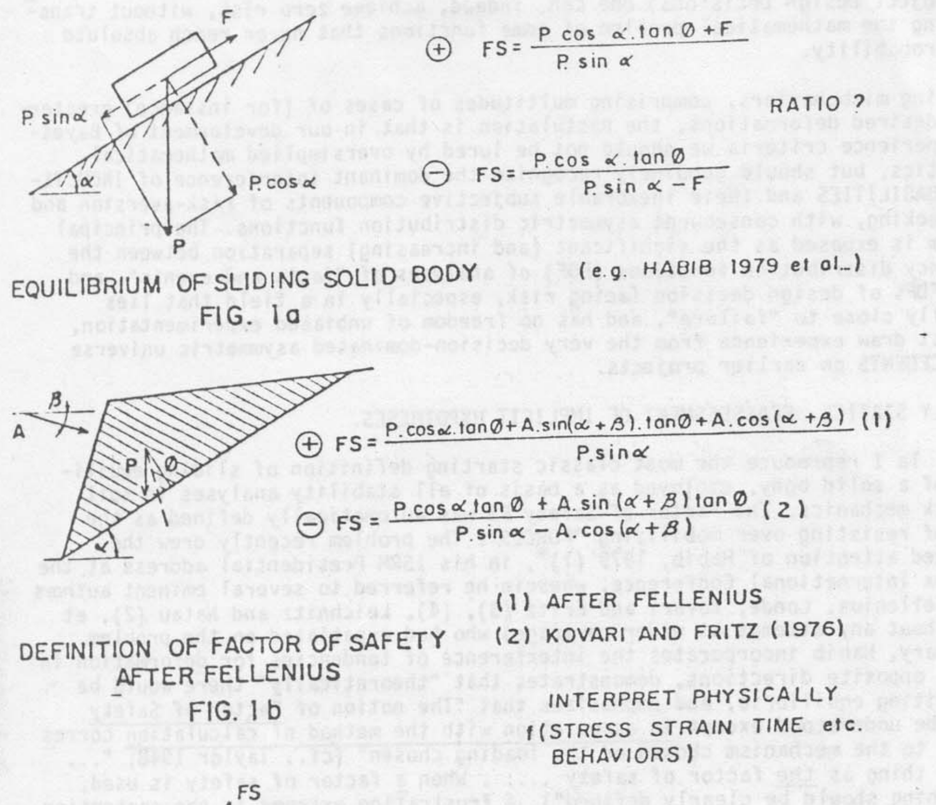


FIG. 1 CONSEQUENCES OF USING TWO DIFFERENT DEFINITIONS OF FACTOR OF SAFETY

(cf. Leonards) is physically more tangible (incremental effects), although the difference of FORCES would have to be adimensionalized (dividing by the "resisting force"?) in order to embrace multitudes of cases. Since conditions tend toward minimized excesses, in Nature and in optimized engineering, the problems of indices as ratios, within close proximity to the (0,0) origin of coordinates, must be averted.

Secondly, the question of the statics-mathematics dilemma simply should not exist in physical reality. The primeval term FORCE is too generic, and only acquires geomechanical meaning through complementary interpretations of stress-

-strain-time behaviors and incremental-rate behaviors. VEGETABLES is a generic category, and one cannot add 5 tomatoes to 6 cucumbers to mention 11 vegetables meaningfully: the need to add Forces as VECTORS, and not scalar quantities, was a milestone, and one wonders why additional qualifications (deformation, rate, etc..) have not arisen in the meaningful composition of the Vectors. In Figs. 1a, b the weight P is a stress-controlled strain-time-independent Force, and we may attribute similar behaviors to the components $P\sin\alpha$ and $P\cos\alpha$. The resisting force $P\cos\alpha \tan\phi$ (Fig. 1a) is inexorably stress-strain-time dependent, and therefore, in analyzing the nature (or significant qualification) of that Force, and its potential for safety, we have to consider the RATES OF MOBILIZATION $\partial F/\partial\varepsilon$, $\partial F/\partial t$ (etc.) with deformation ε and with time t, in starting from the status quo. How can we add significantly two forces that have very different rates of mobilization? The equations reproduced, pertaining to STATICS, incorporate the physical hypotheses of rigid-plastic-instantaneous mobilization, as a basis for permitting the trigonometric supersimplification. In short, neither of the equations should be meaningful since the top one would assume that the Force F would have identical rates of mobilization as the strain-time-dependent resisting force $P\cos\alpha \tan\phi$, and at the other extreme if the force F has identical strain-time-independent rate of mobilization as the mobilizing Force $P\sin\alpha$, its direct subtraction from the mobilizing force (in the denominator, presuming the Ratio maintained) is more realistic.

Note that in Rock Mechanics there has, since long, been a recognized distinction between SOFT and HARD loadings, a definite advance in comparison with the structurally-inherited single type of LOAD in Soil Mechanics, but still unrealistic as all dichotomic qualifications in comparison with the continua of variations.

At any rate, it seems imperative that all hitherto limit equilibrium FS calculations of projects should profit from recalculation. The principal reason why the problem has not surfaced more blatantly is because, as seen in Fig. 1c, with F/P or A/P ratios of less than about 0.25 the two equations give very similar results. Most stabilization designs symbolized by Figs. 1a, b have not required more than about 20% of incremental stabilizing force. The importance is presently one of concept, and should generate aberrations as the forces (F, A) increase.

If a real mobilization rate of F could be subdivided into three parts (in the simple example), one of identical mobilization to the $P\sin\alpha$ force, one of identical mobilization to the $P\cos\alpha \tan\phi$ force, and a final component to make up the total, there would be justification for the first part to be subtracted from the unstabilizing force (presently in the denominator), for the second part to be added to the resisting force (in the numerator), and the third part to constitute the additional favourable or unfavorable handicap. The correspondingly calculated more realistic FS values would lie between the two curves of Fig. 1c.

3. GENERAL CONCEPTS BORROWED FROM STRUCTURES. PROBLEMS PECULIAR TO, AND AGGRAVATED IN, GEOTECHNIQUE. LESSON CONCERNING VALUE OF "COMMON EXPERIENCE" IN COMPARISON WITH NOVELTY SOPHISTICATED COMPUTATIONS.

A question that often arises among Civil Engineers is whether geotechnicians are not mystifying when they claim to face problems quite peculiar to, and aggravated in, geotechnique. One must examine such allegations deeply and with convincing objectivity, not merely because the Limit Equilibrium Statics (and Failure Conditions) of geotechnique derived directly from structural engineering, but also because the two professions continue irrevocably bound to each other, through all misunderstandings, until death do them part.

I contend that there are very significant peculiarities and aggravations in geotechnical engineering, and that the more rapid advances of statistical and probabilistic computations in the fields of structures have increased the misunderstanding and call for divorce, principally because of the geotechnicians' failing in reshaping their own analyses to incorporate the great advances of researches in soil behavior, in stress-strain-time-strength differentiated characterization. Early, conventional, soil mechanics adopted homogeneous isotropic "cohesionless" soils with a constant, single, ϕ value, "cohesive" soil with a constant single c value, and generic idealized soils with single, constant (c, ϕ) parameters, irrespective of stress-strain-time history, stress--strain in-situ conditions, and loading-deformation conditions, etc.: 40 years of research have completely overthrown those simple abstractions in most soils and loading conditions, preserving the pure sands under static loadings as the only material in which the differences between real and idealized behaviors are acceptably small.

In due respect of the readers' knowledge and time, I limit my justification of the above contention to the Chart A summarizing some keyword statements that should evoke the familiar recognitions.

Regarding statistics-probabilities-risks-safety in all geotechnical engineering problems we note the routine emphasis of the need to advance from determinism to the new proposals because of: (a) uncertainties (dispersions) in material and geotechnical parameters; (b) uncertainties in loads (N.B. one should add loading-effects, because structural engineering assumes much too uniform a cause-effect relationship on loadings, for acceptable use in most soils); (c) uncertainties in mathematical modelling and methods of analysis. Thus, there have been growing and ample recognitions of uncertainties, systematic and/or random in input data and hypotheses for geotechnical problems. Since probabilities are derived from prior statistics of cases and events, I am concerned with the geotechnician's ability to note and quantify the events, and to incorporate (deterministically) the well-established important revisions of theoretical analyses that determine the statistical universes to be subjected to analyses of uncertainties. In an age of progressive subspecialization, in which the soil-behavior specialist is not the soil-mass structural analyst, nor the statistics-probability analyst, we must call for deterministic certainty revisions in order to give meaning to the statistical uncertainty formulations within realistically improved universes defined.

Some extensively discussed failure problems have been: (1) embankments on soft clays; (2) bearing capacity of footings; (3) deep supported excavations; (4) landslides.

On the first one, which should be the simplest of equilibrium problems in the rapid-loading case, I refer the reader to the very educational case-history of the M.I.T. performance-vs.-prediction challenge (cf. Hynes and Vanmarcke, 1977 (7)). Figs. 2a, b reproduce the sobering lessons drawn regarding too rapid advances of proposed testing-computational advances (the Eureka publish-or--perish syndrome), without sufficient data-banks for closing the cycle of experience on prototypes. The audience's predictions by "feel" (semi-quantitatively judged under "Bayesian" mental experience-processing) were, on the whole, better than those of the highly specialized purposeful 10 challenge-respondents who relied on methods so new that no experience had been acquired on adjustment factors of computation-to-reality. The better result was reflected not merely in the (fluke) average audience's on-the-dot prediction, but principally because 62% of their predicted ranges included the measured value, whereas the (over-optimistic or over-pessimistic, but inherently self-confident) predictors' responses did not include the measured value in any single case.

CHART A

1. SAFETY IN GEOTECHNICAL ENGINEERING
   1940-'70 SIMPLE, DEFINITE: FROM "STRUCTURES",
   FS ≡ 1.00 FOR STATICS, EXACT FAILURE.

   1970 AVOID "UNACCEPTABLE DIFFERENCE BETWEEN EXPECTED AND OBSERVED BEHAVIOR". HOW TO QUANTIFY?

2. HISTORICALLY FROM STRUCTURES, STATICS.
   RIGID-PLASTIC FAILURE ≡ DETERMINISTIC PHYSICAL COLLAPSE.
   SAFETY, FS ≡ RATIO OF RESISTANCES, AVAILABLE/MOBILIZED
   IN STATICS, NO DEFORMATION

   FS DECISION LEVEL < ACCEPT / REJECT → SET BY CODE, BOOK, PROFESSOR

3. PROBLEMS: NATURAL COMPLEXITY, DISPERSIONS. NATURE'S PROCESS MODELS FOLLOW NATURAL SELECTION, WITH SOME RANDOMNESS.
   CONTINUUM: STATISTICS OF AVERAGES? OR CONFIDENCE LEVELS ?
   DISCONTINUITIES CONDITIONING BEHAVIOR.
   MISBEHAVIORS BY PRE-FAILURE DEFORMATIONS, STATISTICAL UNIVERSE.
   FAILURES: ACCIDENTAL: EXTREME-VALUE STATISTICS.

4. WHAT SAFETY INDICES OR RATIOS FOR SUCH IMPORTANT FAILURES AS: PIPING ? DEFORMATION ? CRACKING, TENSION CRACK = = F(HOMOGENEOUS STRAIN) OR WEAKEST LINK ? SOIL "COLLAPSE", OR HEAVE ? QUICK-CLAY SLIDES ? SEISMIC LIQUEFACTION ? MUDFLOW, AVALANCHES ? CHEMICAL CHANGES ?

5. EXTREME-VALUE EPISODES (NATURAL, HUMAN ACCIDENTAL, FOREFRONT OF THEORY) FURNISH PHYSICAL FAILURE-MODE, BUT NO BAYESIAN STATISTICAL EXPERIENCE.

6. FS STRICTLY NOMINAL, PROGRESSIVE VARIATION, BIAS.

7. WELCOME NOVELTY, PROFITABLY ORIENTED. NEED ACCURACY OF ESTIMATES, AND CONSISTENCY OVER SUFFICIENT PERIOD TO ESTABLISH FREQUENCY OF EXPERIENCE. LIMIT Δ(CHANGE) PER SUCCESSIVE CASE.

8. INDISPENSABLE INDUCTIVE PROBABILITY IN DECISION PREFERENCE.

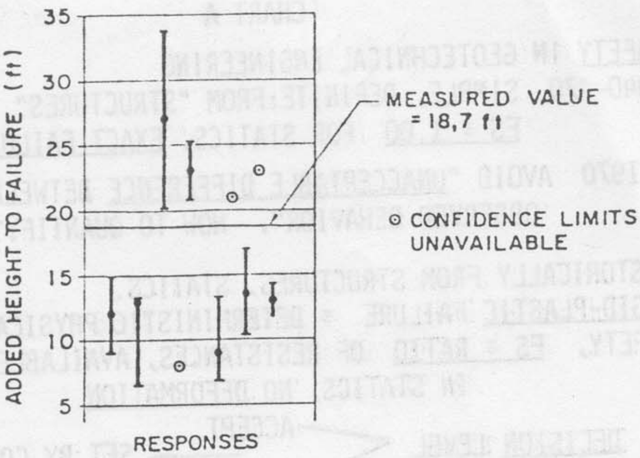


FIG.2a - PREDICTORS' INTERQUARTILE RANGES OF ADDED HEIGHT FAILURE

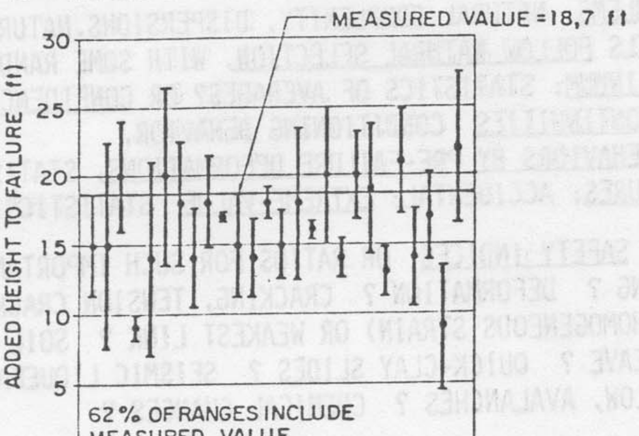


FIG.2b - AUDIENCE'S INTERQUARTILE RANGES OF ADDED HEIGHT TO FAILURE

FIG. 2 HISTORIC CASE OF PREDICTION OF EMBANKMENT ON SOFT CLAY

(cf. HYNES AND VANMARCKE, 1977 M.I.T.)

There is wisdom in civil engineering's recommendations of (1) not changing design procedures too rapidly without accumulating experience, and handing the baton with sufficient superposition from one runner to the next in the relay--race (2) respecting the human experience that is gained principally at what is noted and counts, the complex lumped-parameter result, rather than numerous analytical parameters indispensable to the computations (3) not favouring too rapid a (Δ change) from one prototype to the next.

On the other three routine problems listed, the need for significant revision of the current geotechnical methods of theoretical analysis has been briefly mentioned in various publications (e.g. cf. de Mello 1969 (10), 1972 (18), 1984 (16), etc.), and is emphasized herein. A brief discussion follows.

3.1. Bearing capacity failure under footings.

Fig. 3 summarizes the undisputable difference between the idealized single (c, ϕ) behavior of the loaded soil as postulated by Terzaghi (e.g. 1943), and preserved by all subsequent would-be refinements of bearing capacity analytical theories published, in comparison with the reality of at least three conceptually different ϕ values at play (and corresponding c values of the respective strength equations). Factors of safety of allowable pressures FS = $\sigma_{rupt.}/\sigma_{all.}$ statistically analyzed under the over-simplified starting theory would hardly be of any use for probabilistic risk formulations.

The shape of the failure surfaces, Figs. 3(a)-(d), was theorized from plasticity theory, as determined by the "intrinsic ϕ value", i.e. essentially as given by the effective stress virgin ϕ' from Consolidated-Drained CD tests: let us say, in many a medium plasticity clay $\phi' \approx 28^{0}$. Meanwhile, in the Statics of the derivation there is another ϕ value as representing the $ds/d(\gamma'z)$ variation of in-situ strengths with depth, considering specimens positioned at points 1, 2, 3 etc..: in a critical quick loading condition that would minimize $\sigma_{rupt.}$, this $\phi = ds/d(\gamma'z)$ would be a Consolidated-Undrained CU value for specimens consolidated under in-situ (overburden) stresses; let us say, a frequent value of $\phi_{app.}$ (apparent), expressed in terms of the normal consolidation stress, of the order of 12-15^{0} (?). Finally, there has to be, in the composition of forces, the important ϕ value to reflect the $ds/d\sigma$ generated by the very loadings from the footing: in a generic critical condition in a saturated soil, this would be an Unconsolidated-Undrained UU value, which would frequently be close to 0^{0} under quick loading. Since no formulae have been developed for inclusion of such differentiated strength parameters, an engineering expedient must be applied: for the shape of the failure surface one can forego the predetermined plasticity theory impositions and use minimized sliding mass formulations such as Janbu's etc.., with due respect to appropriate weighted-average (stress-strain-rate-path) equations for the strength parameters. For the appropriate strength parameters one would use some judiciously weighted values between the CU and UU values, depending on the relative importances of (a) depth of failure surface, leading to dominance of CU values (b) stress level due to the footing's loading, leading to dominance of the UU values. Obviously for different trial Janbu analyses, bigger footings and deeper sliding surfaces would start with values closer to CU cases, and, as higher $\sigma_{rupt.}$ values tend to be indicated, the applicable strength values would tend closer toward UU values.

If foundation Failures, Safety, Risks are related to strength limitations (and not deformation) both deterministic and statistical-probabilistic formulations must begin by a significant geotechnical revision. Incidentally, in anticipation of discussions on limiting deformations we note, in passing, that historically the engineering solution (Terzaghi's "local failure conditions") was to apply a reduction coefficient of 1.5 to the strength parameters, in confidence that the plasticity-theory bearing capacity formulations offered, at the time, an acceptable solution.

3.2. Deep supported excavations.

In the case of active pressure calculations of deep excavations (very frequently deeper than 25m etc.) the above-mentioned prior geotechnical problem can be much more crucial because the projects aim at low FS values, the dominant strength equation is the in-situ self-weight one, and the important engineering

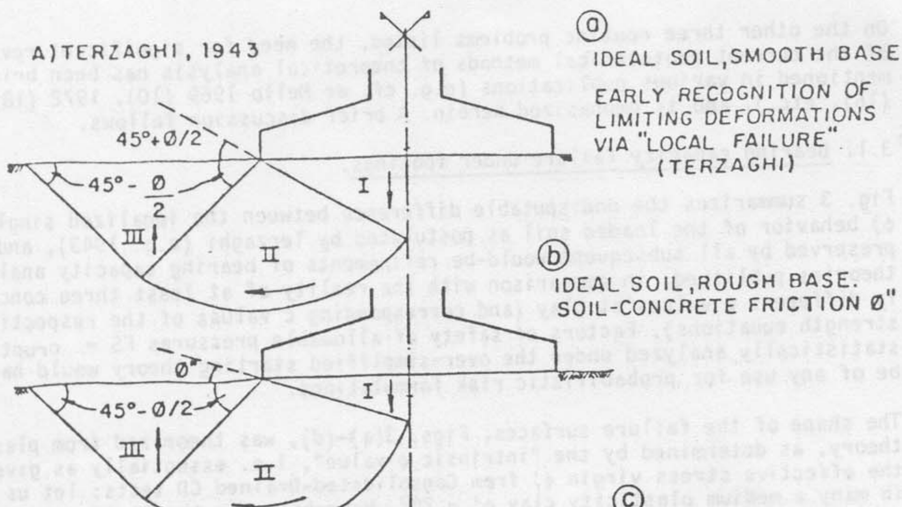


FIG. 3 - REAL VS IDEALIZED "Ø" VALUES IN BEARING CAPACITY FAILURE OF FOOTING

contribution is to be achieved by applied loads with very curious possibilities of ds/dσ values. There are, also, important ds/dt (time) and ds/dε implications, which radically alter the nominal (c, ϕ) values routinely extracted from conventional triaxial tests.

The first decision in excavations of natural soil horizons concerns the separation of the strata or horizons to consider as homogeneous units. The principal concerns for the problem at stake are (only) a reasonably uniform density, and a continuous in situ strength profile within each. In any soil profile many methods have been recommended for testing of undisturbed (block) samples, or in-situ in order to determine the in-situ strengths, but I submit that the recommended concept must be that of carefully (avoiding de-structurations) consolidating (with some definite secondary compression) to a minimum of 3 different stress conditions, analogous to, but higher than, the block's in situ stresses, thereby determining the $\partial s/\partial\sigma_c$ trend, and extrapolating backwards to the presumed in situ consolidation stresses. (cf. de Mello 1972 (18)).

In separating the representative units, traditional subdivisions as to sands or clays etc. merely matter because of the stress-release and loading (generally different) $\partial s/\partial\sigma$ under $\partial\sigma/\partial t$ conditionings, and not with respect to (c,ϕ) parameters presumed applicable from soil classifications, since a highly preconsolid ated clay under low stresses (high OCR) may well behave with a high $\partial s/\partial\sigma$ type of ϕ. The ∂s effects must be recognized as mobilizable only strictly in response to volumetric and deviator strains ($\partial s/\partial t$), which implies the additional non--instantaneous responses of $\partial\varepsilon/\partial\sigma$ and $\partial\varepsilon/\partial(\sigma_1 - \sigma_3)$. The differences between the (c, ϕ) parameters judiciously attributable under such reasonings, and those of conventional triaxial tests "averaged for several samples within the profile" can be astounding.

A case extracted from authoritative publications (e.g. Jamiolkowski 1982 (34)) is represented in Figs. 4, and is used to exemplify the problem, and the solution proposed. The in situ profile permits treatment of the active earthpressure P_A analysis as for a single stratum: however, the statics of the excavated "wedge", if rapid, can only be associated with the (c, ϕ) values of about (0-3 t/m^2, 5^o-21^o) corresponding to the "normal" (σ_{cn}) consolidating stress due to $\gamma' z$. The generalized practice, catalysed by the oversimplified historic theories (Coulomb etc.) would be reasonably valid for sands, in which all ϕ values (of $\partial s_{is}/\partial\gamma' z$ and of UU, CU, CD triaxial tests) would coincide: but the use of CU, CD values would impose questioning, regarding what $\Delta\sigma$ is presumed to be at play (none ?) in generic cases. The abstract calculation of P_A as the force required for limit equilibrium continues to be valid only with regard to the $\partial s_{is}/\partial\sigma_c'$ value of $\approx 6{,}5^o$, since no physical applied force is at play. If, however, a real force P is applied, the ∂s generated by it has to be judiciously considered with respect to the consequent stabilizing effect on the wedge, being (a) rapidly, that derived from the UU tests, (b) slowly, that derived from the CD tests, (c) in no common case, that derived from CU (loading ?, unloading ?) tests. In theory the CU test results were meant to have been analogous to the $s_{is}/\partial\sigma_c$ information. Any vertical unstabilizing loading on the surface would have to be dealt through the (UU) ϕ.

Obviously, before discussing SAFETY, RISKS, FAILURE-PROBABILITIES, etc.. of such structures meaningfully, we should reassess case-histories under realistic and judicious geotechnical parameters. The use of single (c, ϕ) parameters per problem continues to be practical, but the choice of values would have to depend on an assessment (by stress-strain-time evaluation) of the weighted values most appropriate, a heavy responsibility on the GEOTECHNICIAN. Since Truth (nominal ?) is only one, and errors many, once determined, the statistics on Truth should yield much less dispersion.

One point of curiosity is the equilibrium of the really "rigid-instantaneous" retaining structure, for which the P forces applied are generally higher (e.g. 1.3 P_A) for a desired FS (and, hopefully, for control of deformations, presuming to preserve "at-rest" conditions). The statics of the diaphragm wall is fulfilled at two possible points of the soil's stress-strain curve (D_1, D_2) straddling the peak: both at smaller strength mobilizations, either prior to peak (smaller deformation) or post-peak (greater deformation, strain-softened strength). The historic and/or construction-period deformations thus lead to

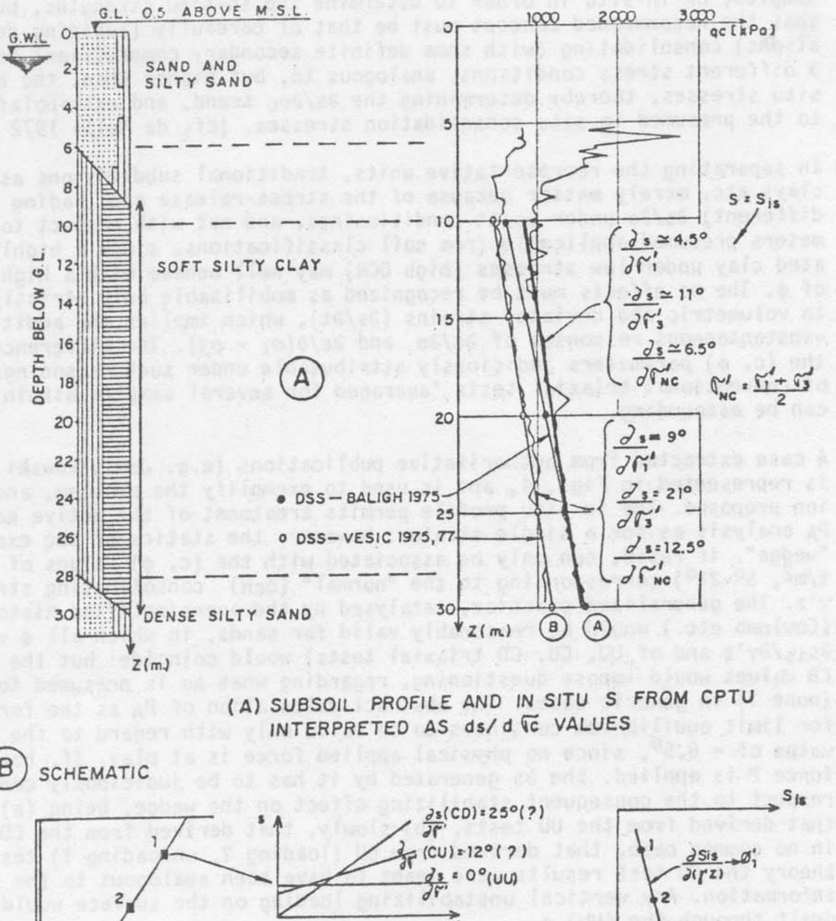


FIG. 4 STRATA SUBDIVISION FOR ACTIVE EARTHPRESSURE P_A : COMPARISON OF (C, Ø) PARAMETERS

equivalent statics with radically different failure risks, because of the prospective follow-up of rate of mobilization of strength (cf. de Mello 1977, (35)).

3.3. Landslide slope stability.

The third broad category of problems mentioned, that of landslides of major slopes due to rainfall infiltration, has been discussed similarly, as requiring a revision in geotechnical bases, before meaningful statistical FDF data can be analyzed for PDF computations on failure risks related to the nominal FS values of routine, but corrected, sliding stability computations. As summarized earlier (e.g. de Mello 1984 (16), 1985 (36) etc.) some major points for revision include: (a) the importance of any thin planar geologic discontinuity in determining the sliding surface, the frequent non-circular surfaces, and the differentiated stress-strain-rate-failure behaviors at different stress levels leading to progressive failure provocations, tensile first etc.., such that the conventional rigid-body statics would not apply; (b) the correction of static equilibria based on compositions of gravity and seepage effective stresses rather than the simplifying conventional hypothesis (valid in linear strength equations and rigid-body equilibrium) of total stresses minus boundary neutral (membrane hypothesis), a correction which in a particular case would change a conventional FS ≈ 1.33 to a revised FS ≈ 0.81 (16); (c) the emphasis that what matters is rainfall infiltration (incorporating suction, cracks etc.), and not intensity of precipitation because, beyond a certain time and intensity of rain, all excess flow unabsorbable by the infiltration flownet (not necessarily saturated) yields excess runoff; (d) the significant conceptual errors that (d.1) effective stresses along the sliding surface, during rapid failure movement can be deduced from u values interpolated from flownet piezometers within surrounding soil mass, i.e., would not include the shear-remolding high transient excess pore-pressures, (d.2) at failure, the forcing of equivalence of the static equilibrium to match FS ≡ 1.00, an error that seems to have been associated with all back-analyses hitherto (cf. Fig. 5A).

After applying the geotechnical corrections of (a), (b) and (d.1), the analyses of failure cases under corrected hypotheses (c) would constitute appropriate causes (process model culprits for the landslides), and, with some judiciously corrected hypotheses (d.2), we would really obtain cause-effect ΔFS data; and the failure condition would imply having gone from an initial status FS_{in} (cf. Fig. 5A) through the FS ≡ 1.00 transient status to a final $FS_{fi} < 1.00$ condition. Presumably the greater the ΔFS and the lower the FS_{fi}, the faster would be the sliding movement.

Once again, the data-universe under present nominal, routine, geotechnical analyses, could only lead to biased and buck-shot scattered FDF of landslide cases, therefore inappropriate for Design Decisions, either on deterministic FS values, or on risk-probability assessments.

4. REDUCTION FACTORS ON STRENGTH PARAMETERS. DIFFERENTIATIONS AS PER DISPERSIONS. TENTATIVE IMPROVEMENTS. SIGNIFICANT CONDITIONINGS, STRESS, STRAIN, OR TIME (?), TESTS VS. PROTOTYPES.

Ever since the ϕ-circle method (cf. Taylor) of stability analyses, the artifice of separate mobilizations of presumably independent (c, ϕ) parameters had occurred, as an artifice. Bishop's, 1955 (5), stability analysis employed factorially "reduced strength" parameters, for improved statics. Generally the analyses were considered validated on assuming FSc ≡ FStanϕ, concomitant mobilization of "cohesion" and "friction". The question arises as to the PROFESSIONAL SIGNIFICANCE of the momentous efforts spent in analytical improvements of the slope limit equilibria. Fredlund 1984 (13) produced a

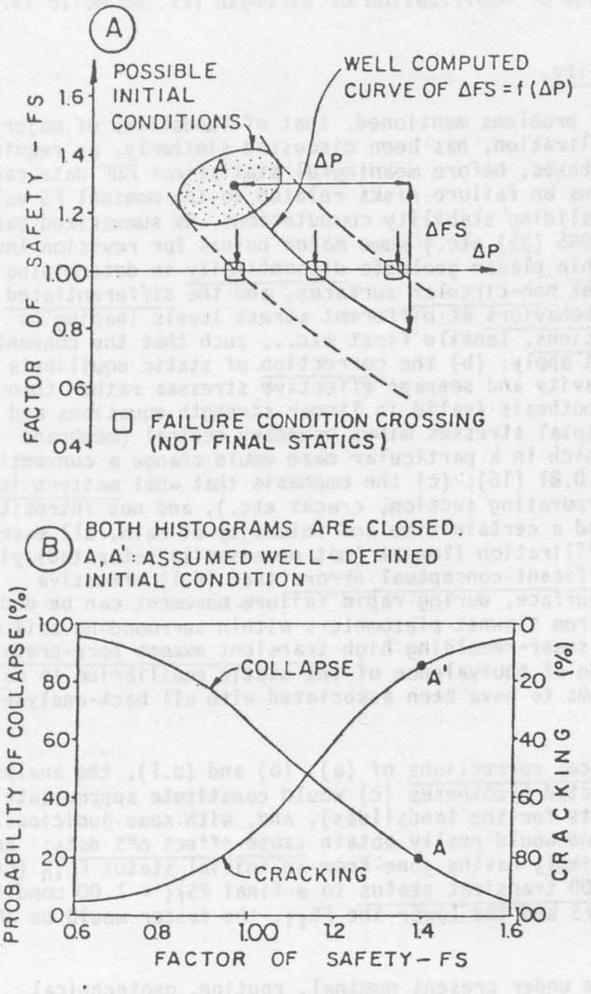


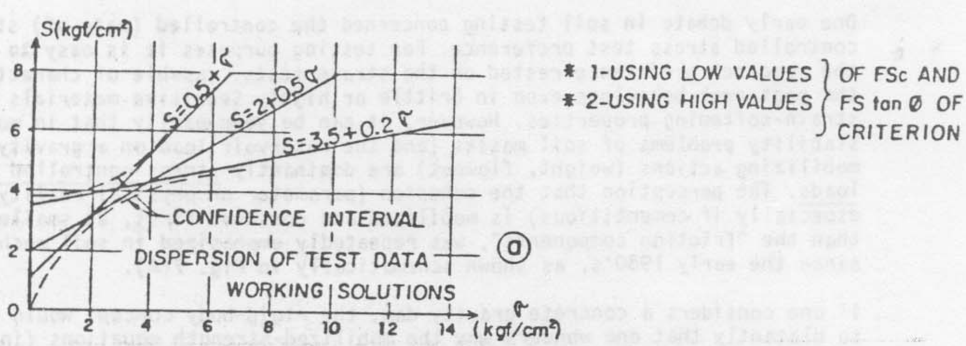
FIG.5 SCHEMATIC INTERPRETATION OF FAILURE CONDITION AS BOUNDARY CROSSING, NOT FS=1.00 EQUIVALENCE

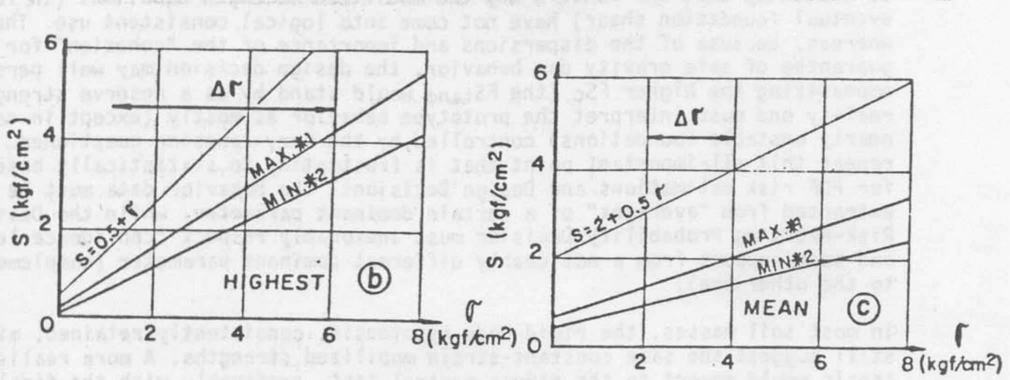
widely-embracing paper which proves that with the current idealizations (e.g. linear strength equation, etc..) a given case would yield results as close as: Highest upper bound FS = 1.45, Janbu generalized FS = 1.37, Fellenius FS = 1.30. One could readily agree that this avenue of refinement could be abandoned. The current and possible DISPLACEMENT FORMULATIONS (e.g. FEM) were not included in that paper.

In Fig. 6 I submit schematically my reappraisal of a criterion that I espoused through several years (~ 1970-80) in rock mechanics of gravity dam foundations and slopes, having learnt it from Manuel Rocha, and considered it very logical and professionally accepted at first sight. In gist, for the purpose of Design Decisions, the criterion proposes use of much bigger partial FS values on c than on tanϕ, observing quite clearly the much wider dispersion of the first than of the second, in typical Mohr diagrams of test results.

On closer look, however, we see in Fig. 6(a) that depending on the stress-ranges of the test results, and to prevail in the prototype, coupled with the Confidence Interval dictated by the test dispersions, the linear equations could easily determine widely different maximum and minimum linear equations around the nominal mean Mohr-Coulomb equation. Thereupon in Figs. 6(b), (c), (d) we obtain two extremes of the possible allowable mobilized Strength Force, using the recommended different FS_c and $FS_{\tan\phi}$ values: the resulting difference as tabulated in Fig. 6(e), would correspond to as much as a 2.58/1.00 ratio of overall FS values for the structure. The separation of the total peak shear strength into independent cohesion and friction parameters has no physical significance: a given strength test result can be expressed either with a low cohesion and high friction or vice-versa. Much greater physical significance should be attached to hypotheses of failures at constant-

1) COHESION, FRICTION SEPARATELY CONSUMED ?
2) BISHOP'S "REDUCED STRENGTH EQUATION" ?





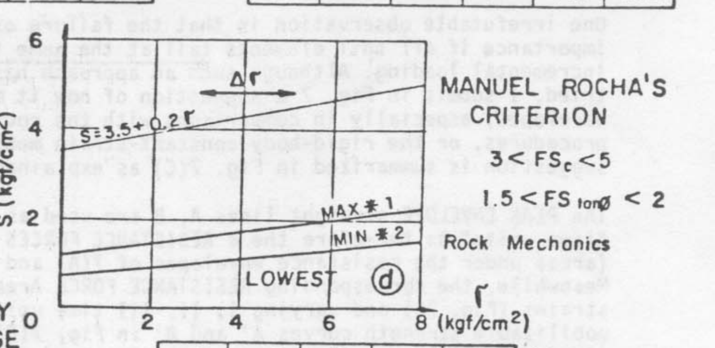
	C_{FS}	$\tan Ø_{FS}$	$\bar{S}$	$\Delta\sigma$	FS
MAX.	0.17	0.67	2.84	6.0	22.7
MIN.	0.10	0.50	2.1	6.0	16.8

	C_{FS}	$\tan Ø_{FS}$	$\bar{S}$	$\Delta\sigma$	FS
MAX.	0.67	0.33	2.0	4.0	16.0
MIN.	0.40	0.25	1.4	4.0	11.2

$\bar{S}$ = AVERAGE SHEAR STRESS FOR $\Delta\sigma$ INTERVAL

$\Delta\sigma$ = STRESS RANGE OF TRIAXIAL TEST (MODEL)

FS = FACTOR OF SAFETY FOR STRESS RANGE BETWEEN 0 AND 8 kgf/cm² (PROTOTYPE)



	C_{FS}	$\tan Ø_{FS}$	$\bar{S}$	$\Delta\sigma$	FS
MAX.MAX. (b)	0.17	0.67	2.84	2.0	22.7
MIN.MINIM. (d)	0.70	0.10	0.10	2.0	8.8

(e) FROM (b) and (d)

→ 2.58/1.00

FIG. 6 DESIGN SUGGESTION OF DIFFERENT FSc, FS tan Ø VALUES, CRITICISED

-strain (rigid body) or constant-time (significant failure mass displacement at a given moment).

One early debate in soil testing concerned the controlled (rate of) strain vs. controlled stress test preference. For testing purposes it is easy to see that the research preference rested on the strain tests, capable of characterizing the post-peak behaviors even in brittle or highly Sensitive materials of sharp strain-softening properties. However, it can be seen easily that in most stability problems of soil masses (and the reservoir load on a gravity dam) the mobilizing actions (weight, flownet) are dominantly stress-controlled soft loads. The perception that the cohesion (parameter or physical reality, especially if cementitious) is mobilized and consumed first, at smaller strains than the "friction components", was repeatedly emphasized in soil mechanics ever since the early 1950's, as shown schematically in Fig. 7(A).

If one considers a concrete gravity dam, the rigid-body concept would stand out so blatantly that one wonders why the mobilized-strength equations (in facing eventual foundation shear) have not come into logical consistent use. Thus, whereas, because of the dispersions and importance of the "cohesion" for guarantee of safe gravity dam behavior, the design decision may well persist emphasizing the higher FS_c (the $FS_{\tan\phi}$ would stand by as a reserve strength), in reality one must interpret the prototype behavior as mostly (except in cases of nearly unstable foundations) controlled by the very cohesion questioned. I shall repeat this all-important point that is frustrating to statistically based FDF for PDF risk estimations and Design Decisions: the behavior data must be extracted from "averages" of a certain dominant parameter, while the Design Risk-Averting Probability Decision must inexorably respect "confidence levels" and seek support from a noticeably different dominant parameter (complementary to the other one)!

In most soil masses, the rigid-body hypothesis, consistently retained, might still suggest the same constant-strain mobilized strengths. A more realistic thesis would revert to the stress-control test, preferably with the final stress increments applied in analogy to the expectation on the prototype (which is why we wisely fear final big and rapid loading increments in our projects). Fig. 7(B) presents schematically a set of such results: note that the strength envelope results somewhat lower than in the strain-control Fig. 7(A).

One irrefutable observation is that the failure of a large soil mass assumes importance if all soil elements fail at the same time, under stress-control incremental loading. Although such an approach has never, to my knowledge, been tried, I submit in Fig. 7 a suggestion of how it might be put forward and developed, especially in comparisons with the routine peak envelope strength procedures, or the rigid-body constant-strain mobilized strength equation. The suggestion is summarized in Fig. 7(C) as explained below.

The PEAK ENVELOPE straight lines A, B are used as independent of strain ε and "time units" t: therefore the s RESISTANCE FORCES (ordinate) are horizontal (areas under the resistance envelopes of 7(A) and 7(B) within the $\Delta\sigma$ range). Meanwhile, the corresponding RESISTANCE FORCE Areas under varying 1%, 2%, 3% strains (Fig. 7A) and varying I, II, III time units (Fig. 7B) determine the mobilizable strength curves A' and B' in Fig. 7(C). The starting condition, with tangential force T, is arbitrarily taken at point R, at a given % strain and nominal time, and obviously lies on the stress controlled soft load curve B'. If an incremental tangential force ΔT is applied, the instantaneous assumption would carry it "vertically" to the past-failure condition $(T + \Delta T)_1$, but any realistic strain-time effects would schematically follow some trajectory representable by an inclined line to past-failure points such as $(T + \Delta T)_2$. Both points are shown well past failure under all criteria. Depending on the stress-

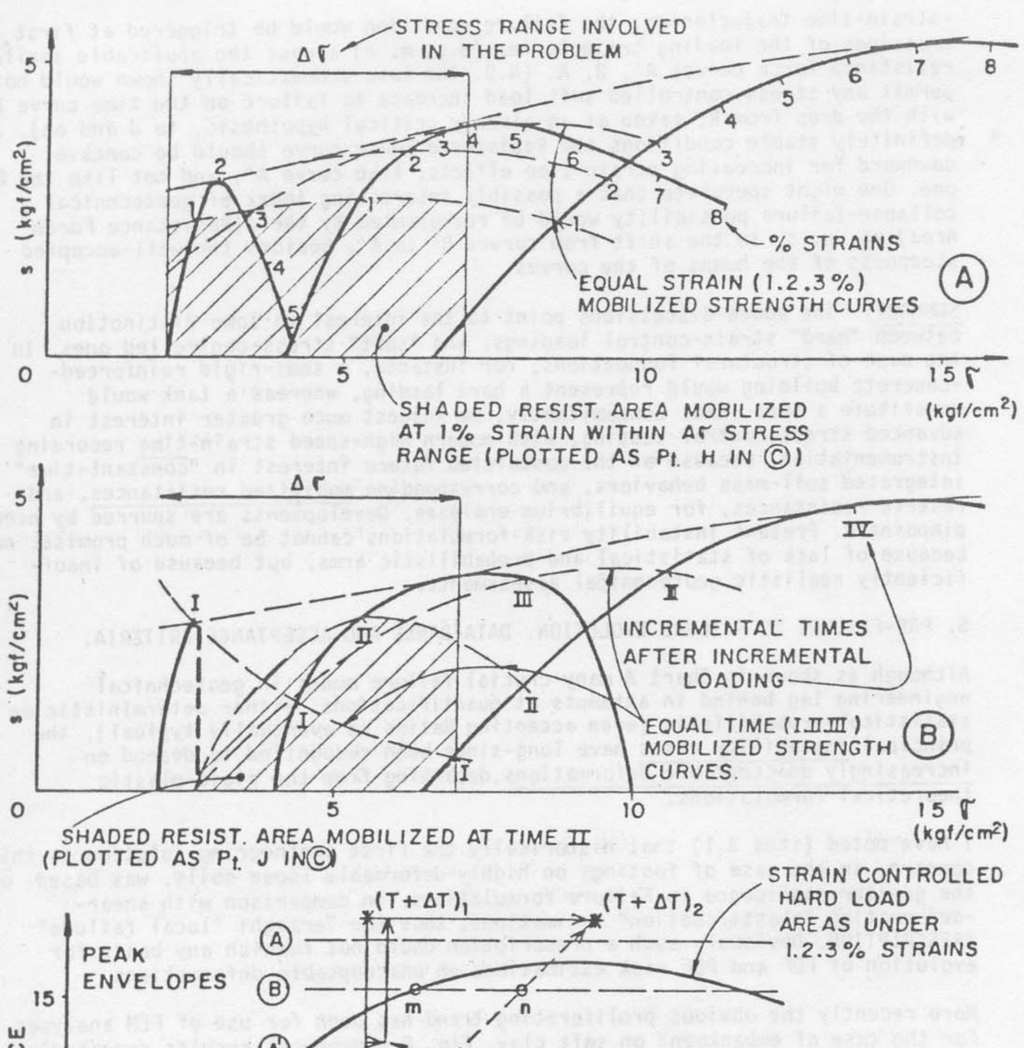


FIG.7 - COMPARATIVE FS CONDITIONS, STRAIN VS. STRESS CONTROL, RIGID BODY, ETC.

-strain-time trajectories, the failure condition would be triggered at first crossings of the loading trajectories (e.g. m, n) across the applicable available resistance force curves A', B, A. (N.B. The case schematically shown would not permit any stress-controlled soft load increase to failure on the time curve B', with the drop from R, taken at an already critical hypothesis, to J and on). In definitely stable conditions the Resistance Force curve should be concave downward for increasing strain-time effects, like curve A', and not like the B' one. One might speculate that a possibly interesting index of geotechnical collapse-failure possibility would be represented by the Δ(Resistance Force Area) vs. ε, t, in the shift from curves B' to A', besides the well-accepted steepness of the humps of the curves.

SUMMARY. The above discussions point to the interest in some distinction between "hard" strain-control loadings, and "soft" stress-controlled ones. In the case of structural foundations, for instance, a semi-rigid reinforced-concrete building would represent a hard loading, whereas a tank would constitute a soft load. Concomitantly, we suggest much greater interest in advanced stress-control testing, with modern high-speed strain-time recording instrumentation, because of the postulated future interest in "constant-time" integrated soil-mass behaviors, and corresponding mobilized resistances, and reserve resistances, for equilibrium analyses. Developments are spurred by needs pinpointed. Present instability risk formulations cannot be of much promise, not because of lack of statistical and probabilistic arms, but because of insufficiently realistic geotechnical assessments.

5. PRE-FAILURE TO FAILURE EVOLUTION. DATA-BANKS AND ACCEPTANCE CRITERIA.

Although as shown in Chart A many crucial failure modes in geotechnical engineering lag behind in attempts at quantifications, either deterministic or statistical-probabilistic (even accepting Ratios as eventually typical), the principal instability cases have long-since been recognized to depend on increasingly unacceptable deformations,departing from the rigid-plastic theoretical formulations.

I have noted (item 3.1) that historically the first engineering solution to this concern, in the case of footings on highly-deformable loose soils, was based on the greater confidence in Failure formulations, in comparison with shear-deformation "plastification" estimations; thus the Terzaghi "local failure" PRESCRIPTION. Obviously such a prescription could not furnish any basis for evolution of FDF and PDF risk estimations on unacceptable deformations.

More recently the obvious proliferating trend has been for use of FEM analyses. For the case of embankment on soft clay, Fig. 8 reproduces results repeatedly published and confirmed. For adequate use of FEM analyses for prototypes, I note summarily that two important stumbling-blocks continue to be (a) the determination, or judicious estimation, of the significant starting in-situ stress-strain conditions (b) the statistical determination of appropriate adjustment coefficients for correction of test-to-prototype deformation parameters ("average" values at play in deformation integrations). Fig. 8 represents a research case, highly successful (and therefore published), but apparently favoured by the simplicity of the profile etc.., and the large movements under consideration. In the cases of soil-structure interaction of footings, diaphragm walls, etc.., where a few centimeters make all the difference between cracking or not, and, for the selfsame reason, the accepted soils are more rigid, the residual in situ stresses, and the deformation parameters, have generally led to much less coincidence between design-predictions and observed performance.

Figs. 8 a,b,c registered the gradually increasing zone of plastification, "local failure" as newly seen. Those shaded areas cast their shadow on the previously

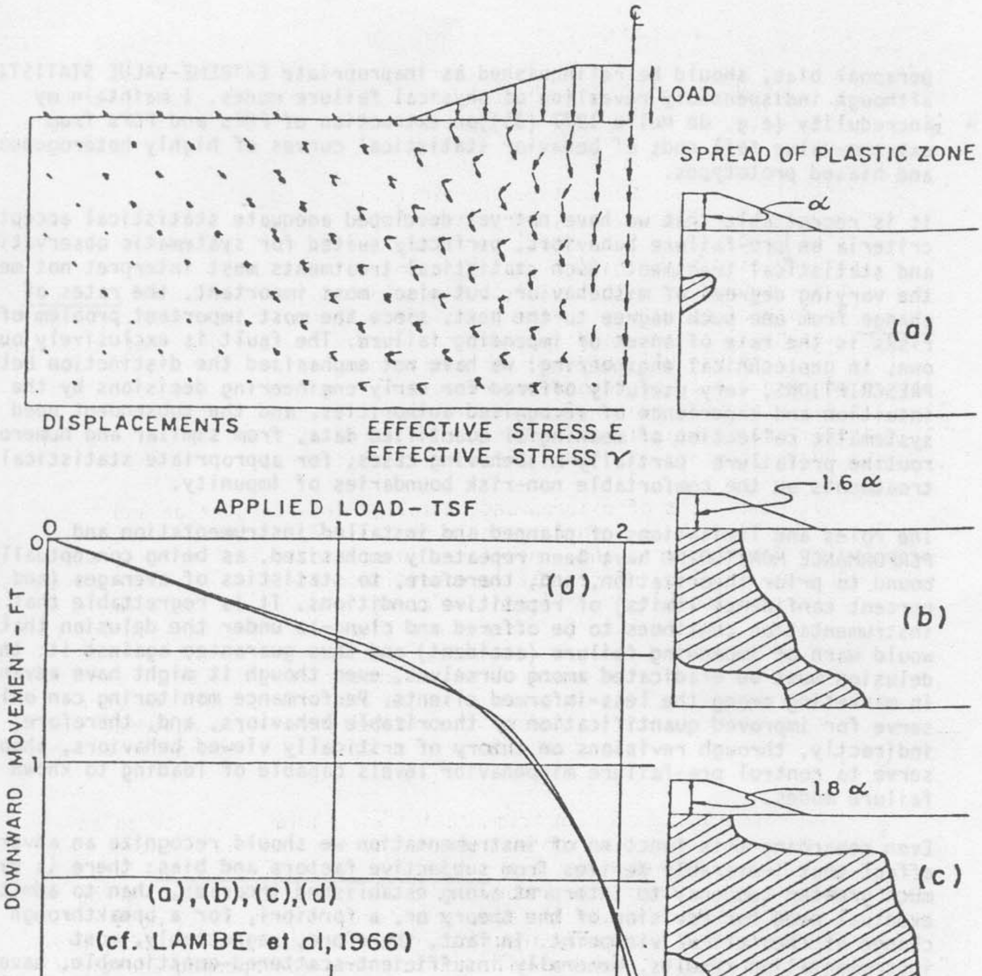


FIG. 8 PRE-FAILURE TO "FAILURE" EVOLUTION

confident limit-equilibrium analyses and the "local failure prescription" that served foundation engineering around 1940-60.

At any rate, the clear needs for risk evaluation in all stability problems accompanied by "significant" deformations, are, as emphasized many a time (e.g. de Mello 1977 (35)): (a) to instrument and observe many analogous prototypes (or better, single prototypes in many different conditions) under different pre-failure nominal FS conditions; (b) thereby obtain adequate statistical data-banks of possible "SATISFACTION INDICES" as associated with FS; (c) finally, establish statistically the boundaries of the accept-reject deformation criteria. It was and is repeatedly emphasized that such statistics of averages in well-defined universes, do offer great promise, whereas the few, accidental, badly recorded, failure cases, generally analyzed under theoretical and personal bias, should be relinquished as inapropriate EXTREME-VALUE STATISTICS although indispensably revealing of physical failure modes. I maintain my incredulity (e.g. de Mello 1977 (35))on extraction of FDFs and PDFs from extreme-value tail ends of behavior statistical curves of highly heterogeneous and biased prototypes.

It is regrettable that we have not yet developed adequate statistical acceptance criteria on pre-failure behaviors, perfectly suited for systematic observation and statistical treatment. Such statistical treatments must interpret not merely the varying degrees of misbehavior, but also, most important, the rates of change from one such degree to the next, since the most important problem of risks is the rate of onset of impending failure. The fault is exclusively our own, in geotechnical engineering: we have not emphasized the distinction between PRESCRIPTIONS, very usefully offered for early engineering decisions by the intuition and experience of recognized authorities, and the subsequent need for systematic collection of meaningful quantified data, from similar and numerous routine prefailure partially-misbehaving cases, for appropriate statistical treatments on the comfortable non-risk boundaries of impunity.

The roles and limitations of planned and installed instrumentation and PERFORMANCE MONITORING have been repeatedly emphasized, as being conceptually bound to prior theorization, and, therefore, to statistics of averages (and percent confidence limits) of repetitive conditions. It is regrettable that most instrumentation continues to be offered and clung-to under the delusion that it would warn of impending failure (accident) and thus guarantee against it: this delusion must be eradicated among ourselves, even though it might have advantages in marketing among the less-informed clients. Performance monitoring can only serve for improved quantification of theorizable behaviors, and, therefore, indirectly, through revisions on theory of critically viewed behaviors, should serve to control pre-failure misbehavior levels capable of leading to known failure modes.

Even regarding this function of instrumentation we should recognize an adverse effect that inexorably derives from subjective factors and bias: there is ever a much greater tendency to interpret along established theories, than to admit the eventual need for revision of the theory or, a fortiori, for a breakthrough change of theoretical viewpoint. In fact, therefore, regrettably, most instrumentation results, generally insufficient-scattered-questionable, have served more for maintaining the prevailing bias, than for opening additional or new vistas.

6. RISKS. IMPORTANCE OF RISK-AVERSION AND RISK-SEEKING. SUBJECTIVE CONDITIONINGS.

Most of us accept the concept that our advances in thinking are basically Bayesian in the qualitative sense inasfar as,in the face of any problem and decision,we possess (and are possessed by) an intuitive "prior probability estimation", and, after obtaining the result, induce that prior probability prediction to be affected by the new result,in order to reach a "digested" posterior probability.

This presentation is not an appropriate vehicle for expatiating on the problem. It is most important to emphasize, however, that any mathematical formulation of the Bayesian prior-to-posterior probability predictions would be grossly at error, if it assumed: (a) symmetrical mathematical functions for frequency distributions (most especially if failure, and catastrophic failure, is at stake) (b) even in the rare cases wherein asymmetric distributions and skew are consciously recognized, and if the "objective frequencies" are adequately established or estimated, the impact of such "cold facts" cannot avoid being subjective: far from a mathematical computation, the revision generated by the added data is absolutely conditioned by the individual involved, and the value at stake. Note, moreover, that "value" really implies "sense of value".

In short, the obvious Bayesian probability revision as a tool for improved decision, even in the case of available "indifferent statistical frequency data" (not hampered by discontinua and/or extreme-value problems), crashes against the problem of: (a) "single-case dispersions" (each decision is important and independent per se) in comparison with dispersions and confidence limits of averages (the "experience" and "callousness" generated by multiple analogous cases); and (b) against the risk-aversion vs. risk-seeking psychology, which varies greatly with value.

Fig. 9 adapted from Kahneman and Tversky, 1982 (23) elucidates forcefully the unsuspectedly high proportion in which these subjective factors interfere. Note the change of attitude as value changes. In comparison with a typical Gaussian normal-distribution curve, the asymmetric psychologically-influenced function (more near to simulation by some Beta distribution) shows that our sense of value for guidance in our judgments, protecting us from illusions, in taking Design Decisions, will greatly condition the progress from prior to posterior--probability risk estimations.

Surely everybody recognizes not only the inexorably strong psychological influence on decision preferences, but also the fact that in major geotechnically-affected structures the sense of value, prestige, risk, catastrophe, responsibility etc.: (a) will yield a very strongly asymetric PDF against risk--seeking (i.e. "eventual risk-accepting") as opposed to risk-averting; (b) will change very radically, as unconsciously applied to a new lesser-known culprit factor, and as progressively applied to presumed increasingly knowledgeable cases; (c) will concentrate failures on smaller, less-studied projects.

We do not have any estimates of what such functions would be in our professional activities, but could well begin to investigate them by appropriate widespread anonymous questionaires. Such functions do not need "case-history facts", but only "opinions", without losing any of their fundamental credibility. However, in investigating "case-history facts" we could to some extent interpret the explicit and implicit design decisions as supplying some data for such functions. Note the emphatic difference between (i) such an FDF on past intentions (asymmetrical, intuitive, and changing with time), in comparison with (ii) the case--history data-banks for FDFs of behaviors of the constructed projects as functions of the nominal FS, (presumably symmetric, frequency-dominated mathematical statistics).

The curious outcome would seem, once again, to emphasize the tendency for failures to belong, not only to the mathematically formulated "extreme-value statistics", but further, to what we might consciously name "absurd-value statistics". We depend on ignorance, collective or individual, conscious or unconscious, in order to reach the change from preparedness for surprise, to the dismay of failures; or, as often happens, we would discover that failures seldom come from a single parameter, being mostly provoked by a queer succession or composition of factors.

The interested readers are invited to ponder on such reflections as "the inductive concept is implicity used in all our thinking on unknown events", "inductive probability ... does not occur in scientific statements" ... but only in judgments about such statements", "inductive probability belongs not to science itself but to the methodology of science, i.e. the analysis of concepts, statements, theories, and methods of science", embodying the very "principle of learning from experience" (cf. Carnap, R., 1945 (12).

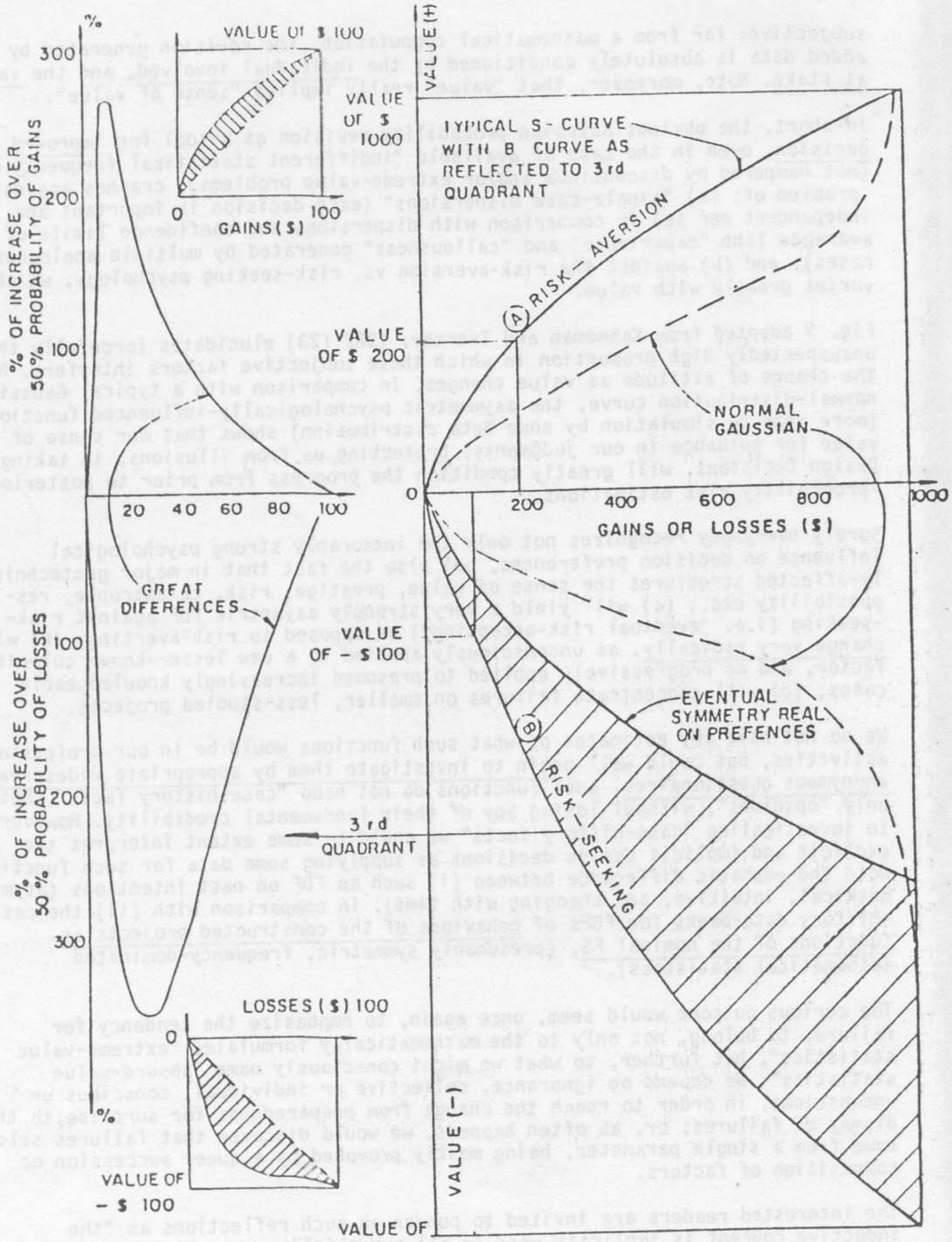


FIG. 9 — PSYCHOLOGICAL INFLUENCE ON PROBABILITY DECISIONS. SYMMETRY OF BAYESIAN PROCEDURES ADULTERATED

Probability computations are impossible without statistics of prior events analysed: but we must distinguish between statistics of nominal events, of progressively improved, perceived, and analysed events (under prevailing theory's bias), in comparison with statistics of preferences-and-decisions, and finally, that of learning from advancing decisions. In a field in which precedents and practice dominate "facts", we may well fall into gradually distancing between the three PDFs, of prior, posterior, and actively advancing probabilities, as the client and social consciousness becomes increasingly severe and blatant against "errors" and "failures".

7. "COMPLETE" CHECKLISTING OF IMPORTANT INTERVENING PARAMETERS. ENGINEERING PRINCIPLE ADVOCATING MAXIMIZING "PRETESTING". DECISIONS, COMPUTABLE OR NOT, TEMPERED BY FACTOR OF HUMILITY IN ACTION.

With advancing technology, obviously there is an increase in the awareness of the intervening parameters, and their conditions vastly more varied and variable than earlier assumed. Statistical analyses, regressional, multivariate, invariant etc., can be made, and are naturally attached to any given moment/ /stage of development of the pertinent "deterministic" knowledge. All factors not suspected or established under deterministic cause-effect functions, are automatically lumped, at that moment/stage, within the assumed "random variations" of the nominally designated fixed statistical universe of that stage of cognizance. So, the processes of advancement imply a gradual extraction of some factors from the "random bag" (without, however, eliminating the randomness of dispersions in defining parameters, and other unrecognized factors that persist in the bag of constancy defined). Naturally, the scatter should be much greater at the beginning, and should gradually decrease, consciously on individually tackled parameter effects, and subconsciously on the overall behavior.

The three important practical recommendations that I offer herein are believed to be applicable at any stage of the continually varying status of knowledge--requirements-optimization (risks of unpredicted costs vs. certainties of design-construction costs):

(1) Begin by preparing as "complete" as possible a checklist of parameters known (or presumed) to interfere, theories involved, ranges of dispersions and doubts, and roughly estimated-computed range of significance of results-effects. Conscious facing of such inquisition is very important: historically the road travelled has been in the direction of accepting everything as simple until proven otherwise, but modern thinking recommends that we make future efforts in the opposite direction, attributing significance and doubts to all factors until proven otherwise. Thereafter, strike out the factors which may be set aside, and concentrate on the ones on which investigation, computation, decision, and engineering action will be applied.

The task may appear formidable and extensive in time and cost, but really is not so for the professional with experience, since most listed parameters will be set aside by a rapid conscious reference to codes, precedents, experience, recognition of secondary importance etc. The incremental work in listing and crossing out is minimal; the important point is the attitude of consciousness about the estimations and decisions.

(2) In the choice-adoption of physical design models:-

(a) Give absolute preference to the physical changes of model (and statistical universe) such as would, in principle, preclude the risk feared: for instance, if erodibility is feared, use a non-erodible element (material-and-condition). It is undisputable that a zero-risk probability can be achieved in design situations that may merit so.

(b) For as many significant anticipated actions as possible, apply, during design-and-construction, the priority design principle of "pretesting", that is, submission of the prototype parts to actions as nearly as possible slightly more severe than as anticipated. The net effect has been repeatedly discussed as introducing the benefits of a FACTOR OF GUARANTEE in lieu of the traditional FACTOR OF SAFETY.

(3) Introduce, wherever necessary, in our analyses and discussions, a Factor of Humility in Action, in substitution for the illusions of a numerically imposing Factor of Safety. A FACTOR OF HUMILITY is subjective, inevitably cultural, dependent on the engineer's interaction with his society and the prevailing sense of values. As a practising professional I submit that there should be no shame in recognizing the dominance of such subjective factors. Moreover, I personally doubt that we will ever avoid some degree of intervenience of such a factor of humility, in the face of Nature and her forces and complexities, even though our efforts will greatly increase the number of problems brought under the domination of numerical, statistical, probabilistic, Indices of Safety or of Guarantee (analogous to currently mentioned Ratio Factors, but more generalized).

In brief clarification of the first concept and recommendation above offered, I submit a schematic drawing concerning the problem of natural slope stability in a tropical saprolite (Fig. 10). I attempt to portray the natural variations in Resisting Factors, knowledge and Analysis consciousness, Unstabilizing Factors from rains and earthquakes, and hypothetical episodes of local sliding failures. Incidentally, at each slide case history attention has been concentrated (to my knowledge without exception) on the analyses of the sliding mass only: I strongly emphasize that much should be gained by concomitant analyses of surrounding areas, presumed analogous, but that did not slide, for assessment of the tangible differences that made the effective difference of performance. A brief mention follows in support of some factors mentioned.

Recognizedly there is a very slow natural loss of strength with time, by weathering. On the other hand, improved sampling and testing in soil mechanics has gradually increased the strength attributed (1950-85). The importance of tension cracks at the top of steep slopes was early recognized as affecting the net available shear resisting area for sliding stability. However, the unstabilizing effect of such cracking during intense rainfalls can be computed to be vastly greater because of infiltrations: on the one hand, the hydrostatic cleft water pressure applying a horizontal force at the top of the potential slide volume; on the other hand, the accompanying noticeable change caused on the infiltration flownet, hitherto by over-simplification assumed to correspond to seepage from the surface through a homogeneous Darcy medium. With increasing shear deformations (generally concentrated along a thin relict-structure plane, often micaceous) there may be aggravation of loss of the resistance, tending towards ultimate or even residual values.

Another, intervening factor recognized in the past decade was the soil suction, increasing stability in dry weather, and rapidly lost on first moistening due to rainfall infiltration. Incidentally, Vaughan 1985 (24) advances Lumb's earlier theory of the advance of the wetting front, during rainfall infiltration, as an important unstabilizing factor in partly saturated soil slopes: a further advance might be associated with the revisions applicable to the infiltration flownet, because it would appear that in most relatively brittle saprolite slopes, during a rainfall episode the time sequence of unstabilizing factors is (1) elimination of suction (2) cracking (3) infiltration flownet, iteratively affected by subvertical cracking (4) with higher rates of infiltrat-

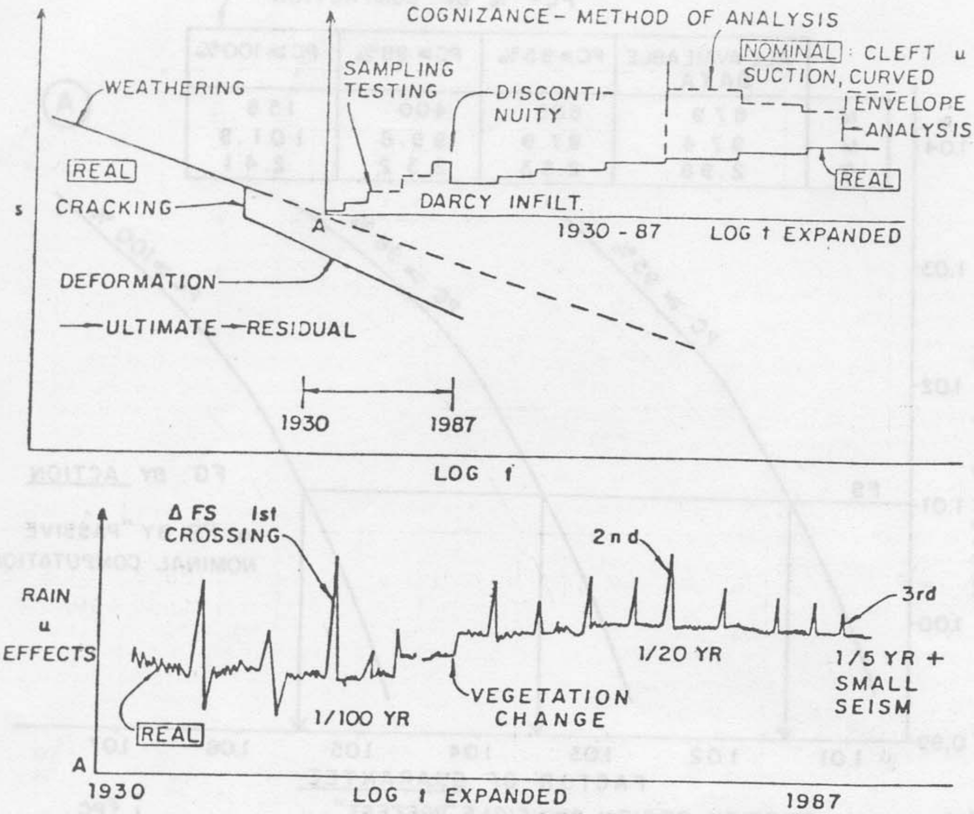


FIG.10 REAL VARIATIONS AND EPISODES: PLUS PROCESS-MODEL CULPRITS AND COGNIZANCE. SAPROLITE NATURAL SLOPE INSTABILITY.

ion, the establishment of cleft water pressures.

More recently the analytical refinements have introduced recognition of the curved strength envelopes at low stresses (OCR > 1), and also of more correct stability analyses of soil masses subjected to flownets (cf. item 3.3), generally increasing estimates of the resisting force. Regarding theoretical recognition it is worth mentioning that not only cracks, but also "canaliculi" appear to be of great importance to infiltration flownets: since there has been a surge of interest in the phenomenon of canaliculi in tropical lateritic soils since ≈ 1980, and their influence is obvious, by being rapidly filled to "hydrostatic three-dimensional tubes" by rain, the example should be mentioned of an item that was published but lay awaiting about 30 years before recognition (cf. Drouhin et al. 1953, (25)).

Finally, some earthquake episodes, and a hypothetical change of vegetation and consequent infiltrations, are schematically introduced.

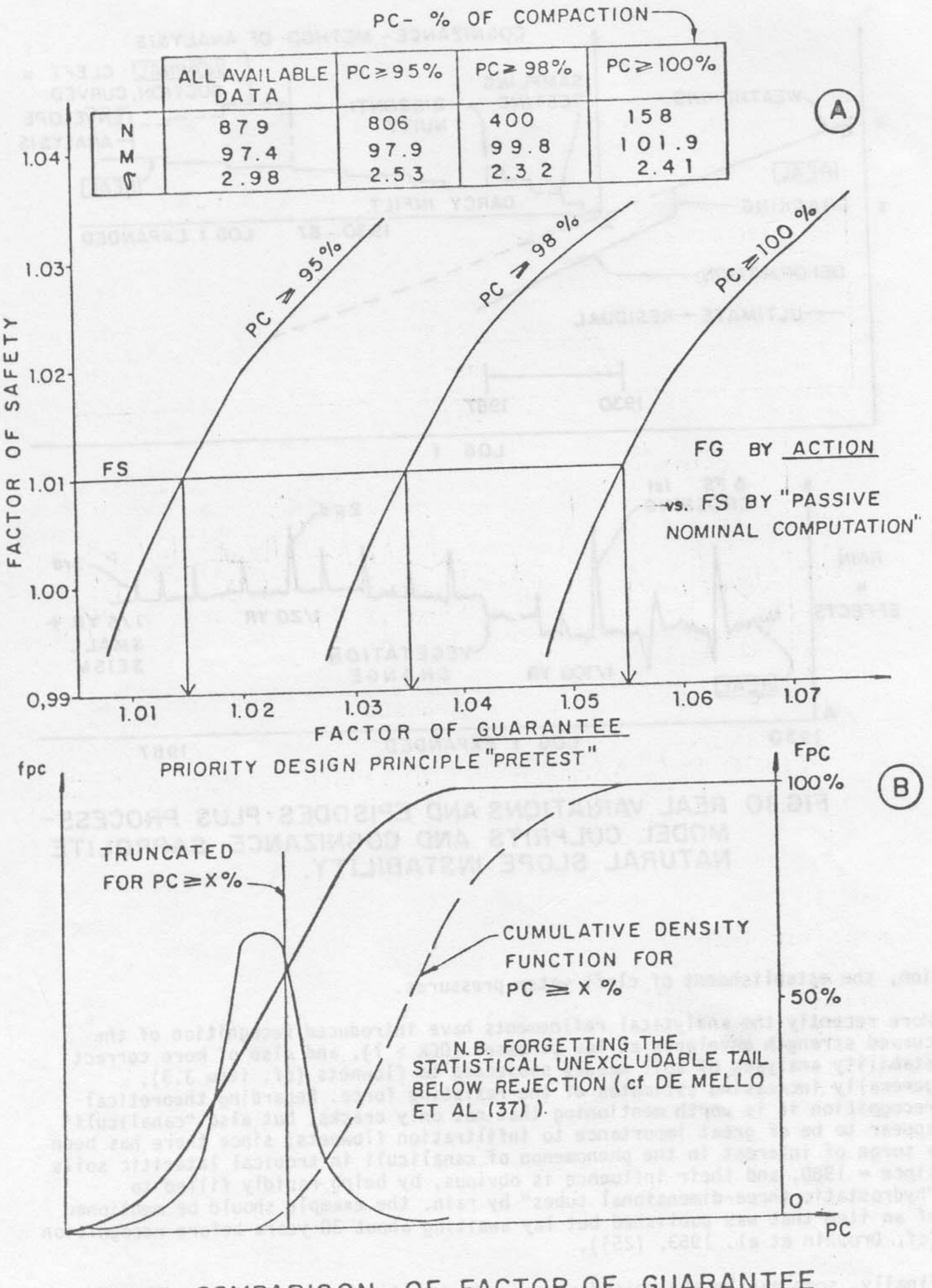


FIG.11 COMPARISON OF FACTOR OF GUARANTEE VS. FACTOR OF SAFETY

The changing universes of presumed reality and corresponding analyses are schematically exposed.

With regard to the second concept and the pretest principle of design, Fig. 11 has been prepared to show, by routine mathematical statistics, how much the PDF of a compacted embankment's percent compaction PC% will improve by applied pretest rejection criteria. The data used are from a dam (cf. de Mello et al., 1959 (37)). There is, naturally, an increase in shifting from prior (FS_{pc}) to the posterior (FG_{pc}) FDFs corresponding to the three rejection criteria applied. The change seems small, and, as a start, one must dispell any false notion, because the strength changes in a 2% or 4% increase in mean PC% are really much greater percent increment (emphasizing the error in employing inconvenient ratios). A much more important point to emphasize, however, is the difference between the gains, via inductive probability as compared with the used frequency probability. As implied by the name Factor of Guarantee and the Pretest Principle, we know that the difference FG $\neq$ FS is not merely numerical, but principally one of quality. An element that has been loaded to stabilization under a given pretest load is physically (not probabilistically) guaranteed to resist future loads up to the pretest one.

Finally in brief expatiation of the third recommendation, I may summarize the following key statements. Engineering comprises decision plus action, aided by nominal computations wherever possible. An engineering work causes a nominal ΔFS to the surrounding geotechnical conditions: one tries to design to replace the ΔFS lost wherever possible. Moreover, Nature and time may cause, or permit occurrence of, other ΔFS conditions: one would attempt to estimate the ΔFS vs. Δt within the useful life intended, and thereupon pre-add the desired ΔFS by routine reinforcing elements. In every design, it is much more realistic and accurate to use calculations of incremental ΔFS effects.

In closing, and in humble recognition of our insignificant and transient contributions, I dare suggest that, far beyond probabilities, Nature is certainly dominant, and in the face of her inscrutable processes, sudden-big up to the limit of infinitesimal integrated with imperceptible slowness over immensities of time, we must ever leave some WORK and WORRIES for our children, and theirs.

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