REVISITING CONVENTIONAL GEOTECHNIQUE AFTER 70 YEARS

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"The old order changeth,
Yielding place to new,
And God fulfills himself in many ways
Lest one good custom should corrupt the world"

(Alfred, Lord Tennyson)

The emphasis of this Geotechnical Workshop is on the "Applicability of Soil Mechanics Principles to structured soils", and attention is further centered on so-called Residual Soils, a sorry misnomer. Incidentally, which are Geotechnique's PRINCIPLES, and which, candidly, but the PRACTICES and PRECEDENTS mistaken for PRINCIPLES?

The needs of the Developing IIIrd World which I find myself insistently impelled to assuage are far too many. I have always felt them, and have mostly acted and voiced in a manner of offering tidbits at buffet-tables of intuitive thoughts, psychologico-socially convinced that the emergence out of colonial IIIrd World dependency comes from the indescribably greater enthusiasms and energies liberated at the first love of one's own choice, and not from the dutiful fulfillment along preferences indicated by teachers and mentors, no matter how well-intended. Nothing matches the effectivity of life's search for itself!

However, the shackled onlooker beside a buffet cannot exercise any choice, and the apparent treat of liberty confuses and frustrates. I have therefore advisedly decided to concentrate on a few thoughts that flashed as being examples of top priority for such a gathering.

1 KARL TERZAGHI, 1925.

Roughly 70 years have elapsed, essentially three generations,since the gestation of his book "Erdbaumechanic auf bodenphysikalisher Grundlage" (Vienna, Deuticke, 1925). Annoyed at the ineffective qualitative GEOLOGY of the time,and hungering for action and responsibility within a context of deterministic physics and mechanics, but nurtured by the persistent self-questioning and patient-pertinacious personal experimentation (that is the label of humility in greatness) in a primitive laboratory(1), he gave two fundamental contributions to Mankind's Development:

a) The Principle of the Effective Stress.

(1) Two points merit emphasis: (1.1) the brain's capacity at extracting the essence out of a set of crude results is often such as to dispense with second-order sophistications, and even to do better without them, for the possibility of their dispersions causing distractions; (1.2) No matter how modern a laboratory, a measure of intrinsic innovation is that to the innovator's needs even brand new potentialities, when bought or fitted-up, are always insufficient to the advancing purpose, and thus always requiring specific adjustments.

b) The life example of incessant zest at facing new challenges as an ENGINEER of practical decisions (2).

It is fundamental to recognize the partnership of the two, because the first, a DEFINITION, ipso facto nominal (i.e. "by name") and therefore unassailable, adopts the sterile fixity of the NOMINAL DOGMA expressly in order to open the doors to multitudes of test-trials of the provable usefulness within the adopted bounds.

As one of hundreds of well-known analogies we could say that Dalton's Atomic Theory (1810) established an irrefutable NOMINAL BASIS for chemistry's early big strides, tested-proved with avidity and exhilerating success: fortunately, however, there occurred the follow-ups of Mendeleev's Atomic Chart and the immense added fertility of the Divisible Atom, without destroying the foundations of Dalton's behavioral indivisible-atom chemistry.

2 The EFFECTIVE STRESS EQUATION, its effectiveness and perspectives, $\sigma' = \sigma_t - u$

In adopting such a definition-dogma there are irrefutable subconscious components: 1) the intuition is prodded by first-order perception of cause-effect behaviors; 2) there is an intense PURPOSE, which is that of usefulness and APPLICABILITY for interpreting and PREDICTING such dominant cause-effect bahaviors; 3) the validity of the definition is pragmatically TESTED with regard to the dominant cause-effect behaviors of the first interpretative and predictive visions; 4) the body of repetitive observations, and consequent "LAWS OF BEHAVIOR", continues to build up for a long while, all second-and third-order intervening parameters appearing as (4.1) dispersions (4.2) recognized and described anomalies (4.3) surprises (some accidents included).

The fact is that upon defining $X = Y - Z$ in so deterministic and simplified a manner we automatically define that any and every parameter present in the environmental immediacy, capable of affecting the cause-effect empirical laws sought are DECLARED TO BE ZERO.

Let us stop and reflect seriously. The fundamental purpose in the equation was for SOLVING ENGINEERING PROBLEMS, that is, by closing the cycle of equivalent-or-analogous experience through LABORATORY TESTING plus confirmatory FIELD OBSERVATION (PROTOTYPE TESTING). Thereupon arises the recognition of the shocking discontinuity in the link sought: in the laboratory, indeed all collateral factors ARE CONTROLLED TO BE ZERO (or constant, cancellable); but in the field prototype, such IDEALIZATIONS must obviously be the STATISTICAL EXCEPTION and NOT THE RULE. How, then, to progress with such a widening abysm?

If the definition had been $f_1(x) = f_2(y) - f_3(z)$, and, if further, the equation had been extended to incorporate several other parameters of presumed (or inferred) likely interference, $f_A(x) = f_B(y) - f_C(z) + f_D(m) + f_E(n) + f_F(p)$ etc.; ... ... on the one hand, the impact of immediate useful applicability would have been lost through maze of dificulties,

(2)See, for instance, Geotechnique, XIV, Mar. 1964, nº1, pp. 57-58, and, besides numerous written discussions, especially such papers as:

1962 "Dam foundation on sheeted granite", Geotechnique, 12:3: 199-208

"Measurement of stresses in rock", Geotechnique 12:2:105-124

" Stability of steep slopes on hard unweathered rock", Geotechnique 12:4: 251-270.

1964 (posthumous) "Mission Dam; an earth rockfill dam on a highly compressible foundation", Geotechnique, 14:1: 13-50

Terzaghi's philosophy is trancribed in his own words, dated Istanbul, 1923, in "ABOUT LIFE AND LIVING", Geotechnique 14:1: 51-56.

but, ... on the other hand the recognition of natural and scientific complexity would have facilitated the gradualism of advance into INCORPORATING ADDITIONAL PARAMETERS into a basic adjustable equation, rather than accumulating more and more exceptions to the original NOMINAL SIMPLICITY. (MEMO A)

For instance, in the laboratory the purity of $\sigma' = \sigma_t - \mathbf{u}$ is essentially untouchable, but in the field it can only be used through assumptions on $\sigma_t = \mathbf{f}(\gamma \mathbf{Z})$. The obvious starting use of HOMOGENEOUS NON-SHEAR-REDISTRIBUTED $\sigma_t = \gamma \mathbf{Z}$ has continued to prevail without exception even after a) multitudinous laboratory and theoretical demonstrations of the importance (moderate, secondary) of $\sigma_1 + \sigma_2 + \sigma_3$, so that behaviors are $\mathbf{f}(\gamma \mathbf{Z} + 2\mathbf{K}'_0 \mathbf{Z})$ and not simply $\mathbf{f}(\gamma \mathbf{Z})$; b) the many reasonably credible determinations of widely different $\mathbf{K'_0}$ values (since Skempton and Sowa, 1963, thirty years ago!); c) the recognition of inexorable statistical variations around a mean, not only through errors, but also inescapably through the facts of reality; d) the flagrant demonstration of all-important hang-up ("silo effect") in differentially compressible earth-rock dam superstructures (since around 1966), a behavior inevitably attributable also to differentially compressible subsoil horizons, but never hitherto presumed of possible participation in any case (e.g. local slides in saprolites?).

The SCIENTIFIC-TECHNOLOGICAL METHOD imposes that: once a hypothesis or definition has been established, an impelling subconscious principle of laboratory investigation is that we work at establishing the cause-effect laws via PARTIAL DIFFERENTIALS; for instance, we test for $\Delta\varepsilon = f(\Delta\sigma')$, all other parameters maintained constant. The term laboratory should be applicable both to the "synthetic" laboratory and to the LABORATORY of job observations: in the synthetic laboratory the control of all parameters to absolute conscious constancy is much better, while investigating the partial differential of the only two allowed to vary; meanwhile the field laboratory would reveal complex reality except for the limitation that REALITY IS WHAT STRIKES THE CONDITIONED EYE OF THE OBSERVER.

It is far beyond the scope of this presentation to inquire into adjustments of a more generalized equation of FUNCTIONS of nominal effective, total and free-porewater stresses, so as to incorporate the known interferences of such first-order parameters as, for instance, highly crushable grain mineralogies, intergranular "nominal contacts" highly susceptible to chemical and electro-colloid-chemical and stress-time changes, stress history, bonding, cyclic stressing hystereses or plastifications, nominally effective pore sizes and pore-size distributions, temperature, porewater electrolytes and concentrations and changes thereof, electrosmosis and electrophoresis, etc.

In principle, if from test data we may express the different partial differentials through appropriate regressions (e.g. exponentials $\varepsilon = \mathbf{A}_0 \mathbf{e}^{\alpha\sigma'}$, etc.) we may attempt integrations if the dominant parameters are "reasonably" independent: thereupon, such EMPIRICAL REGRESSIONS being susceptible to progressive improvement, the basic behaviors could become expressable in terms of some nominal "relative" of the Terzaghian effective stress.

Once we have discovered that the absolute simplicity of the erstwhile PRAGMATIC ENGINEERING DOGMA has been broken in laboratory tests, we obviously must intensify investigation into the "spurious interference". Upon reflecting on the problem and attempting to interpret what has occurred during the past 40 years, I submit that two fundamentally different avenues could have been sought, PREFERABLY THE SECOND FOR ENGINEERING PURPOSES; but apparently neither has been SYSTEMATICALLY EMBRACED AND ADVANCED WITH CLARITY, and, as a net result, the original dogmatic EQUATION-BY-DEFINITION persists as the only backbone. Let us see forthwith the two approaches. However let us first conclude that due to the complexities of multiple really-interdependent parameters, the present status is of a babel of descriptive sub-realities, only integratable (and only qualitatively so) by the EXPERIENCED MIND THAT LEANS OVER BACKWARD. Geotechnique has returned to a crude increasingly expensive first-order technology ON THE SAFE SIDE, qualified in a descriptive status of scores of exceptional behaviors because of the over-simplified idealizations of the ORIGINAL DEFINITION-EQUATION.

Let us first discard the neo-scientific approach that, for the purpose of ENGINEERING APPLICATION, presumes to "understand" the trends and laws of behaviors of level M, N, X, Y, Z, T etc. by researching into laws of behaviors at the level of components dm, dn, dx, dy, dz, dt, etc.. This avenue has been pursued almost to exclusivity, mostly by engineers who are not scientists and scientists who are not engineers, ending up as responsible for the patchwork of EUREKA data. As an interesting example of an exception we may note the "engineering approach" in the use of the Plasticity Indices (Atterberg-Casagrande) of clays to attempt to represent the EXTERIORIZED EFFECT of colloid-electrical -chemical component parameters of clay behavior, without attempting to understand the minuscule interactions at play: we shall make brief mention of the neglect and discredit of these INDEX TESTS because of their preservation in formaldehyde (1928 to date) without progressive engineering adjustment. But, since our immediate aim is to query neo-scientifically the Effective Stress Equation with regard to CAPACITY TO ADJUST to complex realities of "anomalous soils", let us focus on the three milestone publications of meticulous research.

The first two, of outstanding research by A.W. Bishop and co-workers, both published as Philosophical Transactions of the Royal Society of London, focussed on GEOMECHANICAL EVIDENCES (stress-strain) in perfectly saturated soils, descending to the scientific level of taking into account every compressibility, however minute, where the original idealization had assumed incompressible. In "The influence of pore-water tension on the strength of clays" (3 July 1975), setting aside the N-th order hypotheses and imprecisions, the authors conclude that "there is no unique relation between strength and water content" (cf. Fig. 1), [N.B. a routine useful tenet in Engineering, cf. D.W. Taylor, Cam-Clay model, etc.]; and they point to revision, "... the Modified Effective Stress Equation necessary to relate pore-water tensions to the strength of partly saturated soils is outside the scope...". In the following paper "The influence of high pore-water pressure on the strength of cohesionless soils", A.W. Bishop and A.E. Skinner (4 January 1977) the validity of the Terzaghi Effective Stress Equation is experimentally established for cohesionless particulate materials and assumed conditions of intergranular particle contacts, under the postulate(undisputably plausible) that junction growth (instantaneous, mechanical) due to great stress changes is negligible. For the less idealized mineralogies, the questions on interparticle links remain open, under effects of very-long term, or millions of mini-cycles, or colloid-chemistry etc. Finally, for our purpose of more generalized soil conditions the third milestone paper has the closest relevance, "Constitutive relations for volume change in unsaturated soils", D.G. Fredlund and N.R. Morgenstern, Canadian Geotechnical Journal, 13:3, Aug. 1976, 261-276; assuming incompressible soil particles, and "elastic theory" stress-strain relations, after many derivations and statistical regressions on test data, it is concluded that the two constitutive equations FOR VOLUME CHANGE, as written below, "can be used for engineering practice", hysteresis conditions excluded.

Soil structure,

$$\varepsilon = \frac{1}{v}\,\frac{\partial v}{\partial(\sigma - u_w)}\,d(\sigma - u_w) + \frac{1}{v}\,\frac{\partial v}{\partial(u_a - u_w)}\,d(u_a - u_w)$$

Water volume in the element,

$$\theta_w = \frac{1}{v}\,\frac{\partial v_w}{\partial(\sigma - u_w)}\,d(\sigma - u_w) + \frac{1}{v}\,\frac{\partial v_w}{\partial(u_a - u_w)}\,d(u_a - u_w)$$

Both the more general aim, and the overall approach of this third effort tally with my postulations. However, the Figures on principal test data (Figs. 2, 3 herein reproduced) begin by calling attention to THREE IMPORTANT POINTS at least: 1) another parameter (among others, inevitably), TIME, is shown influential though not included in the formulation; 2) the correlation is, in some conditions, rather poor since although % volume diferences are small, for some consequential effects such as (pore pressures) the dispersion may not be of little significance; 3) for engineering practice (cf. all of Terzaghi's solutions included, implicitly) it is important to emphasize that under (inexorably) varying degrees of cognizance we must always work on the safe side, by PRESCRIPTIONS in lieu of CORRELATIONS (unless complemented by % CONFIDENCE BANDS), and, therefore, cannot employ a correlation even if moderately good, but MUST FAR PREFER A "BOUNDARY" EQUATION, which shows that THE CRITICAL BEHAVIOUR e CANNOT BE WORSE THAN.

In concluding my comments on what I have called the NEO- SCIENTIFIC approach, I submit, with due diffidence, the impression that in all three invaluable contributions a point of fine sophism might appear to have sneaked in: is it really an investigation of the GENERALIZABLE VALIDATION of the effective stress definition $\sigma' = \sigma - \mathbf{u}$ if derivations are, throughout, based on use of $(\sigma - \mathbf{u})$? Wouldn't generalization suggest starting with something like $\varepsilon = A_0 (e^{\alpha\sigma''})$, to conclude that for the frequent idealized conditions A_0 and α would tend to 1.00 and σ'' tend σ'? Isn't it somewhat of a condition of begging the question, or, figuratively, of trying to lift oneself by one's own shoe laces?

Taking up the second avenue, that I have declared patently PREFERABLE FOR ENGINEERING PURPOSES, its basic recognition is that second, third, and N-th order complexities will forever exist, and WE ARE ONLY CONCERNED WITH THEM inasfar as they MERIT PROGRESSIVE INCLUSION WITHIN ADEQUATE BENEFIT/COST RATIOS. Thus, one obvious practice is to employ reasonably ANALOGOUS nominal parameters, even if recognizedly COMPLEX-LUMPED and modestly precise, but respecting as rigorously as possible the CLOSED-CYCLE EQUIVALENCE between advances of theory/research, and the corresponding APPLICATION IN PROJECT ANALYSES. Two obvious illustrative examples in conventional soil mechanics were, for instance 1) in a dominantly intuitive-empirical vein, the use of the Atterberg-Casagrande Plasticity Indices, 2) in the analytical -idealized derivation of consolidation theory, the adopted constant Cv, coefficient of consolidation.

In summarizing, Terzaghi's EFFECTIVE STRESS POSTULATION was eminently an ENGINEERING DECISION to concentrate on the then dominant concerns, COMPRESSIBILITY and SHEAR STRENGTH of "young" saturated sediments under idealized relations merely of stress and strain. His interpretable intent was, indeed, to stay away from theoretical complications affecting particle-to-particle contacts, or much more complex cluster-to-cluster and particle-to-particle electrochemical (etc.) attraction-repulsion space equilibria (etc.), and thus to concentrate on a stress, RELATED to the intergranular or interparticle stresses, but NOMINAL, and measured only with regard to EXTERIORIZED EFFECTS, a set of STRESSES to be EFFECTIVE. It is regrettable indeed that less informed geotechnicians proceeded to use the terms effective stress and intergranular stress interchangeably. The emphasis that, prodded by the necessary quest on partial differentials, shifted to the science of geotechnique, presently seems to have lost perception of its NOMINAL ROOTS, always standing on the original ENGINEERED platform ($\sigma' = \sigma - \mathbf{u}$); and, further, seems to have clouded the AIMS AND METHODS OF ENGINEERING. Thus, important early engineering decisions have not been interpreted, and, ipso facto, not been revised progressively; the consequence seems to be a pessimistic proliferation of anomalies on the neo -scientific side, in the face of which the young practising geotechnician is pressed into increasing subconscious conservatism on ENGINEERING.

In passing, lest I be misinterpreted regarding the importance of the science of geotechnique, and correlations, it seems important that I illustrate (Fig. 4) how ENGINEERING is benefitted by improved CORRELATIONS accompanied by tightening CONFIDENCE BANDS, but is not benefitted by mere improved correlations, and is even burdened by presumed better correlations if their confidence bands widen because of sophistication's revealing of more "spurious influences". ENGINEERING must generate the equation(s) to continue to incorporate advancing degrees of dissipation of ignorance within an ENGINEERING (and not SCIENCE) FRAMEWORK; it can do so easier if we continue to work principally in analogous closed-circles LABORATORY-FIELD (job performance).

In analysing the hypotheses and practices of classical soil mechanics and engineering, I cannot but conclude that although the data, knowledge, and hypotheses were inevitably crude, at least the closed-cycle applicability was consistent because the proponents had a definite practical aim, and followed through from hypotheses, through designed tests, to application, and confirmatory analyses; in the following period the subdivisions into tight compartments became the rule, and so most closed-cycle consistency became lost. We are much better documented but wade in confusion, not only in lack of purpose and consistency on the use of such improved variegated documentation in meaningful sets, but also in the very understanding of the aims and methods of engineering.

To illustrate my points regarding loss of direction and ineffectiveness of the multitudinous technical papers, I submit three cases of everyday engineering design-construction practices, and finalize with a topic atuned with so-called unconventional (i.e. not idealized Terzaghian) soils, by commenting on PROFILING IN SAPROLITES and applicable geotechnical parameters.

3 Conventional shoreline sediments. Case history of an exacting driven pile foundation, and unsuspected wasteful conservatism revealed.

All around us hundreds of pile foundations are being designed and executed continually. Everybody regognizes that if we bury 3x instead of x dollars as a first-cost per square meter of useful construction, our inflation is not being helped, nor are we becoming any more competitive or well-to-do. The singular condition of a densely piled foundation rather fully documented, permitted a revealing analysis that should apply to most analogous recent foundations authoritatively designed under the best CODES and teachings. It is a mud-tank structure of tight working tolerances on incremental differential settlements. Narrow compartmentalizations in subtopics, and in time, have marred a sane global view of methods and means of geotechnique as part of civil engineering, and wastes have accrued by casual insertions of arbitrary numbers of Factors of Safety FS devoid of behavioral meaning.

The data analysed pertain to 792 centrifuged concrete precastpiles of 26cm diameter, all individually controlled by simple routine regarding "ultimate dynamic resistance" under final penetration "set", 10 of them also subjected to Dynamic Load Tests DLT using the Pile Driving Analyser PDA, and finally, 4 Static Load Tests, all of them coincident with corresponding DLT tested piles.

3.1 Predicting pile lengths and load capacities for bid-design and budgeting. Direct use of SPT.

This presentation is not a vehicle for noting one principal advantage of driven piling, that of predictability and of quality-control: the statistical data collected, notably rare, are used for pertinent probabilistic reasonings.

For anticipating driven pile lengths as required for guaranteeing code factors of safety on design load capacities two simple statistically derived equations, due to AOKI-VELLOSO (1975) and to DECOURT-QUARESMA (1978) are routinely used in Brazil. Both use UNCORRECTED SPT values, as they come from routine reconnaissance borings. Nevertheless Fig.5 shows good predictabilities (both of load capacity and of driven lengths) achieved by both, despite lack of sophistication. It proves, as happens often, that empirico-statistical correlations INVOLVING ANALOGOUS PHENOMENA (e.g. in this case, driven penetrations) CAN BE VERY GOOD, and consequent ENGINEERING PRESCRIPTIONS, often better than those derived under analytical sophistications.

3.2 Dynamic resistance data available on all piles via DRR, and 10 + 1 dynamic load tests DLT (one repeated after a delay of days). Brief exposé.

Accepting the premisse that the profession's specialists are conversant with pile driving developments associated with wave analyses, we refrain from mention of progressive digressions from the classical dictates and practices of the 1950's.

Old-fashioned pile-driving control, via final penetration "set" of each and every pile, received a significant improvement and credibility when the dynamic formulae were substituted by Smith (1960) model wave-equation PDA (Pile Driving Analyser) applications, with better instrumentation and on-the-spot- -instant preliminary computing. For routine monitoring a special (but obvious) electro-mechanical equipment unit called the Dynamic Rebound Recorder, DRR, (analogous to the electro-optical of Sakimoto, 1985) was developed to record pile head displacements vs. time, wherefrom the pile Dynamic Mobilized Resistance load Rd is derived from rebound records in a manner analogous to the CAPWAP method employing its records of strain and two accelerographs. Thus, in the piling every pile is systematically associated with its Rd value.

Under classical concepts this Mobilized resistance was considered to represent the pile Failure Load, QR, under postulated perfectly rigid-plastic behavior, and with engineering decision on the conservative side. Careful attention is drawn to the fact that with greatly increased pile diameters the same displacement dimensions have come to signify vastly different phenomenological behaviors. Suffice it to record herein that based on many analogous past load tests, and purposely on the safe side, the pile's estimated minimum failure load QR has been set as 1.15Rd (only 15% higher than the mobilized load at final driving "set"). Fig.6 proves that the present case is no exception.

Recognizedly there is no deterministic definition of a single failure load even in a single static load test. The very interpretation of such routine tests presents a variability within roughly $\pm$ 20% of an overall average. Most internationally published criteria, both "analytic" and "graphical" as per references listed in Fellenius (1980), as well as Brazilian Code NBR-6122 under revision, were used for defining the nominal failure loads from the static load tests, although the maximum settlements were small (2-3% of pile diam.). It is important to note that at a historic time when structures on pad foundations accepted settlements of a dozen to a score of centimeters, attention on piles (mostly for the 20-35cm diameter range) focussed on limiting settlements of 1-2cm and only on "failure" interpretations of load-displacement data. Deformability criteria on pile load test data are (almost) non-existent.

Concomittant with the static load tests we have the Dynamic Load Test, DLT, according to a procedure by now repeatedly published (Aoki, 1991). The extensive experience of remarkable similarity of results of the DLT and SLT (cf. Fig.7 as one of scores of cases) is tied to a reasoning, and curiosity, now seemingly obvious a posteriori, that generated the DLT.

Recapitulating: in the classical routine of pile-driving control, the weight and fall (energy) has been kept constant, under the convinced fear that since dynamic $\neq$ static (conventional dogma), it was fundamental to respect avoiding any conscious differences. More recently, however, it was reasoned that if a given (arbitrary) Energy E1 gives the unequivocal dynamic failure load QR = α (Rd), α traditionally taken as 1.0, then any other energy E_2, E_3,... En should also give the same failure load.

Curiosity (and the principle that no two things are ever equivalent) led to questioning this would-be coincidence of the theoretical idealizations; and it was discovered that, quite to the contrary, on using progressively increased energies E1... En at the point of final set, the CAPWAP analyses lead to data quite similar to those of loads-displacements of SLT. The easy and inexpensive test has invited repeated obvious uses. On the project herein reported we have 10 such DLTs, plus one repeated after some days. Understandably the use of DLTs is most inviting in bigger-diameter, higher-capacity, driven piles, of greater modern demand associated with lesser base and head settlements expressed in percentages of diameter. The DLT-SLT similarity is not difficult to explain based on micro-strain equivalence of elasticity moduli, more recently emphasized: the conventional geotechnical tenet of big differences between static and dynamic behaviors must be reinterpreted as inevitably associated with large-strain conditions.

3.3 Frequency distribution of the estimated QR nominal failure loads.

The frequency distribution curve of all the values proves to be strikingly normal-Gaussian as represented in Fig. 8a and also in Fig. 8b of cumulative frequencies. It must be noted that according to code and design requirements about 5% of the piles would have failed to meet the REJECTION CRITERION of failure load ≥ 85 tons required FS = 2 with reference to the design working load of 42.5 tons. In principle in the driven pile foundation under strict quality control easily applicable, those specific piles would be further driven (or redriven later).

3.4 Recurrence probabilities of extreme individual values of the failure loads.

The behavior of each pile is, in this specific subsoil profile (and in most routine cases), justifiably considered independent, and therefore the recurrence probability of each pile's maximum resistance (or nominal failure load) should be analisable under EXTREME VALUE DISTRIBUTION theories of statistics and probabilities. A fortiori the smallest values (of specific interest because of SAFETY) of the universe should be adjustable to one or other of such equations (or plotting papers) in use in engineering.

Among a few trials, the Gumbel distribution (much used in hydrology of maximum yearly floods at a given dam site) was found to give a satisfactory linear fit Fig.9.

The stunning probabilities revealed by the linear regression are approximately that : a) the probability of a single piles's nominal failure load descending to 72.5 metric tons, or a factor of safety of about 1.70, is of the order of 10^{-4} (visible on the graph); b) the corresponding recurrence probability of the nominal failure load getting as low as the design working load of 42.5 tons is of the order of 10^{-5} (computed from the expression, if it still remains applicable at such absurd extremes).

In order to place such conclusions in civil engineering perspective, let us consider the exponential disproportion regarding risks and cost of risks, stating summarily: a spillway is designed for the so-called 10,000-year flood, that is a 10^{-4} recurrence probability of the maximum yearly flood occurring in any year, and therefore, assuming a universe invariable with time, essentially a 10^{-2} probability of occurring within a 100-year useful life; as is well known, if an embankment dam fails by overtopping, material damages can be of billions of dollars, and losses of life can be of hundreds to several thousands. Mean-while if a single pile in a group of 792 reaches a WORKING LOAD equivalent to its nominal FAILURE LOAD, absolutely nothing is at risk (even without considering structural load redistribution); but, to exaggerate in order to quantify something different from zero, possibly one might be risking a fissure of tenth of mm, worth 50 dollars of repairs. Can Society countenance, and unknowingly pay for, such an absurd difference of design "risk-insurance" within the selfsame profession? To those who as members of Committees(1) discussing Codes lightly banter around with changes of FS values from 1.5 to 2.0 (or vice versa) such a revelation should be a loud call for reflection {(1) "... a group of the unknowing, appointed by the unwilling, to do the unnecessary."}.

Of the many absurdities in design practices, one lies in requiring the same FS per pile whether it is alone in supporting a column, or is one of a group for that task. Group vs. single pile soil disturbance is excluded from the present discussion, directed merely at the fact that a group of n independent piles working together should "statistically" behave more as the average of the n , rather than under the full variability-spread, pile per pile. The QR values associated with each pile were plotted on the foundation plan on Fig.10. For a first feel of grouping advantage the plan distribution was considered under two arrays: Firstly, groups of 2x2 adjacent piles, averaged for the available QR; secondly, groups of 3x3 adjacent piles similarly averaged. The resulting Recurrence Probabilities plotted on the Gumbelpaper are shown on Fig.9b, with obvious indications. The median values coincide, but the best-fit line obviously flattens more and more as the number of group-averaging adjacent piles is increased to 4, and further to 9. Obvious: extreme unitary values are attenuated, at both extremes. The risks and fear of extreme-ly low values, (already astoundingly low on single piles), decreases further very considerably.

3.5 Area distribution of QR values.

There is interest in drawing in plan the distribution of QR values pertaining to different magnitude-ranges: consistent differences either due to geological variations, or due to construction differences etc. can be evidenced. In Fig.10 we only show separated the areas of QR values lower than 80 metric tons. The geometric pattern sets aside any subsoil influences. On investigation it was identified that one of the five mobilized pile-drivers was associated with these areas, producing the consistent difference which could have been easily corrected in the process, were it not for the unusual urgency required of the pile driving.

3.6 Judicious evaluation of consequence of less-than- satisfactory performance.

In every structure we must be keenly attentive to a "water- divide" between the early phases when decisions (and risks) are generated on the basis of loads (predictively estimated) and stresses, and final phases when the only thing that matters is deformations and incremental deformations. Engineers aim at creating forms (shapes, functional), and fear de-formations: all the rest is but means to an end. Under such thinking Fig.11 shows that for any unforeseen incremental loading, or any unforeseen insufficiency of load capacity, the only consequence is a minimal RATE OF INCREASE OF SETTLEMENT of the order of 0.1mm per 35 tons. Such an absolutely inconsequent incremental settlement confirms with regard to deformabilities, the already discussed highly overdimensioned condition with regard to FS.

3.7 Miscellaneous

Because of space limitations and the paper's intent, many important issues on practices of design and construction-plus- inspection, plus codes, load tests, etc. cannot be expatiated. For instance: 1) the case concerned piles point-bearing in dense gneissic saprolite, driven through compressible marine clays under fill, and therefore anticipated for negative skin friction, on which factors of safety merit radical rethinking; 2) in big diameter point-piles, the tight (unnecessary) settlement limitations, not justified in the superstructure's tolerances, also lose rationality if presumed derived from the pile-soil load-settlement behavior, which, being of microstrains (justifying dynamic ≡ static) fall far short from failure; judgment and prescriptions must consider settlements as a percentage of diameter; 3) once the mud-tank dead load is totally acting and ulterior sensitive levellings finalized, what incremental loading could possibly require a global FS, and how incomparable is this with buildings of greatly different proportions of final dead load vs. incremental uncertain live loadings?

This study's aim is to employ the statistical data and manipulations of REJECTION CRITERIA extracted from such a documented foundation in order to reemphasize that Geotechnical engineering decisions are not based on averages of correlations, but on rejection criteria, and with progressive changes of construction practices the applicable idealizations for theorizing (and for recommending in Standards and Codes) should have suffered major changes.

3.8 Some typical pile load-test behaviors in a gneissic saprolite profile.

Many suggestions have been published regarding differences for bored vs. driven piles, both of lateral friction and base resistance ultimate loads. By far no generalizations should be permitted in fairness, yet most Codes lend the deafest of ears to varying realities!

Everybody recognizes quite varied execution effects, but at least three other equivalently important interferences are not emphasized quite as much. Firstly, the very values of "intact" in situ strength parameters as intensely investigated, determined, and interpreted, are far from constant or consistent in time and geography (cf. de Mello, 1981a). Secondly, greatly varying are also the stress-displacement behaviors depending on the pile type (execution effects). Thirdly, often much depends on how load tests are interpreted regarding stress distributions, "failures", and settlements, shaft friction with mm, and base with % DIAMETER.

Fig.12 is presented for piles executed for each specialized Company's working load according to best practice, and slightly adjusted to identical dimension (7.5m long, 0.5m in diameter), in a dense gneissic saprolite profile (very susceptible to stress release and densification effects). The slight adjustments necessary and judiciously applied gave results remarkably atuned. The three pile types were *a*) auger bored and/or bentonite-stabilized bored *b*) precast concrete driven *c*) standard Franki type driven without pedestal.

We concentrate on the bored piling, but first some comparative points are of interest. The driven piles achieve considerably increased rigidities, above all, besides the higher ultimate loads (Franki justifiably more): also, dispersions tend to be smaller (seems reasonable). Bored pile stress releases show-up principally in much increased deformabilities within ranges of centimetric settlements mentioned in Codes, and in practically no incremental resistance from the base.

Incidentally, once again (cf. de Mello, 1981b) it does prove increasingly odd to pursue further and further perfections of in situ "intact" definitions without concomittant FACTORS OF ADJUSTMENT regarding execution effects. Factors of Adjustment higher than 1.0 (driven) or lower than 1.0 (bored) must be further discussed regarding the concepts of Factors of Safety.

4 Compacted clayey fill dam and analyses of slope stabilities.

Over the past 50 years many hundreds of compacted clay fill embankments and dams have been constructed with meticulous inspecion and instrumentation. The material is not taylor-fitted into one of the idealized hypotheses of conventional geotechnique, but, counting on estimated billions of m^3 of "industrially-produced" material under closely similar quality-controlled specifications (around Proctor tests) there should have been facility at much improved formulations for better predictions and greater economies. However, Scott's Rankine Lecture (1987) closes with statements irrefutable and appalling, around the enquiry why "when, for a large earth dam, every single degree of flattening of the upstream and downstream slopes costs more than US$ 1 million dollars, earth dams always come out with slope angles of 2.5:1 or 3:1 "; thus, in general we overdesign, with unjustifiable incremental expense, and yet are dismayed occasionally by big failures, obviously dictated not by statistical dispersion but by theoretical misconcept.

To the best of my knowledge, all efforts, repeatedly concentrated on the important topic of slope sliding stability analyses have been dedicated to questions of CRITICAL SLIDING SURFACES, and perfection of inclusion of ALL EQUILIBRIUM EQUATIONS. Doubtless very laudable (cf. Leshchinsky and Huang, 1992). However, they would seem to have been prodded by a guilty conscience complex, of proving a) to structural engineers (of the 1950's) that we are not unaware of full requirements of static equilibrium equations, and b) to the soils, that they should respect our Mathematical rigours. Curiously and regrettably, meanwhile, through the 45 years of significant advances of testing and parameter interpretation of geotechnique, in separate watertight compartments, the historic hypotheses have not been cross-examined, and the consequent updating adjustments have not been made. Professional practice, economy, and field-laboratory-theory compatibilization of instrumentation-observations, have suffered inevitably.

The very important professional topic (involving hundreds of lives and hundreds of millions of dollars yearly) opens much more fertile avenues for additional reflection in sliding of natural slopes, and back analyses, and some recognized sophistications such as progressive failure etc. But our intent is served by restricting to RIGID-PLASTIC sliding stability analyses of compacted clayey fill dams.

In order to revise presently prevalent indications we must begin by imbuing ourselves in the implicit historical premisses. And, recognizing the collateral advances on such items as stress -strain-time paths, importance of stress history, in situ residual stresses, ageing, sensitivities of "structured soils" [(1)], we note that: *a*) limit equilibrium rigid-body sliding, reexamined too often (post Janbu, 1954) in third-order differences, is not in question; *b*) important concepts have not made any advance at all.

In chronological order of much published developments let us list: *1*) while triaxial testing incorporated differentiations of strength parameters as per rational and representative sequential (stress-path) incremental stressing, OBVIOUSLY INCOMPATIBLE WITH RIGID (infinitely undeformable) MATERIAL, the need should arise for comparable adoption of sequential reasoning into analyses of slope UNSTABILIZATION in lieu of slope STABILITY; *2*) obviously if soil element history is of consequence, the genetic condition (in situ residual stresses) could not be overlooked (especially after the milestone demonstration of Skempton and Sowa, 1963, regarding k'_0); *3*) obviously if the body is not "infinitely" rigid, and moreover, suffers from inevitable hysteresis, the routinely simplified conventional manner of considering BOUNDARY vs. BODY FORCES (stresses) must be corrected; *4*) finally, one should recognize and abolish a gross early conceptual error, of assuming that slope sliding failure are automatically bound to correspond to a condition of statics of $FS \equiv 1.00$.

4.1 Regarding compatibilization of triaxial testing and the interpretation of the construction-period unstabilization of an embankment, it is easy to understand the closed-cycle consistency, ON THE SAFE SIDE, of infant soil mechanics. What seems incomprehensible is the dichotomy of recognized importance of in-situ residual stresses and stress-strain-time trajectory (items *1*) and *2*) above) and the lack of compatibilization between laboratory testing and the questioned behavior of soil elements in the compacted fill, especially when compacted at or below optimum water content.

The question may be put forth in a very rapid summary. Assumptions have to be made regarding the lateral pressure σ'_h developed by the compacting pressure, and regarding the suction: it is important to pin-point the key assumptions because that is WHERE THE MOST EFFECTIVE ENGINEERING RESEARCH SHOULD CONCENTRATE. The great importance of reasonably correct measurements of pore-pressures and suctions [(2)] becomes immediately evident if we compare $K_0 \approx \sigma_h/\sigma_v$ and $\equiv \Delta\sigma_h/\Delta\sigma_v$ with intrinsic $K'_0 \approx \sigma'_h/\sigma'_v$ and $\equiv \Delta\sigma'_h/\Delta\sigma'_v$. [N.B. I have repeatedly entreated that we insist on the importance of using K'o, K'a, K'p because of the enormous incidence of errors in the practice of the profession using K factors on TOTAL STRESSES]. Remember that errors in measurements only occur in the direction of attenuations, of both positive and negative pressures.

If we assume a compacting $\sigma'_v = \mathbf{60\ t/m^2}$ and $\sigma'_h = \mathbf{36\ t/m^2}$ together with $\mathbf{u = 0}$, the evaluation of "initial conditions" of the soil elements in the compacted lift (ready to receive the first increment of overburden pressure) can be estimated as was done for

(1) distinguishing between those due to salt lixiviation (Scandinavia) vs. those due to micro-cementations (our interest), grain mineral alterations, etc.

(2) To begin with, in view of the recent (Sept.'92) developments at Imperial College of tensiometers reading instantaneous suctions up to 15-18 bars, all publications based on previous tensiometers, roughly limited to cavitation at 1 bar, should be WITHDRAWN FROM MENTION UNLESS READJUSTED.

undisturbed clay samples via A and B coefficients, etc. Skempton and Bishop, ca. 1948-54 (Geotechnique).

Let us assume that with total relaxation of vertical stress, and small (immediate) relaxation of lateral stress, we might start with ($\sigma_v = \mathbf{0}$, $\sigma_h = \mathbf{10}$, $\mathbf{u} = -\mathbf{15}$) and therefore ($\sigma'_v = \mathbf{15}$, $\sigma'_h = \mathbf{25}$)), Fig.13. Both by reductions of suction and by increase of u, there has to be a gradual decrease of K'_0 (to which the only applicable definition is $\Delta\sigma'_h/\Delta\sigma'_v$, variable). Let us assume that the variation of K'_0 occurs from 0.6, to 0.5, to an asymptotic 0.4 (Fig.13).

The stress trajectory is directly calculable as summarized in the Table and Mohr diagram of Fig.13. The obvious result is that the first increments of overburden do not establish an unstabilizing deviator, as was implicit and is explicit in the infant triaxial testing and stability analyses. Infant soil mechanics, interested in embankment loading of soft saturated clay sediments, had assumed (on the safe side) the testing to start from isotropic stresses, every iota of the embankment loading becoming a deviator stress, driving force: a consistent, conservative closed-cycle hypothesis and testing-plus-analysing procedure. For us, however, for the test-specimen to minimally represent a soil element of a compacted clay dam, two (minimum) conditions must be respected: a) the initial void ratio ε_0 (3) , b) the initial stresses (4) . Thus the compacted specimen would be "prepared" under initial σ'_3 greater than σ'_1 , and the increases of σ_v (accompanied obligatorily by increases of σ_h , respecting K'_0) corresponding to increasing overburden would ONLY BEGIN UNSTABILIZATION AFTER PASSING THROUGH THE ISOTROPIC CONDITION, of logically explained early prudent hypotheses (5).

Thus, for reasonably realistic testing conjugate with unstabilization analysis, the stress path from position 1 (Fig.13) to position 2 (corresponding to topping off at crest) should be postulated, adopting the conservative assumption of "instantaneous elastic loading" (not constant volume, because of pore air compressibility). Thereupon for assessing the still remaining FS, ALTHOUGH NO FURTHER LOADING IS ANTICIPATED, in accordance with Civil Engineering Principles of maximizing unknown interference, the "range" of possible stress-strain-(time) trajectories (shown on Fig.13), up to the strength envelope, should be inquisitively investigated.

In cases of debatable issues, the different hypotheses should be given due consideration: such are the cases, for instance, of the dominance of failure criteria (σ'_1/σ'_3 or $\sigma_1 - \sigma_3$) and stress-strain brittleness, progressive failure etc.

One fact is definite: in deciding on desirable FS regarding construction-period stability we should investigate the RATE OF CHANGE OF FS on the same critical sliding surface as the last increments of height and overburden stress are accrued: if the drop of FS is more rapid, we should take more careful DESIGN DECISIONS. This is much more important, to geotechnical engineering, than the delving into second-order diferences of FS values computed by different THEORETICAL VARIATIONS ON CRITICAL SURFACES AND PERFECTIONS OF EQUILIBRIUM ANALYSES. What covenant does Nature have with our idealized hypotheses?

(3) The "type" of soil being pre-established, the "quality" of the soil element is expressed through its compaction parameters, percent compaction and (water content) leading to an (ε ,S).

(4) If in applying the initial stresses some $\Delta\varepsilon$ interferes, there are many ways of adjusting back to the desired ε_0.

(5) An important detail for the typical unsaturated specimen with suction would be to avoid top and bottom contact with water, possibly using adequade NON-WETTING FLUID (e.g. castor oil, etc.)

4.2 Subsequent unstabilizations: due consideration of change of conditions from starting stability.

Over the past 45 years routines have been established (and especially proliferated via idolized computer programs) that invite many criticisms. To begin with, it hardly need be emphasized that our knowledge is never absolute and abstract, but only relative, compared to some reference. Moreover, because of the unsuspected interfering unknowns, in any calculations our precisions are inexorably better in comparing, by identical procedure, two situations, before and after introduction of an unstabilizing factor, than in running an independent analysis. Thus, for instance, in a dam slope if at the end of construction a $FS \approx 1.3$ has been postulated (and seems confirmed by observational evidence), the subsequent unstabilizing provocations by reservoir filling (and ulterior drawdown) should be computed via CHANGES OF CONDITIONS from each prior "stable condition" (at estimated FS values). The same applies to natural slopes, of course.

In practice, there is NEVER A STABILITY ANALYSIS as implicit in statics, which really implies the "growth" of the weight, normal, and tangential forces on the base of each slice as if starting from scratch. The primitive idealization, soil element going through maximized path [(6)] from isotropic conditions to $(\sigma, \tau)_f$ condition, is ENGINEERING ON THE SAFE SIDE, which was compatibly imitated by the early tests. In updated professional practice the obligatory procedure must be: "Exactly same STATICS must be applied for TWO ANALYSES (1) stable status quo, (2) introduction of unstabilizing factor (which, incidentally, in many cases may be a strength decrease and not a stress increase). Thereby, for any reasonably ESTIMATED INITIAL FS, we obtain a reasonably CALCULATED FS, CHANGE OF CONDITIONS, obviously with much less error. Incidentally, under such conceptual thinking all triaxial testing becomes subdivided into the two phases of "PREPARATION OF SPECIMEN UNDER PRIOR-EXISTING STRESS CONDITIONS" and "DEVIATOR STRESS APLICATION" which corresponds exactly with the family of CU triaxial tests long since recognized and used.

Many important points call for updating comment, but I submit only three: regarding incorporation of pore pressures; regarding expressing strength equations for ENGINEERING; and regarding diametrically opposite trends of "STRUCTURING" in different soils.

4.2.1 Pore pressures. Flownets; cleft water pressures; excess pore pressures due to remoulding at the shearing plane.

The "Basic Seepage Force Relationship" beautifully demonstrated by Taylor 1948 pp. 200-204, and systematically used, cannot continue to be accepted if stress paths are important and the shear strength equation is NOT A PERFECT STRAIGHT LINE. Having persistently emphasized this since 1967, I could not but rejoyce at finding myself recently accompanied by a paper in prestigious Geotechnique, London (cf. King, 1989). The perfect equivalence of [TOTAL FORCES - BOUNDARY NEUTRAL FORCES] = [GRAVITY EFFECTIVE FORCE + (vectorially) SEEPAGE EFFECTIVE STRESS MASS-FORCE] has been used without exception, but CAN ONLY BE VALID if the unit volume considered is RIGID, and the strength equation so straight (and with ZERO HYSTERESIS) that under any and all conditions of reaching failure the $\Delta s/\Delta\sigma$ is a constant. The rate of installation of altered flownets should generally permit confident use of piezometers, and adoption of effective stress STRENGTH EQUATION based on σ'_1/σ'_3 failure criterion.

(6) *Incidentally, if we recognize a ± x% dispersion around the average assumption, the final error of prediction is much greater if we travel the maximized path from (0,0) than if we travel shorter realistic path from $(\sigma,\tau)_1$ to $(\sigma,\tau)_2$*

Quite distinct should be the case of transitory excess pore pressures generated by "compressibility" (including, obviously, construction-period $\Delta u = f(\Delta\sigma)$ of dams), and/or "remoulding" representing fast collapse behavior along the shearing surface at the INSTANT OF SLIDING. This is, indeed, a BOUNDARY NEUTRAL FORCE, and the manner of inclusion of this u or Δu in the statics of boundary forces retains validity. But in the case of shearing Δu, with due respect to its LOCALIZED AND RAPIDLY TRANSIENT QUALITY, for prudent ENGINEERING DECISION the resisting forces should be expressed on the basis of $(\sigma_1 - \sigma_3)_{max}$ as a function of PRE-EXISTING STRESS CONDITIONS, prior to the unstabilizing provocation.

In similar manner, with reference to Fig.14 it is necessary to reflect on how such "instantaneous" applied boundary forces as the load Q and the CLEFT-WATER PRESSURES U_c should be judiciously incorporated into analyses. Their incorporation into the body's static equilibrium is automatic, but the effect on the available SHEAR RESISTING FORCES is a debatable point, because of both geometric and time factors of transmissions from one boundary to the shear surface boundary. The non-rigid body, the importance of stress path, and the curved strength envelope have long since repealed the right to simplicity of analyses as developed in conventional geotechnique.

4.3 Rejection of a presumption that all failures back-analysed correspond to a condition of FS = 1.00.

On looking back at the milestones of the late 1950's synthesized in the ASCE Boulder Conference, 1960, one can well understand that there were very important priority needs of a decisive victory of EFFECTIVE STRESS vs. TOTAL STRESS ANALYSES, and this in the FIELD case histories (simultaneously establishing confidence in laboratory tests with pore pressures, and in field piezometers and observations). Moreover, it was still a period of deterministic credulity, few tests for statistical dispersions, each new test and analysis taken at face value as "the truth". Thus viewed, one can understand the justificable emphasis of those times, of back-analyses "proving" that FS = 1.00 of failed slopes.

What cannot be condoned is that the hypothesis should have become a postulated DOGMA, and PERPETUATED without exception, despite it being obviously wrong, and implicitly presuming on the culmination of knowledge, at any given period-time-case. The perpetuated practice has become a serious deterrent to progress, and to switching from STABILITY ANALYSES to ANALYSES OF UNSTABILIZATION. Incidentally, for the "well-documented" cases then published, the a-posteriori presumed knowledge of u at the failure surface and failure instant (even in Sensitive clays) was a fallacy, and because of the inevitable tendency to under-estimate INSTANTANEOUS-LOCALIZED u, has been compensated by LOWERED BACK-ANALYSED STRENGTHS. Yet another closed-cycle which fails to close as **s** and **u** determinations have improved progressively.

The less incorrect principle seems so clear that I permit myself to be very brief. Failing corresponds to PASSING THROUGH FS = 1.00, and NOT BEING AT FS = 1.00. From a reasonable initial FS, on calculating the unstabilization ΔFS we check if we would go to final FS values below 1.0. A triggering factor leading to $\Delta FS = 0.5$ from $FS_1 = 1.3$ to $FS_2 = 0.8$ should be much worse than $\Delta FS = 0.32$, from $FS_1 = 1.3$ to $FS_2 = 0.98$: both triggering factors would lead to failure.

Incidentally, the differences between calmly re-stabilizing slides, and explosive slides, should be related to ΔFS magnitudes and FS_2 values.

4.4 "Structuring" in soils; postulated diametrically opposite trends likely.

My insistence has been that "mental testing" should precede physical laboratory testing, cases of serendipity excluded. The subject of "structuring in soils" has received very little attention or testing, the principal case being that of the Scandinavian clays. For the case of

TROPICAL SAPROLITES, conditions may be very different, and probably varying with time.

In sedimentary clays conditions of TIME-AGEING have been reported both of INCREASE OF STRENGTH (secondary compression) and Sensitivity, and of LOSS OF STRENGTH (probably via salt lixiviations affecting colloid-chemical attractions) concomittant with increased Sensitivity.

It is of greater interest to focus on this second hypothesis because slope unstabilization can thus be triggered without stress increase, merely by progressive STRENGTH DECREASE. Fig.15 presents the comparative hypotheses schematically. The generally present increase of Sensitivity (7) makes these behaviors extremely important for engineering, because of loading to catastrophic failures. In young saprolites the deterioration from rock to saprolite obviously corresponds to a loss of strength by lixiviation and mineral changing. However, after reaching stable mineralization and going through some minimum soil strength, in very old saprolites there can be a phase of increasing micro-cementation, bonding, corresponding to an INCREASING PEAK STRENGTH.

5 Principles of subsoil profiling; dire abandonment in Tropical Saprolites.

Essential in profiling for engineering purposes are 1) homogeneity 2) adequate characterizations of (2.1) quality-type of material (2.2) physical condition (density) of material 3) stress conditions of soil elements in the profile. Partly by intent and partly by luck the subsoil profiling principle fitted logically in saturated sediments. However, even in sediments, if we move to gross unsaturation and the scale of tens of millions of years (as in our tropical Tertiary strata) we are already dealing with "super-aged residual sediments" with microcementations, macropores, weathered grains, clayey nucleations etc., which are not at all well and representatively characterized by the conventional tests.

If we further move to saprolites, it becomes evident that practically none of the engineering bases of conventional soil mechanics are even remotely applicable.

Firstly, saprolites are marked by extreme heterogeneity (cf. Fig.16) and no well-defined boundaries between horizons.

In fact, to begin with they have been interpreted (de Mello, 1972) as bridging between Soil and Rock Mechanics, requiring CHARACTERIZATIONS both of the soil MASS (itself very heterogeneous) and of DISCONTINUITY FEATURES. In problems of shear, tension, preferential seepages, etc. the discontinuities tend to be dominant, with parameter definition via MAXIMA OR MINIMA: meanwhile, obviously in problems of compressibility, mass permeability, behaviors are dictated by WEIGHTED AVERAGES OF THE MASS. However, since weathering attack is by "natural selection" the starting weakest masses or minerals automatically inviting preferential progressive attack, the mass itself presents enormous heterogeneities between hard lumps, and surrounding or nearby soft volumes.

Needless to say, in every respect the grains themselves are of extreme heterogeneity (in mineralogies, specific gravities, sizes, shapes, crushability, etc.), and sieving or sedimentation testing is criminally unrepresentative because they become totally destructive tests, like killing a humming bird with a bazooka, destroying the very essence of the "structured soil" of grains never yet permitted to develop their full potential lyospheres in free water. So also the Atterberg plasticity tests are absurd: claylike materials that present a granular behavior because of the micro-nucleations, lead to completely misguiding plastic parameters.

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(7) As very inconspicuously shown in my Rankine Lecture, 1977 (Fig.37), I submit that Sensitivity as defined by infant soil mechanics is but a crude index, serving to show when "Brittle behavior" should be more closely investigated. Further, the Brittleness Index suggested by Bishop 1971, does not seem to be as significant as the one that occurred to me as suggestive of progressive failure tendencies of different speeds.

As a side issue to this paper I should mention infant testing along the idea that, SINCE WATER IS THE DAMAGING AGENT, both for "incompressible saturation" and for minimizing destructive testing we should revert to adequate (ref. surface tensions etc.) NON-WETTING LIQUIDS (mostly organophilic, cf. de Mello 1981a). For first-degree approximation the testing used unconfined compression tests in two comparative conditions 1) exposed to air 2) submerged in the liquid immediately preceding the start of loading. Fig.17 shows that in the face of moderately rapid response there are conditions for use of non-wetting liquids. One could imagine using light (to be controlled after due research) jetting by such a liquid for wet-sieving of saprolites, for sedimentation in such fluids for more representative grain-cluster characterization, and even for incremental fluidification for comparative pseudo-Atterberg liquidity and plasticity testing.

The fact has already been mentioned, but calls for repetitions , that in most saprolites (duly identified with regard to parent rock and relict planes based on geologic structure) characterization of geotechnical parameters IS INCOMPLETE AND UNSATISFACTORY if the different parameters regarding MASS and DISCONTINUITES are not tested and quantified.

With regard to the mass, because of the existence of volumes of greatly different "hardness" side by side, some radical revisions were proposed for geotechnical thinking and calculation procedures (de Mello, 1972), which regrettably have not had any implementation through the 20 years. Some fundamental principles may be repeated for desired impact:

(1) Whereas in sediments we start with knowledge of stress (geostatic etc.) and attempt to derive strains, in saprolites the dominating acceptance should be of the PRINCIPLE OF EQUIVALENT STRAINS. Strain differentiation leads to disintegration etc., and, after thousands of meteorological (etc.) cycles the (geologically transient) human scale stability corresponds to adjacent differentiated materials suffering equivalent strains (or being duly sacrificed by "natural selection"). Thereupon, for instance, if we apply any (stress) it should be carried principally by the stiffer lumps (generally arrayed in succession based on the geologic structure), stress distribution between hard and soft adjacent volumes being such that exteriorized strains should be identical. For structural engineers the easy analogy is that of stress distributions between reinforcing steel and concrete in a reinforced concrete column.

(2) Nothing is homogeneous, neither are soil elements in soft recent sediments. Adopting a statistical dispersion, it is inevitable that no soil element in principle needs to become any more rigid or resistant than minimally required to support the overburden stress. The rejection criterion is from the direction of weak growing in strength, and, in accordance with Nature's specification of not squandering factors of safety, as soon as the minimum requirement is attained, presumably there is a stop: thus, in soft consolidating sediments it seems reasonable that the weaker soil elements should dictate weighted-average behavior. If so, one would conclude that the use of less favorable parameters in sediments would not be only a question of prudence, but also a reasonable engineering policy.

Quite in the opposite direction, a saprolite is corroded from rock, in which internal stresses (both mass, and in petrographic structure) are much higher, and gradually release. Now, therefore, the material starts from high strength and internal stresses, and obviously the rejection criterion is from strong to weak. The weaker soil elements are only allowed to weaken in the measure in which the strong ones carry the stresses (incidentally, in the extreme of such reasoning there are the solution cavities sustained only when the surrounding rock permits such natural tunnelling redistributing the stresses). Stresses are dominantly carried by the harder lumps, and (volumetric or weighted-average) stress-strain behaviors are determined by the less deformable masses. Incidentally, rough observations of structures on saprolites have generally shown that deformations in prototypes tend to be very much smaller than anticipated from tests (partly also because our vitiated tendency has been to sample the weaker volumes).

Although it would seem imprudent pending the accumulation of good data, the indication is that in SAPROLITES we should USE GEOTECHNICAL PARAMETERS AS DETERMINED FROM THE HARDER LUMPS, leading to much more optimistic and cheaper solutions. Without some rationale, there isn't even the target or incentive to direct testing and observations in a realistic and effective manner.

Incidentaly, in the application of rejection criteria (never on averages, always on maxima or minima) it stands out that the moderate homogeneity of sediments has permitted long-standing shrouding of GEOSTATIC STATISTICAL STRESS REDISTRIBUTIONS ($\gamma z \pm \tau z$) DUE TO "HANG-UP". The much greater heterogeneity of saprolites (inevitable) immediately focusses attention on the problem: the softening is progressive (cyclic); some seam begins to soften more than adjacent rigid lumps; thereby redistribution of stresses forces further decreases of the stress on the soft volume (below average γz adopted ipso facto in conventional soil mechanics) permitting it to soften even more; and so on.

Ever since the phenomena, consequences, and, analyses of "hang-up" were acutely brought to the fore in higher compacted earth-rock dams (mid 1960's) it gradually became shocking to realize how the sterilizing dogma has avoided geotechnicians recognizing some hang-up redistribution in Natural soils. With regard to the geomorphology of piedmontic flood sedimentation of big boulders side by side with lenses of fine sand (as apparent in Tarbela Dam valley scarps, cf. de Mello 1981c) it would seem inevitable that the foundation materials would be highly loaded on the boulders and essentially loose and unloaded in the adjacent sand lenses.

Adequate sampling and testing in saprolites is difficult enough because of the fragility of microcementations, macropores, water-susceptibility, unsaturation, presumed very low suctions, etc. However the greatest stumbling block has been that the scant testing and mental modelling has been patterned on conventional geotechnique, elasticity continua, homogeneity, etc., that have nothing akin to a postulated REALISM OF PRINCIPLES OF NATURAL-SELECTIVE CORROSIONS FROM ROCK.

6 The high compacted earth-rock dam as an example of a professional engineering challenge.

This case serves many purposes, not the least of which is to emphasize to younger professionals WHAT IS ENGINEERING OF AN ENGINEERED STRUCTURE, AS COMPARED WITH PASSIVE COMPUTATIONS, modernly an ever more dominant occupation, and how we accept analogies, and compromises with "good enough" solutions, in comparison with idealizations that could confuse dominant issues.

In principle the compacted earth-rock dam embodies direct dominant-parameter optimizations of two primordial functions of dams, the rockfill stability of the shells and the imperviousness of the core. In practice, however, it is not easy to use two high-pitch prima donnas in the same opera: the need for transitions was blatant because of collateral, inexorable, parameters.

It is also of conceptual interest to observe that the first transitioning recognized, and fulfilled, concerned problems of failure, serious failure under full reservoir seepage, the Extreme Value type of failure condition caused by piping (de Mello, 1977): thus the primary transitioning sought was with respect to filtering, more severely critical because of the extreme differences in permeabilities and effective pore sizes. One may profitably recall that the extreme value problem found its filtering solution via a physical change of statistical universe, and not by variations on the theme of computations, of gradientes and erosive potentialities, in a fixed physical-statistical universe. The questions that have arisen and persist, with regard to erstwhile filter criteria, are set aside from the present scope, although it behoves one (professionally) to remind that the capacity of rockfill macropores to hide for long periods any muddy water evidence of core erosion does hint at the adage that the higher the rise, the greater the fall; the apex of an ideal solution may yet conceive a worst collapse. The single message herein intended concerns the other needs for transitioning between totally distinct materials.

As is well known, from among the dominant geotechnical behaviors, of strength, deformability, and permeability, questions of strength-stability do not intervene in conventional cross sections: therefore, attention is now focussed on compressibility-deformability, and, for simplicity, considering only the bi-dimensional upstream-downstream condition, although recognizedly the critical condition occurs tri-dimensionally in the core-abutment contact transition, for which, however, an entirely different physical solution is obviously envisaged.

Fig.18 summarizes the Finite Element mesh used for facilitating direct comparisons by substitutions of behavior-parameters within selected elements, and also the characteristic stress-strain curves and concomittant strength equations for shear plastifications. The filter-transition column is by far the most rigid, and controls the core hang-up (silo effect) that leads to tensile cracking and tendency to hydraulic fracturing. On the rockfill side there is no problem except that the downward shear overload due to the greater rockfill settlement may result in hearing of the filter-transition zone if it is not sufficiently robust: such a possible behavior would be particularly dangerous on the downstream side, if the continuous integrity of the filtering panel is broken.

The mental model of the solution, as schematically represented in Fig.19, suggested itself from typical "LOOPS" used in industrial pipelines to give them the desired average deformability by much "softened" localized loop deformabilities. The conceived solution had been firstly checked by rough elasticity (Hooke's law) calculations of the adjacent "columns" of the different materials. In order to avoid dispersions in the comparisons, identical densities were used in all materials. In the FEM program used, displacements are given along the boundaries, and stresses at specific sampling points within the elements. The example has been much idealized (more than initially desired) because of time constraints, but furnishes also a background for mentioning some of the accomodations required in the dialogue between physics-engineering near realities, and FEM-mathematical idealizations.

The FEM mesh set up started with some premisses anticipated to be realistic, and optimized, but which gradually served for gaining experiences, pin-pointing details to be revised, as is being done for an immediately forthcoming publication of more direct recommendations for dam specialists. For instance, as a simplifying start, with great benefit/cost advantages, the typical case can be represented as symmetrical, concentrating on much more detail in one-half the cross-section. Meanwhile, for the present purpose, the very reasonings, trials, dissatisfactions, and adjustments undergone are thought to fit within the intent of this paper, of emphasizing computations (analytic or numerical) as MEANS AND NOT ENDS in service of engineering.

Interest concentrates on core elements near the top, where the feared effects will be felt. Bottom layers, that transfer lesser contributions to the top, can be thicker; only the top layers have to be as thin as possible. Along the cross-section one would wish for very narrow elements at the filter-core interface, or even plan the use of JOINT-ELEMENTS: the interest in the interface behavior is justified in a "scientific quest" to interpret the local plastification that can be realistically anticipated, but that in computations depends too much on constitutive equations etc.. In the ENGINEERING ANALYSIS we accept the local facial failure, that would decrease minimally the width of undamaged core, and concentrate attention on stress changes at points somewhat further within the core (cf. sampling points shown in the crest detail, Fig.18). For clear comparisons the footing-loops were made equivalent to mesh elements: however, the recognized need for such "footings" of width not less than about 3x the height, jeopardized anticipated results somewhat by being too wide; finer meshes, and compositions of adjacent elements is the forthcoming adjustment. In the schematic comparisons of a narrow vs. wide core it transpires that the compressive longitudinal stress transmitted to the filter transition as a column calls for the loop solution very much more in the narrow core case, while the wider the core the more the filter-transition deformations tend to bend smoothly, because of the thick cushion of deformable core vertically underlying, thereby decreasing the requirement of footing-loop compressibility of the rigid column: the forthcoming mesh incorporates subdivision into more positions of inclined filters, simultaneously subdividing into narrower elements for improved definitions of variations. Reflection on some of the results achieved to-date point to the need of changing the (E, μ) parameters on (real) stress releases in comparison with those effective on loading, more than merely by 50% as adopted.

One point of interest regarding computational techniques transpired after a few comparative runs of homogeneous embankment vs. conventional markedly zoned earth-rock embankment: the stress redistributions resulted much smaller than reasonably anticipated, when using the conventional stepped construction. The fact is that the mathematical stresses-strains being instantaneous (which, to different degrees, reality never is), only small redistributions occur corresponding to each incremental thickness, and the top of each layer establishes an elevation platform at equilibrium, for superposition of the next layer. This effect shows up all the more when the top layers are made thinner. Thus, in trying to benefit precision for the top we thinned the top layers, and, thereby, ipso facto altered the desired top redistributions, attenuating them. In order to eradicate this effect we used the technique of using normal stiffer parameters during construction, and forcing a "collapse" of the material afterwards.

Note the difference of principal results (vertical stresses in core and filter) in two comparative cases (Fig.20): 1) building the zoned embankment to the top as per routine; 2) after reaching the top of the 7^{th} layer (just as an example) constructing the 8^{th}, 9^{th}, 10^{th} layers hypothetically first as a homogeneous embankment of filter rigidity, and only after reaching the top, "collapsing" the core to its true modulus.

Incidentally, for the computational technique of forcing a "collapse" of any element or zone, it is necessary to apply an imaginary (stress) by increased gravity in the element, as is done, for instance, for collapse settlements of rockfills of upstream shoulders on submergence by reservoir. The $\Delta\gamma$ and ΔE for the case are judiciously chosen, respecting the product for desired strains and stresses. In the cases of the major "collapses" required to represent the footing-loops, the total collapse envisioned had to be subdivided into steps in order not to derange the computations.

The stress redistributions achievable by footing-loops in specific layers (3^{rd}, 5^{th} etc.) or combinations of layers, are summarized in Fig.20. The capacity to benefit stress redistributions in the core near the crest is well demonstrable, and optimizations will be parametrically analysable for design orientation.

It appears logical that the effectiveness should only start with footing-loops around mid-height (where compressive increments are highest) and upwards (for closer transmission of effects).

Adjustments of positions, thicknesses, and moduli, seem quite rationally indicated. The point is that all attempts at solutions without altering the basic geometry-physics-geomechanics of the section a) on the one hand, tend to cause second-order effects only b) tend to cause opposing trends, damaging in one direction while improving in the other c) do not provide a SUPERABUNDANT SOLUTION that can be adjusted at will to meet real-case demands.

The worst of all such intuitive attempts at adjustment comprised the compaction of the core to wetter, "more flexible", lower percent compaction GC% conditions: it can be seen to result dangerous for many reasons, and especially so because of the higher construction pore presures and RETARDED CONSOLIDATION SETTLEMENTS (more damaging), as against the instantaneous settlements of the granular zones. So also, making the filter--transition more robust involves unquantifiable effects, some favorable, some not. But principally one notes the Trojan horse with respect to filter-transition specification. Increased care in minimizing porosimetries of the filter (especially continuous grainsize curves, and higher compaction) only decreases the deformability: that is, a systematic improvement of filter transitioning irrevocably leads to aggravation of the deformability-discontinuity.

Note, in passing, that FEM embankment analyses to-date do not give trustworthy tensile STRESSES, but merely tendency to incremental STRESSING in the tensile direction, under the geostatic increments of loading by overburden.

7 Conclusion

Few are the examples that can be brought to a given sitting, hearing, reading, and focus. But the message stands, of life and spirit as a wayfaring and restless flight that should ever be exalted. As I expressed at the San Francisco Conference Presidential address, no function merits being recognized as VITAL if it is not very much alive, creative, progressive, ... therefore SELF-EFFACING, as the principle of Life imposes Death. Let us ever remember that absence of evidence is not evidence of absence. In the wisdom of Hippocrates (c. 500 BC) "Life is short. Art is long. Knowledge is elusive. EXPERIENCE IS TREACHEROUS. Judgment is difficult", but to judge, right or wrong, is a perennial imposition of life, of Life's search for Itself, in Bayesian microadjustments YES-NO.

Great are, indeed, the responsibilities that we geotechnicians and professionals of South America must shoulder on behalf of our fellowmen and societies, in extracting the essence of advancements from richer societies, while challenging and decrying any automatic application of their regulations and rituals.

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cf. BISHOP et al. 1975, 77 :

"... THERE IS NO UNIQUE RELATION BETWEEN STRENGTH AND AND WATER CONTENT."

"...THE MODIFIED EFFECTIVE STRESS EQUATION NECESSARY TO RELATE POREWATER TENSIONS TO THE STRENGTH OF PARTLY SATURATED SOILS IS OUTSIDE THE SCOPE..."

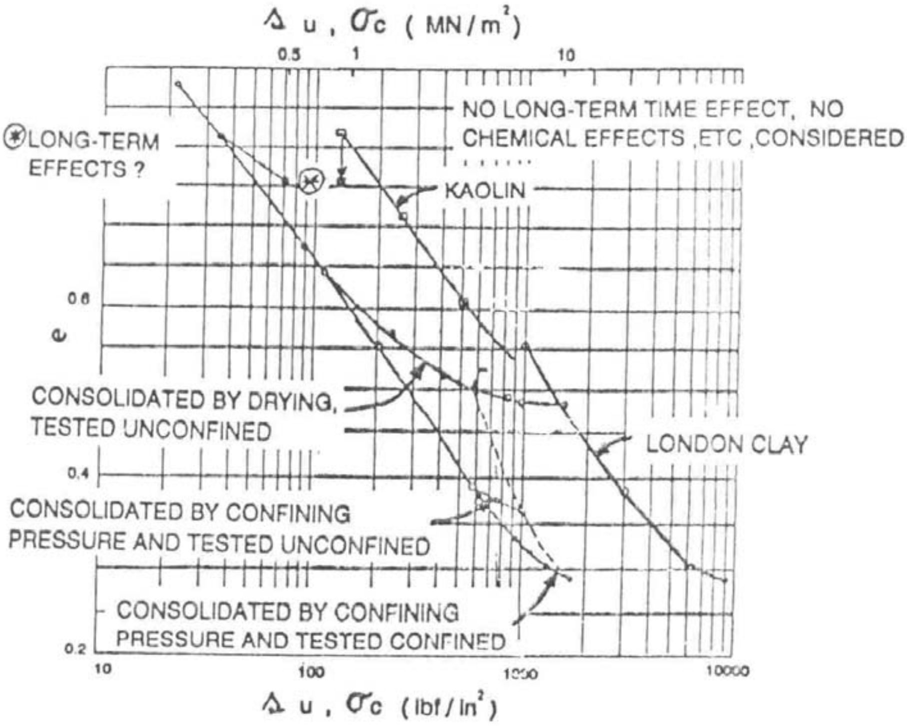
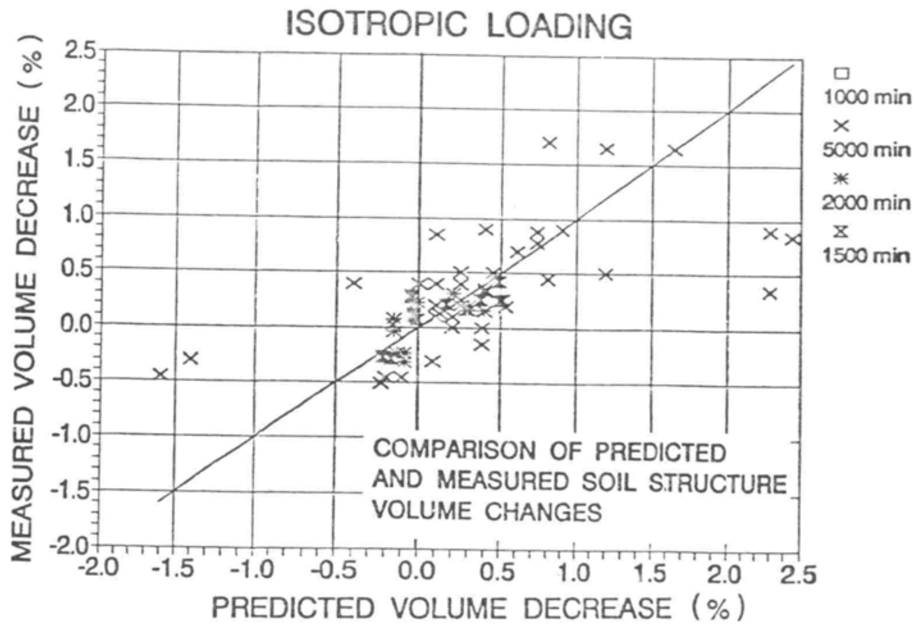
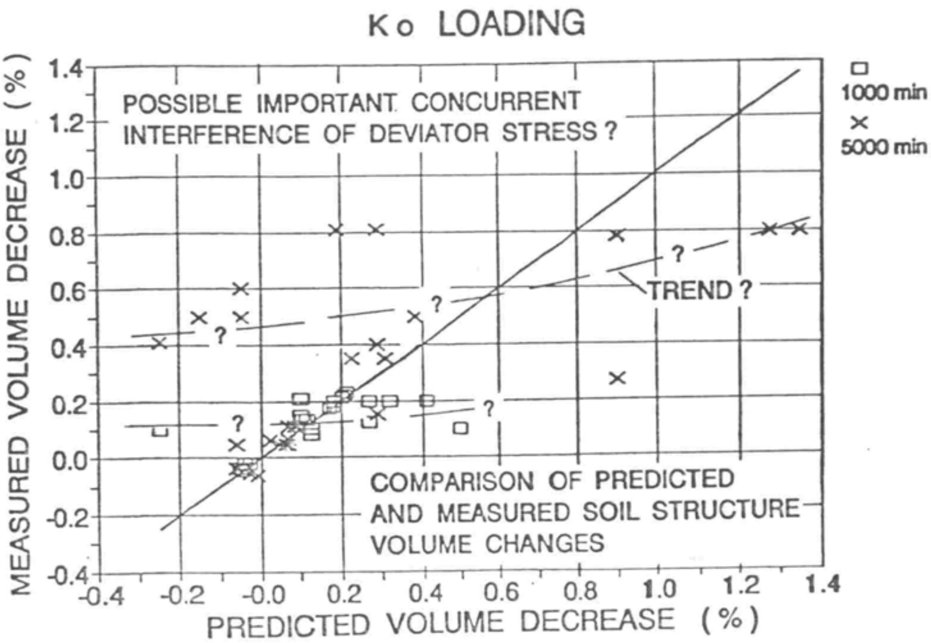


Figure 1 - Void Ratio vs. Consolidation Pressure and vs. "Cohesion"


ISOTROPIC LOADING
MEASURED VOLUME DECREASE (%)
PREDICTED VOLUME DECREASE (%)
2.5
2.0
1.5
1.0
0.5
0.0
-0.5
-1.0
-1.5
-2.0
-1.5
-1.0
-0.5
0.0
0.5
1.0
1.5
2.0
2.5
1000 min
5000 min
2000 min
1500 min
COMPARISON OF PREDICTED
AND MEASURED SOIL STRUCTURE
VOLUME CHANGES


Ko LOADING
POSSIBLE IMPORTANT CONCURRENT INTERFERENCE OF DEVIATOR STRESS ?
TREND ?
COMPARISON OF PREDICTED AND MEASURED SOIL STRUCTURE VOLUME CHANGES
1000 min
5000 min
MEASURED VOLUME DECREASE (%)
PREDICTED VOLUME DECREASE (%)
1.4
1.2
1.0
0.8
0.6
0.4
0.2
0.0
-0.2
-0.4
-0.4
-0.2
-0.0
0.2
0.4
0.6
0.8
1.0
1.2
1.4

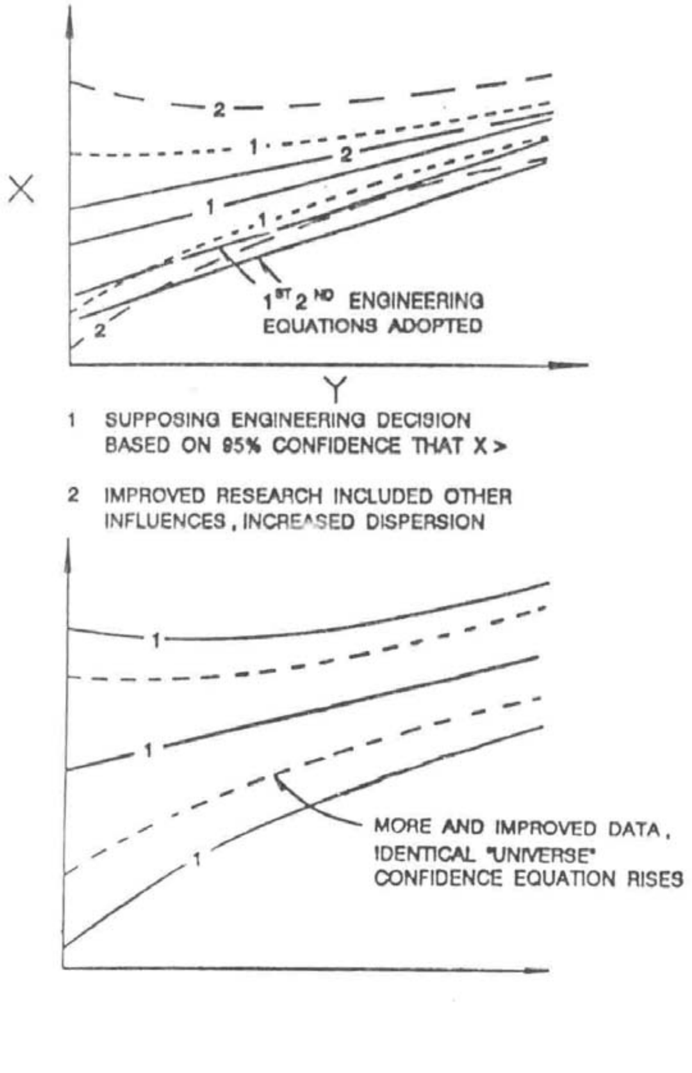


Figure 4 - Engineering Decisions as Affected by Statistical Dispersion

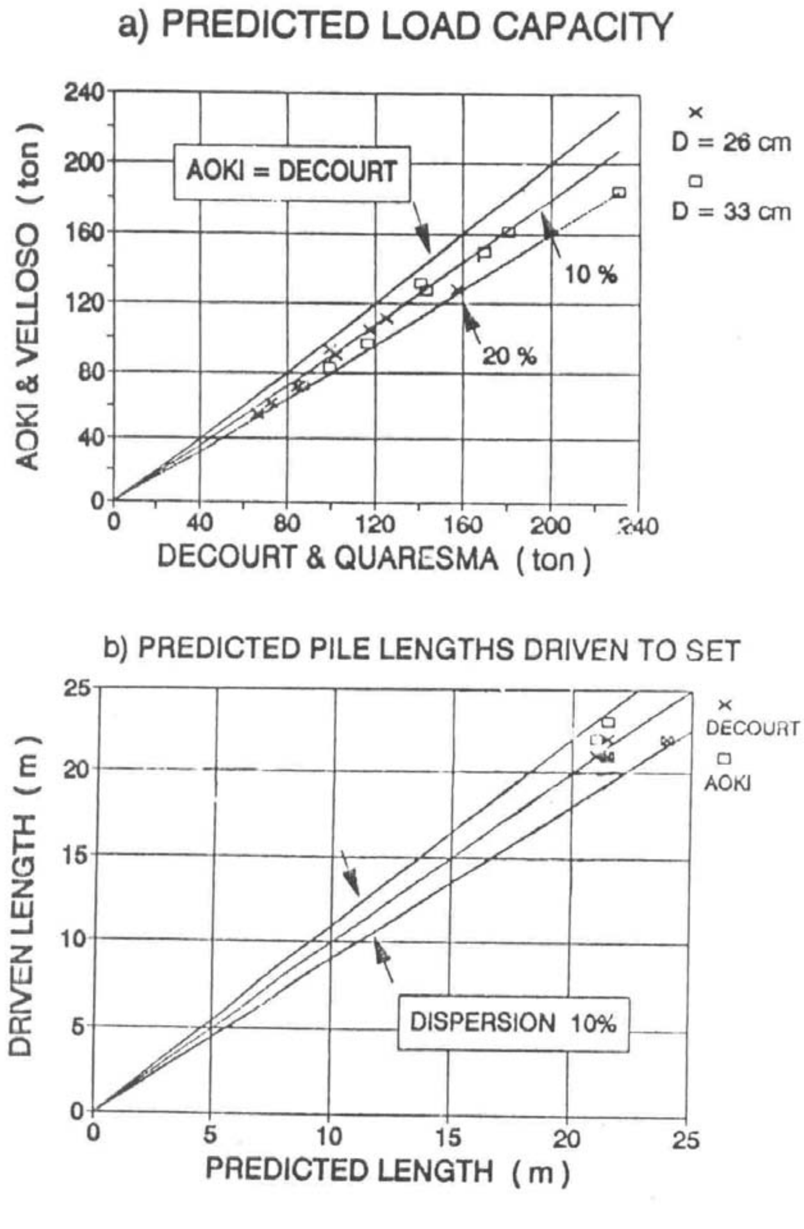


Figure 5 - Predicted vs. Real Based on SPT Statistical Prescription

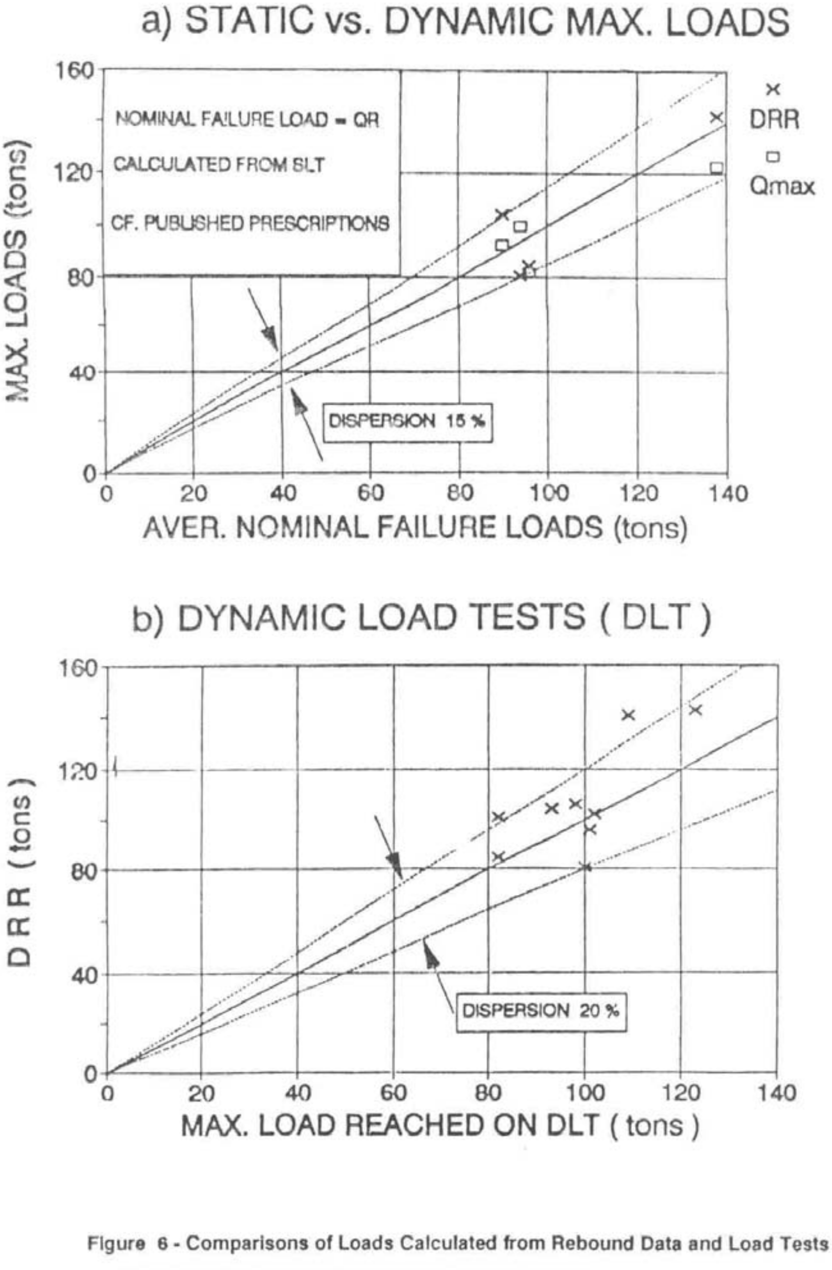


Figure 6 - Comparisons of Loads Calculated from Rebound Data and Load Tests

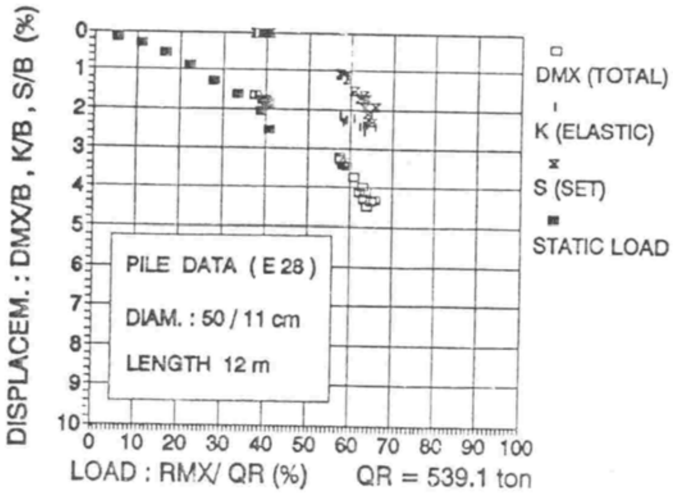


FIG. 7 Widely Published Experience, Similarity of Dynamic (DLT) and Static (SLT) Load Test

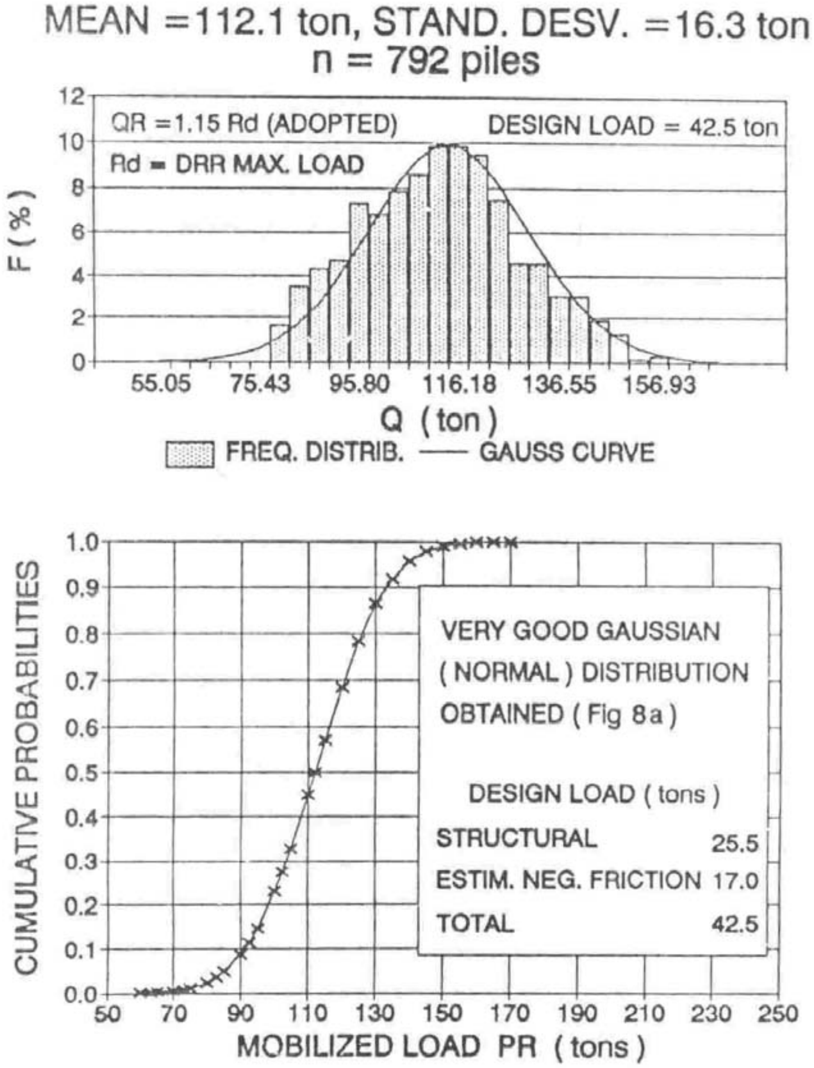


Figure 8 - Statistical Analysis of DRR Data

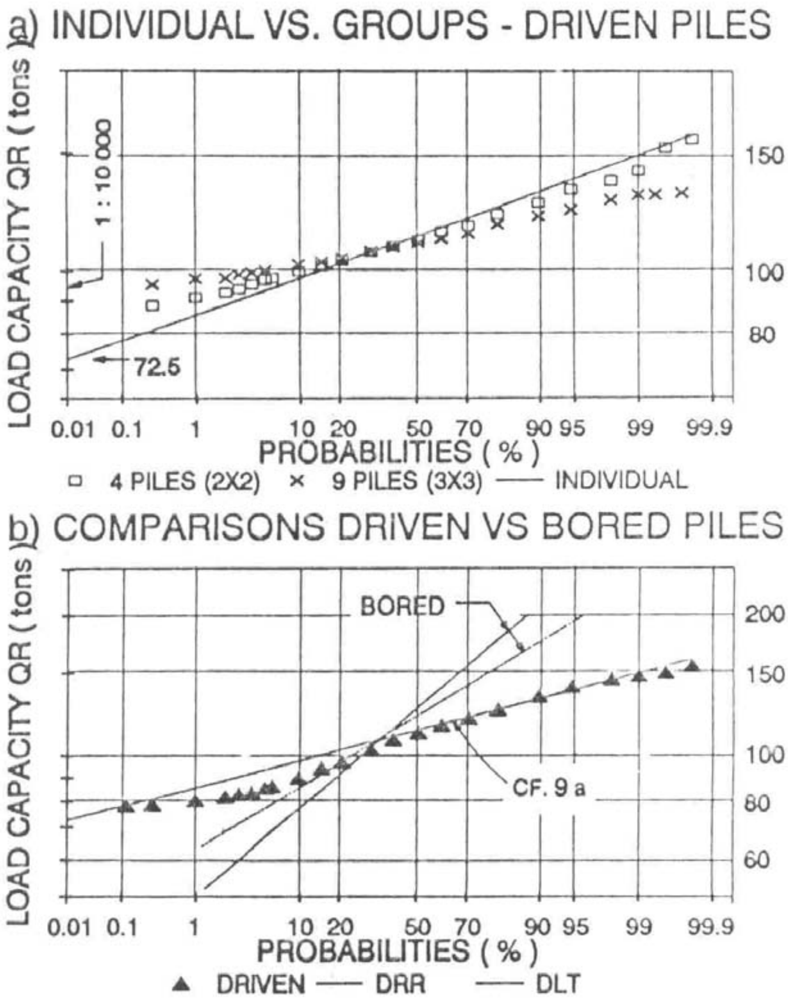


Figure 9 - Probabilities of Lowest QR Cases Analysed by "Extreme Value" Distribution

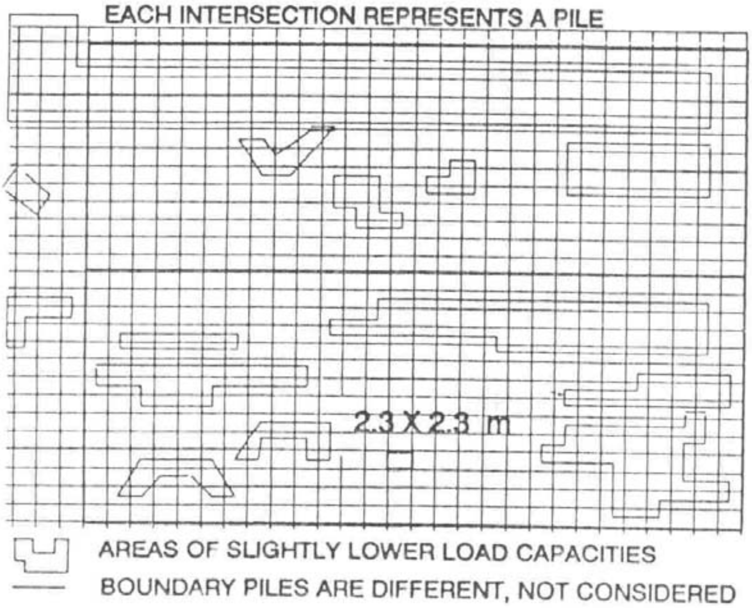


Figure 10 - Plan Distribution of Piles

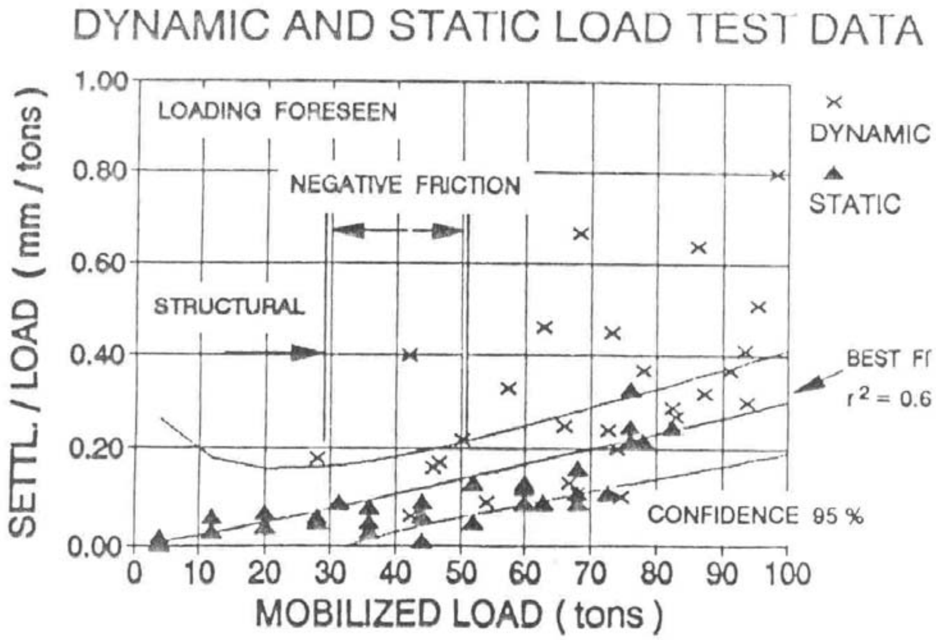
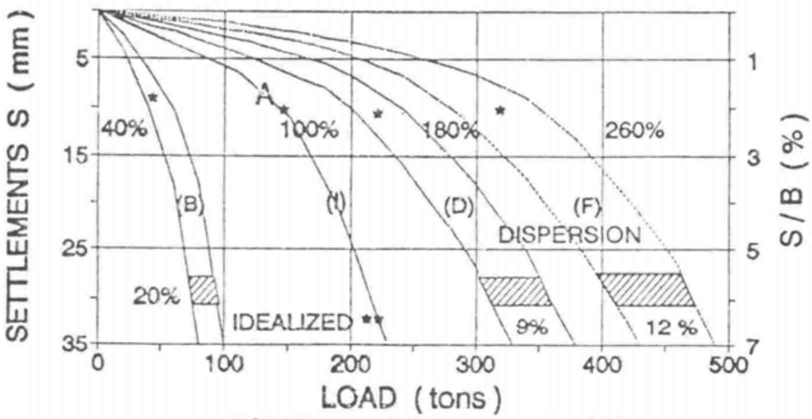


Figure 11 - Interpretation of Load Test Data Ref. Incremental Loading & Consequent Settlement

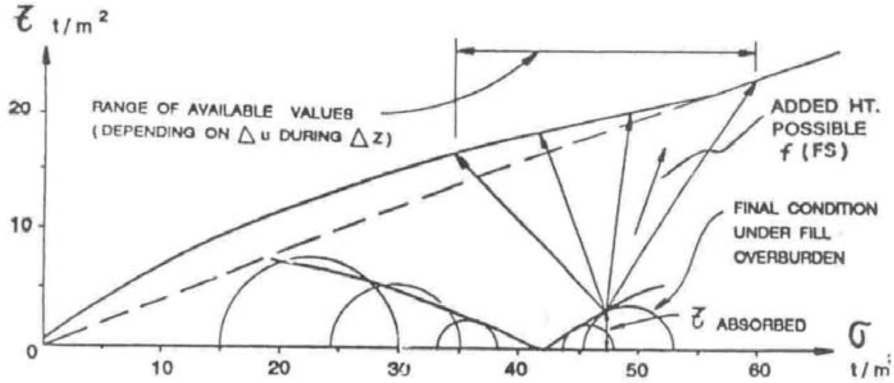


NOTES : —— BORED —— DRIVEN —— FRANKI

* REFERRED TO A

** SLIGHTLY ADJUSTED CURVE BASED ON "VERY GOOD" TEST RESULTS USED FOR IN SITU INTACT BEHAVIOR. DISPERSION, INEVITABLE, NOT REPRESENTED

FIG. 12 COMPARATIVE LOAD – SETTLEMENTS OF ANALOGOUS PILE IN DENSE GNEISSIC SAPROLITE



	FILL OVERBURDEN INCREASING				
	INITIAL	5 m	10 m	20 m	25 m
σ_v	0	10	20	40	50
u	-15	-15	-13	-8	-3
σ'_v	15	24.5	33	48	53
K_o	0.5	0.5	0.48	1.42	0.4
σ_h	30	34.8	38.6	43.8	45.2
σ'_h	15	20.3	25.6	35.8	42.2
σ'_v / σ'_h	0.5	0.7	0.85	1.09	1.17

$\Delta\sigma_v$ VALUES TAKEN SIMPLIFIED AS γZ IN LIEU OF THE MORE CORRECT $I\gamma Z$

CONTINUED REALISTIC FOR SOME ΔZ WITH:

$\Delta u \approx 0$

$\Delta\sigma_v \approx \Delta\sigma'_v$

$\Delta\sigma_h \approx \Delta\sigma'_h$

$\approx 0.4\Delta\sigma'_v$

CONTINUED MOST CRITICAL CONVENTIONAL APPLICATION $\Delta(\sigma_1 - \sigma_3)$ WITHOUT INCREASE OF $\Delta\sigma_3$ IN TRIAXIAL

SOIL ELEMENTS AT DIFF. POSITIONS HAVE DIFF. INITIAL LATERAL STRESS RELEASE, AND DIFF. STRESS PATHS

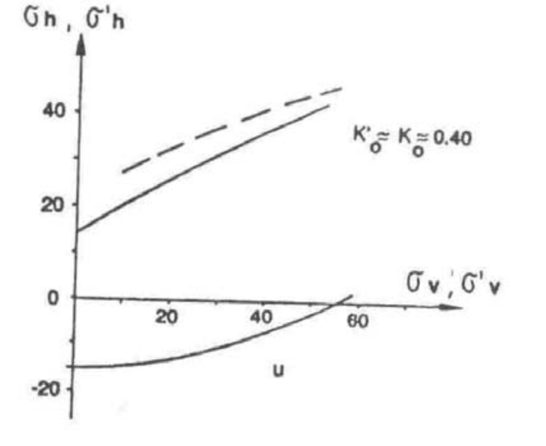


Figure 13 - Unstabilization of Slope During Construction

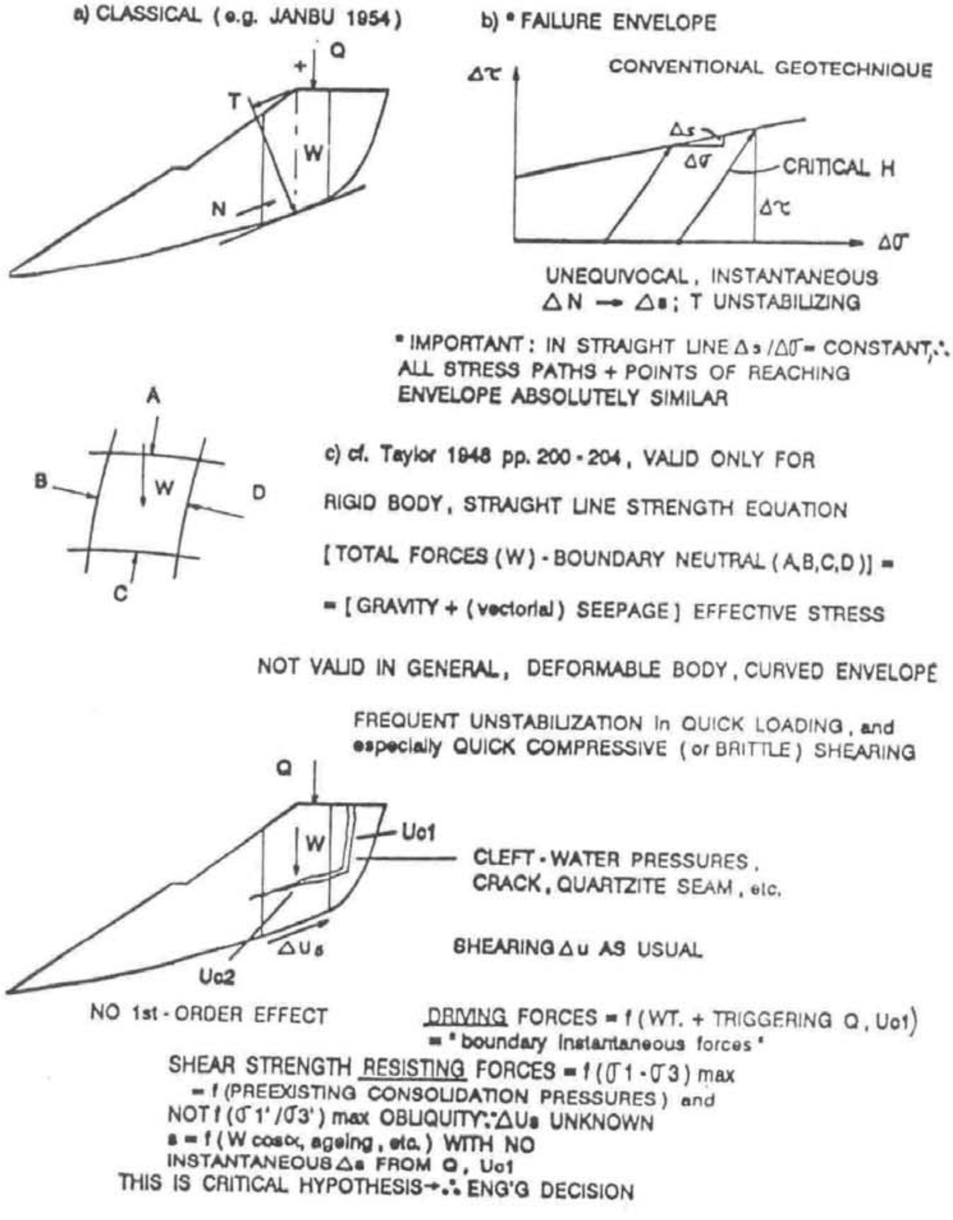


Figure 14 - Statics of Stability Analysis Equilibrium and Revisions on Unstabilizations

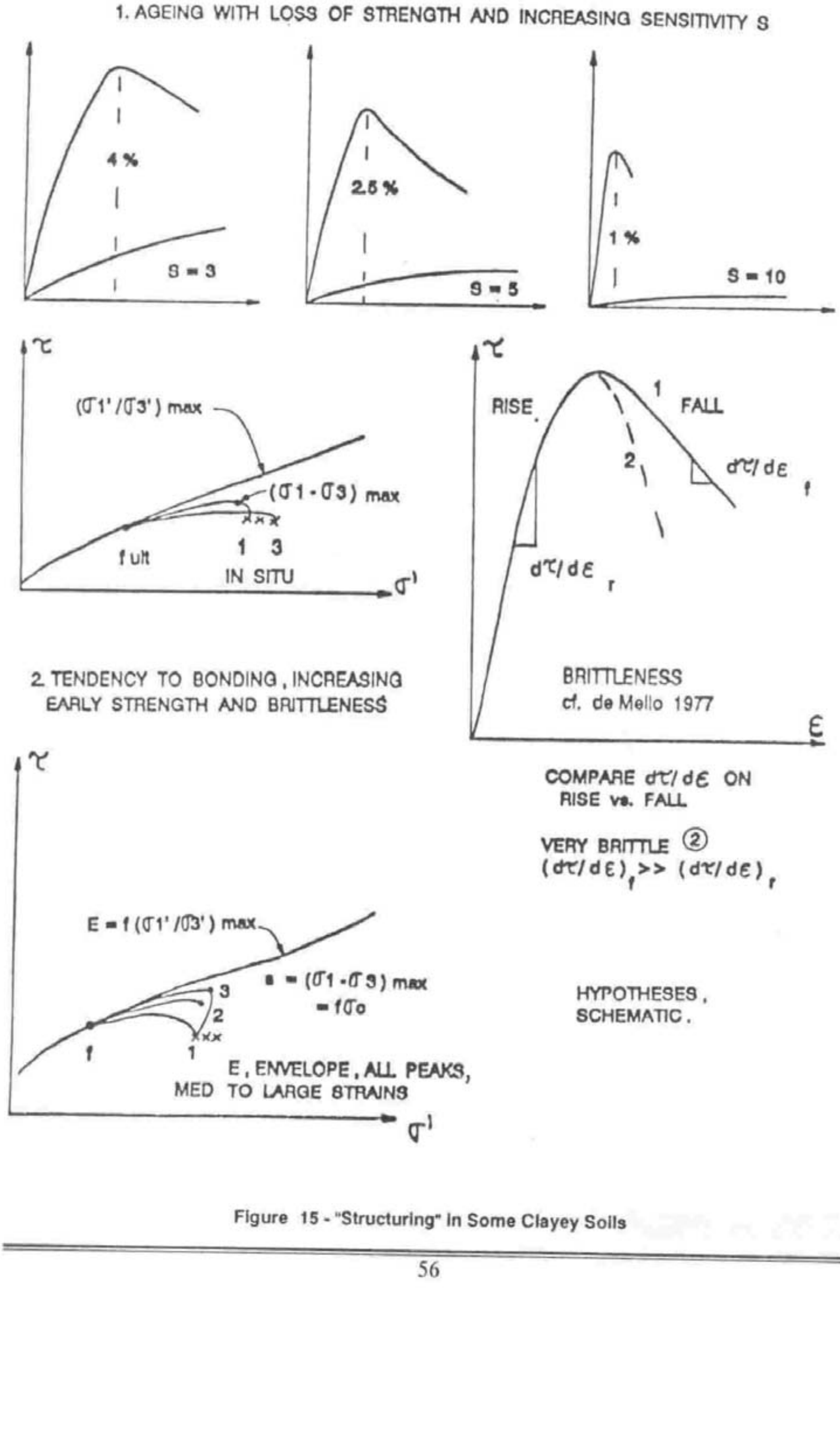
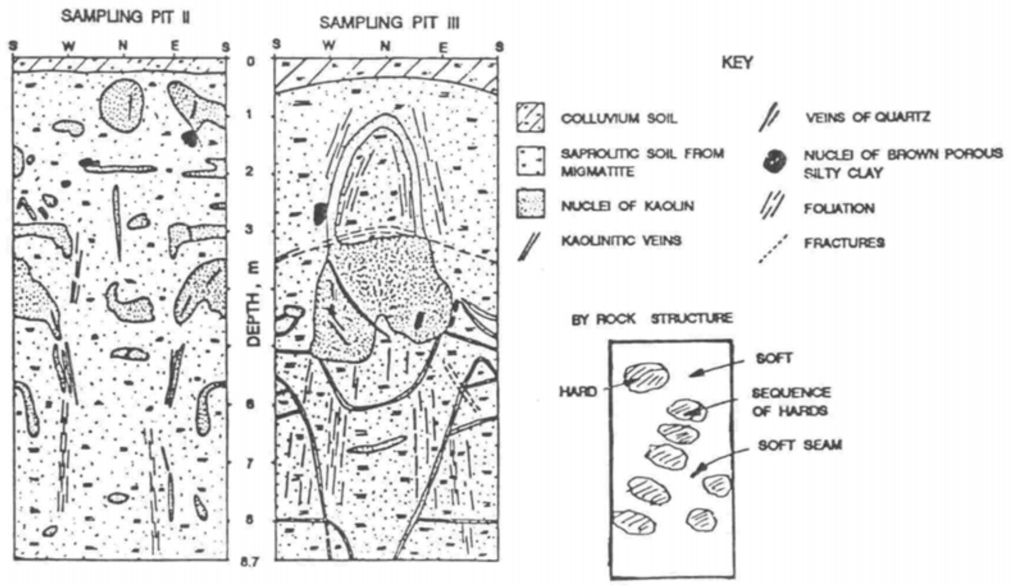


Figure 15 - "Structuring" in Some Clayey Soils


SAMPLING PIT II
SAMPLING PIT III
S W N E S
S W N E S
0
1
2
3
DEPTH, m
6
7
8
8.7
KEY
COLLUVIUM SOIL
SAPROLITIC SOIL FROM MIGMATITE
NUCLEI OF KAOLIN
KAOLINITIC VEINS
VEINS OF QUARTZ
NUCLEI OF BROWN POROUS SILTY CLAY
FOLIATION
FRACTURES
BY ROCK STRUCTURE
SOFT
HARD
SEQUENCE OF HARDS
SOFT SEAM

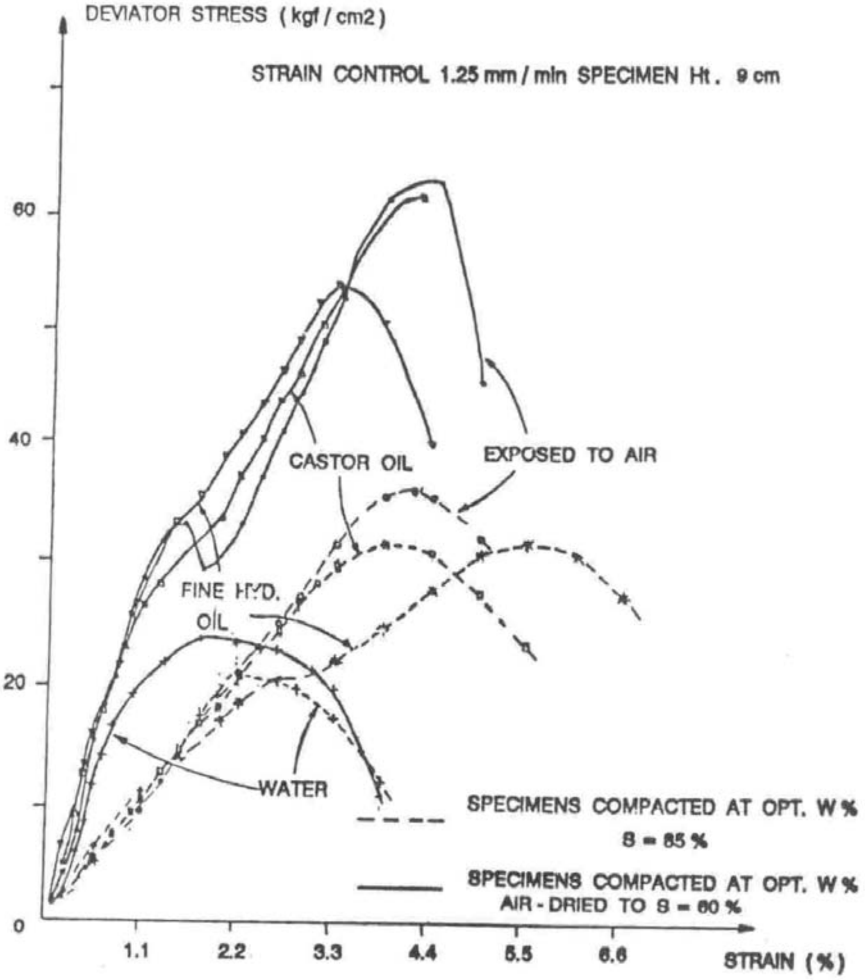
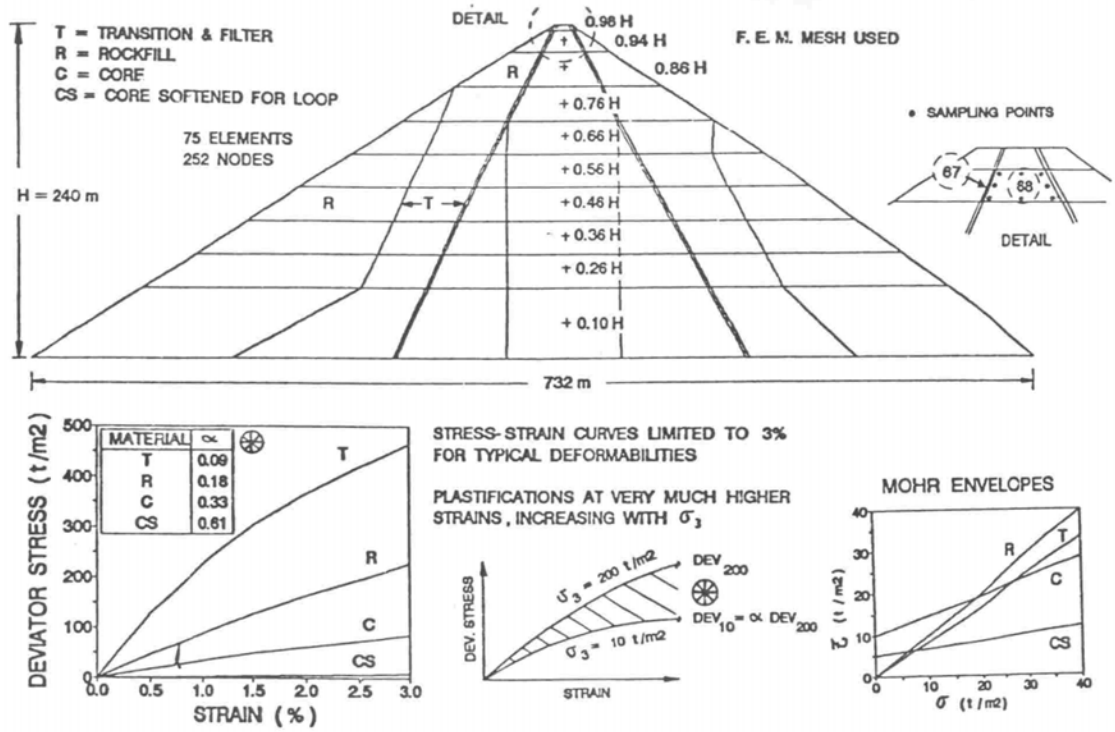
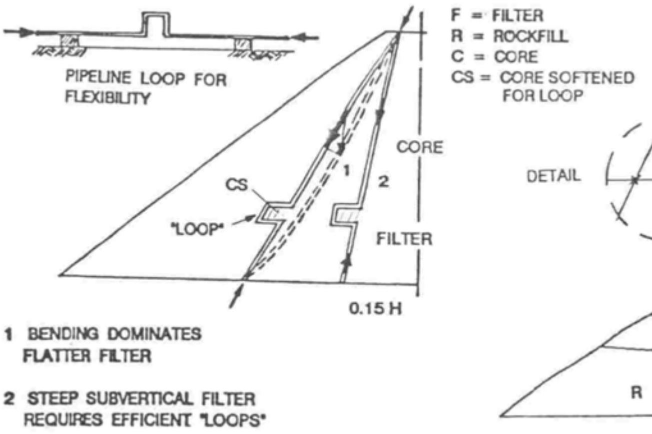
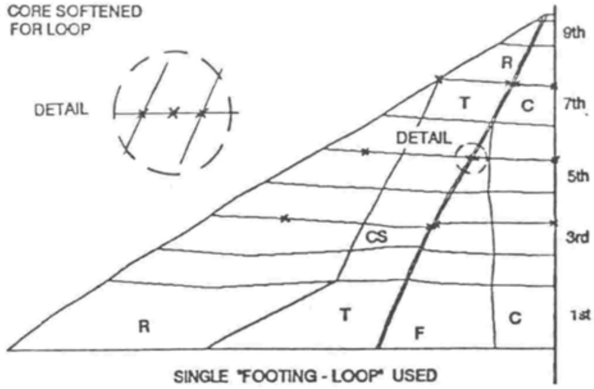


Figure 17 - First Approximation Testing of Non-Wetting Liquids in Unconfined Compression


T = TRANSITION & FILTER
R = ROCKFILL
C = CORE
CS = CORE SOFTENED FOR LOOP
75 ELEMENTS
252 NODES
DETAIL
0.98 H
0.94 H
0.86 H
+ 0.76 H
+ 0.66 H
+ 0.56 H
+ 0.46 H
+ 0.36 H
+ 0.26 H
+ 0.10 H
F. E. M. MESH USED
H = 240 m
732 m
R
T
SAMPLING POINTS
67
68
DETAIL
MATERIAL
α
T 0.09
R 0.18
C 0.33
CS 0.61
DEVIATOR STRESS (t/m2)
STRAIN (%)
CS
STRESS-STRAIN CURVES LIMITED TO 3% FOR TYPICAL DEFORMABILITIES
PLASTIFICATIONS AT VERY MUCH HIGHER STRAINS, INCREASING WITH σ3
σ3 = 200 t/m2
σ3 = 10 t/m2
DEV200
DEV10 = α DEV200
DEV. STRESS
STRAIN
MOHR ENVELOPES
τ (t/m2)
σ (t/m2)





SINGLE "FOOTING - LOOP" USED

SETTLEMENTS (m)

CASES	TOP OF 3rd LAYER					TOP OF 5th LAYER					TOP OF 7th LAYER				
	R	R/F	F	F/C	C	R	R/F	F	F/C	C	R	R/F	F	F/C	C
H	0.91	3.22	3.25	3.27	4.37	2.22	5.32	5.38	5.45	6.43	1.45	4.23	4.27	4.30	4.45
Z	1.07	0.82	0.83	0.85	2.23	0.53	1.41	1.44	1.48	2.52	0.24	1.16	1.20	1.23	1.54
ZC	1.08	0.82	0.84	0.86	2.35	0.54	1.48	1.51	1.55	2.77	0.26	1.45	1.49	1.53	1.94
L	1.11	3.04	2.63	2.29	2.17	1.38	2.34	2.37	2.39	2.90	0.52	1.48	1.51	1.55	1.84

H - HOMOGENEUS Z - ZONED (CONVENTIONAL)
ZC - ZONED WITH COLLAPSED CORE L - LOOP IN 3rd LAYER

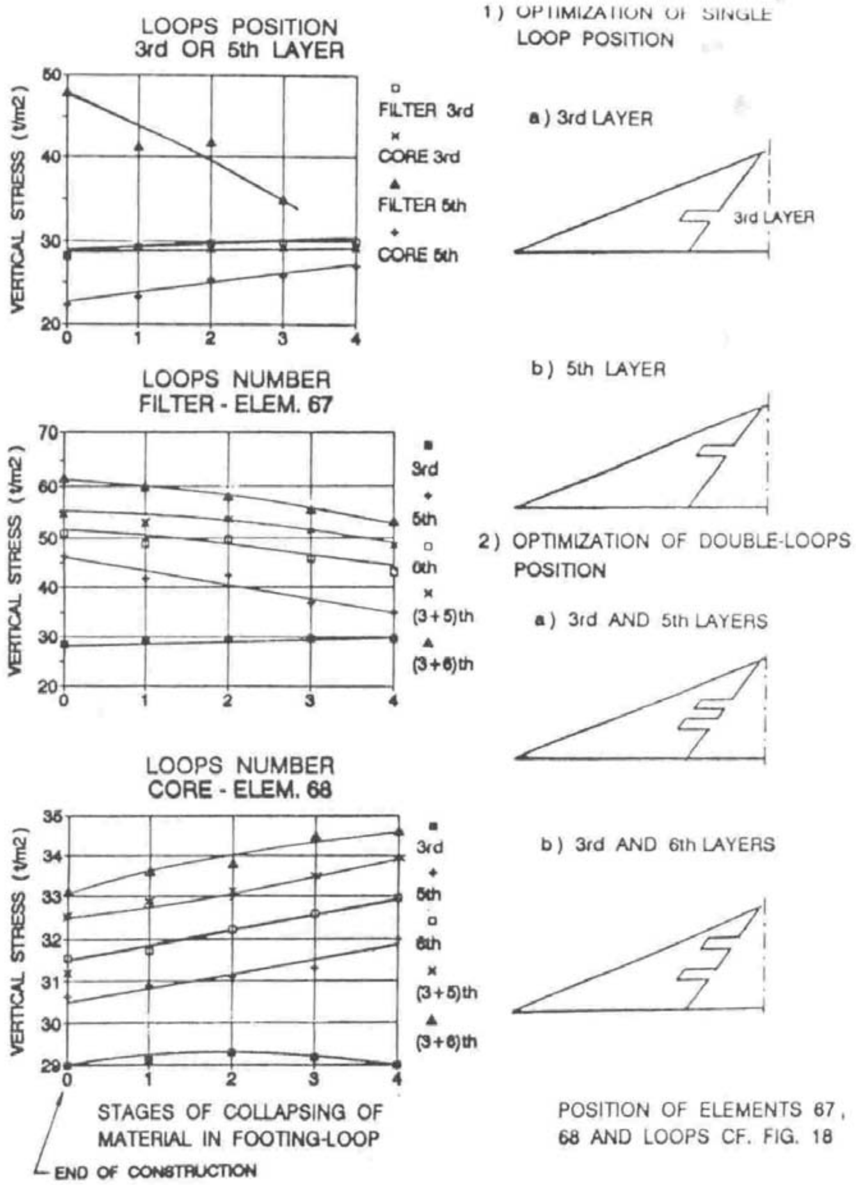


Figure 20 - Influence of Loops on Vertical Stresses Near Dam Crest