Reflections on needed logical unifying of basic geotechnical prescriptions: simple examples

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1 PROLOGUE

Upon thinking of a puny belated tribute to one of the great pillars of the international geotechnical community, Professor José Antonio Jimenez Salas, my deep feelings of esteem fall short of the admiration for two well-recognized personal marks: his dosed mixture of philosophy, science, mathematics and practical engineering; and his having created more than engineering works, an enviable school of brilliance that pervades the Iberian-American world. So, in my eager effort to emphasize the NEED FOR A RENEWED START, I was attracted to aligning myself with his holistic vision and strategy. For we must begin by perceiving and proclaiming the chronic viral epidemic unleashed unto Civil Geotechnical Engineering of a one-legged support, dispensing the two other supports of triple-legged firmness. Firstly, the recognition that every case is ab limine different, however reasonably similar to others we may make it, for drawing statistical conclusions on confidence bands, that condition our decisions regulated by maxima and minima. Secondly, the importance of costs and benefit/cost ratios in controlling decisions in our socially-oriented executive profession. And thus only in third order of merit, the profiting of the sinews and stumbles of three-score years of success-addiction in case histories published under natural selective psychology, as a tremendous weight of production befalling us.

Anathema to days of total freedom that brought us to present uneconomical chaos wherein for any challenge of Design Prediction vs. Performance n learned participants optimally employ as many as (0.7 n) different methods, mostly reaching solutions several times more conservative than prescribed or suspected!

In the face of the welter of existing proposals, insufficiently proven as to credibility, welcome be the call of halt, for the purpose of checking, comparing, and discarding the illogical indices and postulations that clutter textbooks and publications, and well-intentioned proposed practices that led to no benefit ! The call for erudite Doctoral theses is misdirected, in comparison with our need to discard and distill, preeminently to avert a suicidal course to the specialization. Within our fold, the lack of honest debate of preferred solutions suppresses a sense of purpose and drive towards the global problem, plus intellectual tribalism. From without our confines there is a growing trend to foregoing the refinements of honest specialized effort, both by satisfied use of our own offered transient PRESCRIPTIONS, and by the exponentially growing industry of SOIL TREATMENTS that almost dispense the knowledge of the soil, as secondary partner.

With full respect for the intentions that paved our past, I venture to discuss, criticize and dismiss some sample prescriptions, in a humble but courageous obligation to stimulate our successors to exercise anew some of the freedom of reflection and decision that were the dominant feature of geotechnique's early days. The only proviso is that henceforth we should be intrinsically aware of the great numbers of globalized colleagues that also strive along same or correlated topics. Thereupon, what the profession needs is group action, as results of well-debated workshops. Within this innate conviction, my presentation of some presumed examples is, in itself, a bad example, for which I beg excuses...mea culpa. May this appeal be merely a rivulet heading down to the sea, while joined by more and better tributaries. The sea pooling together our efforts will not leave us dejected at a calm of apparent sterility: it should represent another condition of energetic swells and sublimations, for recycling into

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new precipitations and flowing streams and rivers.

2 INTRODUCTION

Geotechnical research on behaviors of soil elements has produced an unwieldy amount of data. Taken piecemeal from "homogeneous specimens", and deterministically analyzed, the countless Doctoral theses have offered ever more "idealized theories". I do not recall any case in which a succession of (presumed) identical tests in sufficient number (e.g. ≥ 10) have been scrutinized as to statistical dispersions. Dispersions and confidence bands detected as affecting a limited range of variation of any significant parameter will greatly increase when the range is extended in the self-same soil, and much more so when the concept deduced is extended to other soils, presumed similar.

Conscientious self-analysis as Civil-Geotechnical Engineers should have shocked us long since! The infinite variabilities of Nature should have imbued us with a philosophy that every soil element is different, unless proved sufficiently similar to permit pragmatic grouping into a "statistical universe". Moreover, since each job prototype is different, and ALL MUST BE SAFE as delivered to Society, our Factors of Safety, FS (and of Ignorance) must assuredly cover the maximum (or minimum) critical confidence band.

The immeasurable capacity for numerical solutions FOR SOIL MASSES, based on idealized equations and computer software, has advanced a score of years beyond the experimental bases ON SOIL ELEMENTS. But every step includes interfering dispersions: in the idealization, in the theorizations for integrating soil (elements into masses), in the soil-structure interaction, in computing the predictable global behavior, and in taking and implementing the Design-Construction decisions. The global dispersion multiplies at every step.

It is therefore fundamental, and overdue, that we reexamine critically the kindly-offered and gratefully-received early PRESCRIPTIONS, for validation, limitation, or discarding. And the profession should henceforth determinedly reject any and every publication that disregards the fundamental precepts of "transient truths" statistically formulated, WITHIN LOGIC AND NUMERICAL CONFIDENCE BANDS.

Conscientious OBSERVATION during execution is an obligation in any profession: thereupon, the proclaiming of an OBSERVATIONAL METHOD of conducting continually-revised design-construction constitutes an unacceptable burden in a world of fixed-price bidding, with "experienced" risk-hedging, hopefully quantified in confidence bands, or contingencies. The recognition of the importance of EXPERIENCE should not irk our zealous successors if we recognize that the interpreted reality is that our indices, parameters, and formulae really fail to register and transmit much of what we "neurologically infer and know"! The first step to improvement is a humble recognition of our insufficiency.

3 PLASTICITY CHART CLASSIFICATION OF CLAYEY SOILS. A SIMPLE BASIC EXAMPLE.

Casagrande's early efforts at classification of soils essentially "for use on airfield projects" (for the U.S. Engineer Department, in the period of Dec. 1942 to 1944 as developed for instruction in Army courses on "Control of Soils in Military Construction") underwent an eager and rapid extension as recorded in his 1948 paper "14. POSSIBLE EXPANSION OF AC SYSTEM. General. Numerous suggestions have been received for the expansion of the airfield classification system...". It was a time of faith, youthful freedom, and adulation (e.g. R. Fadum, loc.cit. pp.931 "The author has referred to...as the Airfield Classification System. This name connotes a restrictive meaning that does not do justice to its general applicability"): quite understandable from recent ex-students. The answer seems easily explainable: the "experienced" consultants hardly looked at index test data in comparison with the DEDICATION TO DIRECT FEEL OF THE SOIL ITSELF (always emphasized in the early days), and thus were oblivious of any need. Meanwhile the young enthusiasts believed the indices and focussed their zeal, eager for prestige, on numbers and sophisticated novelties. So, prescriptions unquestioned became dogmas.

I must shun flogging a dead horse. Two starting failings of the Plasticity Chart are too salient and have been repeatedly emphasized (e.g. de Mello 1999b) independently of the crudity of the very tests[1]. It is generally recognized that both liquid and plastic limit tests are too rudimentary, whatever their reassessed intent.

Anyhow, the Plasticity Chart points blatantly to dependence on 2 parameters. Thus, if two parameters are more determining (for c', φ', φ_a, s_u, E, M, k, etc..) than a single one, priority should be given to two INDEPENDENT TEST VALUES, e.g. w_L and w_P (?), and not to the third (often alone), least acceptable Ip, a difference-value subject to greater errors. Then there is the criticism of the Chart having employed a graph compressed in 45° when/if the interest is to separate, distinguish...the logical aim of any classification[2].

In due justice to Casagrande's milestone paper it is necessary to quote his observation (1948) that "the results of limit tests on a NUMBER OF SAMPLES FROM THE SAME FINE-GRAINED DEPOSIT...the points lie on a straight line approximately parallel to the A-line". In

[1] I forego mention of the illogical over-extension of the same classification, based on totally remolded material, to foundation soils, wherein the importance of Structure and Sensitivity had been Casagrande's own victorious joust, and subsequent research has emphasized ageing.

[2] In principle, moreover, one should start from the general to the particular.[The urgent practical wartime needs fully justified the condition, itself often dominating a given civil engineering project need]. Thus, reasonings should begin established from near-extreme soil-data, and subsequent pinching-in and filling-in. The starting data (not plotted or tabulated) seem to have been mostly from the range of $23 < W_L < 90\%$.

fact, by using the most cited CAM-clay hypotheses on proportion of shearing resistances between w_P and w_L, and generating the varied water contents by mixing inert grains with a given clay mineral, it was proved (cf. de Mello, 1993, 1999a) that the linear variation of I_p vs. w_L is a computational result, crossing the A-line at a relatively small angle[3]. This finding matches Casagrande's statement, applicable to "the same fine-grained deposit" likely to have varying mixtures of the same clay-mineral(s) and inerts. Obviously the linear connections between w_L and I_p are a consequence of the linear connection between w_L and w_P.

However, logic obliges our repudiation of any such playful interest in relationships between two indices, that SHOULD BE SOUGHT AS PROVIDING, FOR BETTER IDENTIFICATION, A PAIR OF WIDELY DIFFERENT ANGLES OF VISION. Two questions never researched (in support of the Chart or otherwise) even in remoulded materials for an easy starting step, are: one, already queried (de Mello, 1999b) the variation of parameters of interest by synthesizing at same w_L the widest range of w_P (or I_p), and at same I_p (or w_P) the widest range of w_L; the second, the eventual similarities resulting associated with an identical w_L using (a) a given clay-mineral + x% of inert, vs. (b) as if from another deposit, another clay-mineral + y% of the same inert.

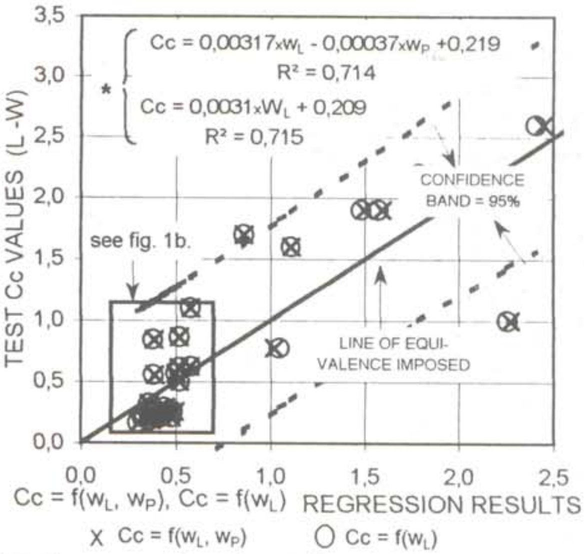
Anyhow, the basic point is that the chart is to rely on 2 independent parameters, for aid in assessing preliminary parameters of use in geotechnical engineering. Why were no TWO-PARAMETER regressions ever attempted for use, when such branches as health, economics, etc.. use multiple-regressions of scores of parameters? For the optimized cross-checking desired, the tries are here made using the tabulation of Lambe - Whitman (1969, pg.322) on a wide variety of clay minerals, and aiming at the virgin compression index Cc, earliest and most used. In fairness the checks were also made for the narrower universe $23 < w_L < 120\%$ associated with the experimental data of the time[4].

Several single-parameter regression equations were tried (de Mello, 1999b) for comparison with the Terzaghi-Peck (T-P)(1948) classic equation $Cc \approx 0.007 (w_L - 10\%)$. Among most common equations, that resulted in $0.61 < R^2 < 0.79$, the linear first intuition seemed passable, $R^2 \approx 0.71$. Salient conclusions, however, were that for the "full-range" of data the T-P relation went very pessimistic beyond the 1948 data range. For these, curiously the average result proved near coinciding, but dispersion absurdly high ($R^2 \approx 0.4$). We recall the likely greater dispersions of those days. And the main lesson for empirical correlations is to avoid extending beyond the universe of data.

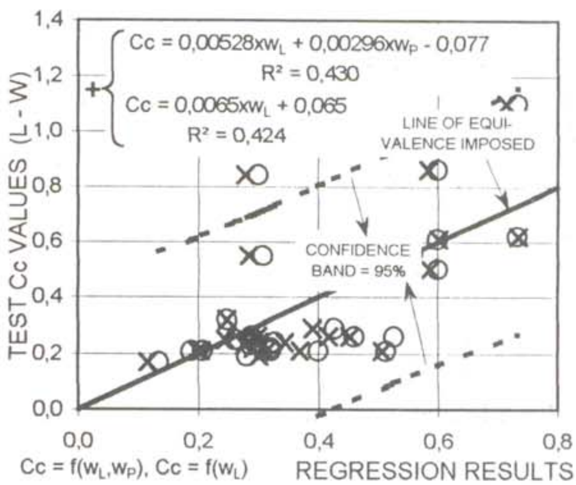
The attempted double regressions, of $Cc = f(w_L, w_P)$, summarized in Fig.1 show no changes or improvements at all. For easier visual perception, the graphs impose the line of equivalence, so that the effective quality of the regression correlations reflects in the R^2, the width of confidence bands. These are shocking, about ± 50% for the full range of clay-minerals, and ± 100% for the 1948 range. The significant revelation is that w_P is inconsequential: the double vs. single regression equations superpose, differing about 1 – 7% and 4–7% for the full, and 1948, data ranges. Separate regressions of $Cc = f(w_P)$ worsened. Indications are too clear (from these data) that w_P is "genetically too similar" to w_L so that it adds no identifying contribution. Discarding I_P as the worst, could existing correlations be adjusted to $f(w_L)$ for tolerable average estimates?

Would such analyses decree belated mercy shots to Atterberg Limits and Plasticity Chart single-legged?

Much smaller pangs of conscience accompany the indispensable mercy shot on a pseudo-index, COMPRESSION RATIO, $Cc/1+\varepsilon_o$ spread through much of geotechnical engineering as a weed from the Harvard roots (apparently R.E. Fadum 1941). If we accept that Cc is related to CLAY QUALITY (w_L etc.) and



(*) Full range of Lambe-Whitman (L-W) 1969 data.



(+) L-W data, in range of 1948 $23 < w_L < 120\%$.

Figure 1 - Questioning double vs. single regressions. Absolutely useless.

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[3] Subsidiary ensuing deductions are herein set aside.

[4] Fair scrutiny should delve into the test details, including the important influence of the water content of initial remoulding compared with the W_L, etc.

reasonably constant in a stratum, the ε_o is ipso facto a physical index of IN SITU STATUS, variable with consolidation pressure (depth, or overconsolidation). Therefore, if we have access to both test results, physical index ε_o and oedometer (or w_L) for Cc, it is illogical, unacceptable to propose mixing an INTRINSIC QUALITY WITH A SPECIFIC CONDITION[5] .

However, the fact that the pseudo-index persists, in many publications including so authoritative a textbook as Terzaghi-Peck-Mesri (1996), called for a brief cross-checking, principally because of the already stated variability of w_L within a given stratum, by variations of inerts. The concept was investigated and proved discardable (de Mello, 1999b) as inferred by logic.

4 STANDARDS AND CODES IN DRIVEN PILE ALLOWABLE LOADINGS; DESIGN CONSEQUENCES. TWO EXEMPLIFYING CASES.

It was inevitable that a multitude of different practices sprouted in different cities and countries, since pile-driving is millenially older than geotechnique. And it is also natural that the Standards and Codes established therefrom should have expressed conditions mostly on the conservative, uneconomical side. So very many are the perspectives of necessary logical revisions faced, that any presentation, and this one specifically, must restrict itself to key exemplifying points, and with focus on possible savings without decreasing safety. Comments arise from two cases of Brazilian practice, and reference to the ENV 1997-1 Eurocode 7 Geotechnical Design, with its basis of the ISSMFE Suggested Method of Axial Loading Test, ASTM 1985.

The origins of many a codified practice must be interpreted, for conscientious systematic revisions. Since a building's performance doesn't know whether it is founded on footings, piles, piers, or rafts[6], why is it that the settlement-limited codified prescriptions are so much tighter for piles than for footings (of historic buildings, pre-conventional geotechnique) ? Obviously because early piling projects used smaller diameters (say 25 to 35cm) and settlements beyond about 15-20 mm already signified failure: the presumed settlement limitation was really because of FS worries. As we "know" (since > 1960) that friction failures occur with about 10 mm, while point bearing with about 10% D, how is it that Codes generalized on constant limiting settlements, independently of friction vs. point, and of diameters? Possibly because early driven penetrations achieved the desired modest loadings mostly by friction, and 10% of the 30 cm diameters did not expose salient differences of friction and point contributions. And so on.

But the realities of professional practice that I expose are, in short, that:

1. Considering the design/bidding decision process, the first need is of deciding on the lengths of piles (budget). In the face of any doubt or risk it is automatically preferred to drive somewhat deeper (to a tighter "refusal") than hypothetically necessary. Thus pile driving/penetration experience is strongly biassed toward excess, if related to subsequent loading tests (rarely achieving "failure"). Empirical correlations, though oriented pseudo-theoretically by highly simplified coefficients (friction and end-bearing) may lead to results quite consistent, but always for a prospective behavior better than aimed by design.

2. The hypothesis of prior driving/testing of preliminary piles (as per ISSMFE Subcommittee recommendation) is quite out of the question in 99% of the cases, and never to be countenanced in principle, because check-testing should never be done /accepted in any but the job's routine logistical conditions.

3. Taking into account the fact that every pile has a fair degree of control (via demeaned, but still resilient, dynamic formulae and "refusals"), the foundation's concept implies Factors of Guarantee FG in lieu of the less assured Factor of Safety FS (de Mello, 1981). Thus limitations on differential settlements (and, therefore, settlements) are in concept too conservative for driven piles in comparison with bored piles.

4. Regarding standards for load-testing, and resulting codified FS values, there is a real lack of rationality in testing procedures, and their logical relationships to dead vs. live loadings, margins of ignorance and contingencies on both, and resulting influences on risks and decisions. For instance, the Brazilian Code requires each loading-step to be maintained until settlements have decelerated to a near-stop (formally defined): times at each loading are thus different, longer at higher loads, smaller FS. Accepting the low probability of dual comparable tests, a revision has been repeatedly suggested (not yet implemented): up to the working dead load, one wants to maximize settlement, and therefore the present procedure should be maintained; beyond such loading, the unknowns and risks impose maximizing tendency to failure, to check on guaranteed FS, and thus, the rapid constant rate of strain test should take over. Ponder on the differences between tanks and silos, vs. buildings, and, in these, the different proportions and rates of dead to live loadings in skyscrapers, US-steel vs. Brazilian concrete.

5. The repeated "fundamentalist" repudiation one hears of the dynamic load tests (Aoki / de Mello, 1993b) because of classical static vs. dynamic strength-deformation behaviors in some soils must be openly REEXAMINED AS AN IMPROVEMENT, AND NOT THE ULTIMATE AIM; at the micro-strain levels "volume-displacement masses" taxed below the pile point

[5] Of course, there may be some thin strata so highly preconsolidated as to merit a constant average ε_o, but such and all cases could continue to justify the simplest of operations of division.
[6] except for much faster settlements of piles, important to damages by differential settlements.

are far from involved. The reaction developed is dominantly the side-friction; and the judgment must be based on whether the indication achieved is on the safe side, or not.

6. Without incorporating group effects (which may vary from very damaging, in highly sensitive clays, to improving, in densifiable strata) no tangible benefit is recognized or granted by any Codes (of my knowledge) to the greatly decreasing probabilities of risked unfavourable behavior with big foundation statistical universes: this imposes earnest reconsideration of the historic arbitrarily fixed FS numbers.

The following presentation exposes the absurdly uneconomical realities that are demonstrable from cases of prospective live loading near zero.

In various pile foundation design practices, the accumulated effects of stringent requirements and increased costs flagrantly call for revisitation of the origins, as distinct from subsequent trajectory.

Fig. 2 firstly illustrates two principles repeatedly proclaimed. (I) Even if a lumped-parameter INDEX is crude, if it is used and systematically adjusted to another lumped-parameter that AVERAGES ANALOGOUS BEHAVIORAL PHENOMENA, correlations and prescriptions can become quite good. (II) Whenever deformation criteria ρ are tight, tending towards zero, and consequent rates of changes of increments $\partial Q/\partial\rho$ ipso facto become magnified, correlations become good. At microstrains predictive abilities improve, but lose practical significance: unfortunately foundation costs correspondingly increase. A WORTHWHILE ENGINEERING CORRELATION SHOULD PROVE UNDER SIGNIFICANT MAGNITUDES, that permit, and risk, significant differences.

Only the briefest mention can be made herein of the messages of the two graphs (a), (b) of Fig. 2 extracting data from driven point-bearing concrete piles of the structures of Fig. 3. Published prescriptions by Aoki-Velloso (1975) and Décourt-Quaresma (1978) have been very much used in Brazil. They were based on uncorrected SPT values from routine reconnaissance borings, and were aimed at predicting the driven pile lengths L, that would guarantee satisfactory load capacities as per static load tests and the FOUNDATION CODE (settlements at failure and allowable loads set at 15 and 10 mm respectively). Moreover, most of the piles were of small 25-40 cm diameters. It can be seen that both methods give good results for the purposes: (a) similar predicted Load Capacities (often taken as 1.5x the maximum load reached, a frequent limitation because of cost, and the vicious circle of dimensioning the test reaction load to 1.5x the desired/predicted pile design load); (b) good prediction of lengths of driven piles; (c) because of microstrain conditions at play in the pile data, and the tremendous available rate of change $\Delta Q/\Delta L$, the stage was set for easy success by routine overdesign of driven pile lengths, always self-justified in vicious-

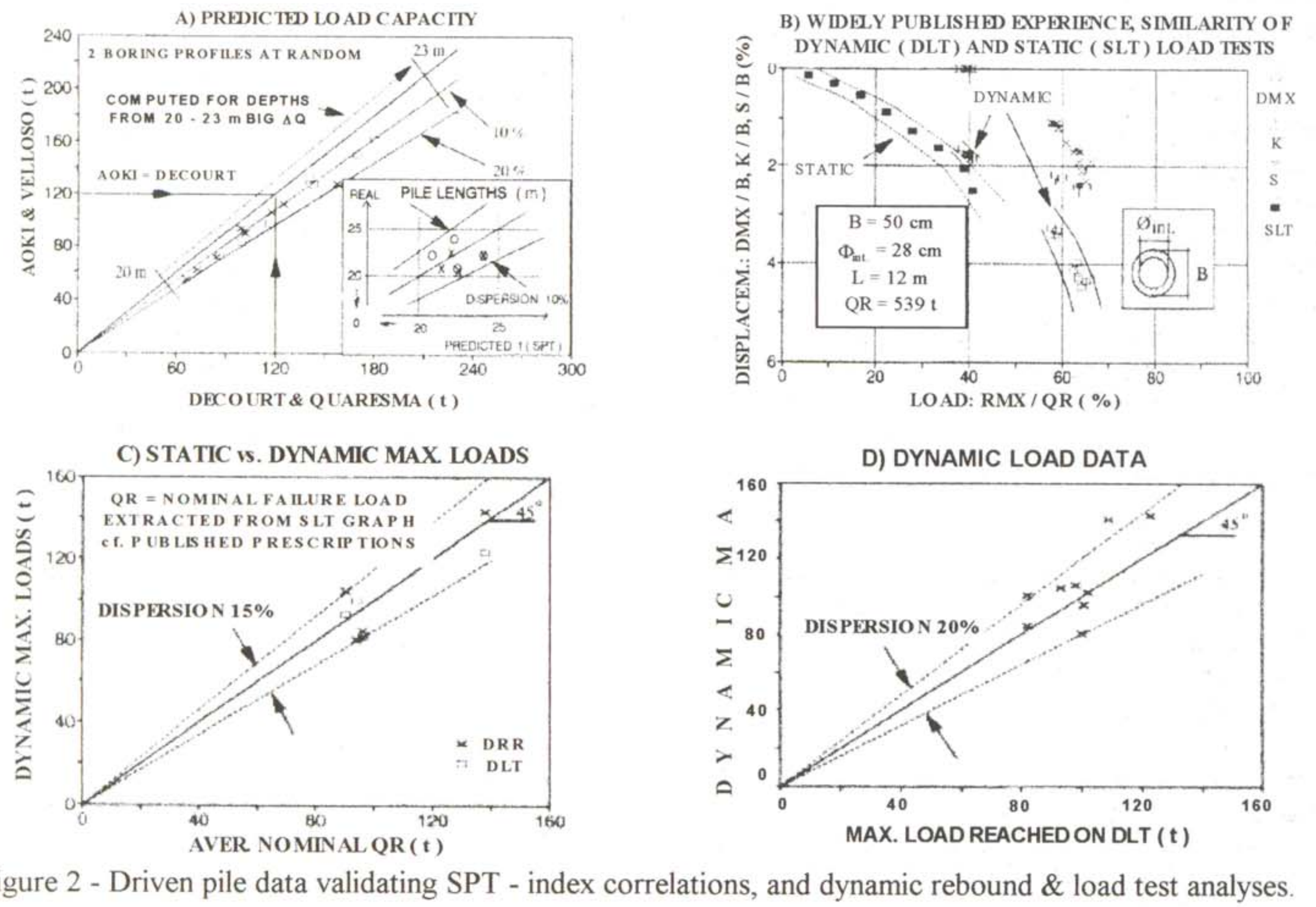

Figure 2 - Driven pile data validating SPT - index correlations, and dynamic rebound & load test analyses.

circle by the tight code: only statistical analyses can permit assessing the consequence.

Fortunately the same microstrain condition of test, code, and design prescriptions, have annulled, in routine practice, the historic dicta of the dominantly abysmal dichotomy between STATIC and DYNAMIC behaviors, which really pertained to macrostrains. Conditions thus recently began to provide the numerous data for revealing statistics. One gains understanding by reasoning stepwise: the points will not be expatiated herein because they have been submitted and accepted through many publications over the past decade.

Firstly, there was the very successful introduction of the Smith (1960) model wave-equation PDA (Pile Driving Analyser) applications, with better instrumentation, and on-the-spot-instant preliminary computing, to improve and validate the old-fashioned pile-driving control of each pile via final penetration "set". With a special (but obvious) electro-mechanical equipment unit called the Dynamic Rebound Recorder, DRR (analogous to the electro-optical of Sakimoto, 1985) the pilehead recording of displacements vs. time have been systematic: therefrom the pile Dynamic Mobilized Resistance load R_d is derived, from rebound records, in a manner analogous to the CAPWAP using records of strain and two accelerographs. Thus, in driven piling every pile is systematically associated with its R_d value. Finally, using many load tests purposely interpreted on the safe side the pile's minimum dynamic failure load QR, out of the DRR data, has been set, for the centrifuged concrete precast piles as 1.15 R_d (only 15% higher than the mobilized load at final driving "set"). The data identified as DRR are such dynamic QR values (Fig. 2c).

Meanwhile it need hardly be recounted that at similar microstrains the pile-driven Dynamic Load Test DLT has been repeatedly validated, both in comparison with the DRR and with reference to the Static Load Test SLT (as interpreted through the most divulged tight code prescription) (cf. Figs.2b, c, d). 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 ≠ static (conventional dogma), it was fundamental to respect avoiding any conscious differences. More recently, however, it was reasoned that if a given (arbitrary) Energy E_1 gives the unequivocal dynamic failure load QR=$\alpha(R_d)$, α traditionally taken as 1.0, then any energy E_2, E_3,...E_n 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 E_1, ...E_n at the point of final set the CAPWAP analyses lead to data quite similar to those of loads-displacements of SLT (Fig.2b).

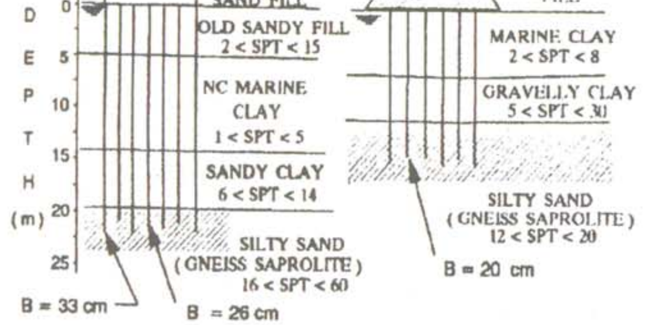

Figure 3 – Two cases of multitude (> 700) of driven concrete piles well documented.

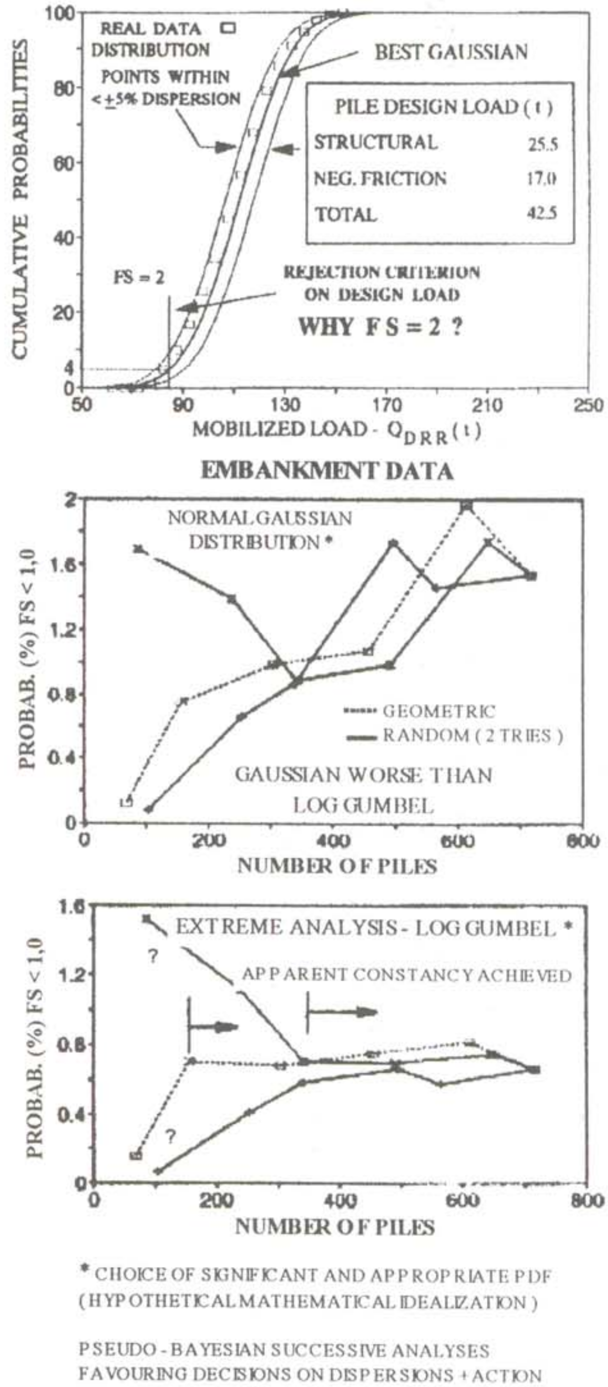

Figure 4 – Tank piling. Sample statistical analyses and conclusion.

The easy and inexpensive test has invited repeated obvious uses. Thus on the TANK T Project we had 10 such DLTs (plus one repeated after a few days): the DRR-DLT-SLT rough equivalences were again confirmed.

The same driven-pile concepts were also used for support of a compacted expressway EMBANKMENT E, in essentially similar subsoil profile (Fig.3). These two cases of multitudes of driven concrete piles well documented are used, with more than 700 contiguous piles each, for extracting the statistical lessons. Fig.4b firstly serves to recall that for extracting some minimal benefits put at our disposal by statistics we not only document with Confidence Bands (CB, not incorporated, to avoid crowding the drawing) but must (a) choose from among the Probability Distribution Functions PDF available, the one that on trial proves most profitable (b) test the existence or not of secondary trends by Bayesian successive analyses. It transpires that apparently the Gaussian PDF is less fertile than log-Gumbel PDF. Also, that with less than 160 piles (advancing geometrically in the piling array) or 340 piles (taken at random) the statistical conclusion does not reach an apparent constancy (in the exacting level exposed by the scale of the drawing, far more exacting than of significance to the foundation).

The use of an extreme-value PDF proves appropriate, as is shown in Fig.5, of graphical trends preferably linearized for easier interpolations and extrapolations. In this specific subsoil profile (and inmost routine cases) the behavior of each pile is justifiably considered independent, and therefore the recurrence probability of each pile's maximum resistance (or nominal failure load) and, a fortiori, the smallest values of that universe should be most appropriately adjustable to EXTREME VALUE DISTRIBUTIONS. The two graphs are convincing enough, and reveal logical trends, and astounding magnitudes. We can forego pointing to the obvious trends: the Embankment piles are quite logically a little less exigent than the Tank piles, but both are far more exigent than should be required by judicious safety and serviceability criteria.

The two principal facts exposed are, firstly, the very low probabilities of any single pile having a nominal FS dropping to 1.0; that is, the Design Loading (usually estimated with pessimism) increasing to become equivalent to the nominal failure QR of DRR values. A probability of 0.04% is most astoundingly and unjustifiably low for such an inconsequential "overloading" in comparison with such catastrophic and sudden events as a 1:10000 flood risk for a dam and spillway. Even more important a lesson derives from the obvious demonstration that with the typical, routine, "deterministic interference" of the pile-driving foreman, in improving the "set" of piles to suit specifications and his experience, the probability PF of any single pile reaching FS = 1.0 becomes far too low for either physical or mathematical meaning.

Do the learned writers of prescriptions and codes realize how much and how unjustifiably they increase the conservatism and costs of such driven piling? The subject demands further analysis, and can only be analysed via historic justifications, coupled with failure to adjust judiciously because of inexistence of statistical revisitations. Misunderstood pronouncements, and a few visible failures, have weighed thousand times more than the tremendously more important silent record of cases that did not merit study or publication.

Fig.6 shows that even in the unnecessarily tight mi crostrain range, most of the divulged prescriptions for

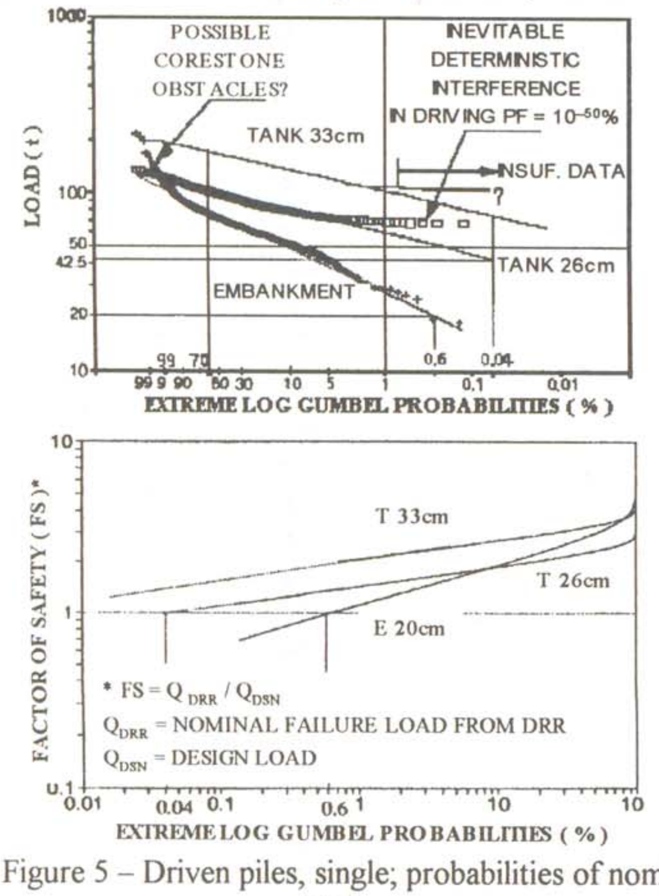

Figure 5 – Driven piles, single; probabilities of nominal failure.

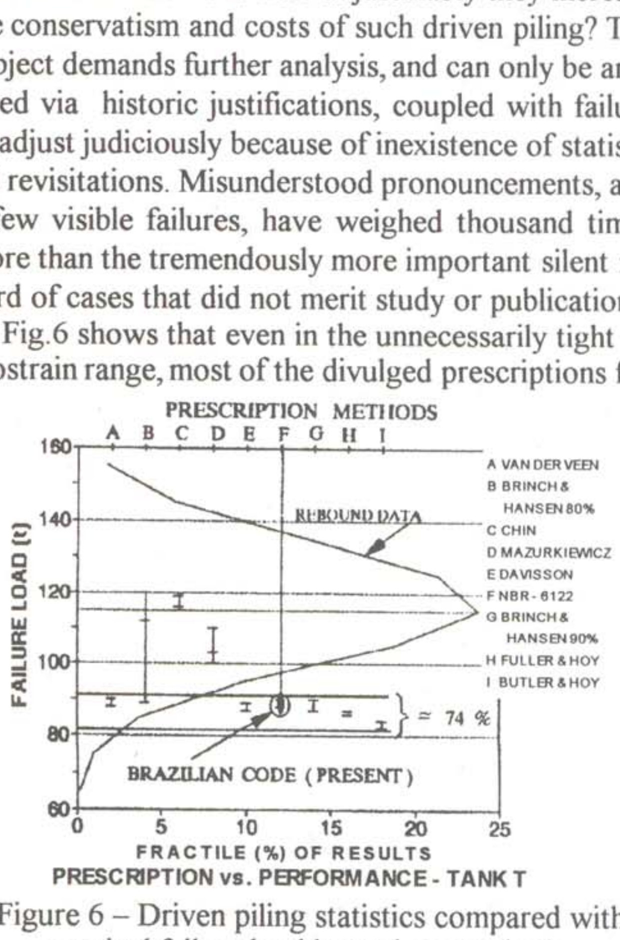

Figure 6 – Driven piling statistics compared with nominal failure load by various methods.

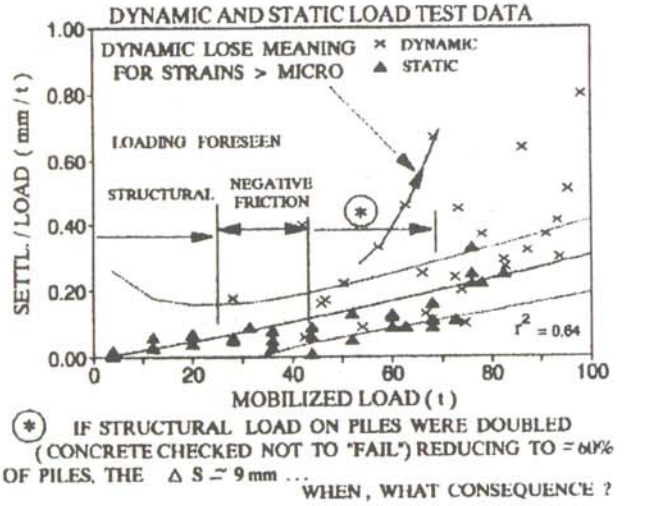

Figure 7 - Analyses of consequence if nominal load on pile is exceeded.

procedural interpretations of the load test graphs include a further 1.35 FS with regard to the microstrain QR from DRR data. Fig.7 shows what would be the physical consequences BEYOND THE NOMINAL FAILURE postulated. Cases of brittle failure emphatically excluded, all that happens if the condition of FS < 1.0 begins to set-in for the INDIVIDUAL PILE, is that a minimal inconsequential rate OF INCREMENTAL SETTLEMENT of 0.1 mm per 35 tons would begin to force some redistributions of loading (cf. Fig.7). Regarding savings in the foundations it is seen that if the total number of piles were reduced to 60% of the designed array, an increase of (flexible, first-load) settlement of 9 mm would be the only result. Finally, by using the individual DRR data of contiguous piles and, without any structural redistribution, merely considering the arithmetic average QR of groups of 2 x 2, 2 x 3, and 3 x 3 contiguous piles, the PF% of FS = 1.0 drops to about one-tenth of the corresponding PF% established for the individual pile. Barring geotechnical disturbances of one pile to others nearby (a quite separate point), one of the absurdities in design practices, is requiring the same FS per pile whether it is alone or one of a group in supporting a column.

In short, by using simple statistical analyses on a

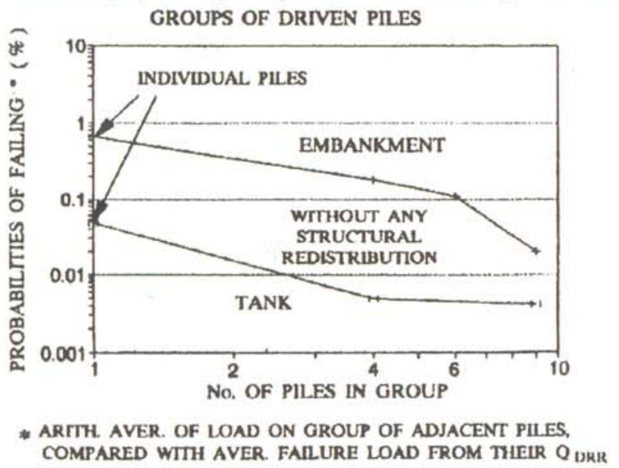

Figure 8 – Driven piling. Reduction of probability of failure groups vs. single.

documented piling foundation we reemphasize that our engineering decisions are not based on averages of correlations but on rejection criteria. With progressive changes of construction practices the applicable idealizations for theorizing (and for recommending in Standards and Codes) should have suffered major changes. Having systematically failed in this priority intent, the resulting absurdities and greatly increased unjustifiable costs have become a plague. 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) 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?; (3) how can Committees, discussing Codes, lightly banter around with changes of FS values (e.g. from 1.5 to 2.0, or vice-versa) without any statistical data to evaluate the magnitudes of the consequences? The fact is that in placing our conclusions in civil engineering perspective two aspects become salient. (a) The exponential disproportion regarding risks and costs of risks in comparing a spillway failure to cope with a flood, and the piling's failure to cope with the assigned FS. (b) The great increase of unnecessary first cost. In the case of a real FS lower than assigned, absolutely nothing is at risk: 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?

5 FUNDAMENTAL QUESTIONS ON SLOPE DESTABILIZATIONS, EXEMPLIFIED FOR UPSTREAM SLOPE OF HOMOGENEOUS COMPACTED DAMS. MERE APPETIZERS.

Respecting space limits, brief reference is added on another momentous topic, through yearning to encourage younger colleagues ever to enjoy the difference between decisions that have been acceptable, and the tighter truth ever playfully inviting, ever in the offing. Slope destabilizations invite scrutiny, on principle and practice. Upstream slopes of homogeneous compacted clayey fill dams, HCCFD, invite exciting globally didactic reappraisals, both for updating theories, and by perspectives of immeasurable benefit/cost ratios, especially for modest height ($\leq$ 60m) dams or dam stretches. The historic imprint, and hysteretic behavior, impose that we compute changes ΔFS from a prior to a posterior condition, due to changes of causative factors, $\Delta\sigma$, ΔS, Δu, Δs, $\Delta\tau$, Δstrain, etc. The sequential condition of a HCCFD slope destabilizable mass is especially didactic from GEOTECHNIQUE'S FIRST PRINCIPLES. Assertions have been published scattered through the past 25 years. They are merely listed forthwith, in frank challenge/entreatment: one by one, to be recanted or rechanted ?

1. Geotechnique implies love for accompanying changes through successive steps and stress-strain-time. No soil elements in any other prototype are better know(n) (able) than in HCCFD regarding classification and characteristics, and in situ starting stresses.

2. Residual in situ stresses are know(n)(able). Measured values, especially of suctions, must be improved, but advances do not change present assessments. Each layer's residual stress after the roller leaves starts with $\sigma_h > \sigma_v$, $\sigma'_h > \sigma'_v$.

3. In situ parameters can be amply and meticulously investigated, determined.

4. Progressive rise of fill applies $\Delta\sigma_v$, firstly leading to isotropy, and only beyond a certain overburden height giving the (σ_v - σ_h) deviator the tendency to destabilize. Construction period pore pressures Δu_c change from initial suction to positive values, EVENTUALLY DESTABILIZING. Conventional UU tests for predicting Δu_c lie far from approximate realism.

5. Judicious inclusion of suctions and compacted residual stresses results in perceptibly increased construction-period slope FSs.

6. Limit-equilibrium must abandon vertical slices and the $\sigma_1 = \gamma z$ vertical $\Delta\sigma$ hypotheses, condonable in historic flat slopes, small slices. Appropriate wedge-slices kinematically admissible must be queried (Sarma, 1979).

7. Queries on Slope Destabilization.

7.1. It is accepted unquestionably that effective stresses determine behaviors and stability. However, I question that we have really known, measured, or credibly predicted, the concomitant pore pressures AT THE FAILURE PLANE DURING CRITICAL INSTANTS OF STRAIN, especially if fast, more damaging. Therefore, stability analyses should be incremental, final stages of ($\Delta\sigma$, $\Delta\tau$) $\rightarrow$ ΔFS being conducted under TOTAL STRESS INCREMENTS AND ACU STRENGTH INCREMENTS (for prudence, engineering).

7.2. In back-analysed prototype failures I have further strongly questioned the hypothesis of equivalence, FS $\equiv$ 1.00. Failure signifies $\Delta FS = FS_i - FS_f$ positive, and PASSING THROUGH 1.0, not standing ("statics") at 1.00. Destabilization potentialities must, therefore, go through 2 or 3 STEPS OF CONVENTIONAL STATICS. Fig.9 illustrates all points.

7.3. Thus, for a starting first approximation the likely critical surface is computed (by unquestioned Limit Equilibrium updated methods, judicious general surface) for the HCCFD having reached the crest. Presumed FS $\geq$ 1.3 should result. Thus, the "rigid solid body" does not become "isolated, for statics".

Then, coming back with same critical surface recompute FS_1, for, say, 70% H. Proceed to recomputing FS_2, FS_3 for 85% and 100% H. However, for these ΔFSs, since the DAM CONTINUUM continues to prevail, the $\Delta\sigma$ and $\Delta\tau$ values on the surface SHOULD NO LONGER BE TAKEN BY THE VERTICAL overburden

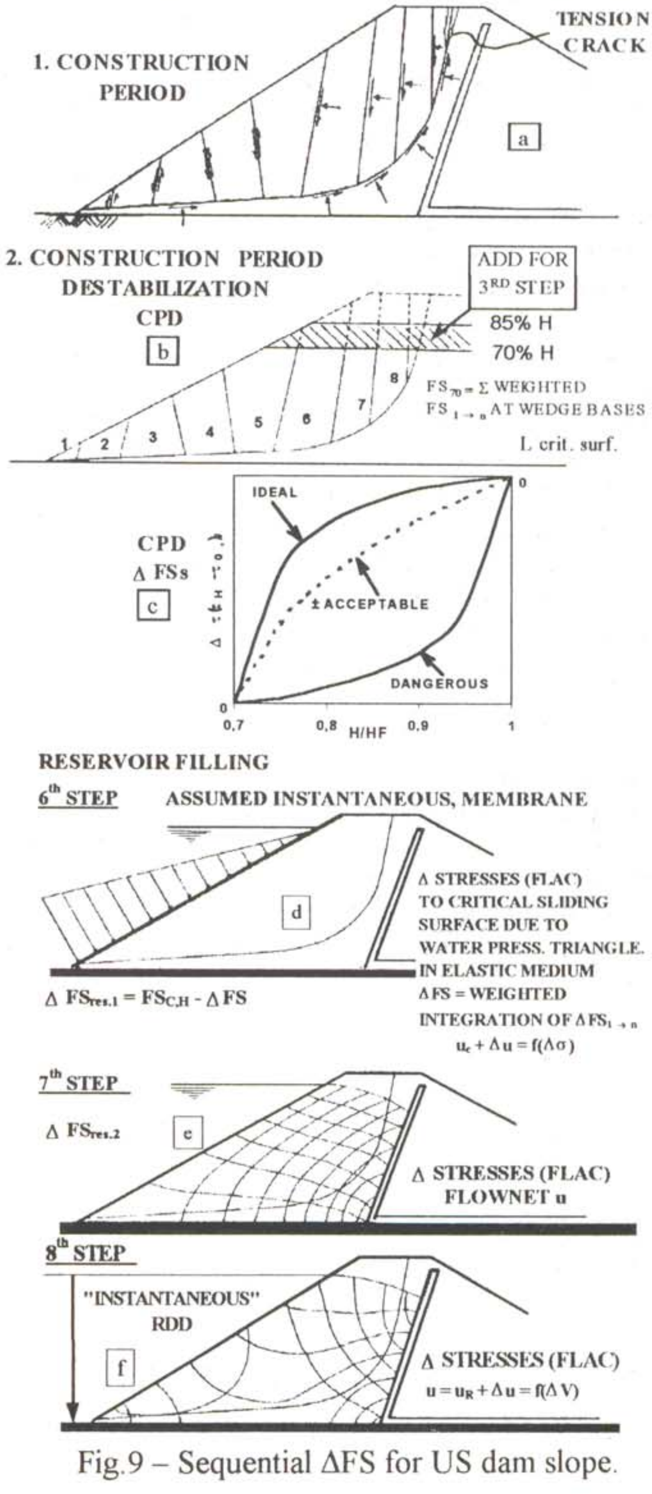

Fig.9 – Sequential ΔFS for US dam slope.

heights of the slices, but by analyses (e.g. FLAC) of the Influences of the added fill-weight trapezoids.

Results are quite different from conventional. Engineering prudence on the project slope is required if the successive ΔFSs increase rapidly (Fig.9c).

8. Reservoir filling, with respective flownet and flownet-compatible effective stresses in the continuum, generally introduces a favourable ΔFS to the end-of-construction ΔS_c. Saturation mostly requires high back-pressures (e.g. 6-10 bars, depending on porosimetries) and is too pessimistic, inapplicable to the common shallower sliding masses. In principle, again, the new FS is obtained via ΔFS.

9. Rapid drawdown pore pressure changes again call for treatment via ΔFS. Despite modest unsaturations, the continuous pores are sufficient to allow the POTENTIAL CHANGE from one flownet to another (Biarez et al, 1991); and, for conservatism, this tendency may be mentally applied as instantaneous. However the resulting TENDENCIES TO CHANGE of effective stresses (in the continuum, on the hypothetical sliding plane) are temporarily altered by the transient Δu due to ΔV, unfavourable if contractive and closer to saturated. The resulting ΔFSs must consider these compressibility Δu, and adopt the short or long-term case, whichever turns out more critical. It is unacceptable, technically/economically, to persist with simplified idealizations of Rapid Drawdown RDD u values that disregard so dominant a purposeful feature as the filter-drainage chimney affecting both flownets, full reservoir, and tendency-to-change on to lowered reservoir. (de Mello, 1977).

10. Naturally several hypothetical critical surfaces should be tried. On any one of them the change of conditions, reflected in ΔFS, tends to be a more reliable index, than by allowing the computer to select separate critical surfaces (different) of secondary importance, bearing in mind the imposed hypotheses.

In summary, the recommended revisions are significant, and in several cases studied, would permit significant savings, with steepened slopes.

6 EPILOGUE

Why have such hypotheses remained by the wayside? In foundation engineering because of the arbitrary separation of professional endeavours. In dams, because generalist Civil Engineers dictate on layouts and principal external features of dams: geotechnique's early conservative prescriptions and the geometries of sections, have lent them spatterings of knowledge thought sufficient. How can we entice research and development if we ourselves do not challenge, nor seek refinements? Good luck to geotechnique.

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