Proceedings of the

GEOTECH - YEAR 2000

Developments in Geotechnical Engineering

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November 2000

GEOTECH-YEAR 2000
Developments in Geotechnical Engineering
27-30 November 2000, Bangkok, Thailand

Overview of hypotheses not pluked or pursued. Merit recanting or rechanting ?

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SYNOPSIS Fully and gratefully respecting the past, that has brought us to our present competences, we ennoble it more by emulating its courageous creativity, than by imperceptibly standing subdued into laborious pursuance onto dead ends. Where do past and future separate, in the continued challenge of choosing, adjusting, promoting, discarding, and standing back for periodically looking anew? Examples are put forth, as always subject to being superseded, especially in the complex-responsible engineering obligation of deciding despite doubts, on the ever-singular prototype itself. Queries are briefly broached on subsoil characterization, shallow foundations, urban tunneling, and embankment dam slope destabilizations. No item is simple or complex enough to be avoided. Subsoil characterizations for general sand-silt-clay soils, even merely sedimentary, merit queries of significance for minimal parametrizations: for profile interpretation, Terzaghi's emphasis on historic-geologic relevance is recalled. Footing foundations, even on pure sand, offer much ground for reorientations both on bearing capacities and on settlements. Urban tunneling imposes composite analyses of limit equilibrium zones, followed by altered moduli as functions of (FS,TIME), and consequent settlements. Movements tolerable for buildings are queried. Compacted clayey dams offer optimal conditions for general-soil research, both for foundations, and for successively phased slope destabilizations. Plasticity theorizations appear more as an illusion and hindrance, than as a boon, because of varied complexities in shear strengths: these can be judiciously incorporated in sequential equilibria, and stress-strain distributions in continua. May the enthusiasms, competence, and energies of younger geotechnicians be unfettered unto new vistas and visions of service to Society, to be proven or not, under narrowing statistical dispersions, in favor of economies without foregoing priority safety.

1. INTRODUCTION.

The turn of century and millenium is an incentive for many singular initiatives. For some, to recall the past; for others, to attempt, or to influence, projections into the future. I shall try to tie past and future, within a viewpoint that **acts of decision are required** for any fertile renewal, generated from the past, and aimed at the future. The day-to-day drone, ever more exacting and speedy, numbs us into unquestioning repetitivity. And we hardly notice two subliminar effects at play: on the one hand, the stifling of courage, challenge, and creativity, in the areas of greater responsibility; on the other hand, the progressive degeneration of conventional practices, handed on and on with decreasing zest, and often restrictively dictated by **codes**.

Quests and qualms abound in the vast numbers of common professional cases, judged to dispense specific geotechnical testing because of cost, schedules, and presumed sufficiency of **correlations** for parameters, and solutions by **prescriptions**. Majorities of jobs continue with age-old practices, since élitist geotechnique devotes little attention to optimizations of generic solutions. Heavy-investment cases fill our pages of mostly successful case-histories. Aren't there worthy challenges in the vast middle ground, repository of the greatest sum-total of investment ?

We must reflect with gratitude on the dominant trend encompassing most scientific-technological endeavours. It applies to the typical cycles of progress, wherein past, present, and future intertwine. Successive steps depend upon:

1) intuitive breakthrough or serendipitous discovery, frequently a leap forward that ensures longer persistence of the presumed but transient "new truth";

2) coupling this impulse with approximate models, and practical first-order "test" observations, that don't descend to details and precisions that risk confusing, or inviting refutation;

3) establishing conventional truths, and yes-no, dichotomic-deterministic **prescriptions for decisions**;

4) developing methods and precisions for tightening dispersions in results;

5) systematically accumulating data to sort out yes-no cases in prototype practice, accept-reject criteria of safety and serviceability, for the cause-effect cycle of hypotheses-decisions-results;

6) Humbly recognizing that **in reality**, bounded between beautiful mathematical formulations of (1) idealized equational behavior, and (2) probability mathematics, there is the fact that nature's behaviors are statistical/ probabilistic in concept and in dispersions, while "fitting into our laws".

In a profession very social-affecting, of each case ipso facto distinctive, until demonstrated sufficiently analogous to be treated as same, a period of collation of statistical documentation is much needed. Therein it is gratifying that the first-degree theory's success entices broad basic loyalty. Also in this period of less-questioning acceptance, one begins to sense influences of second-order factors that should lead to the intuitions of the following phase of closer scrutiny, for recycling cause-effect assumptions. The emphasis of experienced consultants on **judgement** arises from such perceptions of intervening factors, not yet incorporated into the conventions of the profession. Global competition dwarfs our individual aptitudes and efforts. Competition forces us to shed off erstwhile laudable prudence, and superfluity. Every other field, much more dynamic than age-old everybody-knows Civil Engineering, has continuous and stunning breakthroughs. Have we reached a culmination asymptote? Or do we enjoy the dynamics of a challenging trek?

Present hypotheses and practices are tied to the priority concerns of the past, which were: i) Failures, and Factors of Safety; ii) medium-scale Deformations, decimeter(s). The first world advances have shifted the goals to Micro-Deformations, (in the scale of millimeters to a few centimeters) and their allowable limits for Serviceability; a noticeable opposite exception being the case of large-strain consolidation of sedimented tailings and hydraulic fills. The past three decades have been prolific in: (a) simulations, numerical modelling, and computational expertise, reaching out decades ahead of the profession's capacity to **diagnose** and finalize with "Dense" Engineering **decisions**; (b) ground treatments, inciting the feasible dispensing with intimate knowledge of the soils and behaviors in situ; (c) reporting strings of case-histories, case by case, as if single case experiences can establish the needed confidence in statistically confirmed "laws" of practical theory.

This message is intended for the abounding binary geotechnicians, academic / professional, now beating in retreat, in demand and scope of opportunities in the market-dominated profession. What to expect of centripetal influence, in favour of specialized core refinements, if the market decisions are mostly formulated by non-specialists? We must aim at the **basics revisited**, that is, basics reviewed for the idealized soils, and thought-afresh for the so-called unconventional ones: and thereby influence the market by a renewed centrifugal energy, as first emanated by the starting mentors. Few examples suffice, ranging from the very first items of soil mechanics, on to design practices, laid dormant, on real problems of important works.

In my estimate 95% of geotechnical engineering is exercized for the support of "conventional solutions" via the teachings of the 50's and 60's (and degenerating, from conscious simplifications/idealizations, into undebated rules of practice); therein lie the damages from neglecting the importance of a historical view, and of prototype Civil Engineering's obligation to start from the prudent end of conscious conservatism. Therefore, while applauding each step of progress we must demand good statistical correlations of novel procedures with those previously respected, to preserve the accumulated prototype experience...the only one worthy of respectful reanalyses.

However, we must first inquire into the validity of concepts **subconsciously pervading**, of Method vs. End-Product, specifications and/or uses of results, **in their consequences to theory**. In a technology so deeply dependent on important stress-strain-time (and many complex cause-effect) influences, we greatly regret the lapses in logic that have so often caused stumbles: the use of **a homogeneous method** (start) does not lead to a **homogeneous end-product** (really, ipso facto, just the opposite). Consequences to pratice, and felt in construction and performance are frequent: but my intent is to cite the invariably forgotten interference of a **constant method**, acting on a varied or varying "universe', in misleading theoretical conclusions.

Two cases, already published, suffice. (1) In characterizing a "homogeneous" thick stratum across its depth, we satisfy ourselves with a **constant** ("undisturbed") **sampling** abridged description: and plot the comparative parameters as if all the samples are of equivalent quality, not influenced systematically by the changing depth, and erratically by components of handling, specimen cutting, testing etc... In truth, because the increasing depth inexorably increases effects of stress release (etc.) the theories on the history effect on the sedimented stratum (or, inversely, of the weathered saprolitic horizon) are ipso facto distorted by the need of a **varying adjustment factor**. The profession sorely needs systematic research and statistical data on such adjustment factors for representative cases. (2) The other published case, is that of the homogeneously compacted, nominally designated, "homogeneous earth dam", compressed into varying parameters by added overburden .

2. METHOD VS. END-PRODUCT SPECIFICATION: AND STANDARDIZATIONS IMPOSED, IN LIEU OF USED AS MERE BASIC REFERENCE.

For many a reason, including the important case of performances to be judged, forever insufficiently perfect compared with the future, however proven sufficient for the past, it is fundamental to **reject** once and for all **the often cited, and even lauded, method specification**. It is illogical.

A design (intent, desire), of each component (exploration plan, sampling, testing, parameter-selection, calculation, project-synthesis) inevitably seeks, imposes, a prospective **performance**. The only valid principle acceptable is the **end-product specification** really tying predetermined purpose to behavior parameters. Every step is vectorized, from what, by and through what, to what, for what. Only by respecting these links can we retain consistence in our always partialized contributions, either parallel in teams, or sequential.

For instance, a subsoil exploration design is not fulfilled by specifying a plan of borings and (conventional) identifications: these are but methods, appropriate or not to different degrees, depending on the goal, i.e. the best possible practical geologic-geomechanical configuration of the subsoil conditions, for use in necessary mental models for analyses, for design-construction of the job, and ultimately for meeting the job's technical-economic operation goal. With the end-product purpose clearly established, a cognizant "design forerunner" may and should **recommend presumed predicted optimized methods** for its achievement: but recommendations cannot be imposed or restrictive; means should never supersede the end. Many a failure, of dam foundation and tunneling, derives directly from this.

As for **standardizations**, always advancing at exponential rates because of the successes of **industrial multiples**, the diametric distinction from Civil-Geotechnical Engineering needs loud emphasis. Standardizations disrespect the individuality of each prototype, coupled with ingenious engineering's creativity. A common **reference standard** for communication and comparison is desirable, indispensable; but never an obligatory procedure to stifle the multiplicity of singular end-purposes within a **means** (itself generally levelled to least common denominators, and of time-lags of 15-20 years past).

Incidentally, as sorely as we criticise the invasion of imposed Standards/Codes, we also deplore the disrespect for the dire need of the **reference standard** to accompany varying procedures, for preserving the comparative communication. There is no culmination of knowledge and perfection; so, since everything is relative **(to different degrees)** we **must relate across different practices and experiences**. It seems, for instance, that so meritorious an initiative as the Bothkennar Soft Clay Test Site, U.K., engrossed with efforts at using the very best **(present)** samplers, did not: (1) run parallel sampling-testing with 3-4 most common so-called "undisturbed" conventional samplers (2" shelby, 3" shelby, 4" Osterberg etc..) that, unquestioned 50-40 years ago, accumulated countless case-histories of experiences **that will never be repeated**, (2) discuss comparative sample-quality indices, (3) suggest one such index as now preferred for **reference**, (4) indicate approximate consequences to design parameters deduced from other indices of current practice. Are we so well documented as to be able to despise revisiting and re-distilling all past prototype experience? At what cost to Society in its needs for the immediate future?

In summary, require: (a) **end-product concept specification**
(b) **recommendations on methods, varied and varying**
(c) **common reference standards used for communication/comparison**

3. LEAST GEOLOGIC BACKING TO SUBSOIL INVESTIGATION/ SYNTHESIS. ROUTINE CHARACTERIZATIONS.

Let us begin by considering the simplest and most documented case of quaternary marine clay strata.

How rare are the professional jobs, and respective reports and publications that (1) abandon the untenable geometrically distributed boring locations, for geologically oriented ones? (2) ever include at least a starting paragraph on geology, with estimated ages (calendar years, not geologic eras), of importance so stressed in geotechnique? Definitely, most geotechnicians need strong persistent reminders not to tackle subsoil in situ geotechnique without a sensible feel of the local geology: Terzaghi's lessons need loud rechanting.

If however realistic ages can be neglected for our purposes, many authoritative papers would merit recanting. How to know, unless scientific-technological quest draw pertinent data? Doesn't the profession (100000 earnestly working engineers?) merit having the question roughly quantified, before further ado? Is it not worth more than another DSc paper on a further variation on a constitutive equation? Incidentally, almost never (to my knowledge, Bothkennar excepted) have different ages been attributed from bottom to top of a clay layer. And, assuming 4mm (consolidated) thickness deposited per year (as in some varved clays, and Mexico City montmorillonites, etc.), a 10m thick stratum, implies a 2500 yr span.

And what is to be sentenced about subsoil profiling? Preconsolidation pressure? Method Specification for sampling "as undisturbed as possible"? Overconsolidation Ratio OCR? Dried crust? Aging? Precompression of sands?

How growingly serious and pressing it has become, to recognize and stress the generalized interference of **variabilities of procedures/results** across time and geography! As a result, the digestion and transfer of experience grossly lacks suitable statistical correlations for adjusting to **uniformity of communication**. On points recantable it is secondary, but not so on those involving principles, and principal parameters[1].

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[1] *Regarding all-important* ***geologic*** *context, one might mention one extreme example of abyssal lack of logic in the links, geomorphological-geostructural-tectonic-seismologic, affecting safety and economy of* ***dams****, so important to humanity: (a) Effects, transmitted and felt (by humans, accelerometers, structures) dependent on local* ***intensities****; (b) dramatic multiplicity of postulated* ***magnitude-intensity*** *relations, and their impracticably broad dispersions, hidden under* ***deterministic*** *number-magnitudes communicated (without specifics, nor iota-dispersions); (c) diametrically opposite logic of* ***recurrence pseudo-statistics****, taken analogously to cyclic hydrology, in comparison with phenomenological hypothesis of episodic abrupt stress-release* ***breaks*** *within a pseudo-continuum of tectonic-movement* ***stress-energy accumulation****; and blatantly (d) the intuitive recognition that since the* ***bias*** *of* ***dams-reservoirs*** *is of rivers as geomorphological expressions of weakness, and* ***dam-layouts*** *as sited on geologic singularities, the inferred statistics (hopefully fertile) is of how rare have been the seismic damages to the thousands of dams, tens of hundreds of thousands of dams x years worldwide, of sites* ***retro-analysable regarding comparative seismic danger indices****.*

3.1. Routine soil identification, characterization, and classification, for conclusion on first-degree predictable parameters.

Even for saturated sediments, the difficulty of a two-legged (and not pyramidal) system for cause-effect classification (grainsize distributions for silt-sand inerts, and plasticity limits for sub-silt fines) continues to thwart the ability to estimate design parameters[2]. The doubts, and mistaken predictions, arise in the **all-too-frequent intermediate soils:** when is it a clayey-sand, and when a sandy-clay; and depending on the label, what parameters of strength, compressibility, and permeability to attribute? One notices a bias towards worrying about the clayey component. Unfortunately there has been no effort to organize the multitude of data, for closing the cycle of experience, on reasonable correlations/dispersions for fundamental parameters derivable from the historic classifications. Meanwhile Academia continues to focus on "pure" sands and clays, dispensing even the "pure silts".

The historical intuitive "triangular classification charts" (of % fractions of sands, silt, clay) rightly fell into disuse long since.

We herewith put forth just two examples of reasonings that have progressed only as meek side issues. One, the fundamental importance of **porosimetries** (not merely gross porosities, secondary to solid contents); and the other, in saturated sandy-clays or clayey-sands, the derivable inferences from so-called "double-sedimentations" (sand grains into a mud, or muddy water into a sand-sand grain structure)(cf. Fig.1a).

Porosimetries dominate many important behavior parameters, more especially so in unsaturated soils: some appear more influenced by macropores, others possibly by micropores. Since over 30 years ago there have been progressively successful probabilistic solutions of poresize distributions of inert grains of ideal geometries[3]. There have been also successful test techniques determining porosimetries in fine-grained stiffer specimens. Predominant methods such as the intrusion methods of non-wetting fluids (e.g. mercury) would appear to overlook occluded pores. The importance of these to pore-pressure coefficients (and possibly to suction) raises the challenge to distinguish, via refined suction and pore-pressure research, between continuous and occluded pores.

The following exercises in cross-correlations, assumed much documented and

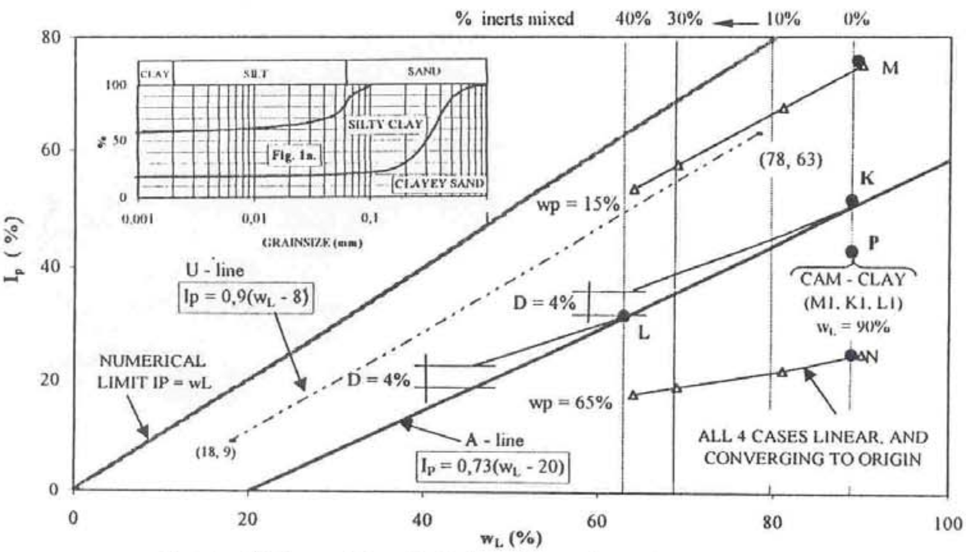


Fig.1b – Varing positions for 4 given clays, admixed inerts increasing.

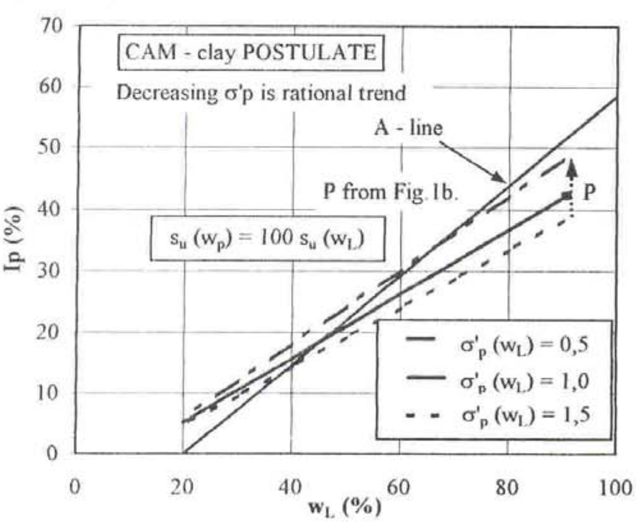


Fig. 1c – Influence of $(\sigma'_p)\ w_L$ on position and slope of line.

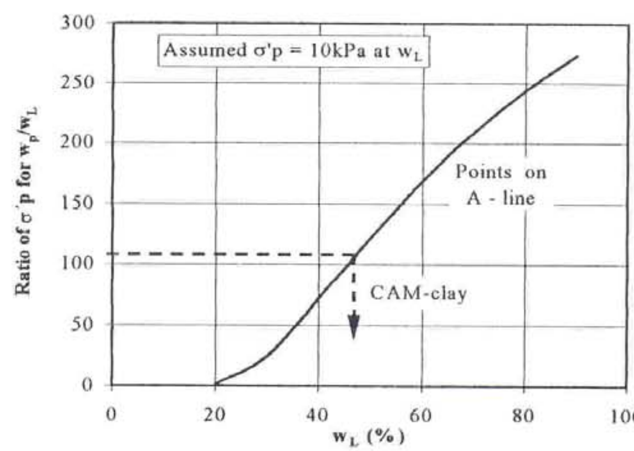


Fig. 1d – Ratio of σ'_p pressures for wp/wL for points on A-line.

Fig.1 – Interpretative exercises on clay Plasticity Chart.

[2] *For lack of space we forego mention of such gaps in typical* ***standards*** *as sedimentation grainsizes on the fraction passing the # 10 sieve, and plasticity tests on the minus # 40; and inviting periodic revision (laudable) of a standard, without requiring minimum set of* ***tests to correlate the future with the past.***

[3] *It is a pity that laboratory confirmations of the probabilistically-derived porosimetries have been indirect, via behavior parameters. Simple it should be, with the array of materials and solvents nowadays available, to use grains of material A, fill the pores with liquid B that solidifies with equivalent volumes to solids* B_1*, insoluble in solvent C; and finally to use this solvent C to dissolve grains A into pores* A_1 *pertaining to the packing structure of the grains* B_1.

divulged, are offered as a query on (a) the Plasticity Chart (b) the CAM-clay model postulates on Liquid and Plastic Limits (c) limiting fractiles of clay, and sands, on sandy-clays vs. clayey-sands. One lacks systematic research on a clear influence of different I_ps at the same w_L, and different w_{LS} at the same I_p, for ratifying the use of two-parameter regressions, as belongs to the Plasticity Chart Classification. Note that the historic tactile-visual classification is considered tied dominantly to "shearing at atmospheric pressure" (feel)[4].

Sandy-clay "families". These may be reasoned via effects of different proportions of "inerts" in a dominant matrix of saturated clay. Direct calculations of changed physical indices, by substituting particles of $\delta = 2{,}67$ g/cm^3 for equivalent volumes of saturated clay (γ sat $\approx 1{,}5$ g/cm^3, say, in a given case) lead to new values of the gross water content for equivalent behavior: i.e. equivalence of the undrained shear strengths at the two Limits, no matter what value is implicit. Fig.1b shows the results for 4 arbitrary points of (w_L, I_P) values: two, M, N, near upper and lower limits of common data, and two, K, L made to start at the A-line. The variations, of independent calculations on w_L and I_P, are linear, passing through (0,0). The suspicious curiosity had arisen some years ago that the A-line, put forth as strictly empirical, may have been induced by plain logical deduction: note that a slight difference of slopes (of about 1% I_p per 10% of inerts) arises.[5]

Next, we were prodded to check validity and dispersions of the CAM-clay Model tenet (of secondary importance to principal Cam-clay deductions) whereby "The value of the tests...they are rough strength indicators: the shear strength ... at its plastic limit may be roughly 200 KN/m^2, but only 2 KN/m^2 at its liquid limit. The plasticity index, therefore, is the increment of water necessary to reduce the strength of a soil **roughly a hundredfold.**" The calculations are limited to most-quoted test data, overlooking greatly disperse values, and CAM-clay's origins from inactive clay.

It is widely quoted that the w_L measures s_u values around 17-25 g/cm^2: indeed, in the Casagrande device the shear strength is associated with a "slope destabilization" (with some adulteration by impacts, avoided in the preferable Swedish cone test); in short, let us adopt the 2 kN/m^2. The other interconnected empirical correlations are: (1) $C_C = 0.007$ (w_L - 10%) derived from and for remolded clays (no data limits, statistics, and dispersions given)[6]; (2) accepting the ε vs. log s straight line as parallel to the C_C, confirmed also by the widely used $s_u/\sigma'_p \approx$ constant ≈ 0.22 (say); (3) equivalent consolidation pressure σ'_p at w_L estimated as 1 t/m^2. In first trials we calculated the w_p for an adopted value of $w_L = 90\%$ (points M, K, N, Fig.1b) and assuming the conventionalized data (σ'_p t/m^2, w) of (1, w_L): with increased inerts (10%, 30%, 40%) and consolidating the clay matrix to reach the 100-fold shear strength from w_L to w_p the results aligned linearly, to an identical w_p and I_p. Point K was purposely taken on the A-line because of **an inkling** that this empirical line really followed a reasoning corresponding to **families of points of a stratum** reproducing the recognized positions roughly parallel to it. Figs 1b and 1c show the lines systematically sloping a little different from the A-line, steeper for higher positions (fatter clays), and flatter for lower positions (silty and lean clays). Moreover, it is of curious interest that all lines converge to the origin (using the single parameter relation of $C_c = f(w_L)$). Incidentally, under the other adopted calculation constraints, and with a correlation oblivious of the chart's second parameter, I_p, the curious linear plots cannot but follow as obvious.

In summary further delving we present:

(1) In Fig.1c, the CAM-clay point P of Fig.1b was checked for any influence of the σ'_p value at w_L, recalculating for 0.5 and 1.5 t/m^2. The resulting trend seems rational, whereby fatter clays, higher I_p, would reach the 100-fold strength and better-positioned "lines", with lower slurry starting σ'_p.

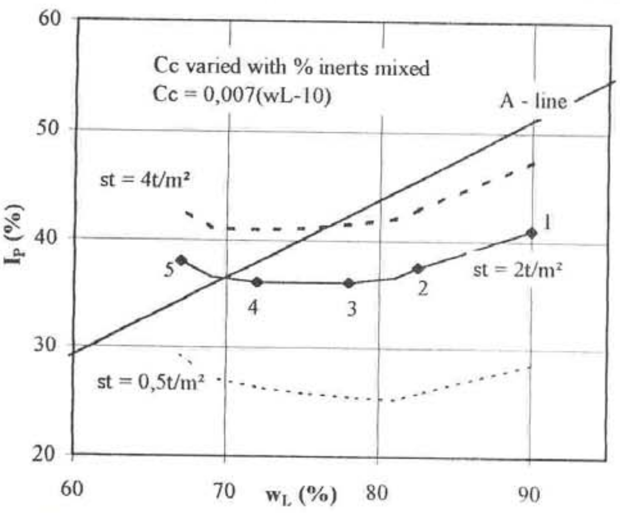
(2) Fig.1d questions the 100-fold consolidation pressure ratio for points on the A-line. For a lean clay of $w_L = 45\%$ the postulate roughly matches. But the ratios vary greatly with w_L, and at $w_L = 90\%$ it goes up to 273. Such a ratio, so much higher than mostly quoted, seems justified for the "tougher" behavior. The real σ'_p at w_L will be partly compensated by the finding of Fig.1c: much hinges on whether or not the w_p measures an undrained strength of roughly 20 t/m^2.

One further try is presented in Fig.2. It pertains to the intuition (cf. footnote 4) that w_p really measures a nearly **constant tensile strength** of the Brazilian test type. Three hypothetical strengths, referred to unconfined compression strengths were used. On the center curve the sequence of points is plotted, derived from the intervening variables, as tabulated. The $C_c = 0{,}007$ (w_L - 10%) and w_L at $\sigma'_p = 1$ t/m^2 were kept unvarying. The calculation sequence was: (1) with the % inerts (column a) the altered w_L is deduced as in Fig.1. (2) with the tensile strength s_t adopted, and ratio (s_t/s_u) w_p (column b) the s_u value is obtained. (3) assuming $s_u = 0{,}22\ \sigma'_p$, the consolidation pressures σ'_p at w_p are deduced (column c). (4) finally, with (σ'_p) w_p, the Δe and Δw corresponding to $w_L \rightarrow w_p$ is deduced, and the w_p and I_p (columns d, e) follow directly.

Meaningful positions on the Plasticity Chart may seem enhanced.

Conclusions on Sandy-clays. (1) The Plasticity Chart merits purposeful interested quest, for vindication or recanting. If not rationalizable, it should be abolished. (2) Single parameter correlations must be abolished as **strictly illogical.** Through them the w_L and I_p become directly bound to each other. By theory of errors (besides testing crudities) the I_p is the worse parameter. (3) general claims from CAM-clay cannot be extended to the variety of clays.

Clayey-sand "families". Such gap-graded sediments are often reasoned to



Points	% inerts	s_t/s_u	$(\sigma'_p)_{wp}$	w_p	I_p
	a	B	c	d	e
1	0	0,1	91	49	41
2	5	0,1	91	45	37,5
3	15	0,085	107	42	36
4	25	0,055	165	36	36
5	35	0,025	363	29	38

Fig. 2 – Varying positions assuming w_p tied to fixed tensile strength, s_t.

[4] *In slightly more refined evaluation, it seems that the w_p condition corresponds to a tensile failure ("first cracks") upon excessive shear distortion, a sort of "Brazilian test" tensile cracking: and thus should be affected by different ratios of tensile to shear resistances, smaller with increasing inerts. Seemingly the Shrinkage Limit would belong to quite different phenomena of fabric, suctions, and interparticle attractions-repulsions, and cannot be generalized as being lower than the w_p, with which it has absolutely no consistent relation.*

[5] *Since the heavier inerts lower the index-resistances a little (especially in the slope impact destabilizations of the Casagrande device) it seems reasonable for the clayey resistances to increase a little, with decreased (w_L, w_P) water contents. Pending special testing the effect on I_p is not clear, as a difference between two values that decrease, differently.*

[6] *Note that this has been postulated as compatible with the Gouy-Chapman diffuse **double-layer theory**, including the initial slurry void ratio, and a **modified effective stress concept** for interparticle attractions-repulsions.*

arise from "double sedimentation": in higher velocity turbulent flows, leading to erratic packings, the sands deposit forming an independent structure; therein the muddy waters deposit the silty-clay slurry. In principle one needs to distinguish between sandy-clays dominantly behaving as clays, and clayey-sands, behaving principally as sandy. What are the limiting proportions for the approximate shift from one to the other? Depends on the sand structure porosity, and the consolidation pressure absorbable by the slurry that filled the voids: and thereby the clay-type constituting the slurry interferes directly.

Assume that the sand structure can establish porosities between 30 and 60%, but for a mere example let us adopt 45%. Again, assume that the clay slurry can have $50 \le w_L \le 130\%$, but limit the example to 80% (Fig. 3a). Trends are discussed for plus or minus variations, both of n% and w_L%. Incidentally, the grainsizes and grain shapes also matter, as do, in principle, other factors presently disregarded. Calculations used the common relation $C_c \approx 0.007$ (w_L - 10%).

The reasonings that appear applicable involve the following sequence: (1) as a start the slurry total pressures (fluid) apply only to the slurry even within the sand's pores; therefore the clay dry weight percentage is deduced from the consolidation pressure's compressing the slurry into the pore volume. In Fig.3a we see that while 30% clay fits into the pores with a $\sigma'_p = 6$ t/m², with 40% the σ'_p would have to reach the very high value of 135 t/m² (point P).

Higher sand porosities obviously permit higher % fines, while higher w_L % force much greater limitation of % fines. At a sample value of $\sigma'_p = 3$ t/m² the following table gives the calculated numbers:

n (%)	% clay for w_L = 130%	50%
30	10,5	19
45	28,5	37,5
60	37,5	68,5

(2) Obviously a σ'_p of 135 t/m² applied as "fluid pressure" entirely on the slurry is recognizable as preposterous. There is, at present, no basis for estimating how a total overburden pressure (and respective effective stress) distributes between "resistant pore fluid" and sand-grain structure. But one could reason that for the joint behavior there should be some compatibility of mechanical behaviors, deformability and strength. For the consolidated slurry, the shear strengths are taken roughly as: undrained, 0.2 σ'_p, and drained, 0.46 σ'_p. The nominal elastic modulus of the clay is taken as roughly $500 \le E \le 1000$ times the undrained strength. For pure sands it is taken from most updated authoritative publications, as plotted in Fig.3b: corresponding frictions are taken as, loose, $\varphi' = 25°$, $\tan^{-1} = 0.466$, medium dense 35°, 0.7, and dense 45°, 1.0. In the same graph are plotted the c_u, c' and E values of the clay.

(3) Let us accept that beyond roughly $\sigma'_p \approx 3$ t/m², $c_u \approx 0{,}6$ t/m², $c' \approx 1.4$ t/m² the pressures begin to be transferred to the sand skeleton by way of micro-shear relative displacements along the sand grain surfaces.

Beyond (σ', c_u) values of the order of (3, 0.6 t/m²) the slurry is no longer a fluid receiving the total overlying fluid pressure exerted all-round on the grains as additional sediments deposit. Vectorized effective stresses take over in dominating behaviors. At present we lack clues for estimating how the stresses distribute between the sand structure and the clayey pore infilling. An analogy for visualization by civil engineers may come from reinforced concrete columns: the concrete-steel bond (shear) ensures the apportioning of compressive stresses to the steel and the concrete; the stresses absorbed are such that both "elements" considered separately, with their distinct moduli, suffer absolutely equivalent strains.

Fig.3c considers, for ease of a mere example, the case of combining the stiff-clay infilling and the medium sand. In sample condition M a common E = 10000 t/m² is obtained with $\sigma'_{vs} = 25$ and $\sigma'_{vc} = 50$ t/m² (the subscripts are v = vertical, s = sand, c = clay). Another condition N yields values of $\sigma'_{vs} = 85$ and $\sigma'_{vc} = 85$ t/m² for E = 17000 t/m². If for conventional simplicity in the face of declared ignorance of effective partial areas we adopt, for a force equilibrium $A\sigma'_v = \alpha A\sigma'_{vs} + (1-\alpha) A\sigma'_{vc}$, the hypothesis of reducing to nominal pressures

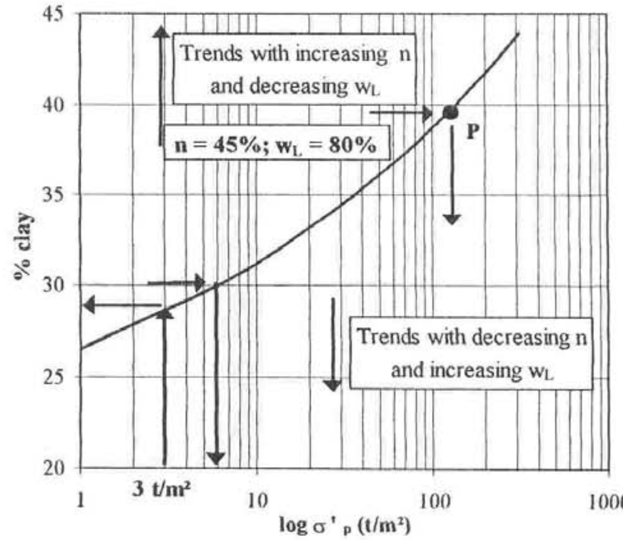


Fig. 3a – Consolidation pressures required to compress % clay fines into pores of sand remaining unaffected.

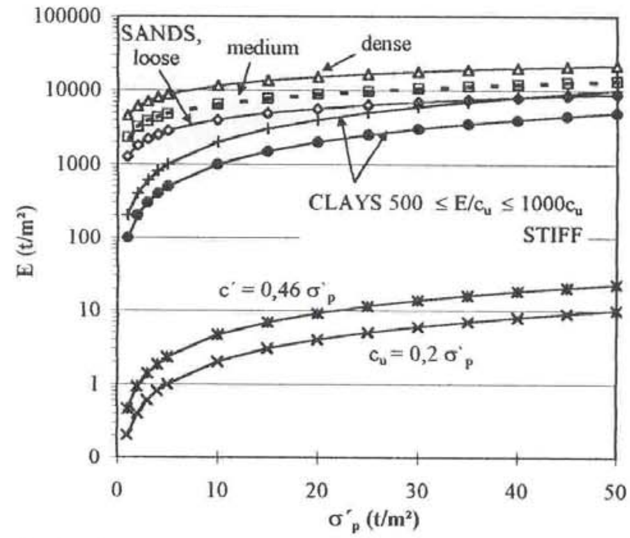


Fig. 3b – Approx. comparative deformation and strength parameters.

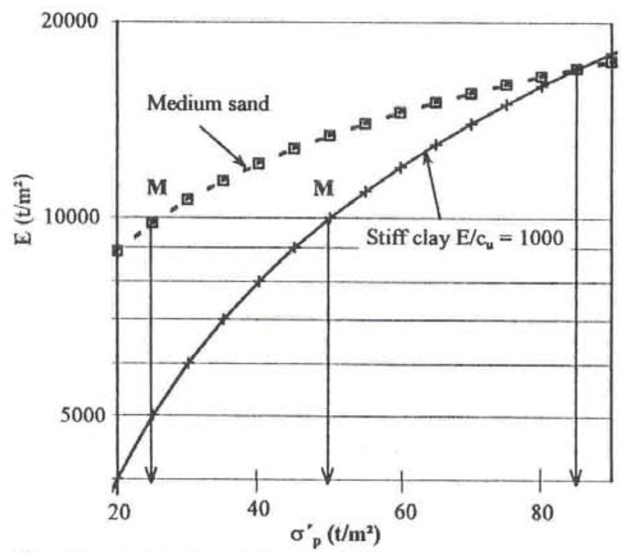


Fig. 3c – Hypothesis of nominal compatibility of moduli of sand and pore.

Fig. 3 – Exercising on classifying clayey-sand.

on the total area A, we would hypothesize effective overburden pressures of $\sigma'_v = 75\ t/m^2$ and $170\ t/m^2$ respectively for sample conditions M and N.

For M, going back to Fig.3a we would infer that in equivalent coparticipation the pores would be limited to 34% of clay fines of $w_L = 80\%$. Percentages smaller than 34% would imply increasingly more the behavior of a sand with secondary clay slurry: higher percentages would lead to behavior as sandy-clay.

The influence trends from n, and w_L have already been indicated. Point N hints at a further intervening factor, which is the gross consolidation pressure. For the higher value of $170\ t/m^2$ the nominal limit of clay fines would rise from 34% to 38%. These mental exercises are decried as to specific credibility on numbers. They only suggest interest in interrelating principles of possible deeper feel on geotechnique, that has been reduced to elasticity, plasticity and equations. The desired hints are of the: (a) wide-open door to investigations and redefinitions; (b) caution regarding visual-tactile classifications at near zero pressures.

3.2 Interpretative reevaluation of profiles of overconsolidation pressures.

The importance of differentiated behavior of normally and over-consolidated soils, both sands and principally clays, has greatly increased through the past decades. In some situations, such as for calculations of settlements, the parameter sought is the Δq, the margin of pressure between the assumed initial geostatic in situ vertical stress and the test-determined "yield preconsolidated stress" up to which the smaller compressibility prevail. Respecting space limitations we forego mention of the unlimited succession of queries that persist regarding in-situ stresses, and sampling-testing disturbances, and concepts regarding the significance of the σ'_p pressures determined across a profile: let it be assumed that the Δq value is real. The more frequently used parameter, in innumerable correlations, is the **normalized overconsolidation ratio**, $OCR = (\gamma' z + \Delta q)/\gamma' z$. And we recognize unabashed that, by unquestioning inertia, the majority of geotechnicians and collateral engineers imagines a constant OCR of a stratum, coupled with an increased OCR "at the top" as a presumed "dried crust" (which opens another array of interesting queries).

One is again enticed into enjoining truly loving geotechnicians not to forego seeking interpretations of the imprinted past, because of the **indefatigable interference of stress-strain-time behaviors**, historic, past, and near future.

Only two well-documented cases are herein tackled as mere examples: (a) the Bothkennar soft clay test site (Geotechnique, 1992); (b) a site in downtown Boston (Hashash & Whittle, 1996). It need hardly be noted that the OCR ratio is **numerically obliged** to swoop down from high values at near-surface, of $\gamma' z$ near zero, to asymptotical near-encounters with the overburden effective stress at depths increasing with increased Δq (cf. Fig.4, detail). This pertains to "infinite" influence factor $I = 1.00$ loaded area with Δq.

The Bothkennar clay reasonably fits this common interpretation (Fig.4 line A). Moreover, since the test-data regression suggests a definite progressively higher Δq with depth (curve B), and there is information on secondary compression coefficients and estimated ages of bottom-to-top depositions, one has the incentive to try incorporating the increased preconsolidation due to the Leonards-Bjerrum secondary compression. Fig.4 curve C summarizes the first-order approximation tried. It seems proven that there is interest in such interpretative exercises, fully respecting the **engineering precept** of subsequent simplified idealization for satisfactory/safe project solution.

Curve C was calculated under the routine premises: (1) a constant Δq assumed (line D), necessarily smaller than the published $1.5\ t/m^2$; (2) the first portions of $\Delta\varepsilon$, corresponding to virgin primary compressions, necessarily result somewhat smaller at depth than near top, starting from assumed normally consolidated profile; (3) incremental secondary compressions were taken for $C_{\alpha\varepsilon} = 0.04$, and a maximum nominal difference of 2500 years from bottom to top (N.B. references were found to 1000, 1500, or 2500 years age difference: clarification and certification are of no consequence to the present conceptual submission); (4) the consequent total $\Delta\varepsilon$ values (primary plus secondary) result greater at bottom than near top, and corresponding σ'_p and $\Delta\sigma'_p$ values also result greater at the bottom. The statistical regressions of the data points tally reasonably with this modest reasoning. The numbers surely require corrections. The principle is what matters: profiles of good test data must be submitted to **consistent geologic-geotechnical interpretation**, inasfar as possible, before being adapted to convenient engineering simplifications.

The Boston test-data (taken unquestionably) present a strange scenario. Fig.5a reproduces the data and authors' adopted trends, alongside with two regressions herein tried, linear and exponential. Three points arouse strangeness: the very much better exponential regression than linear; the very high near-top σ'_p values, reaching 800kPa (about 80% of σ' often associated with w_p), equivalent to 130m of "infinitely" wide submerged overburden; and thirdly, the abrupt apparent change at about 26m depth, implying the "dissipation" of the overpressure between depths of 13 to 28m.

Barring other more unusual hypothetical causes, the σ'_p profile might hint at a pressure-bulb condition, of an unusually high surface pressure on a limited loaded area (Fig.5b). The less unlikely results would pertain to a hypothetical loaded surface at sand-top elevation, before the profile's fill. Regressions were tried with different influence values, areas, applied pressures. A first assumption (of unlikely coincidence) was of stress transmissions under the center of the loaded area. For curiosity, trials were repeated for influences

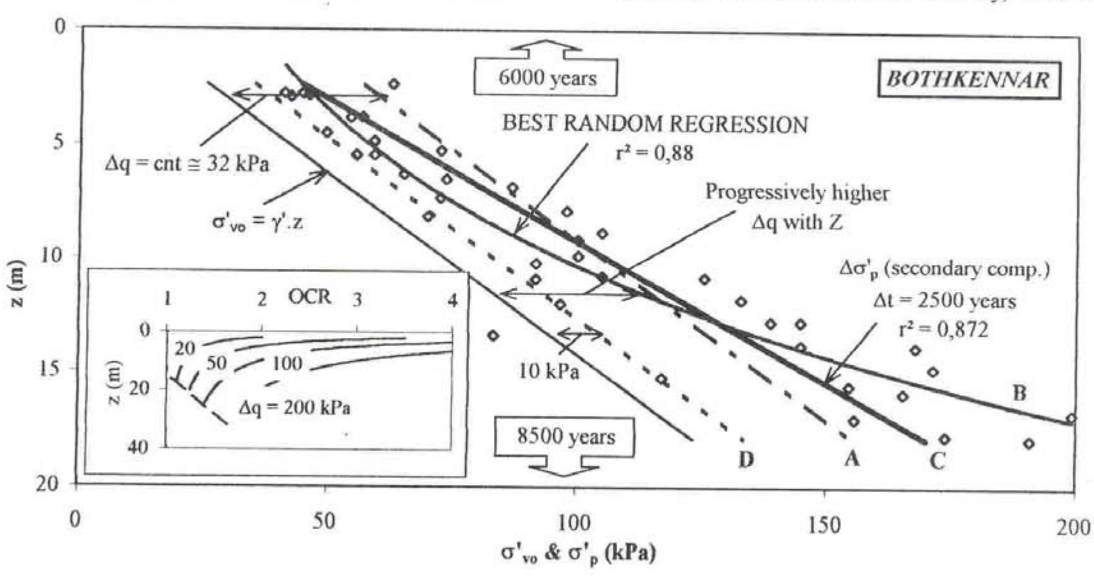


Fig. 4 – Investigating the Bothkennar preconsolidation profile, an exercise

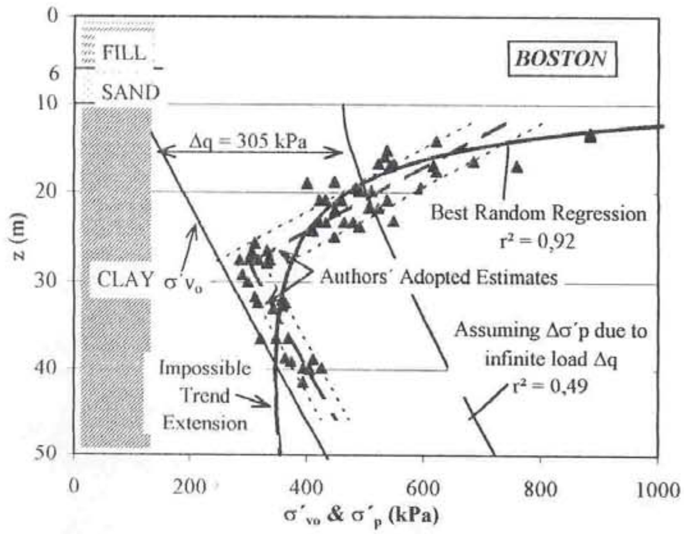


Fig. 5a – First interpretative trials of overburden consolidation.

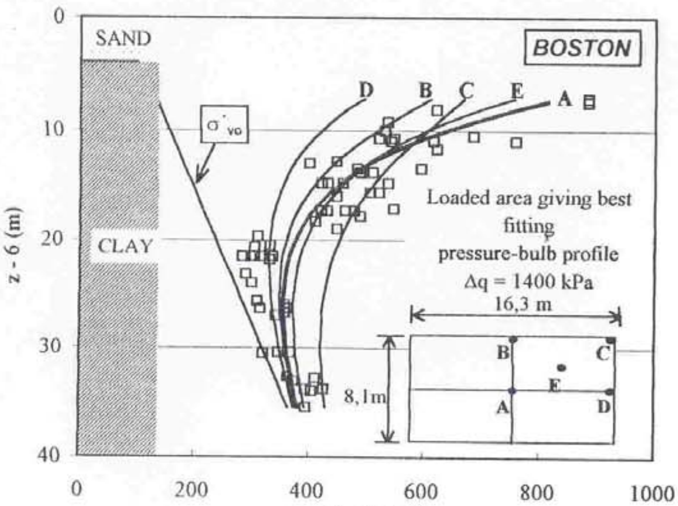


Fig. 5b – Investigating insinuated absurd pressure bulbs.

Fig. 5 – Profile preconsolidation analysis; strangely high OCRs.

under several points asymmetrical in a rectangular loaded area. The regressions hint at considerable interest, but the area dimensions and applied pressures, would be quite beyond explanation. The applied pressure of 14 kg/cm^2, however unconceivable, was assumed constant over the area. **The principle is not revocable**, that the profile of credible test data must firstly be interpretable as to presumed subsoil history.

3.3 Tenable correlations, and dispersions, respecting professional uses.

Every professional problem passes through successive stages of approximation, first estimates relying mostly on **correlations** and **prescriptions**. Subsequent phases should depend on benefit/cost perspectives, taking into account increased costs and time, for better definitions of behaviors, under intended **unchanged factors of safety**. The existing, widely divulged, correlations, are seriously affected by: (a) general disregard of assumed theorized trends (to be ratified, adjusted, or even possibly refuted); (b) omitted delimitation of the range of the test data that generated the correlation: the risks of extrapolations to other materials and ranges of properties must be minimized; (c) single-parameter correlations, when more than one **dominant** causative parameter is well recognized; (d) "average" correlations sketched-in by eye, instead of calculated for "best statistical regressions"; (e) generalized oblivion of dispersions and correlation coefficients, or preferably, percent confidence bands, disregarding that meaningful Factors of Safety depend on **ignorance and confidence bands**.

A few items summarized forthwith are mere examples calling for readjustment, revalidation, or rebuttal. Erstwhile well-intended, they presently leave the demeaning impression that geotechnique cannot surmount the "rule-of-thumb" dicta, memorized from the "experienced consultants".

Fig.6 reproduces a repeatedly proposed correlation between sin φ' and I_p. What reasoning, what dispersion, why the preference for I_p in lieu of w_L (and, especially, how to forego the double regression on (w_L, I_p) if the Plasticity Chart has merit)? How to compare, or tie-in, with other analogous exponential correlations of φ' directly with w_L or I_p? Many published regressions, of similar criticisable bases, invite similar challenges; such as those of φ'_{res} vs. I_p or w_L, without incorporating the physically reasonable effect of platelet clay-mineral particles forced into slickensiding.

Fig.7 repeats my (1979, 1981) proposed minimal adjustment on a linear correlation offered by Massarsch (1979) between K'_o of normally consolidated clays, and their I_p. Lacking data, both of specific points used, and of other clays, the exercise was merely given to note that a linear correlation should be queried by reasoning that a condition of a very fat clay slurry should have an asymptotic trend towards $K'_o \rightarrow 1,0$. Obviously the search for a double regression would prevail, rendering the exercise of 18 years ago barely pardonable. But better exercises lie awaiting, or my postulated principle calls for recanting. The importance of φ', and K_o, in professional practice cannot be underrated; nor the untenable negligence on ignorance and dispersions that persist.

One of the classic authoritative near-correlations of greatest use by professional practice has been of the type $c/\sigma'_p \approx a + bI_p$. Possibly the oldest set of numerical values, since five decades ago, has been a ≈ 0.11 and b ≈ 0.0037. The numerical values have been questioned, and altered, in cases, but time and again without questioning the cross-connected dictates. With due respects, many intervening factors merit reflection and research. Many of the early c values were taken from unconfined compression tests $2c \approx R_c$; and sampling-testing qualities were primitive, though perfection-seeking. It should not be immaterial to relate merely to I_p instead of (w_L , I_p) since the suctions retaining c close to c_u should be more efficient in the fatter clay than in the lean silty-clay of same w_L: also, to relate only to σ'_p without considering the composite (σ'_v, $K'\sigma'_v$). One cannot avert including the more updated tendency to normalize $c_u / \sigma'_p \approx$ (roughly 0.2 to 0.25), and the collateral relations, that abound in routine tests, between $\varphi_{sp} \approx f(\varphi')$ and $\varphi' \approx f(w_L, I_p)$. In short, such relations c_u/σ'_p, of great use to practice, need prompt conscious revisions (recanting or rechanting) under statistics theoretically moulded by so many limpid cross-correlating behaviors.

The final example herein cited concerns the repeatedly quoted empirical correction factor "to be applied to the results of vane tests" to establish the "shear strength mobilizable in the field" for problems of failures of embankments on soft clays. This correction factor μ is given as a function of I_p: its trend becomes secondary, in the light of (a) presumed logic (b) number of failure cases that might have served for the proposed factor $0.55 < \mu < 1.0$ for I_p varying from 20 to 120. As for logic: what miraculous relation might tie a crude index parameter I_p, from two tests on fully remoulded material, to the in-situ strength behaviors of undisturbed clays[7], of sensitivities that may vary from near-infinite (quick clays) to about 4-10 ? As for data, doubtless few, and dispersions, wide-range: what matters it to find any **random statistical correlation**, such as the classic irony on a high correlation coefficient between the numbers of tons of pig-iron produced in Philadelphia in 1890-1900, and of births in London in the same decade? In favouring the zests of geotechnique's challenges, in lieu of the comforts of prescriptions (hallowed), one should recant on this postulate (as an example).

[7] *We are bound to set aside, herein, the broad complementary range of queries on the base-failure analyses that generated the in situ strength values.*

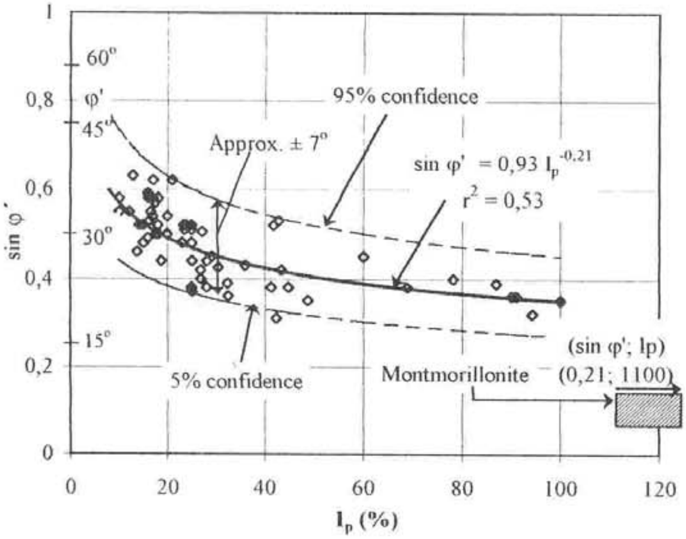


Fig. 6 – Repeatedly published correlation $\varphi' \approx f(Ip)$.

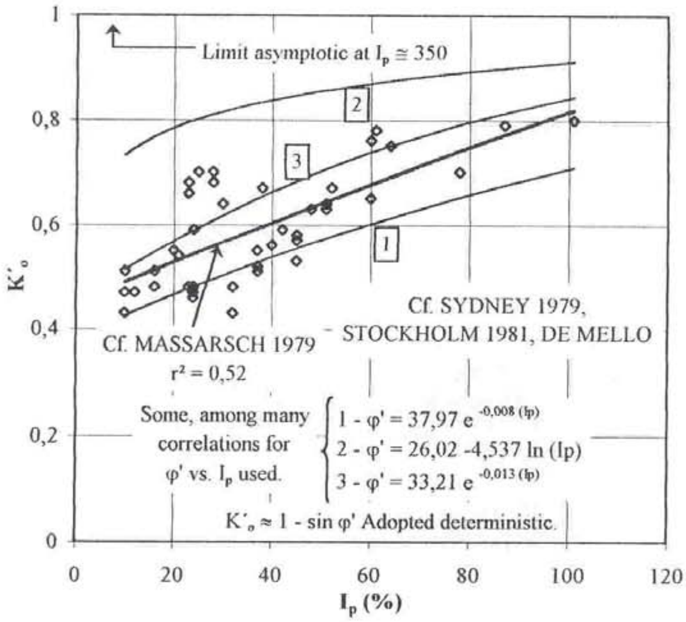


Fig. 7 – Repeatedly questioned correlations for K_o in clays.

4. SHALLOW FOUNDATIONS: BEARING CAPACITIES, "FAILURE" AND "TOLERABLE" SETTLEMENTS.

One recognizes that in most structures on bigger footings the controlling criterion is one of tolerable settlements, especially so in advancing societies. Thus, it would seem that interest in bearing capacity and Factors of Safety FS centers on lesser structures, with smaller footings, regarding which forefront geotechnique relaxed attention. Socio-psychological reasons persist, however, for giving first attention, by professionals, to "failure" analyses. By far the greatest number and economic volume of construction concerns lesser structures. Even for aiming at settlements, one needs to "feel" the approximate FS involved, because of decreasing moduli with lower FSs, and larger "plastifying" slower settlements than the "short-term" pseudo-elastic ones. Most professionals can estimate strengths more reasonably than deformabilities. Philosophically no civil engineering structure can dispense estimating the FS, including therein the **factors of ignorance**. In short, accumulated experience ties to allowable pressures, much associated with presumed FS values: if the latter are too low, there is perceptible cracking, but if they have been set too high, there is a call for improvements, in favour of economy.

Bearing capacity solutions must, in principle, be as **near true as possible, while truly safe**: the latter dominates the first steps, leaving the former as the persistent challenge. True to what? And in what partial steps, to be progressively improved, especially in the light of greatly advanced updated computational abilities, in all collateral branches ?

There was no escape from the reality that professional practice imposes the need to quantify approximate FS values on "failure". This is so even if pin-pointing a failure pressure (associated with great incessant settlements without pressure increase) becomes nebulous (cf. "local shear"). The fearful concept of failure, loss of **static equilibrium**, is too deep-rooted and codified: and it is understandable that historically the more frequent cases of slope failures (including those with retaining walls) should have followed the progressive concerns and refinements on the slope-stability analyses by rigid-body statics (Fellenius, and countless subsequent authorities). In comparing with early "plastic equilibrium ideal-solid theoretical" solutions (Prandtl 1921, Frohlich 1934 etc.) the Wilson (1941) asymmetrical base failure (cf Fig.8a) merits practical interest as successfully analysed, although merely via Fellenius' circular limit-equilibrium failure surfaces.

Since bearing capacity solutions, and slope stability ones, bifurcated very early into distinct avenues, a brief preamble of comments appears profitable:
(a) The 3-dimensional (3D) condition, as compared with 2D, occurs in both problems, though much more markedly in foundations. Through decades, the slope limit-equilibrium solutions progressed under 2D conditions, the 3D adjustments undergoing implementations more recently. The bearing capacity plasticity solutions had to use 3D shape-factors since the beginnings.
(b) Possibly the far greater number of computation-requiring cases (buildings, and different footing dimensions in each) induced the preference for plasticity-theory solutions, more amenable to tabulations and charts of complex factors.
(c) Possibly the lures of plasticity theories may be interpreted as having been irresistibly seductive by a historic coincidence in timing. Prandtl's (1921) exact and unique solution[8] for a smooth strip footing, a Mohr-Coulomb (c, φ) soil, although weightless, must have been an irresistible boon to Terzaghi's

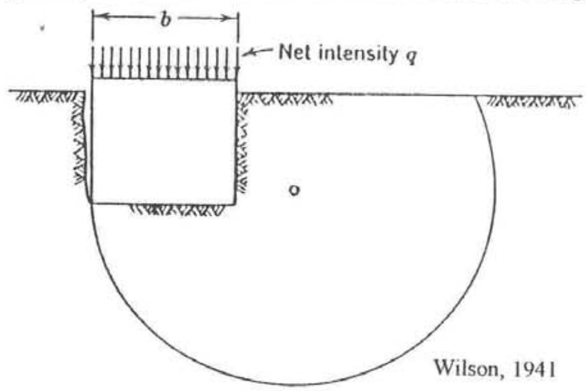


Fig. 8a – Limit equilibrium analysis via Fellenius – Wilson.

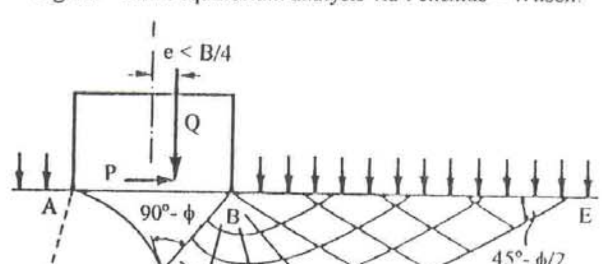


Fig. 8b – Curious extreme case of method of "lines of characteristics".

Fig. 8 – Different approaches to shallow foundation failure.

[8] *Simultaneously upper and lower-bound, by ulterior concepts, and analyses.*

needs. Rigid-body destabilizing movements[9] were the rule in all analyses, slope-stability, retaining wall pressures, etc... There was no (anticipated) questioning of the later Drucker-Prager (1952) dictates of associated vs. non-associated flow rules, normality postulates, statically and kinematically admissible fields, velocity fields, and so on. The two problems signalled, of **footing smoothness**, and **weightless soil,** seemed easily tackled by then current engineering expedients[10]. So, it is inviting to infer that a main avenue opened to a welter of respected publications on Plasticity Theory solutions for bearing capacity problems, improved and /or confirmed and bounded.

(d) My questions and objections to be raised in these matters are unfortunately repeated from a distant candid outcry (de Mello 1969). Concisely, one observes worldwide that foundation practitioners report back to the roots, Terzaghi in essence, despite the undisguisable slips of infancy. On behalf of a caring professional interaction between leaderships, academic and forefront foundation designer, I appeal for an **unabashed abandonment of plasticity theory solutions**, their postulates and results to be courageously recanted[11].

4.1 Limit Equilibrium solutions, bearing capacity formulae and factors, and finite element analyses leading to Limit Solutions.

To begin with, case-histories of failures of wider foundations (tanks, silos, storage yards, etc.) have been irrefutably asymmetrical (N.B. it is a dominant mistake in intellectual circles to respect truth as mathematical and symmetrical)[12]. These cases, and those of embankments on soft soils, have always been analysed by limit equilibrium methods. The early, and initially persisting, shortcomings have been progressively diagnosed and corrected; incidentally, much earlier, better, and more convincingly than those of plasticity theory. "Theoretical impurities" have been questioned, profitably bounded, and systematically adjusted and removed. So very many geotechnical works are confidently dependent on these analyses, that the strangeness of the priority given to footing bearing capacity via plasticity might be queried, and answered with regard to four questions:

(a) the reasonable realities of 3D vs. 2D analyses[13];

(b) the dissociation of deformation phases from ultimate failure analyses[14];

(c) the great disproportion of applied stresses to initial ground stresses. This invites **very special attention** because not only it appeals strongly in favour of Limit Analyses rather than Plasticity solutions, but also it raised the key questioning of 1969. Does the comment call for recanting ?;

(d) computational facilities have increased exponentially. This facility could be equally valuable for any intended line of attack, but tends to attract more towards sophistication. Erudition evinced is admirable, (though possibly unnoticed by Nature's simplicities) but admiration accepted can be stifling.

No matter what analysis, geomechanically reasonable, is adopted, and what degree of sophistication it reaches, let us humbly recognize that we will never avoid the ever-present statistical adjustment factors of theory-to-practice. Thus, the more tantalizing the efforts towards scientific and mathematical perfectionings on ideal soils, the more we postpone facing reality.

Fully 25 and 20 years after Terzaghi (1943) and Terzaghi-Peck (1948) put forth the longed-for recommendations that helped my own initial, deeply concerned, steps on footings for highrise buildings, I submitted (Mexico 1969) some professional concerns, which are now rechanted;

(1) interests and needs of professional practice abound in "general soils" of **varying (c, φ) parameters nominally linearized**, and greatly varying deformabilities and **responses to rates of loading.** But hitherto most challenges, theoretical (especially plasticity theory), experimental (model and field, quite rare), and prediction-performance, have systematically continued to avert any but pure (c = 0) sands, and pure (s = c) clays.

(2) In 1969 it was reminded that in early days one could condone the presumption of a single Mohr-Coulomb equation $\tau_f = c + \sigma \mathrm{tg}\,\varphi$[15]. In all plasticity theory derivations this hypothesis continues unaltered (and unalterable?) despite 50 years of recognition that it is untenable. Are we playing with our colleagues' responsibilities, and/or our credibility ?

Permit me to set aside the incremental problems not rendered current, of extracting second-order parameters from current tests (e.g. angle of dilation, etc... cf. ASCE Workshop at McGill Univ., 1980). The 1969 warning was that in the mother-derivations of general bearing capacity factors, N_c, N_q, N_γ, the **three conventional types** of tests intervene in different degrees and different "applied stress ranges": the "true" effective stress φ' in determining (by innocent plasticity theory geometry) the shape of the failure surface; some "appropriate" consolidated-undrained strength envelopes (of varying conditions along the presumed surface) in influencing the ds/d γ'z of in-situ strength with depth; and the unconsolidated-undrained strength equations for different soil elements, in influencing the ds/dq of strengths affected by the stress level of surcharge and **foundation loading itself** (prudently taken as fast, undrained, for contractive materials, at least for live loading).

Mention must herein be made that for one ideal material there has been a partial move in the direction above summarized. That is the development by de Beer (1963) of the realism of using the curved strength envelope for sands under progressively increased high stresses under pile points.

The unquestionably exemplary data of Fig.9 on saturated London Clay do not reach the full extent of generalization of typical cases of professional practice (incomparably less documented), because unsaturation and possible suctions are not incorporated. But they are sufficient to pose the unanswerable challenges on **what parameters, equations and resulting coefficients** might be used to fit in by the Plasticity Theory method. Meanwhile, and on the optimistic side, we may note that Limit Analysis solutions permit the desired flexibilities, even without obviating the common solid-body hypotheses.

First and foremost, any and every shape can be used for arriving at the most critical shearing surface. Moreover, these can change at successive steps of the loading sequence[16]. The dominance of angles φ' and ψ in imposing failure

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[9] *i.e. at "very small strains", and under mathematical obligations, of $\Delta V=0$ through the shearing-dislocating masses, and implying Hvorslev φ_e of ΔV.*

[10] *such as Caquot's transfer of axis, applying surface surcharge, and changing shapes of wedges and directions of principal stresses to respect the angle between principal stresses and failure plane.*

[11] *In truth, it is the assumed deterministic laws adapt to soils, and not vice-versa.*

[12] *Symmetrical punching-in of bases occurs when columns (above ground, or piling) sufficiently long/rigid, force a vertical failure settlement and high stresses dwarfing into similarity the ever-present minor natural soil differences.*

[13] *This problem occurs, to different degrees, with different "engineered compromise solutions" in many a case: advances are not lacking. For 3D slope stability analysis refer, for instance, to Lam and Fredlund, 1993.*

[14] *This problem is as old as the "original sin" of Terzaghi's benificent "local failure" fudged solutions (analogously by Vesic's "rigidity index" for deep foundations). It occurs in all analyses heretofore, being recently tackled through finite elements that abut in limit analyses (e.g. Frydman and Burd, 1997).*

[15] *In the light of generalized acceptance of the reasonable unique effective stress envelope (as seen, cf. Fig.9 herein, through most of the stress range of London Clay data from the Ashford Common Shaft, Geotechnique 1965, 3), for this envelope to be used it need be duly accompanied by pore pressures (monitored in the foundation, and to be calculated in design problems). Both hopes are preposterous, at present and through a foreseeable future, because of the tremendous variation of stress values and rates along the failable pressure bulb. At the other extreme one would find the other strongly backed premise, of tests (and results) based on the stress-strain-time-path principle, with specimens tested under foreseeable complex anisotropic consolidated-undrained conditions; therein strengths are expressed as functions of consolidating stresses, and pore pressures disregarded in analyses, because assumed already embodied in the effect on the undrained specimen strengths. Not only are both alternates impossible to apply, but it can be seen clearly that short linearized equations would be varying along the real curved envelopes.*

[16] *As is expatiated in Section 6 regarding dam slope destabilizations, one of the basic principles of geotechnique is the importance of "stress-history", whereby for each provocation we have to analyse the pre-existing status, and thereon the change of condition caused.* $\psi = \sin^{-1}\left[\left(\varepsilon^{P1} + \varepsilon^{P2}\right)/\left(\varepsilon^{P1} - \varepsilon^{P2}\right)\right]$ *(Potts et all, 1987)*

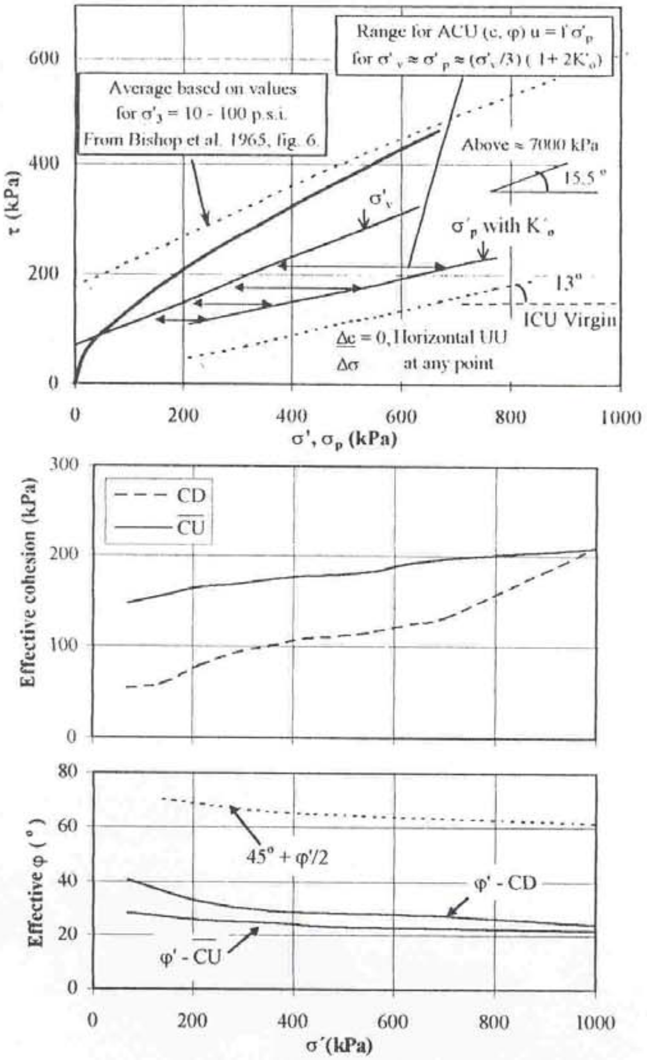


Fig. 9 – CHALLENGE: for thoroughly documented London Clay, and intended depth, dimensions, and Δ stresses, **choose weighted single applicable Mohr-Coulomb** equation for plasticity approach.

shapes disappears (as it never appears in slope destabilizations). And, assuming **rapid** (undrained) **loading**, as well as, **possibly rapid failure shearing**, the strength parameters will be the ACU and UU type. At the start of loading by the structure dead load, the stability is dominated by the different s_i (initial consolidated-undrained strengths point by point, with the respective γ'_z in the ground profile). Beyond a certain (partial) applied pressure the dominant strengths will be given by UU envelopes. And especially for live loads, typically considered rapid in structural engineering, the equilibria should be attained with UU strengths, with soil elements presumed mostly consolidated under the dead loading (already stabilized with dissipated pore pressures).

Even if we redirect all theoretical and computing energies in this direction, strongly recommended as the most logical and fertile, it will take very many D.Sc. and M.Sc. theses to normalize the useful bearing capacity results and charts. The **necessary conversion** is what will take more effort, debate and time[17]. Allow me to repeat, the present status is a sham and shambles for foundation designers.

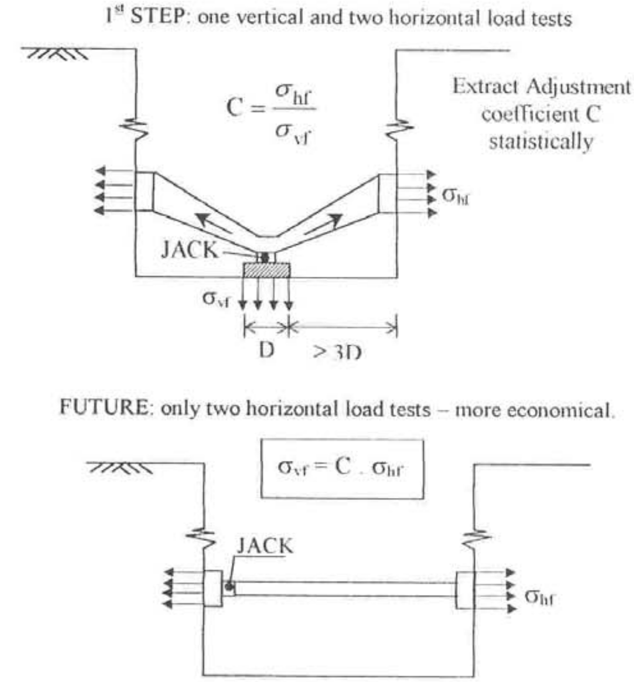


Fig. 10 – Indication of scheme for simultaneous horizontal and vertical load testing promoting great savings.

4.2 Settlements of shallow foundations on sands, predictions, observations, predictability and tolerable limits.

The classical first-approximation teachings had been that settlements were of concern in clays, and not sands. But serious publications progressively revert the emphasis, and, in parallel with the frustration on bearing capacity (cf. 4.1) one queries the status on this problem, since intolerable and failure settlements are but a sequence. The comments that follow were suggested by the provocative Prediction Symposium on Five Spread Footings on Sand (Eds. J.L. Briaud and R.M. Gibbens, 1994), rightly prefaced by emphasizing the important savings achievable by any concerted effort towards improvement of the bases for designs of footing foundations. One must stress that the greatest difficulties for this worthy aim lie in that shallow foundations have absolutely no sponsor, geotechnical or of vested interest: also, designs are mostly overconservative, codified, and would hardly envisage any monitoring.

Conditions could not be any nearer to absolutely ideal: pure uniform medium-to-fine, above W.L., quite homogeneous. The array of test data, both conventional, and complemented by updated special tests of personal preferences, is completely unrivalled worldwide through foundation history. There were 31 academic/consultant predictions, from 9 countries, using 22 quoted authoritatively recommended methods. Yet the results summarized by the authors (as stated, still subject to worthwhile discussions) are near shocking: (1) one prediction led to FS<1 (that is, 6.5‰ of analogous foundations, immeasurably less documented for design, would suffer failure!); (2) the average stated prediction FS of 5,4 (as detailed in Fig.11) confirms the uneconomical conservatism; (3) only one predictor essentially clung to the classical postulates based on theoretical parameters. The latter two conclusions reflect present realities, of prudence from widespread recognition of collective ignorance, and of professionals naturally fleeing from sampling/ testing of sand strata for theoretical parameters. But surely we could affirm that failure

[17] *The profession sorely needs field testing, in optimized conditions, for stimulus and ratifying. I offer the suggestion that nowadays the best known and accessible "general soils" are to be found in the very many millions of homogeneously* ***compacted cores of dams.*** *Fig. 10 suggests a schematic set-up repeatedly recommended (hitherto unsuccessfully, because dams and shallow foundations, are not related, in the broken-up Civil Engineering household) for concurrent tests horizontal (much cheaper) and vertical. Upon establishing suitable statistical correlations, horizontal tests at prospective footing horizons will multiply greatly, enriching this sector of the profession, mendicant, but of greatest responsibility and cost/benefit prospects.*

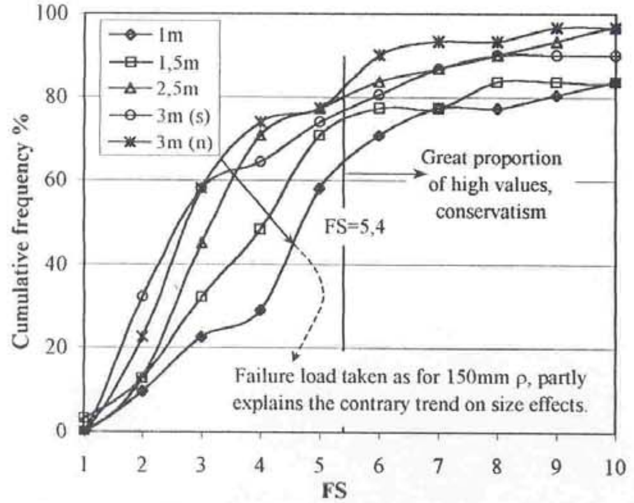


Fig. 11 – Predictors' FS (Table 9, Briaud and Gibbens 1994).

of footing foundations on a 35° sand would afflict less than 1:1000 cases, and not 6.5 times so. Problems of footing foundations have been left unattended, but not as badly as reflected by this challenge and results. Some hindrances in the case arose from conduct and interpretations constrained by code-dicta and hypotheses, in lieu of unfettered quest on geotechnical behavior.

The confusions, however well-intended and prestigiously backed, are so many that it is difficult and risky to sort out the wheat from the chaff. But the appalling scatter on most prescriptions imposes courage or foolhardiness. And Donald Taylor's (1948, p.584) beckoning regarding "Analysis of Size Effects for the Case of Cohesionless Soil Surface Footings Loaded to a Given Fraction of the Ultimate Loading Intensity" with the zestful assertion "There are no better examples of true soil mechanics than those offered by these studies, and there is nothing more important to the soil mechanics specialist than the intuition for soil action which studies of this type help to furnish" allows no shirking. Fig.12 on prediction frequencies on pressures is revealing.

Allow me to begin by discarding any reference to Relative Densities Rd, a parameter justly hated (by me) as a necessary evil, accepted in some conditions, but subject to too much erraticity, and especially irrelevant for microdeformations, precompressions, etc. As a regretted forceful second step, permit me to postulate harms from too much sophisticated data and religious faith therein. No professionally designed foundation on a "homogeneous" stratum would differentiate settlements of two 3x3m footings 8.5m apart c-to-c, as per different numerical test results. Fig.13 with 60% predictors' computations to differences less than 10kPa indicates filigrees, of loss of sense of purpose and proportion. An engineering prediction within ± 20% is "perfection" beyond most other links of the chain, humility must temper egocentric faith unbounded, and if structures are so specious/delicate as to mind such differences, the problem and solution pertains to them. Another important point regarding **differentiation of precompressed vs. normally-consolidated sands** will follow brief comments on the **five-footing challenge.**

The challenged predictions, load-test procedures, and respective phenomenologically interpreted data suffered from the following constraints, understood to have arisen from design codifications: (1) a 150mm settlement was used as **nominal failure** irrespective of footing dimension; (2) the 1-min. settlement was assumed to pertain to a phenomenological difference (pseudo-elastic?) from the 30-min. incremental settlements: (3) these short-term incremental settlements were assumed as constituting "the start of long-term creep" irrespective of the **footing-size influence on spreading of the shear strains**; (4) the cumulative effects of 30min. compressions/shears, and of load-unload cycles, could not have been qualitatively overlooked.

An attempt at recapitulation may result elucidative: (a) Terzaghi-Peck, 1948, estimated differential settlements for different footings of given buildings. Design routines use a **constant allowable pressure**, independent of FS on σult, guarding against failure risk on minimum width, and against over-settling on maximum width. Commonly building loads do not vary more than from 0.5 to 2 times the average, so dimensions hardly vary more than from 0.6 to 1.5 times the average. The small plate (1 ft) load test was the crux.

(b) Bjerrum-Eggestad, 1963, specifically mentioned again the basis of a **constant applied pressure** for their indications (Fig.14). The extrapolation for ratios of 100 times or more is what raises questions, principally because the dominant phenomena (compression and/or shear

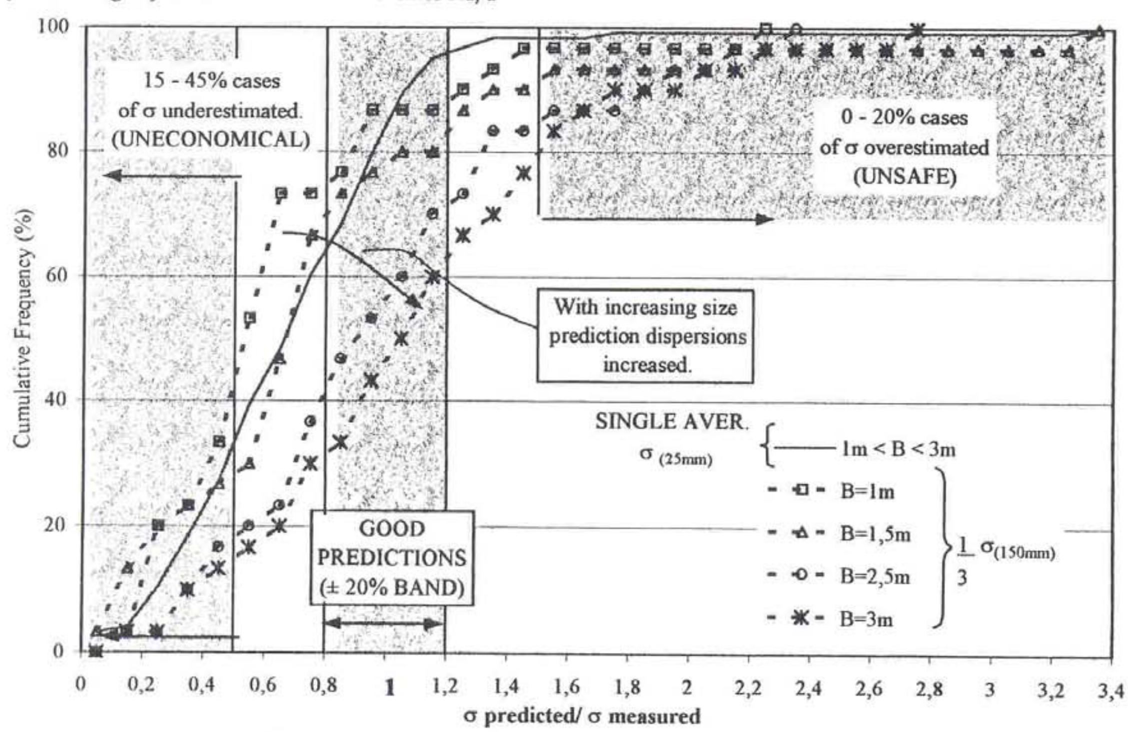


Fig. 12 – Details on predictions and trends on pressures.

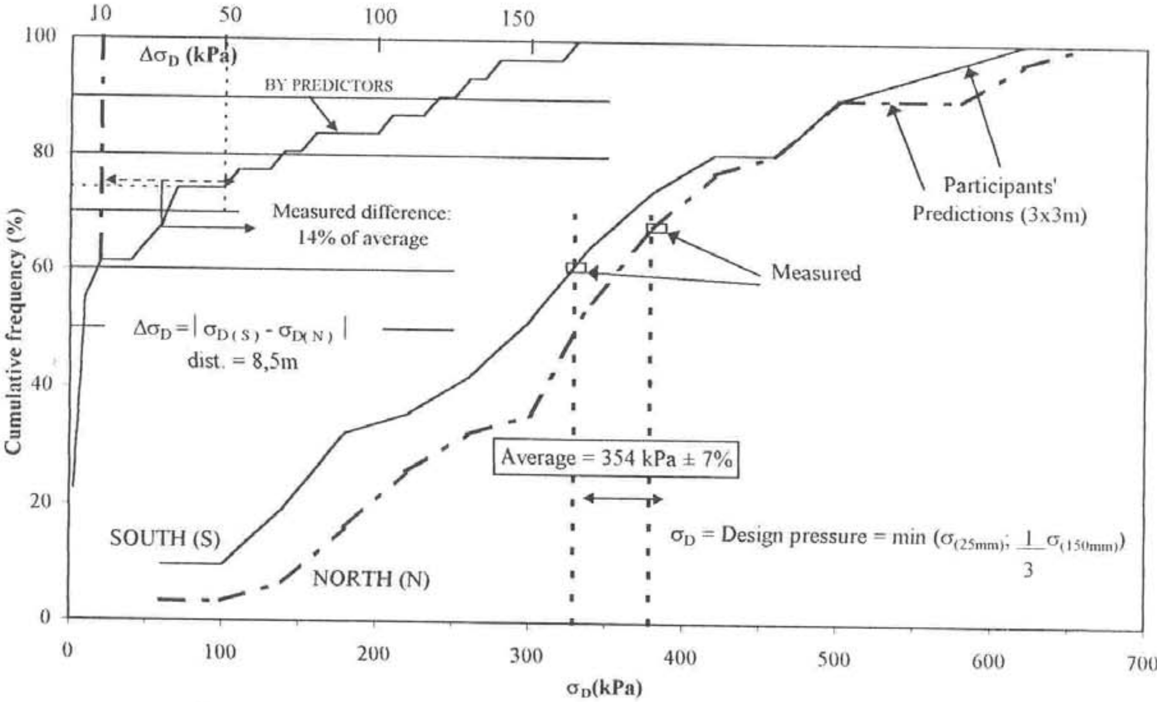


Fig. 13 – Similar footing behaviors, vs. widely different specious predictions.

distortions[18]) change greatly with increased areas under lower pressures, and one wonders what point is used for the settlement mentioned, in rigid vs. flexible bases. The principal points to consider are: (i) the extreme variations of FS with size (shown in Fig.14, using Terzaghi's $N\gamma$ values for assumed φ=25°, loose, and 44°, very dense); (ii) corresponding great variations of secant **"nominal modulus"** (or proportional **"subgrade compressibility"**); (iii) the importance of precompressions[18] in sands, because of near-zero rebound, (Fig.17 insert).

(c) In building practice load-test pressures are limited to a given settlement considered unacceptable (to the superstructure). Understandably no load testing seeks higher pressures and costs (reactive load, and time) than usable in design as per this limit-settlement. Thereby the **geotechnical understanding** of the sand behavior under wider footings was impaired, leaving the phenomena called "immediate settlement" and "failure" nebulously defined.

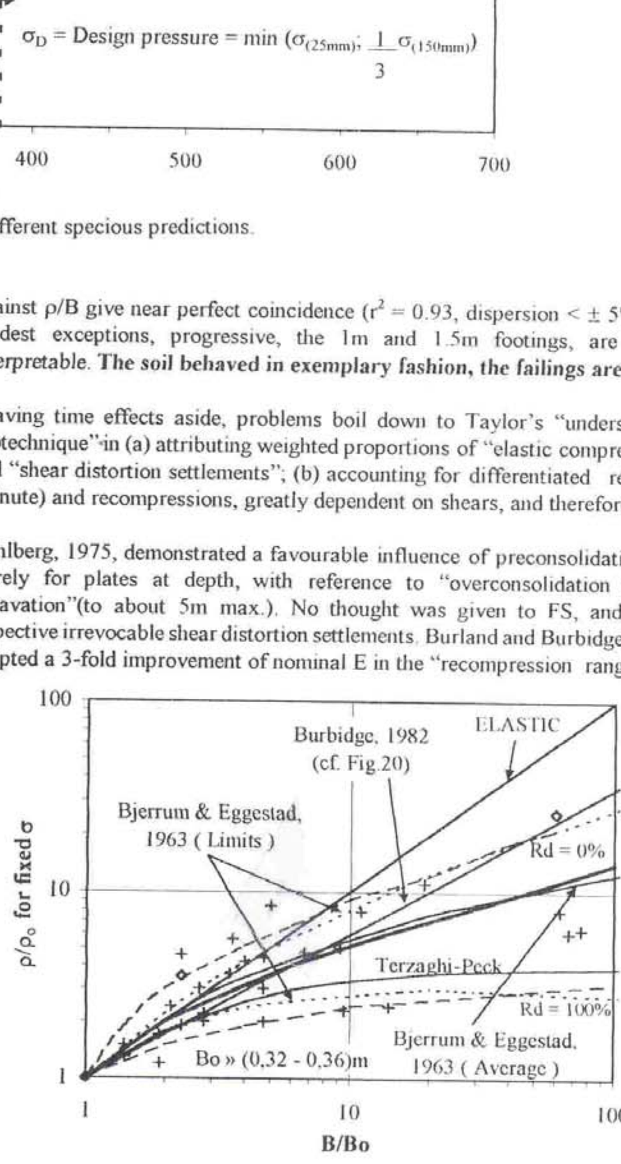
Priority belongs to fact, knowledge, wisdom: practices and codes follow, for conveniences, transient and varying. Barring my ignorance, one has to return to the classic Plantema-pile, 1948, for thorough knowledge: 141 load tests, across 14m of sands varying in resistances across 3 times, all tests pushed to 0,25 D settlements of D = 42.6 cm (favourably compared with 1 ft plates). Firstly we derived the cumulative frequency distributions of nominal Es at different positions on the Plantema exponential curve, with these positions defined by their FSs: and conclude that the frequencies remain very similar whether "final failure" is taken at 0.23B or at 0.1B (and the intermediate 0.2B used by Arnold, 1980). The ratios E/E_{max} of secant nominal Es at different FS and E_{max} at FS ≥ 10 (thereafter essentially constant, compatible with commonly adopted linearity) are tabulated as:

FS	1.2	1.3	1.5	3	5	10	30
E/E_{max} %	14	34	62	82	92	97	≈100

The 5 load-test curves calculated for a Plantema normalized regression of σ against ρ/B give near perfect coincidence (r^2 = 0.93, dispersion < ± 5%). The modest exceptions, progressive, the 1m and 1.5m footings, are clearly *interpretable*. **The soil behaved in exemplary fashion, the failings are ours !**

Leaving time effects aside, problems boil down to Taylor's "understanding geotechnique" in (a) attributing weighted proportions of "elastic compressions" and "shear distortion settlements"; (b) accounting for differentiated rebounds (minute) and recompressions, greatly dependent on shears, and therefore FSs.

Dahlberg, 1975, demonstrated a favourable influence of preconsolidation, but merely for plates at depth, with reference to "overconsolidation due to excavation"(to about 5m max.). No thought was given to FS, and to the respective irrevocable shear distortion settlements. Burland and Burbidge, 1985, adopted a 3-fold improvement of nominal E in the "recompression range" (for



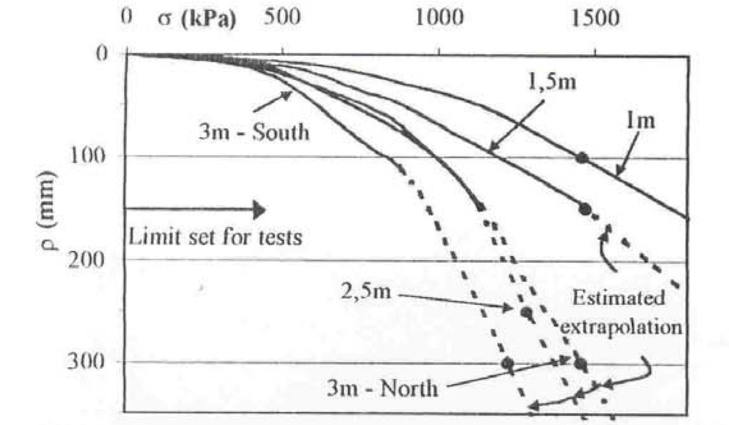
USING TERZAGHI'S $N\gamma$ VARIATION (B/B_o=100)

Rd	ϕ'	$N\gamma$	FS_{Bo}	$FS_{B/Bo}$	E/E_o *
0%	25°	10	1,5	10	1,57
100%	44°	260	1,5	260	1,68

* from Plantema Pile

Fig. 14 – Size effects on FSs and nominal Es.

[18] Precompression benefits are much smaller when caused merely by "infinite area" overburden, than when resulting from shear distortions under small loaded area. The point merits attention, cf. Fig.17. I chanced to stress (1969,1971) that strength and indeformability go roughly together, but there was (is yet?) no in-situ testing capable of distinguishing, in microstrain range, between pre-and virgin-compressions, and between pseudo-elastic and plastifying conditions.



B (m)	Regression type	R^2	ρ (mm)		σ_{ult}(kPa)
1	Polynomial 2^{nd} deg.	0,99	100	For 0,1B	1463 (measured)
1,5	Polynomial 2^{nd} deg.	0,98	150		1475
2,5	Potential	0,99	250		1287
3-North	Polynomial 2^{nd} deg.	0,99	300		1461
3-South	Potential	0,99	300		1227

Fig.15-Regressions for extrapolations of curves to "geotechnical failure, 10%B".

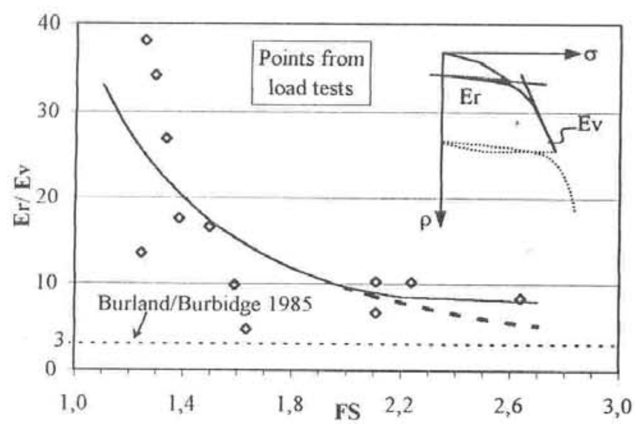


Fig.16 – Ratios of recompression to virgin nominal moduli.

FS>3 thus associated with dominant pseudo-elastic compression). Using first-order approximations taken from the load-unload-reload cycles of the 5 load tests one confirms (disregarding yet the inevitable time effects) the geotechnically expected (Fig.16) for Er/Ev ratio of recompression to incremental-virgin nominal moduli (a) vary greatly with FS, (b) are much higher with lower FSs that caused greater irreversible shear accommodations, (c) were higher (e.g. 8-10) for FS ≥ 5 than the prudently adopted 3.

The data from Burbidge 1982 are summarized in Fig.17 with regressions for the influence of size. It is not a graph for constant σ, as per Bjerrum-Eggestad, but for a **constant ratio ρ/σ, adopted applied pressure for "accepted/ observed" settlement**. The sand density groupings give a reasonable trend of increasing settlements (roughly 20 to 30 times at the limits) in moving from very dense, dense, medium, loose, to very loose. Thus the 20-30 ratio applies either for pressures at same settlement, or for inverse of settlement at same pressure, in very dense to very loose sands. It is of interest to ponder on the close parallelism for different densities. Apparently all FSs (unknown) are sufficiently high for nominal pseudo-elastic behaviors (FS ≥ 5).

Ahtchi-Ali and Santamaria, 1994, incorporated "all" previous data published. The resulting poor general regression (Fig.18) invited inclusion of the great FS influence. The frequencies of FS values (presumed at stake) were taken by relation to a first-approximation hypothesis of applicability of the σ_{ult} at 0.1B

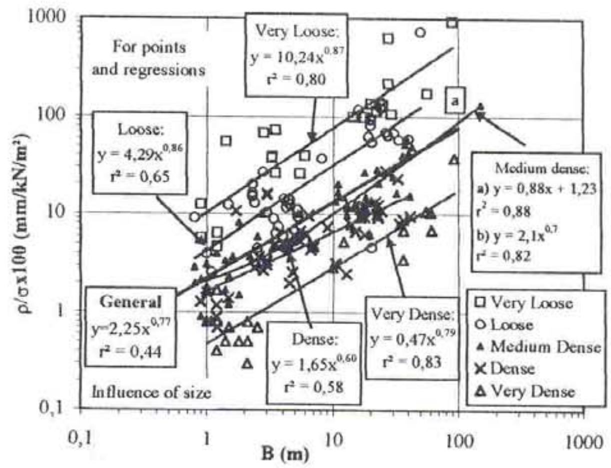


Fig. 17 – Burbidge 1982 (Fig.4.6) reanalysed statistically, general (poor) and grouped (modest).

of the Plantema equation. The data, thus separated by FS groupings, give suggestive separate regressions. Incidentally, even the variations of r^2 values seem suggestive, being lower at the extremes (a) when closer to failure, (b) when incorporating a broader range of high FSs.

A final passing reference must be made to the specious questions of long-term creep settlements, broached both by Burbidge 1982 and the Briaud-Gibbens 1994 data and challenge. The log-log graph offered by Burbidge (his Fig.4.12) is not reproduced herein because it embraces too many variables and results too broadly scattered, although tentatively subdivided into three bands of comparative densities. The points purport to cover from a few days to about 15 years. The settlement rates are given in mm/year x pressure, and magnitudes vary from about 20cm/year for $1t/m^2$ to about 10^{-5} times that rate. The general regression (Fig.19), fraught with implicit queries, would suggest a creep settlement of about 9mm in 30 years under constant pressure of 0.1 t/m^2 ($1kN/m^2$); a behavior extrapolated to 9cm for 1 t/m^2, which seems highly unrealistic. With typical much smaller rates the behavior becomes an inexorable part of life, especially if all footings are **under equal σ and not differentiated**

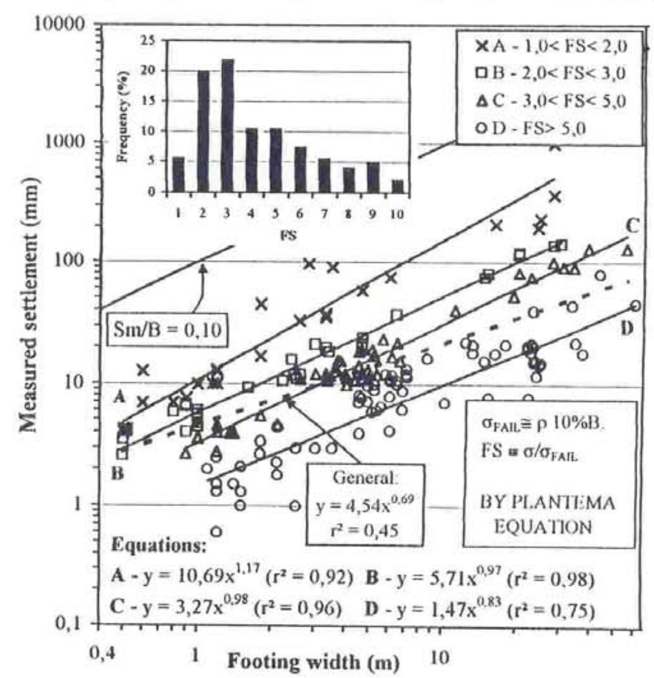


Fig. 18 – Reanalysis of "all previous data" on size effects (Ahtchi-Ali/ Santamaria, 1994) including FS estimates.

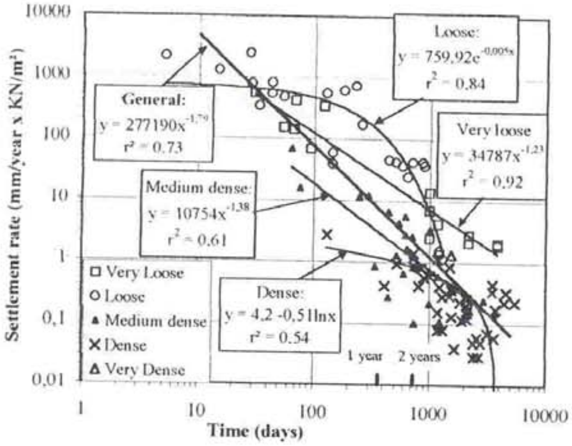


Fig.19 – Burbidge 1982 data on "creep" settlement rates reanalysed.

by size and FS, thus presumably behaving much alike. Most construction materials suffer no damage from rates of deformation (total, and smaller differential) easily accommodated by their own strain-stress redistributions. Accepting the data unquestioned and unimprovable, separate regressions have been derived for different densities: trends of likely differentiations begin to sprout. The instinctive conceptual criticism is suggested by the 5 footing load-tests' nominal (codified?) acceptance that the 1-30 min settlements already belong to the "secular creep" phenomena. One cannot extract conclusions to precisions of grams with measurements made in kgs. Precision measurements and graphs of fractions of mms. across fractions of mins., will begin to permit differentiation of **"non-instantaneous body deformations"** and ulterior microstrains from grain-structure rearrangements. The path from crude, codified, to precise, researched, cannot be reversed.

5. URBAN TUNNELING, AND CONSEQUENT SETTLEMENTS.

To geotechnicians associated with tunneling, there will be nothing novel in the following summary of the problems and symptoms attached to design and construction decisions for tunnels. The novelties appear in profusion along the line of inventive and developmental procedures and equipment, achieving soil reinforcements and face confinements that tend towards controlling settlements to near zero. It is a conscious, valid, engineering strategy: **when a behavior eludes reasonable predictability, aim at zeroing it**.

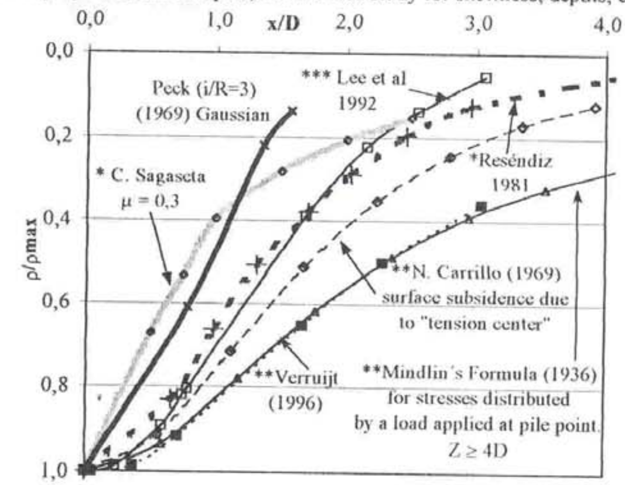
Indeed, we could therefore declare "sweet are the uses of adversity". But humble allegiance to the fundamentals of our art (strutted on science) forces decrying an original adversity seriously felt. So it was, roughly, with the starting mental experimentations on total vs. effective stresses, importance of uplifts to gravity dams, and so on. Not only by 30 years elapsed in frustration to professional practice, but also under present orientations of sterility disguised, the perspectives for a radical promising revision seem small and decreasing.

The good effects of remarkable developments that would dispense with geotechnique will not be mentioned. At least three major problems regarding present trends need emphasis. Firstly the investments, and costs, increase greatly, and choices between different options represent significant differences in costs and logistics, generally crucial to successful bidding, and/or to averting localized "change-of-conditions" accidents of very disproportionate consequences. Secondly, a net subconscious result is the problem's shift heavily onto the contractor's need to choose and decide, with no direct support from computable cause-effect behaviors. Thirdly, the principles of adjusting by help of systematic monitoring ("observational method"?) have degenerated visibly, through lack of cause-effect indications, while along the tunnel length external conditions are far from constant, or readily adjustable via theories.

In a single terse declaration of intentional impact, however respectful of intents and past, I propose: **it was very unfortunate** to geotechnique of soft ground tunneling **that the settlement trough should have been defined as a Gaussian probability curve**. It should be recanted authoritatively and in unison. The similarity with other theorizable curves is mathematically and visibly so obvious (Fig.20a) that it distracts from essential principles and purposes. Its repeated success in **retro-fitting to case-history** monitored data, only permitted continuing the flogging of the dead horse. Engineering is for predicting, desiring, designing, and deciding, through use of geomechanical parameters and cause-effect relationships, available, or pin-pointed as lacking (therefore to be sought and developed). Fig.20b shows how the surface trough (much more frequently monitored) has been nominally transferred into the ground (under the clearly unlikely constant volume hypothesis).

Herewith follows a personal interpretative (and apologetic) summary of my principal points in support of the above earnest appeal.

Litwiniszyn's (1964) conclusion on the probability trough for the equal spheres (Fig.21) seems inexorable. Since nothing but rigid geometries was involved (highly idealized for rock blocks), if interest persisted[19], other shapes, contact-properties, anisotropies, etc. merited study for skewness, depths, etc.



* Numerical elasto-plastic, ** Analytical elastic, *** Analytical elasto-plastic

Fig.20a - Clear analogies (of elastic, elasto-plastic, responses) to Gaussian curve, sterile.

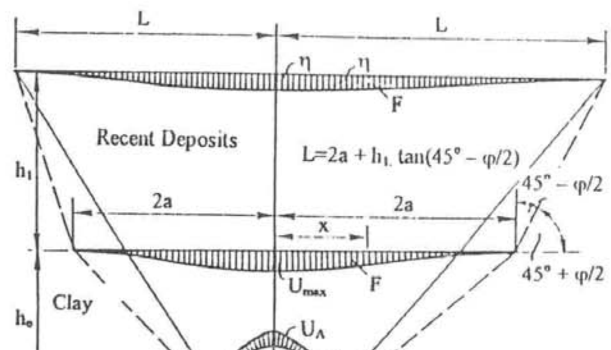


Fig.20b - Example, typical idealized geometric distribution and downward transfer of surface subsidence curve, $\Delta v = 0$ (transverse).

Fig.20 - Surface settlement trough, transverse: hypothesis, inferences.

[19] *It appears that the approach was not extended to umpteen less idealized conditions visualizable, even geometric-probabilistically.*

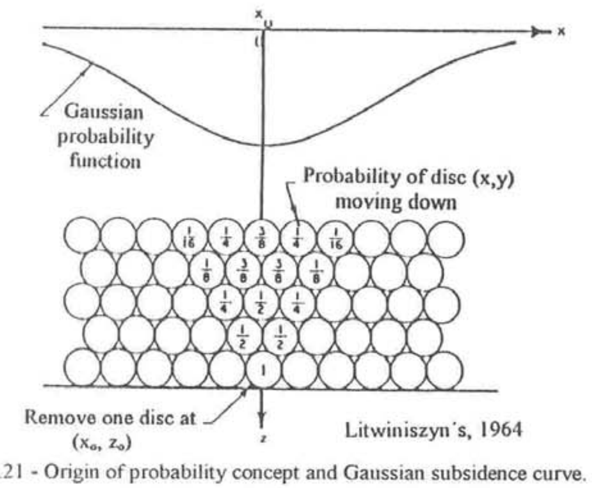


Fig.21 - Origin of probability concept and Gaussian subsidence curve.

Unfortunate orientations are a part of life, most often arising by historic contingencies, and systematic oblivion of provisos and precautionary advice. B.Schmidt (1969) confessed "In many instances, elastic treatments of the problem might be more appropriate on theoretical grounds, but in view of the complexity of the elastic solutions, the simpler stochastic approach is preferable" and Peck (State-of-the-Art, Mexico 1969), who set the important innovative milestone on the problem, emphasizes "the settlement trough ... can usually be represented within reasonable limits by the error function or normal probability curve. **Although the use of this curve has no theoretical justification**, it provides at least a temporary expedient for estimating the **settlements to be expected at varying distances laterally from the center line of a tunnel"**. The historic contingencies were of Schmidt transplanting from Europe (and Rock Mechanics) the stochastic hypothesis to Illinois, where Peck, at the subject's infancy, induced back-analysing his files of data, seemingly overlooking the lacking keys to geomechanical parameters, elastic or strength-deformability. Resultant trends were (a) successful back-analysing, though with dwarfed aim at first-order effective predicting, and strange oblivion of stand-up time, FSs on destabilizing potentialities, construction techniques and logistics, shape, depth/diameter ratios, heterogeneous profiles, etc...; (b) no mention of longitudinal settlements; (c) no hint of displacements beside settlements; (d) strange fixation on the trough's shape and point of inflection, a side-issue; (e) inability to attempt incorporating the conceptually separate components of elastic adjustments and of ulterior partial plastifications; (f) etc.

Success is addictive irrespective of deviated vector, and prestige is seductive. The near-totality of job-cases possess and lack the selfsame data as available to Schmidt-Peck. No wonder, therefore, that the validation of the pseudo-probabilistic curve proliferated, servile and to no service. Meanwhile through time and efforts on hundreds of kilometers of tunnels, the bottom of the trough has been progressively reduced in common successful cases, through design-construction expedients quite empirically oriented. The unreliable empiricism is spitefully reminded by occasional dramatic collapses.

A present unbiassed retrospective analysis forces note of at least three important points on concepts and recognized practices in collateral engineering solutions (always insufficiently right, but satisfactory for the intended purposes):

a) first, there existed, at the time, quite a number of analogous behaviors (and mathematical solutions thereof) that exhibited the same shape of curve in idealized homogeneous elastic isotropic media, for which nominal geomechanical parameters could be adjusted (Fig.20a);
b) second, it is infallibly accepted that **"adjustment factors"[20] represent an absolute necessity in engineering practice,** even in situations considered "perfectly" modelled and equated. An extreme example to be recalled involved even transforming problems of excessive deformations into reduced strength parameters,[21] for the footing bearing capacity "local shear" condition.
c) third, in the intervening period many publications arose (such as, O'Reilly & New - 1982; Durand et al - 1994; Soliman & Darrag - 1994; Swoboda & Moussa - 1994; Baki, et al - 1996; Kotake & Taji - 1998; Malato et al 1998, Marques et al - 1998; Targas, et al - 1998) developing/presenting finite element solutions, with adopted geomechanical parameters, for study and back-analysis of given specific cases. The phenomena, geomechanical, not probabilistic, are unquestionable: the obstacle and lack is yet of generalized equations/charts for assessing changes from section to section of advancing tunnels.

Meanwhile, the dominant trend of a long list of authoritative publications continued to back the unfortunate association with the Gaussian shape. Among that long list I take the liberty to transcribe only a few (Sáenz ,J.T. - 1971; Cording, E.J. and Hansmire, W.H. - 1975; Attewell, P.B. – 1982; Fujita, K. - 1982; Fang, Y.S. et al - 1994) from different countries and justly respected schools of professional orientation. Thus much work was spent, in parallel, on two irreconcilable lines, Gaussian random retrofitting, and finite element special-case analyses using geomechanical concepts/parameters.

It is undeniable that around 1966-9 the development of geomechanical generalized solutions (either analytical or finite-element-numerical) was no inviting task. Moreover, the cruder tunneling techniques that then prevailed in case-records, led to much greater proportions of plastification ("loss of ground") effects, thwarting direct "elastic solutions".

While advocating that the dead-end road be abandoned, it is comforting to signal that academic-professional redirecting has been progressing. In fact, at the Mexico 1969 Conference, the discussion by N.R.Cuevas (3, 365-7) proposed another stochastic (sorry?) approach with a viscoelastic continuum including time effects. A good milestone came from Sagaseta (1973), subsequently furthered, offering "generalized, generic solutions" by finite element analyses, starting under rather idealized conditions. Now since much is to be done to give the profession the bases for rapid responsible successive comparative decisions, preferably via charts of normalized complex lumped coefficients[22] tied to the intervening geomechanical parameters, some comments should help bridge the wide gap between the sterile Gaussian hypothesis and "full (continually completable)" finite element case-solutions:
1- Longitudinal settlements **ahead of the excavation face** (Fig.22) cannot be neglected, as regards firstly worsening in-situ conditions and parameters;
2- In fact, depending on the destabilizing drop of FS of excavated face (incorporating stand-up time, reinforcements, drainages, etc..) the most frequent failure, most often skylighting as craters of widely different (justifiable) shapes, the degree of deterioration of geomechanical in-situ

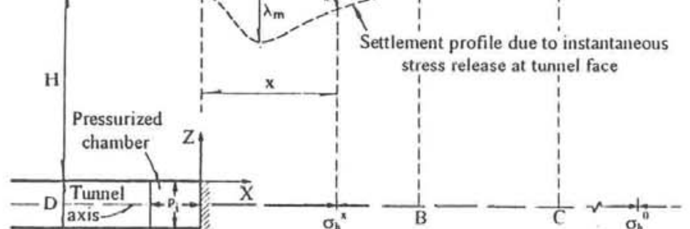


Fig.22 - Example of derivation of frontal longitudinal settlement trough.

[20] *These are so inevitable that they even go unperceived: examples include test-model-prototype, and also calculated vs. real, correlation adjustments.*

[21] *Although clearly difficult to digest, this contorted solution stood for five decades, with no overt rejection.*

[22] *It is noted that the profession is favorably served (and reciprocally interacts) by such charts of comparative indicators. Obvious examples are: for slope stability, Taylor's (1948) charts (b) for footing bearing capacities, Terzaghi's (1943) factors N_c, N_q, N_γ and charts (c) for earth pressures on retaining walls, the Caquot-Kerisel tables; and so on. This prevails indispensable, for starting approximations, in all cases facing frequent/great variations, with causative factors dominating (tunnels being a prime example).*

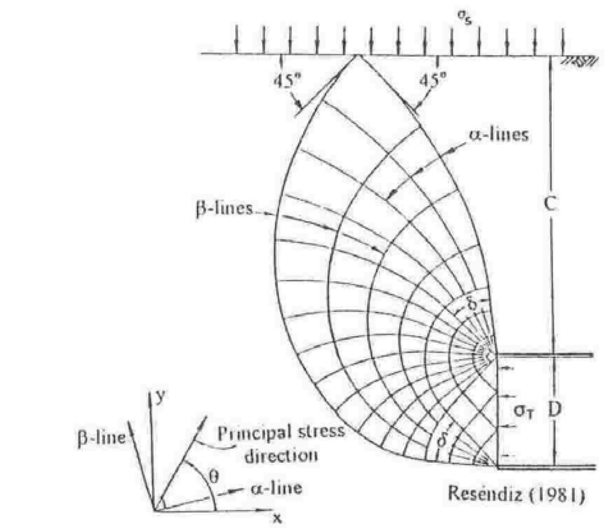


Fig.23a – Extreme example by Plasticity Theory Lines of characteristics.

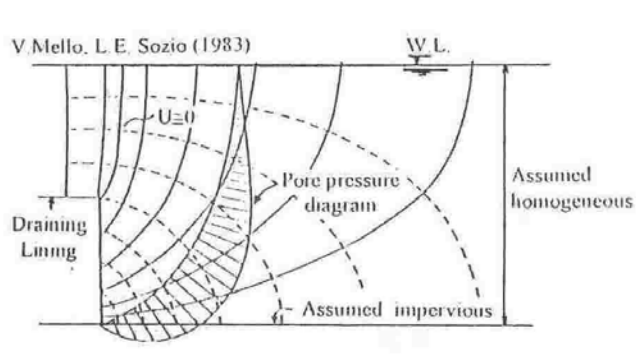


Fig.23b – Example of Limit Equilibrium viable attempts.

Fig.23 – Frontal destabilizations and proposed analyses.

parameters might be estimable as insinuated in Figs.24, 25).

3- Irrespective of criticisms on limit equilibrium solutions (3D affecting greater widths than tunnel diameter, therefore subsequently affecting lateral 2D deformations and stability), I submit that it is totally indefensible to employ pseudo-erudite **Plasticity Theory** solutions (Fig.23a) in comparison with the **Limit Equilibrium Analyses,** of great capacity to include widest imaginable flexibilities of intervening parameters, and irrefutably **valid for comparisons**. Fig.23b reproduces one of the early simplified examples published, de Mello 1979, 1983.

4- The adjustment factors for standup time and progressive deteriorations of nominal elastic moduli, depending on FS (cf. Fig.16) and time-delay, might be derivable via anchor pullout testing within tunnel excavations. Lacking such data de Mello and Sozio, 1983, resorted to analogy to plate load tests, inverted, as in Fig.24, schematic. Progressive deterioration of moduli can be applied judiciously in numerical computations.

5- Mention need not be made of the many scattered efforts, dead-end though intense, of progressively completing and purifying Plasticity Theory solutions. What stands clear is that even elasto-plastic solutions diligently **applied to the entire medium** fail to reflect the deepened trough, Gaussian-like (Fig.20a).

6- An advance in the finite element solutions came with the recognition of the so-called "loss of ground volume", transformed into a "gap" encircling the nominal geometric excavation (visualisable in fractured rock, blasted, besides the authors' gap around shields). Such solutions (e.g Lee et al., 1992), one of which is represented in Fig.20a, obviously lead to enthusiastic receptivities as representing job realities more closely. Thus they also to revive support for the Gaussian trend because of the improved similarity with deepened troughs.

7- The crux of plausible reasoning and solutions (cf. 4.1, 4.2) lies in the **enormous difference of nominal moduli in function of FS and TIME**. Applying elasto-plasticity, modest moduli changes, etc...to the continuum, or to zones (e.g. annular) poorly assumed deteriorated by intuition, does not lead to logical and/or monitored trends. The volumes suffering near-failure must be judiciously diagnosed, analysed by Limit Equilibria FSs, and ulterior time effects (Fig.25). Fig.26 shows that the deepened trough is thereby achieved, as monitored, proving the association of Gaussian troughs to dominance of plastified-zone reduction of moduli.

It is, indeed, regrettable to play the dog in the manger: I recommend that such initiatives be abandoned, unless and until the "ground loss" volume begins to be associated with destabilization FSs (both front, and lateral), with preference for sequential limit equilibrium analyses.

In closing comments regarding urban tunneling conditioned by tolerable limits of total and differential settlements, one final crucial problem must be emphasized, kept under the carpet in subconscious hypocrisy. Assuming that destabilization FSs are not allowed to fall too low, ultimate failure being thus averted, every item of design, construction, and economy hangs on the predictability of (FS, time) deterioration of parameters, and consequent movements. Settlements, convergences, displacements are industrially and diligently monitored. What criteria and suggested prescriptions might have been offered regarding warning limits for these observations? And, moving on to the feared effects on buildings (the ultimate aim of tunneling improvements), is geotechnique documented regarding limits for these cases, compared with the much-quoted published tolerable limits for settlements of foundations (e.g. Skempton and McDonald 1956, Bjerrum 1963, etc.)?

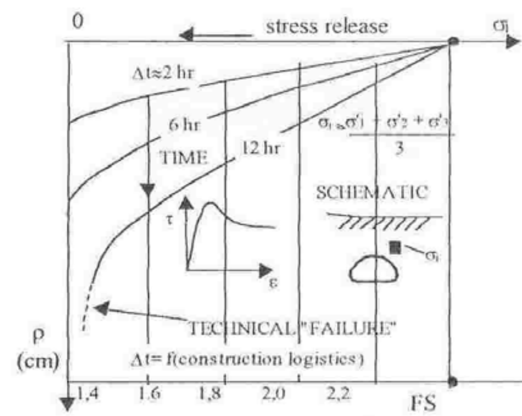


Fig.24 – Conditions surrounding tunnel, stress-release destabilization, "stand-up-time rheologies" pending investigations.

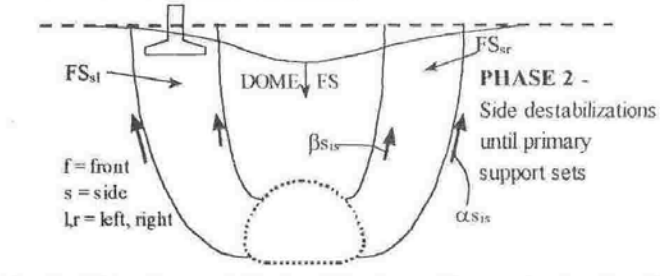


Fig.25 – Combining front and side destabilizations with stage-deterioration of strength.

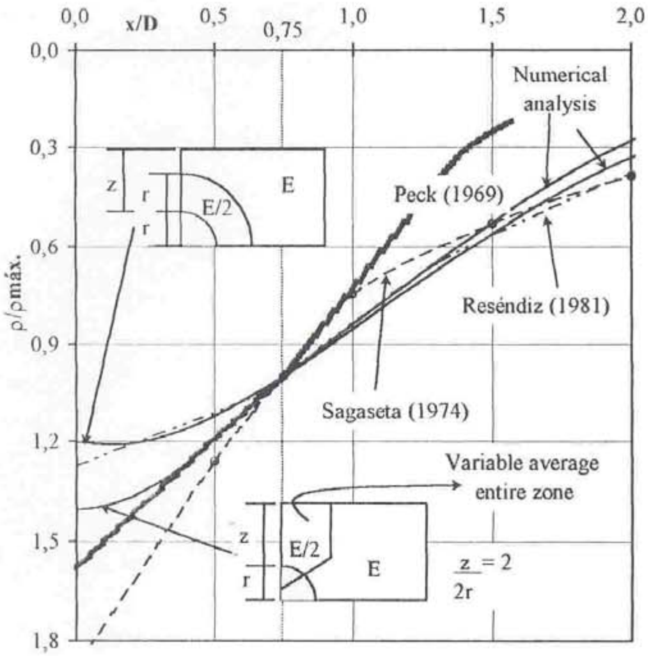


Fig.26 – Analyses (FLAC, etc.) with plastifications differently applied a) to general continuum; b) annular. Fail to match deepened trough; c) in destabilized zones (Fig.25) match Gaussian.

For observations within tunnels, references are almost exclusively to percentages of the diameter. Of course, a bigger excavation de-stresses and destabilizes a bigger external mass. But, is it not illogical to make no reference to the external mass and pressures, that are the causative factors? Is it not indispensable to refer to geomechanics of the surrounding mass and loads, for evaluating with some logic? This inescapable fact must lead to rapid recanting of over-simplified statements, and to the needed pressing for non-Gaussian non-Plasticity Theory Idealizations, but geomechanical solutions, generalizedly oriented and normalized.

Even for buildings, available for observation by the thousands (each one at every few floors), the well-intended gratefully received **prescriptions offered as a start** should be recanted aloud. Many questionings and provisos have been published by many. My own (1969) ventured the thoughts that:
(a) calculated and observed settlements are ipso facto different, if the walls and structures crack (demonstrating stress redistributions of semi-rigidity); and cracking is reported from observations, whereas design decisions to avoid cracking are based on pre-construction calculations. What adjustment factors apply? This gives some tolerance, possibly very significant in highrise buildings of big height/width ratios, in using calculated settlements for deciding on tolerable pre-damage differentials.
(b) on the other hand, since most building settlements are monitored from the subsurface foundation, it takes time to build-up floor by floor, and also, for an instantaneous building load to develop its settlement; and since no floor can suffer from settlements developed prior to its materialization, and, effectively, the n^{th} floor becomes the "foundation" to the $(n + 1) \rightarrow (n + m)$ floors, the **cracking in any of these upper floors derives from much smaller differential settlements than the building's totals** (de Mello, 1994)... all of this reduces, possibly greatly, the effective tolerances of the building's elements to real differential settlements. The profession knows nothing about these realities (in large part abetted by the structural engineers who know everything, while we question with incredulous curiosity), although the ease and benefit/cost ratio point to the wealth of information available for collection and analysis, by settlement-recorders on numerous floors from innumerable buildings.
(c) finally, the limits allowable to tunneling activities are far, far tighter, both because buildings suffer under already deteriorated, unknown-unknowable aged conditions, and because movements are rapid. Incidentally, quite understandably horizontal deformations are more damaging to esthetic cracking, although of lesser consequence to structural stability, than cracking caused by differential settlements, redistributing loads and stresses.

Should I be corrected, and recant on these postulates extended over 30 years, despite the personal respect and esteem for the earlier writings? Will gladly so, if so suggestable; priority resides in the revival of the essence of geotechnique and its benefits to civil society. Elsewise, let it be proclaimed that a decided recanting on those prior tenets is sorely needed, to unfetter renewed potentialities.

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

At risk of tiring, beyond breaking 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, with zest and 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 apllies $\Delta\sigma_v$, firstly leading to isotropy, and only beyond a certain overburden height giving the $(\sigma_v - \sigma_h)$ deviator the tendency to destabilize. Construction period pore pressures Δu_c change from initial suction to positive values, **eventually destabilizing**. Present conventional $\overline{UU}$ 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, ASCE GT12, pp.1511-24). Failed slopes show trapezoidal blocks.

7. Fundamental queries on Slope Destabilization Analyses.

7.1. It is accepted unquestionably that effective stresses determine behaviors and stability. However, I earnestly 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 $(\Delta\sigma, \Delta\tau) \rightarrow \Delta FS$ being conducted under **total stress increments and ACU strength increments** (for prudence, engineering).

7.2. In back-analysed prototype I have further strongly questioned the hypothesis of equivalence, FS ≡ 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**.

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

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

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 pressures changes are also treated via ΔFS. With modest unsaturations, the continuous pores allow the **potential change** from one flownet to another; for conservatism, this tendency may be assumed 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.

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.

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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