REVISITATIONS ON SAMPLE FOUNDATION DESIGNS

REAPRECIAÇÕES SOBRE EXEMPLOS DE PROJETOS DE FUNDAÇÕES

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ABSTRACT

Reference is firstly made to two highrise buildings on footings in São Paulo, 1952 and 1992, in order to praise the comparative daring, in favour of economy, with which Odair Grillo designed foundations, and to extol the *Book* of his professional life, sparse in publication and entirely dedicated to challenging works in design and construction. Follows a critical analysis of what happened through the past 40 years: deformations misunderstandingly restricted to micro-deformations, consequent validation of the similarity between static and dynamic micro-deformations, the consequent availability of wide statistical universes of load-settlements of driven piles, and finally demonstrating the implicit exaggerated expense, without analysis or need, in our current practices. On typical *Prediction vs. Performance challenges on piles, as formulated by Academia, absurdities are exposed. While* facetiously ridiculing some of our postulations, notwithstanding their very good intentions, fellow geotechnicians are invited to reexamine repeatedly what has already been built and proven, in comparison with the sterile academic thirst for propositions forever startingly raw.

RESUMO

Começa-se por referir a dois casos de edifícios sobre fundações diretas em São Paulo, 1952 e 1992, para louvar o arrojo comparativo com que Odair Grillo projetava fundações, e exaltar o *Livro* de sua vida, abstêmia em publicação e inteiramente dedicada a realizações de projetos e obras desafiantes. Segue-se com uma análise crítica do ocorrido nos últimos 40 anos: deformações incompreendidamente limitadas a micro-deformações, consequente validação da semelhança entre micro-deformações estáticas e dinâmicas, a consequente disponibilidade de grandes universos estatísticos de carga-recalque de estacas cravadas, finalmente demonstrando o exagerado ônus implícito, sem análise nem necessidade, em nossas práticas correntes. Demonstra-se o absurdo dos desafios *Previsão vs. Comportamento* de estacas, montados por acadêmicos. Expondo jocosamente o ridículo de algumas de nossas postulações, conquanto muito bem intencionadas, convoca-se os colegas a repetidos re-exames do que já foi construído, em comparação com a sede acadêmica estéril de proposições continuamente novatas.

1. INTRODUÇÃO

I feel bound to start by using the occasion to compliment the Brazilian Association for Foundation Engineering, ABEF, for having instituted the Odair Grillo prestige Lecture, a long overdue tribute to the geotechnician who not only introduced Soil Mechanics and Foundation Engineering in Brasil, but also generated persons and institutions locally imbued with the zest for achieving successful foundations in emulation of his own unrivalled example. Odair Grillo made a single foray into the international geotechnical community in presenting the infant group of Brazilian enthusiasts at the 2nd ICSMFE, Amsterdam (Grillo, 1948); but, having with his dynamic kaleidoscopic personality left a strong impression that persisted for decades, he withdrew from conferences and publications into what was his cherished arena, the design and execution of challenging, successful foundations, by the hundreds, thousands. In professional life he maintained through roughly a 15-year span the intensity of forefront activity that should be unhesitatingly assessed as that of the most experienced innovative foundation engineer in the world. To those of us who have had the privilege of living as world citizens in geotechnique, foundations, and embankment dams, the unbiased evaluation seems fair and explainable on four counts: - the rate of growth and civil engineering construction that took place in Brazil, around 1947-72; the challengingly different subsoil conditions compared with everything taught and published in 1st world media; very incipient and modest supporting investigations and pseudo-theoretical testing locally available; the relative freedom from encumbering dictates of precedents, practices, and codes.

I am grateful for the honour of having been charged with the present responsibility, and in tribute to him with whom I worked through 16 formative years, my principal mentor in Foundation Engineering, submit my lecture/paper in two parts: firstly recounting my initiation into the company *Geotécnica S.A.*; and secondly in appealing to our colleagues to more proud revisitations and redigestions of our own immense park of data-cases, of the silent majority of modestly documented successes that have not merited theses and publications.

2. SAMPLE REVISITATIONS ON ROUTINES ESTABLISHED IN FOUNDATION DESIGN

Have we progressed, and in what manner? Or have routine practices suffered in efficiency and cost?

It seems that because of illogical and grossly oversimplified index-interpretations of early foundation design practices and prescriptions that spread across the world through the 1946-60 period, and were incorporated into burocratic *Codes* without any adjustments to progressively varying realities, the present situation is inconceivably stringent both as regards *Failure FS* and principally as regards criteria of the *Limiting Settlements That Should Be Really Significant.*

Thus the call for revisitations, purposeful and radical, is blatant. The point is *First Exemplified* as follows: (1) one example of comparative pad foundations of two buildings, in 1952 and 1992; (2) the interpretations statistically extracted from two cases of very many medium-size driven piles; (3) the comparative load test data on piles executed in the *Special* (ABEF) *Foundation Test Site* in gneissic saprolite profile at Polytechnic School, São Paulo; (4) and the analysis of the *Prediction* vs. *Performance* challenge on Pile Foundations, ASCE 1989. Thereupon, the point of the resulting absurdly increased conservatism is *Analysed* as *Maybe* derived from misinterpretations of historically justified *Prescriptions* that should have been adjusted, both based on evolving conditions and experience, and, principally, on the logic associable to the physical phenomena at play.

The early index prescriptions were perfectly justified as related to idealized extreme cases of "purely cohesive" and "purely cohesionless" homogeneous subsoil foundation behavior, such behavior associated to settlements, dichotomically interpreted either as signifying *Foundation Failure*, or as representing *Intolerable Distortions* and degrees of cracking of the buildings. The said prescriptions were also simultaneously related to two additional factors of great consequence, quite logical though somewhat recondite, and therefore have not been emphasized, nor have made any inroad into practical research-development and more economical design practice: they are (a) the implicit "flexible structures applying instant non-redistributed loading" (b) a most unfavourable hypothetical condition of the building, subject to damage, being *Constructed Weightless* in order to be available for thereafter absorbing the fullness of the gravity-on cause-effect relationship of *Load-Settlement.*

Thereupon it becomes inexorable by logic that for the vast majority of foundation subsoils and matchbox-like highrise buildings, the reality of structural and foundation behaviour should result very significantly less unfavourable. The first incumbent collateral demonstration is of the astounding level of overdesign resulting in modern practice: in the case of driven piling the behavior results restricted to microstrains quite dissociated from early intent of FS against failure. Collaterally one must inquire into the sense of researching ever more "perfectly" the behavior of the intact soil-element in situ, while developing ever more potent and grossly varying equipment-procedures for destroying the intact elemental behavior by *Execution Effects*. Thereupon one confirms the bias distance, and dispersions, that separate design predictions and observed performance. Finally, one reflects on the curious abdication of all logic and theoretical reasoning with which the early index prescriptions failed to benefit from the irrefutable prototype experience in fast growing cities, e.g. even in a city like São Paulo, where thousands of similar buildings were put up in roughly 40 years.

3. ANALOGOUS HIGHRISE BUILDINGS WITH SPREAD FOUNDATIONS

By a curious coincidence exactly 40 years separate the cases of two similar typical reinforced-concrete highrise residential buildings in São Paulo: the first, 1952, the 22-storey M.F. building which marked my frightened initiation under the brilliant and experienced prodding of Eng. Odair Grillo, the originator of Brazilian Geotechnique, a foundation engineer ever intent on pushing the frontiers of impunity to the limit, in favour of economy; the second, 1992, the 19-storey R. building, of analogous spread foundations, competitively designed to avoid expensive piling alternatives, despite the progressively more stringent settlement-limits presumed imposable.

The client of the M.F. building sought a cheaper alternate to the 17m Franki-Pile designed foundation; and Grillo without hesitation decided to use footings designed for a bearing pressure of 4,0 kgf/cm^2 (t/ft^2)! Reporting to Terzaghi-Peck (1948), only available support in those pioneering days, I could only conclude that with SPT $\cong$ 13, the respective "stiff clay" could not accept more than 1.00 - 2.00 (!!) t/ft^2. After sleepless hours of repeating calculations through all imaginable variations on the theme, even resorting to the side issue of calculating "net pressures" etc. (cf. Taylor, 1948) and adding the weight of the footing, etc., I timidly put forth to Grillo my minimal fear that his stated limiting bearing pressure would be exceeded, reaching 4.5 kg/cm^2. In truth, I secretly nurtured dismal premonitions that my first foundation responsibility, designed by an M.I.T. D.Sc. geotechnician about to be unmasked, alas, the proud building-to-be was doomed to result in a pile of rubble smack in the center of town! Grillo scorned the 10% increase in bearing pressure, and said that the client would be saving so very much, that he would gladly consent to our running two plate load tests and a series of conventional laboratory tests on undisturbed block samples; sufficient to allay my fears, he insisted.

That is how it came about that earliest conventional testing was made available for the M.F. building; and pseudo-theoretical calculations more than confirmed Grillo's experienced confidence. Incidentally, the load test execution and interpretation as installed in Brazil incorporated an imported respectful mixture of the Boston Code and also of the well taught/learnt preference for a bigger plate, so as to minimize errors of installation and of representative pressure bulb. Since similar well-intended gestures must have been transplanted to other areas of influence of other respectful disciples, it is worthwhile emphasizing that, from the very start, as regards pressures conditioned by allowable settlements, the adoption of the 0.8m diameter vs. 0.3 x 0.3m plate, associated with *The Same* 10mm *Settlement Criterion*, already incorporated a roughly 200-230% greater margin of conservatism than the presumed experience embodied in the Code derived from Boston's buildings pre-1945. Note that in essentially no subsoil profile does the perfectly flexible (circular) base on perfectly homogeneous elastic half-space ever apply, notwithstanding the *Routine Historic Adoption of Clays* as constant strength (c, $\phi = 0$) soils (UU behavior, unwittingly overextended across three generations of geotechnicians, also with the presumption of an associated constant modulus with depth). Even in the "conventional pure clays" normally consolidated, the Gibson half-space (of finite depth) becomes imperative, with variations of settlements and differential settlements "part-way" akin to those of "cohesionless sands". Therefore, no matter in what subsoil supporting spread foundations, the (*Simpler*) extrapolation of estimated settlements (for same bearing pressure) approximately as proportional to the diameter is a fallacy, and extremely conservative. *Conservatism I* (progressively we shall note the sum of conservatisms).

Figure 1 summarizes the data of reconnaissance borings, with SPT blowcounts, with which 98% of design decisions have been taken in São Paulo. Note that excepting for an unsaturated (MF 1952) vs. submerged (R, 1992) condition of the supporting tertiary clays, on most counts the 1952 design transpires as more daring. Further data on conventional tests are summarized in Figure 2. The 1952 load tests were limited to twice the intended allowable bearing pressure, as required by the code for the assumption that the conditioning criterion might be the failure capacity (*Conservatism II*).

Experience is acquired by dedication and

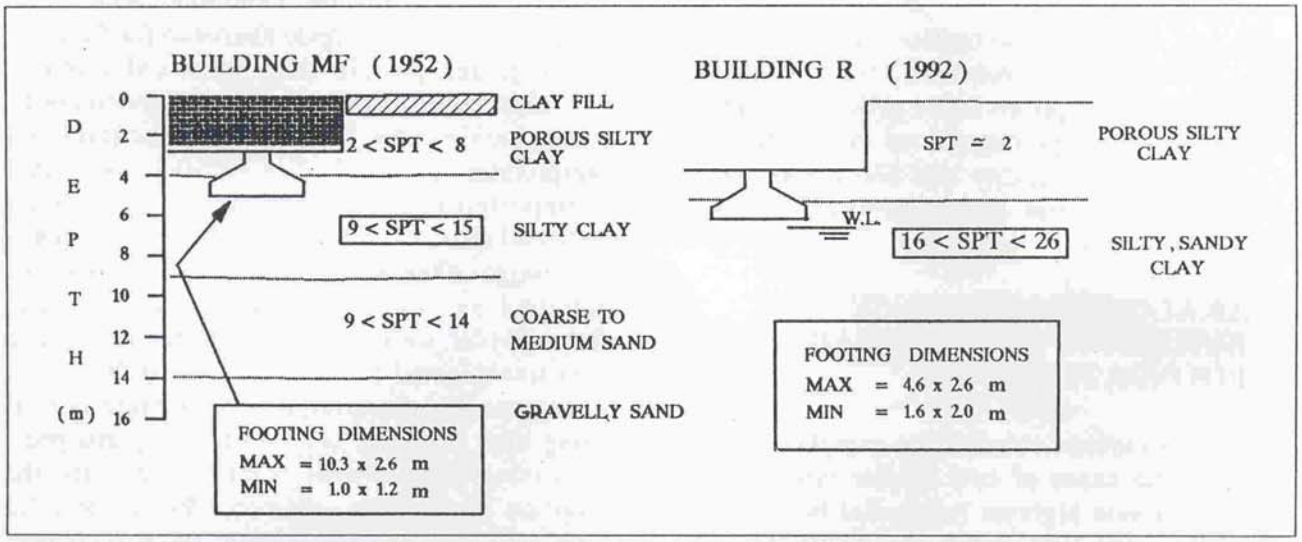

Fig.1 - Basic data, comparative spread foundation cases - 1952 & 1992.

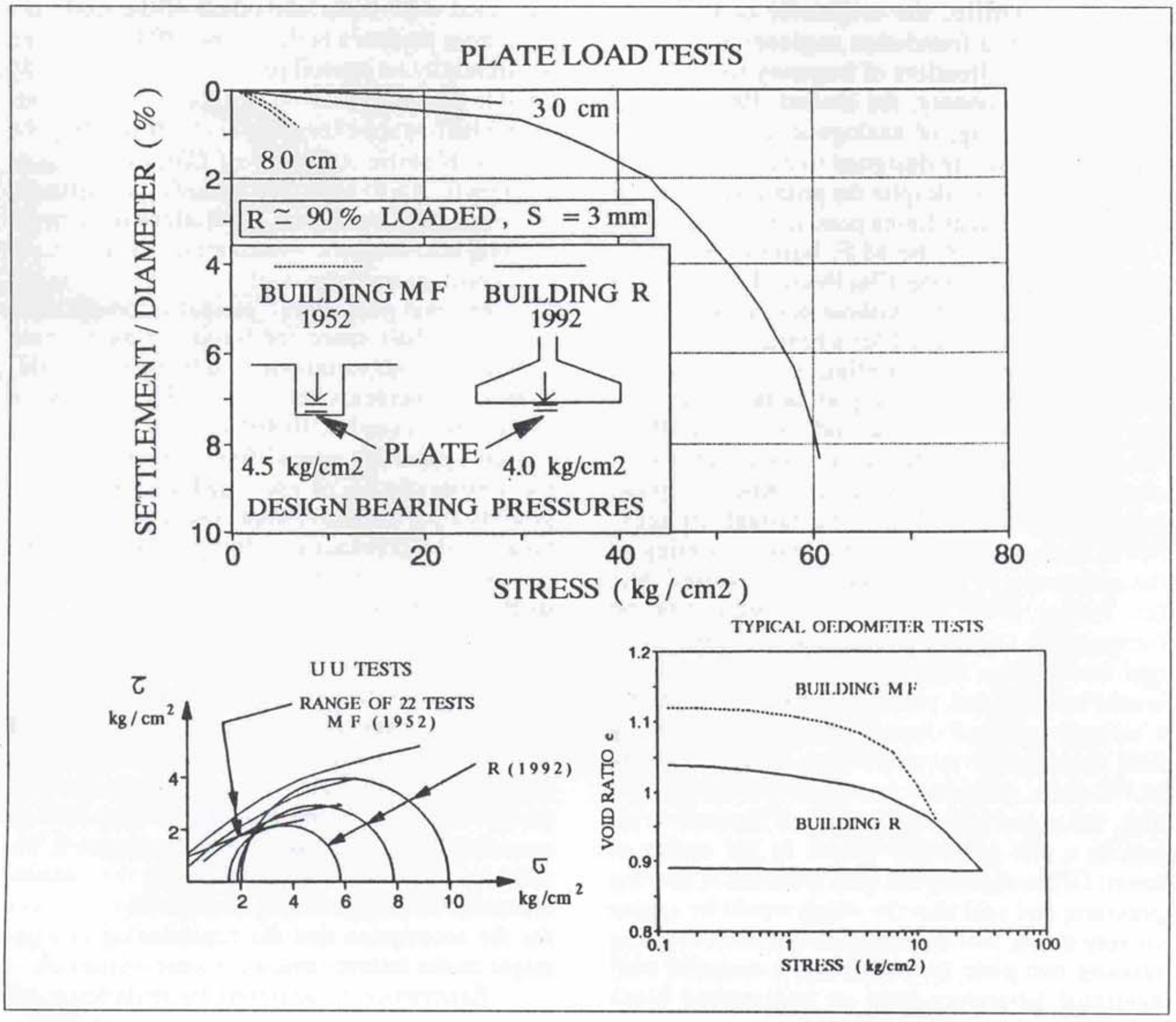

Fig.2 - Plate load test and conventional laboratory tests on block samples.

worry. Once or twice a week my wife and I used to go with our baby son to have dinner at my mother-in-law's, down-town. After dinner, with invented excuses of having to go to the office, I regularly found the need for walks in anonymity around the building's sidewalk while its floors and walls piled up, one every 20-25 days. Feigning no interest at all in the building I would search for signs of any tilt and/or visually perceptible displacement between column bases and the sidewalk concrete. Not a single sign:... relief, or postponed doubled concern?

How much did MF settle? From the experience of buildings well observed in the 1940's, right after Grillo's introduction of infant Soil Engineering from Boston, I would estimate something of the order of 4-8 cm; instantaneous, unperceived, imperceptible, and immaterial. Just another case collected, among thousands, with settlement performance satisfactory and not worth measuring: it belonged to the immense silent majority of cases that constitute definite experience, but, regrettably, are not thought worth any thesis or publication.

Note that the data from the R building (Figs.1 and 2) indicate very high safety against bearing capacity failure, and but mms. of settlement of the biggest footings at almost full loading. Such an overconservative performance obviously was neither sought nor felt necessary: it resulted... from understandable prudence bias, associated with a lack of revisitations of multitudes of cases from the past, *With Simple First-Approximation Tools*. For instance, it is axiomatic that undisturbed block samples can be taken, representing bearing conditions of spread foundations; and it has always been recognized that one should be able to estimate plate test stress-strain behaviours by some *Coupled Coefficients of Adjustment* applied to triaxial and oedometer stress-strain curves. Are not many hundreds of such sets of data available for statistical multiple regressions, so that for routine practice the geotechnical engineer could ward off the expense and delay of plate tests by systematic use of good conventional sampling and testing?

Incidentally, it is intrinsic to such a philosophy that the gradual improvement of sampling and testing techniques (if and when applied), and consequent results, become ingrained into our trajectory, past, present, and future. And the parallel scientific pursuit of new, *Different Theories And Tests*, would enrich and not confuse the experience backbone. How many of us have noted, with due respect, that in classifying clays of 8 < SPT < 15 as stiff, with consequent indicated allowable pressures of 1.00 - 2.00 t/ft^2, there was also the statement "compression tests should always be made on the spoon samples" (Terzaghi & Peck, pg. 300, 1948)[1]? The roughly perceivable performance of the buildings, satisfactory or not, Is *The Ultimate Basic Fact*. The varying SPT or compression-test indices are the complicating interference in our *Knowledge Distribution*. Micro-strain measurements in the middle of the triaxial specimens, as also by tell-tales inside the pressure bulb, are gradual refinements; so also the recommended use of *Bender Elements* to control sample quality, and resonant column tests: all such have to be sponsored and lauded and must be gradually incorporated into improved understandings, and for *Readjustments of the Backbone Statistical Coefficients of Adjustment*. They should not entice moulding

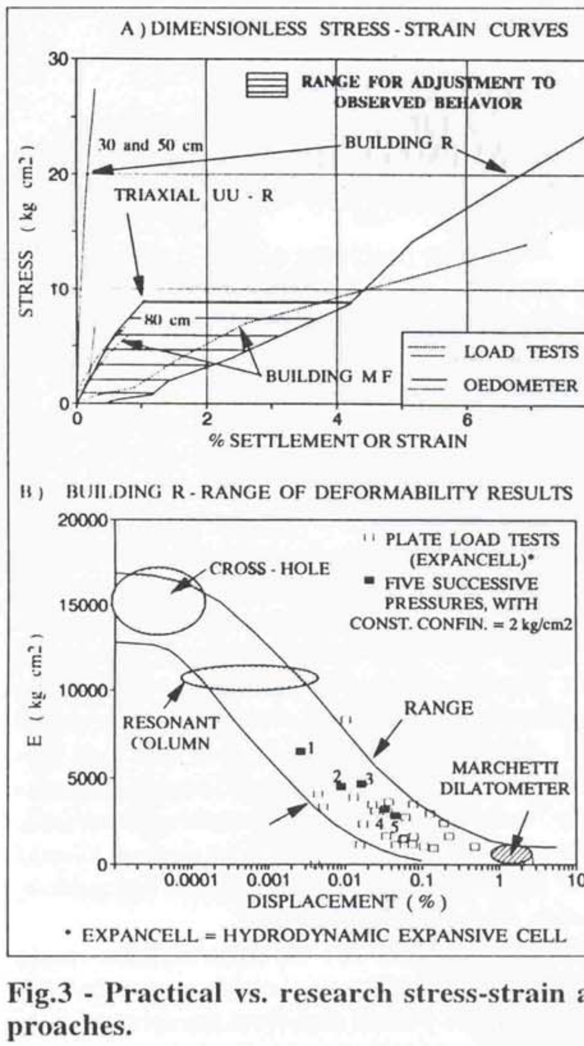

Fig.3 - Practical vs. research stress-strain approaches.

[1] An improved accreditation of the SPT resistance index by Torque measurements, the SPT-T constitutes an important judicious complement in use by Luciano Décourt, Brazil, and followers.

totally new embryo backbones, or fracturing the existing one except for judicious reconstitution.

Figure 3 summarizes data, for Building R, from much additional testing of types currently promoted. Do such data directly favour design decision for the foundation or the building? No: not for a long time to come, and not unless our aims are redirected. One practical conclusion is that microstrain moduli testing should be of professional interest by offering for *Design Decision* an upper bound indication of rigidity, especially in micro-cemented tropical saprolites and laterites, long-term aged soils, etc.. Experience shows that such soils have always been treated far too pessimistically because of low moduli given by most testing: overestimates of settlements by ratios of 5 to 10 have been frequent over more than four decades, and have not changed perceptibly by changing *in situ* or laboratory tests.

4. INTERPRETATIONS STATISTICALLY EXTRACTED FROM TWO CASES OF DRIVEN CONCRETE PILE FOUNDATIONS

In various problems of pile foundation design practices, the accumulated effects of stringent requirements and increased costs are flagrant in calling for revisitation of the origins, as distinct from subsequent trajectory.

Figure 4 firstly illustrates two principles that have been repeatedly proclaimed. (I) Even if a lumped-parameter *Index* is crude, if it is used and systematically adjusted to another lumped-parameter that *Averages Analogous Behavioral Phenomena*, correlations and prescriptions can become quite good. (II) Whenever deformation criteria ρ are tight, tending towards zero, and consequent rates of changes of increments $\partial Q/\partial \rho$ *ipso facto* become magnified, correlations become good. At microstrains predictive abilities improve, but lose practical significance: unfortunately foundation costs correspondingly increase. A *Worthwhile Engineering Correlation Should Prove Under Significant Magnitudes*, that permit, and risk, significant differences.

Only the briefest mention can be made herein of the messages of the two graphs (a), (b) of Figure 4 that extract data from the driven point-bearing concrete piles for the *Tank-T* structure of Figure 5. Published prescriptions by Aoki-Velloso (1975) and Décourt-Quaresma (1978) have been very much used in Brazil. They were based on uncorrected SPT values from routine reconnaissance borings, and were aimed at predicting the driven pile length L, that would guarantee satisfactory load capacities as per static load tests and the *Foundation Code* (settlements at failure and allowable loads set at 15 and 10 mm respectively). Moreover, most of the piles were of small 25-40 cm diameters. It can be seen that both methods give good results for the purposes: (a) similar predicted Load Capacities (often taken as 1.5x the maximum load reached, a frequent limitation because of cost, and the vicious circle of dimensioning the test reaction load to 1.5x the desired/predicted pile design load); (b) good prediction of lengths of driven piles; (c) because of microstrain conditions at play in the pile data, and the tremendous available rate of change $\Delta Q/\Delta L$, the stage was set for easy success by routine overdesign of driven pile lengths, always self-justified in vicious-circle by the tight code: only statistical analyses can permit assessing the consequence.

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

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

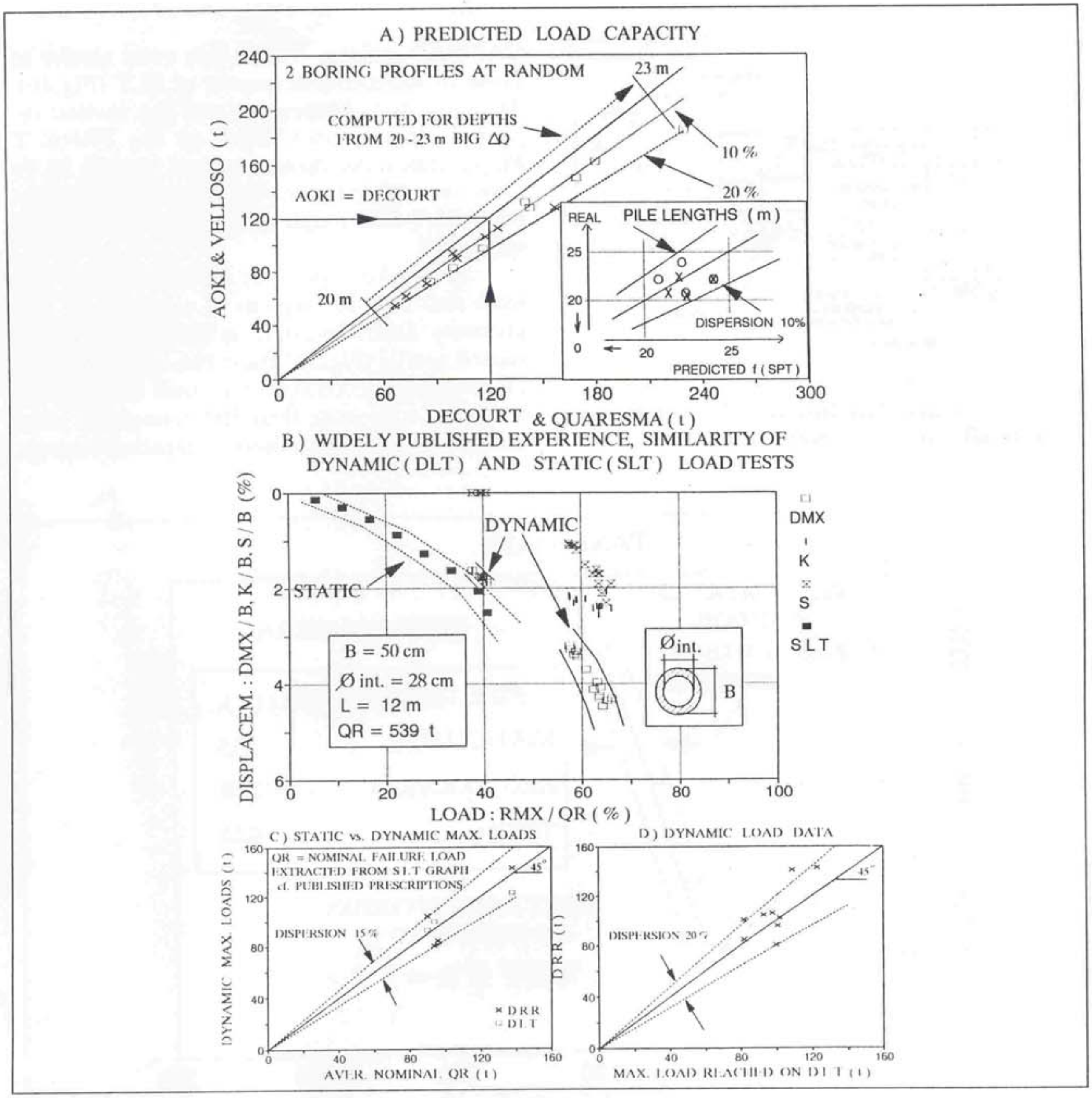

Fig.4 - Driven pile data validating SPT - index correlations and dynamic rebound load test analyses.

"set"). The data identified as DRR are such dynamic QR values (Fig.4c).

Meanwhile it need hardly be recounted that at similar microstrains the pile-driven Dynamic Load Test DLT has been repeatedly validated, both in comparison with the DRR and with reference to the Static Load Test SLT (as interpreted through the most divulged tight code prescription, cf. Figs.4b, c, d). Recapitulating: in the classical routine of pile-driving control, the weight and fall (energy) has been kept constant, under the convinced fear that since dynamic ≠ static (conventional dogma), it was fundamental to respect avoiding any conscious differences. More recently, however, it was reasoned that if a given (arbitrary) energy E_1 gives the unequivocal dynamic failure load QR = α (R_d), α traditionally taken as 1,0 , then any energy E_2, E_3, ... E_n should also give the same failure load. Curiosity, and the principle that no two things are ever equivalent, led to questioning this would-be coincidence of the theoretical idealizations: and it was discovered that, quite to the contrary, on *Using Progressively Increased Energies* E_1,... E_n at the point of final set the

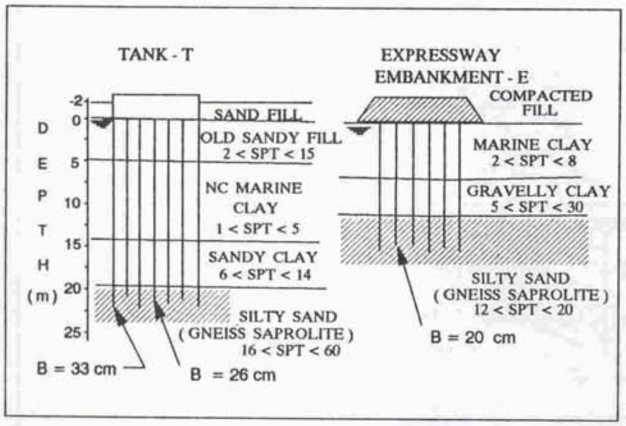

Fig.5 - Two cases of multitude (> 700) of driven concrete piles well documented.

CAPWAP analyses lead to data quite similar to those of loads-displacements of SLT (Fig.4b). The easy and inexpensive test has invited repeated obvious uses. Thus on the TANK T Project herein commented we had 10 such DLTs (plus one repeated after a few days): moreover the DRR-DLT-SLT rough equivalences were again confirmed.

Meanwhile, the same driven-pile concepts were also used for support of a compacted expressway *Embankment* E, in essentially similar subsoil profile (Fig.5) . These two cases of multitudes of driven concrete piles well documented are used, with more than 700 contiguous piles each, for extracting the desired statistical lessons.

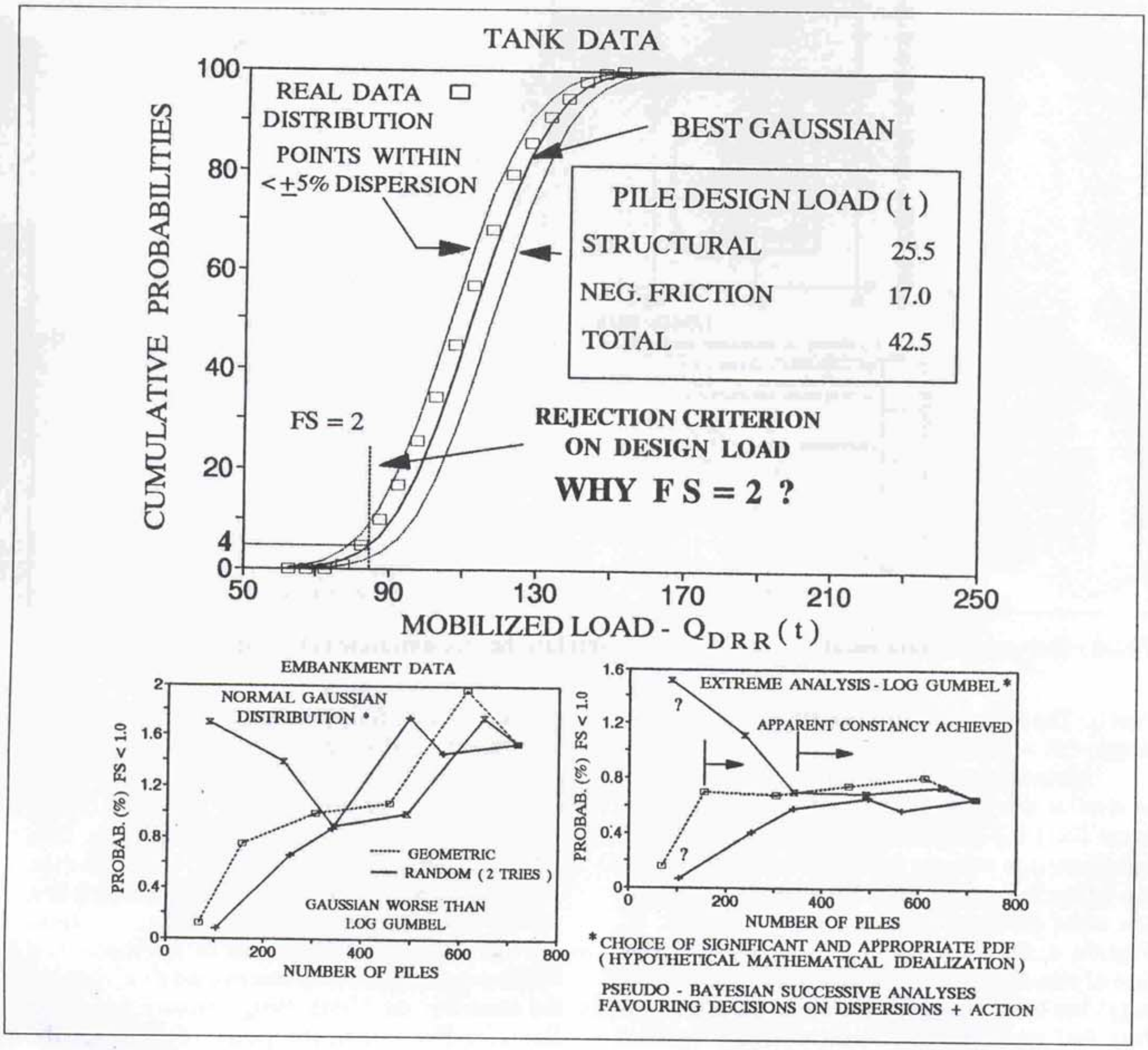

Fig.6 - Tank piling: sample statistical analyses and conclusions.

Figure 6b firstly serves to recall that for extracting some minimal benefits put at our disposal by statistics we not only document with Confidence Bands (CB, not incorporated, to avoid crowding the drawing) but must (a) choose from among the Probability Distribution Functions PDF available, the one that on trial proves most profitable, (b) test the existence or not of secondary trends by Bayesian successive analyses. It transpires that apparently the Gaussian PDF is less fertile than log-Gumbel PDF. Also, that apparently with less than 160 piles (advancing geometrically in the piling array) or 340 piles (data taken at random) the statistical conclusion does not reach an apparent constancy (in the exacting level exposed by the scale of the drawing, far more exacting than of significance to the foundation).

The use of an extreme-value PDF proves appropriate, as is shown in Figure 7, of graphical trends preferably linearized for easier interpolations and extrapolations. To begin with, in this specific subsoil profile (and in most routine cases) the behavior of each pile is justifiably considered independent, and therefore the recurrence probability of each pile's maximum resistance

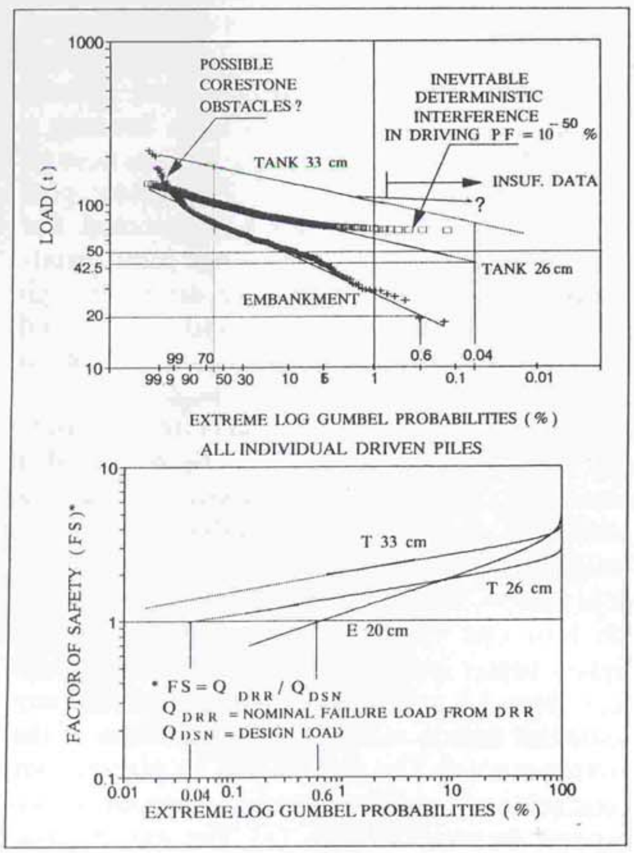

Fig.7 - Driven piles, single: probabilities of nominal failure.

(or nominal failure load) and, a fortiori, the smallest values of that universe should be most appropriately adjustable to *Extreme Value Distributions*. The two graphs are convincing enough, and reveal logical trends, and astounding magnitudes. We can forego pointing to the obvious trends: the Embankment piles are quite logically a little less exigent than the Tank piles, but both are far more exigent than should be required by judicious safety and serviceability criteria.

The two principal facts exposed are, firstly, the very low probabilities of any single pile having a nominal FS dropping to 1.0; that is, the Design Loading (usually estimated with pessimism) increasing to become equivalent to the nominal failure QR of DRR values. A probability of 0.04% is most astoundingly and unjustifiably low for such an inconsequential "overloading" in comparison with such catastrophic and sudden events as a 1:10,000 flood risk for a dam and spillway. Even more important a lesson derives from the obvious demonstration that with the typical, routine, "deterministic interference" of the pile-driving foreman, in improving the "set" of piles to suit specifications and his experience, the probability PF of any single pile reaching FS = 1.0 becomes far too low for either physical or mathematical meaning.

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

Figure 8 shows that even in the unnecessarily tight microstrain range, most of the divulged prescriptions for procedural interpretations of the load test graphs include a further 1.35 FS with regard to the microstrain QR from DRR data. Figure 9 shows what would be the physical consequences *Beyond the Nominal Failure* postulated. Cases of brittle failure emphatically excluded, all that happens if the condition of FS < 1.0 begins to set-in for the *Individual Pile*, is that a minimal inconsequential rate of *Incremental Settlement* of 0.1 mm per 35 t would begin to force some redistributions of loading (Fig.10). Regarding savings in the foundations it is seen that if the total number of piles were reduced to 60% of the designed array,

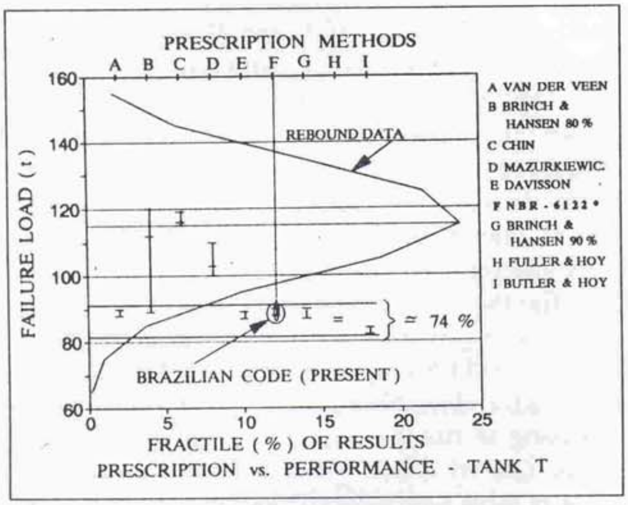

Fig.8 - Driven piling statistics compared with nominal failure load by various methods.

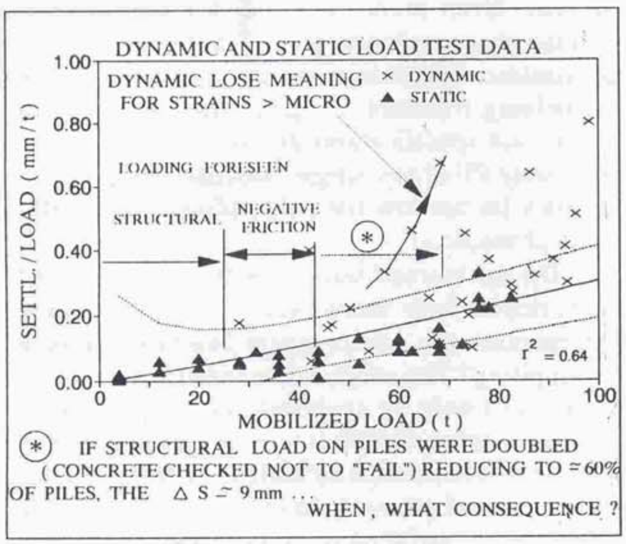

Fig.9 - Analyses of consequence if nominal load on pile is exceeded.

an increase of (flexible, first-load) settlement of 9 mm would be the only result. Finally, by using the individual DRR data of contiguous piles and, without any structural redistribution, merely considering the arithmetic average QR of groups of 2 x 2, 2 x 3, and 3 x 3 contiguous piles, the PF% of FS = 1.0 drops to about one-tenth of the corresponding PF% established for the individual pile. Barring geotechnical disturbances of one pile to others nearby (an entirely separate consideration), of the many absurdities in design practices, one lies in requiring the same FS per pile whether it is alone in supporting a column, or is one of a group for that task.

In short, by using simple statistical analyses

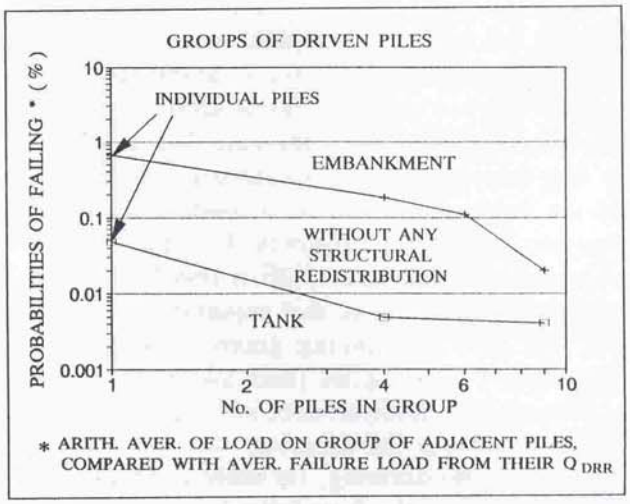

Fig.10 - Driven piling: reduction of probability of failure in group vs. single.

on a documented piling foundation we reemphasize that our engineering decisions are not based on averages of correlations but on rejection criteria. With progressive changes of construction practices the applicable idealizations for theorizing (and for recommending in Standards and Codes) should have suffered major changes. Having systematically failed in this priority intent, the resulting absurdities and greatly increased unjustifiable costs have become a plague. Many important issues on practices of design and construction-plus-inspection, plus codes, load tests, etc. cannot be expatiated. For instance: (1) the case concerned piles point-bearing in dense gneissic saprolite, driven through compressible marine clays under fill, and therefore anticipated for negative skin friction, on which factors of safety merit radical rethinking; (2) once the mud-tank dead load is totally acting, and ulterior sensitive levellings finalized, what incremental loading could possibly require a global FS, and how incomparable is this with buildings of greatly different proportions of final dead load vs. incremental uncertain live loadings?; (3) how can Committees, discussing Codes, lightly banter around with changes of FS values (e.g. from 1.5 to 2.0, or vice-versa) without any statistical data to evaluate the magnitudes of the consequences? The fact is that in placing our conclusions in civil engineering perspective two aspects become salient. (a) The exponential disproportion regarding risks and costs of risks in comparing a spillway failure to cope with a flood, and the piling's failure to cope with the assigned FS. (b) The great increase of unnecessary first cost. In the case of a real FS lower than assigned, absolutely nothing is at risk: but, to exaggerate in order to quantify something different from zero, possibly one might be risking a fissure of tenth of mm, worth 50 dollars of repairs. Can Society countenance, and unknowingly pay for, such an absurd difference of design "risk-insurance" within the selfsame profession?

5. BROADER RANGE OF PILE TYPES- CONTINUALLY REFINING SUBSOIL KNOWLEDGE VS. PREDICTING DIFFERENT PILE TYPES.

The scientific urge to know more precisely the real conditions and behavioral parameters of intact elements in a subsoil is obvious and laudable, not only for reasons of science, but also for such geotechnical engineering works as depend on natural profiles without submitting them to alterations (natural slopes, shallow foundations, embankments on various soils, some significant masses in underground excavations, etc.). However, in the case of pile foundations, the irony over the past four decades has been that as the scientific definition of intact soil elements has become increasingly "precise", the capacities of equipment to alter completely the intact conditions by installation/construction effects have been exponentially increased in the opposite direction.

The ASCE Pile Prediction Symposium (1989) offers data of interest for a revisitation. Figure 11 summarizes the minimal data on the subsoil profile, and the three pile types chosen for our presentation. The routine and special profiling tests

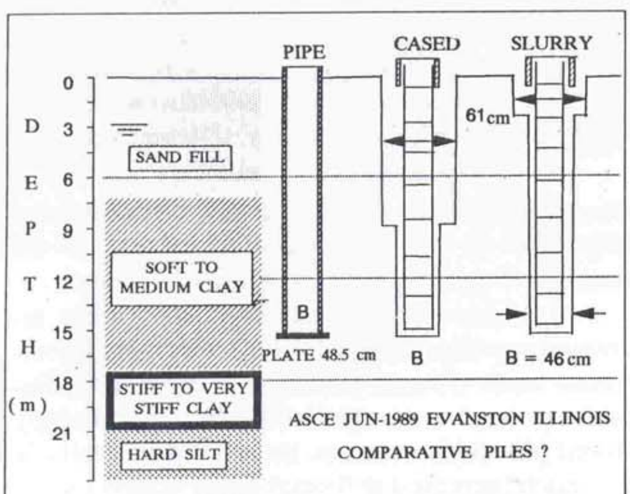

Fig.11 - Summary profiles on ASCE Pile Prediction Symposium (1989).

are not reproduced: what matters is the essence, and not the details. It is important to emphasize that the predictors did not submit questions or reservations in advance. This reflects seriously on two sore trends of more recent geotechnique: (1) the relative inexperience of designers and academia in the rough working details of investigation procedures, and all the subordinate inabilities to apply judicious adjustments to the data; (2) the dismaying acceptance of data at face value, in lieu of the indispensable engineering-science (and Terzaghian) concept that all test values are always more or less wrong, unless and until assessed to have acceptable and judgeable coefficients of adjustment, to experience and/or (presumed) reality.

The SPT borings were of very questionable quality, incorporating both consistent and erratic errors apparent. Field vane, pressuremeter PMT, Marchetti dilatometer DMT, and cone static penetrometer CPTU profilings appear to have been introduced under obligation, and not under comforting intimacy. Pseudo-undisturbed samples are summarily described as 3" and 5" "tube samples": judging from stress-strain behavior, one should qualify the sample quality as moderate. Laboratory tests concentrated on UU and CK_0U triaxial, and direct shear: how and why programmed? No testing on sensitivity, or remoulded-reconsolidated samples. Pile driving records furnished are routine.

The three pile profiles shown in Figure 11 invite inquisitive attention because: (a) exactly the same depth (and diameter) was used (the lower stretch being in the saturated clay in which depth would be of secondary relevance!) with transparent intent of similarity of the piles; (b) the wider-diameter base plate in the pile and the "collars" in the sand in the other two piles, should produce significant effects annulling the presumed similarity.

The statistical analyses of the 23 predictors are summarized in Figure 12 (a, b, c, d). Almost nothing seems to have been contributed by the wider spectrum of investigation testing: obvious, and alerting. The predictions were best on the Pipe Pile possibly because of more routine experience with such piling. The predictions on the Cased and Slurry Piles seem extraordinarily absurd; the cause seems to lie in the "collars". Would it not appear that, similarly to the M.I.T. 1974 Embankment case, this prediction challenge also suffered from digressing, in many respects, from what would be local routines, more consistently handled and averageable?

Further data on the wide range of execution

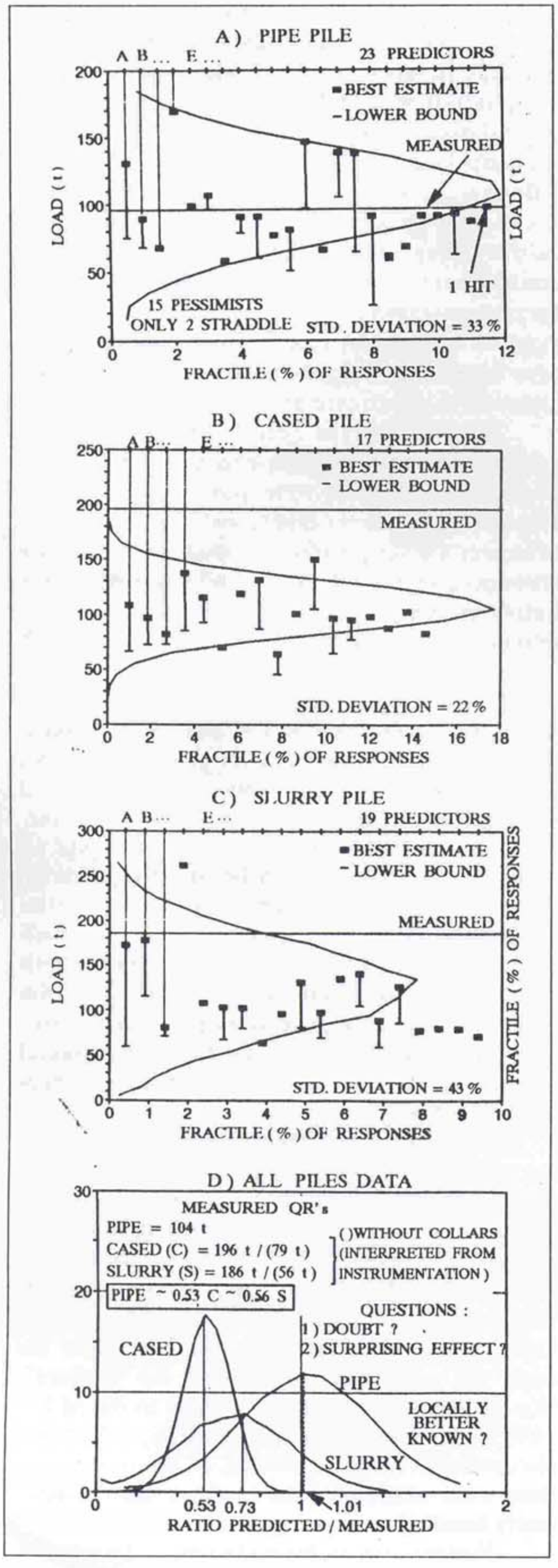

Fig.12 - Statistical analyses of predictions.

effects are extracted from the ABEF 1989 Experimental Site in a gneissic saprolite profile. In connection with the Rio 1989 ICSMFE, the Brazilian Society for Foundation Engineering and Engineering Services, ABEF, conducted a far-sighted, broad-scope research on foundation engineering and emitted a first publication on the results. The dense gneissic saprolite profile, very susceptible to stress release and densification effects, besides presenting slickensided fissures as relict joints, is fully equated as to geotechnical parameters; the purpose of the present revisitation is limited, however, to exposing the widely different execution effects.

Figure 13 is presented for piles executed by and for each Specialized Company's working load according to best local practice. The data have been slightly adjusted to identical dimension, 7.5 m long and 0.5 m in diameter: the slight adjustments necessary and judiciously applied gave results remarkably atuned. The three piles

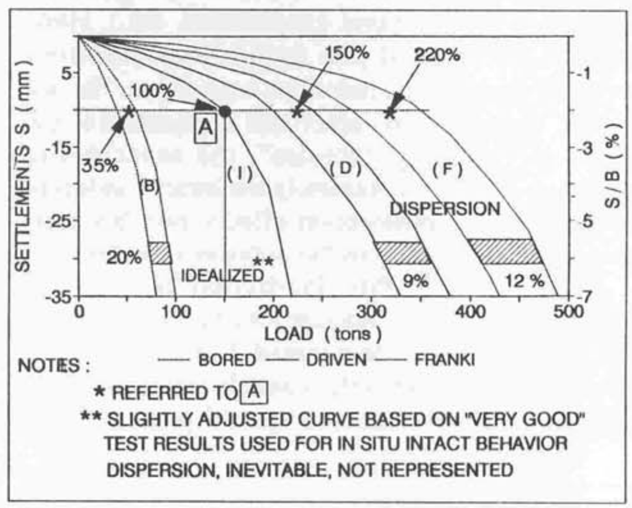

Fig.13 - Impressive pile execution effects in gneiss saprolite (ABEF experimental site, 1989). Use of intact parameters thwarted.

chosen are (a) auger-bored, and/or bentonite-stabilized-bored, (b) precast concrete driven, and (c) standard Franki-type driven, without pedestal.

The driven piles achieve considerably increased rigidities, above all, besides the higher ultimate loads (Franki justifiably more); also, dispersions tend to be smaller (seems reasonable). Bored pile stress releases show-up principally in (1) much increased deformabilities within ranges of centimetric settlements mentioned in Codes, and (2) practically no incremental resistance from the base. The tremendous differences, of necessary Coefficients of Adjustment (herein ranging from 40% to 260%) regarding execution effects, in referencing to the idealized *Intact* load-displacement curve predictable, surely stand as a grave cause for reflections and revisitations, when considering the pursuit of better defining intact parameters. At greatly increased costs, do we not principally change the target continually, never permitting the acceptable closure of the cycle of experience? Both from the investigation-interpretation side, and, more potently, from the side of piling types, the production of wasteful confusion has been astounding.

We need not emphasize the influence on *Design Decision* and *Costs* exerted not only by the *Bias on the Average*, but also by the big, and noticeably different, *Dispersions.* The concept, and method of analysis, expatiated in connection with the embankment on soft clays, continue directly applicable; and they become greatly aggravated by the difference between real failures and the nominal failures attributed to piles.

In broaching the important influence of interpretations of load tests on piles we must revert to the historic concepts and origins of practices that became consecrated in codes. The bifurcation into (a) real failure, and (b) serviceability limits on settlements, prevails. Three factors intervened, quite understandable for the past 30 years (cf. ICE Large Bored Piles, 1966), but not yet incorporated. Firstly, the small limiting settlements (e.g. 10-15 mm) used for defining loads were based on failure, and valid for driven smaller length-diameter piles. Secondly, there was confusion in extrapolating to increasingly bigger diameters, because lateral friction and point bearing have absolutely different stress-deformation behaviors, the friction quite constant irrespective of diameter, and the base essentially proportional to the diameter. Finally, regarding limiting deformations, the failure risk having been definitely averted, it should be reasoned that if buildings (e.g. on spread foundations) have well accepted settlements of 10-15 cm, there should be almost no reason for them to discriminate between the different foundation types that cause the said settlement at the bases of the columns.

Figure 14 illustrates how to compose, in first-degree approximation the load-settlement curve, using the distinct adhesion and base contributions (the latter assuming constant E). Under these same criteria (obviously requiring second-degree corrections in many a profile), Figure 15 illustrates the unfair consequences to bigger diameter piles and piers, and particularly to bored vs.

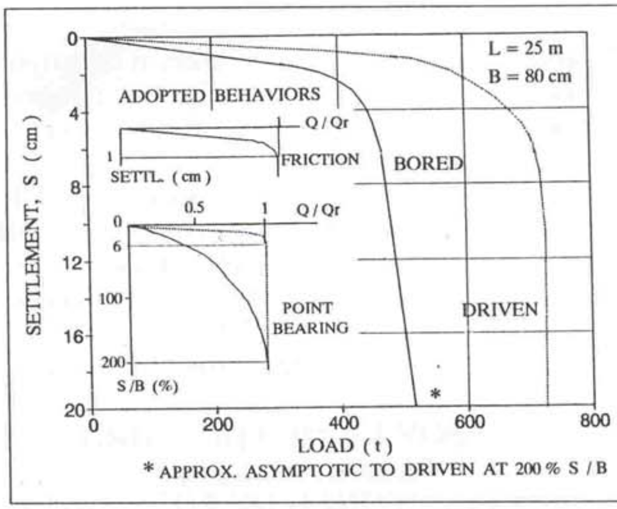

Fig.14 - Updated idealized composition of a load-settlemente curve.

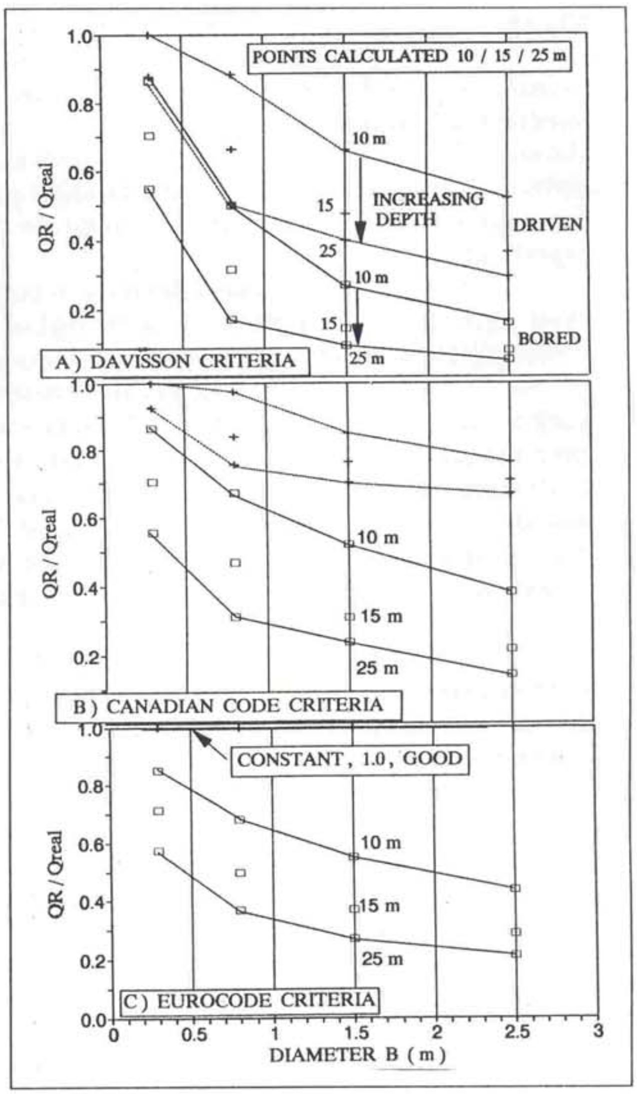

Fig.15 - Sample comparisons of criteria influencing nominal vs. real pile failure loads.

driven piles, in using some respected prescriptions and codes, historically established on the basis of real failures of driven piles, of smaller lengths and diameters.

When we run Dynamic Load Tests DLT on longer-larger diameter piles, especially if bored (which require load testing more than any other), all that is being tested in the microstrains is the lateral friction. But why forego partial knowledge, when all knowledge is forever partial?

6. THE POWER OF THE WORD AND COMMUNICATION, IN THE WORLD OF THE *VARIETY* OF THE *MAYBE VISION*

Except *Maybe* for the *Cost Consequences*, all of the above has been repeatedly said and written. How and why do I presume to encroach on your time? Failure, the primeval fear, can be averted: it should be so at least cost, and not at unperceived increasingly higher costs. It is the chronic HIV virus of the vicious circle of serviceabilities-costs that plagues our buried routine professional activity. The burden is greatest on the eagerly-growing societies.

History gave the pioneers the right to build, even monuments and palaces, with rights to settle gallantly: pseudo-historians that cannot see the charming ironies of recently lived history deny us the ladder to development. History even gave the privilege of having unusual, undesired settlements become great sources of income: who does not know of the Tower of Pisa, or the Palace of Fine Arts (Mexico)? Who has assessed, by eye, that the huge walls of the main hall of Grand Central Station, New York, settled at least 30 cm? Meanwhile, such laudable efforts as published by Skempton and McDonald (1956), Bjerrum (1963), De Mello (1969), Grant, Christian and Vanmarcke (1974), Burland, Broms and De Mello (1977), and others in offering first-rung discussions on *Unacceptable Differential Settlements*: have they really blocked the ladder of progressive questions-answers, or principally opened them to fertile revisitations?

Similarly, some suggestions on key geotechnical profiling parameters introduced into the profession by illustrious colleagues, have they not called for reviewing by independent lateral thinking? Everybody is invited to exercise the right and obligation of challenging transient quirks with queries on pragmatically varying logic.

7. UNACCEPTABLE DIFFERENTIAL SETTLEMENTS

Scattered through the publications, all points have been made repeatedly. Two principal things appear to be lacking: one, to associate the statements with the data and theories of the time, in order to readjust them; the other, to try to use *Words of Impact*, the caricature, the hit in the eye for *Communication*.

For instance, in the Skempton-McDonald analyses (and throughout the bifurcation of conventional soil mechanics into *Cohesive* vs. *Cohesionless)* the logical bases of references were of perfectly flexible circular rafts on elastic homogeneous half-space medium. Essentially no soil has the idealized constant c and E with depth; the neo-Gibson (1974) improved Carrier and Christian (1973) soil model should be an obligatory adjustment of revisitation since 20 years ago. Figure 16 shows the trends of some changes of conclusions.

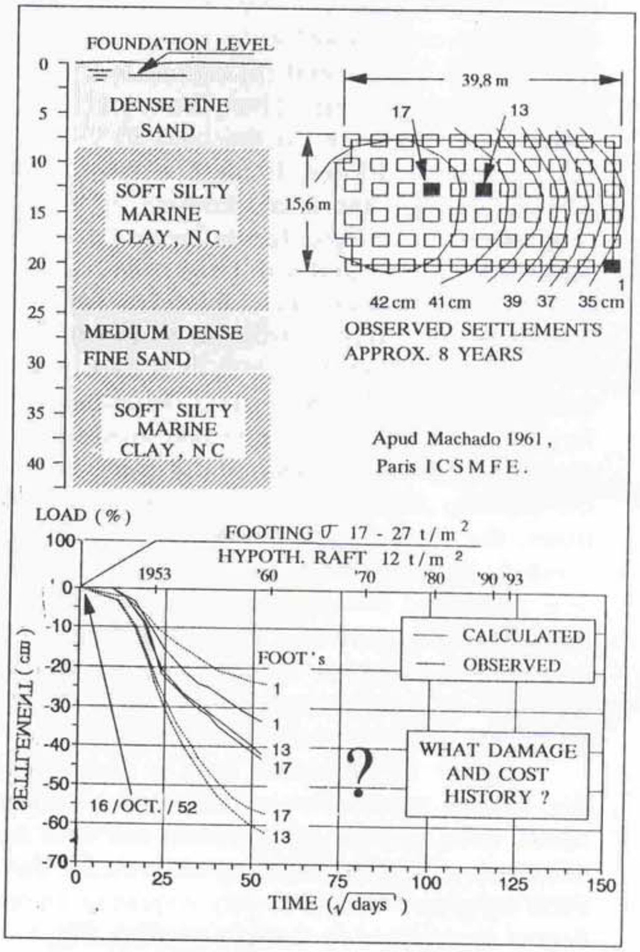

Fig.16 - Change from idealized constant E to realistic Gibson subsoil changed influence of raft diameter on settlements.

We assume that obviously for any shallow foundation the ease of testing just below surface establishes a fixed known adjusted E value at the top. The conclusion is that even with only a Gibson soil the settlements and angular distortions decrease over most of the central area *(Conservatism III)*. Meanwhile the angular distortions increase significantly towards the edge only, where some special reinforcement can be concentrated.

Grant et al. (1974) emphasize Terzaghi's discussion on the Skempton-McDonald original *Milestone*... "the audacity with which the authors had drawn their final conclusions... Instead of stimulating thought and observation in the difficult field..., the conclusions were likely to have the opposite effects". Indeed *Audacity at Temporarily Accepted Conclusions is an Engineering Obligation*. The fact is that in order to open the field to fertile reanalyses, avoiding the incubated virus of unnecessary incremental costs to society, the conclusions should risk being *Optimistic, Audacious*.

The call for observations and reevaluations is emphasized: and, all the more so towards the obligation to supply (appended) the tabulated data on the cases studied, in order to permit revisitations. The failing is general, regrettable. As a mere example: on the Grant et al. study one would wish to reassess under maybe premisses that the *Evidenced Building Rigidity*, taken as function of "maximum specific distortion" $(\delta / L)_{max} / \rho_{max}$, could be better associated with the *Difference* of such parameters between the *Totally Flexible* case and the *Semi-Rigid Case*. Also, for instance, separate single-parameter correlations might be substituted by multiple regressions, such as associating the building's idealized structural rigidity with H^3 / L^4 (H = height, L = length).

However, any and every such consideration is puny in the light of the surprising effect of *Communication By Habit* that is exemplified in Figure 17 (a, b) and that: (1) has, in my experience, caused enormously wasteful foundation costs in most cases analogous to *Machine Foundations*; (2) calls for a *Radical Revisitation on Buildings*. The word "foundation" automatically evokes "subsoil": let us urgently adopt the substitute "support"; the stringent requirements of machines only apply to the "top of block support", and that only after the machine has been anchored. Regarding buildings we recognize that a "first cracking on finishes or panel walls" cannot be correlated with "total settlements", most of which may have occurred long before the walls or finishes started existing *(Conservatism IV)*.

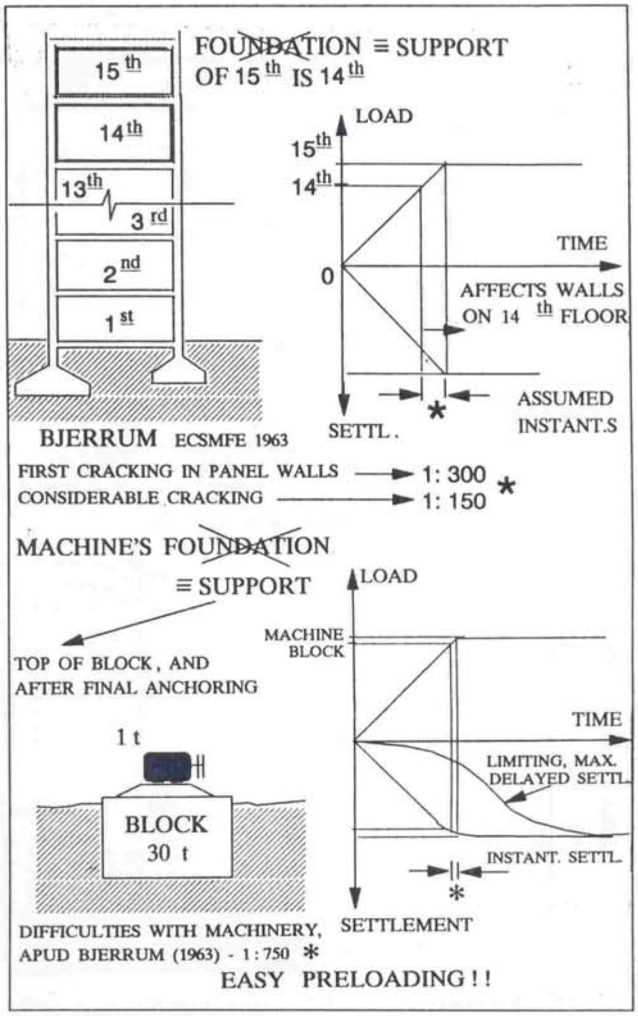

Fig.17 - Recommended use of "*Support*" instead of "*Foundation*" for improved logic and great savings on many cases.

Terzaghi's premonition seems to have been vindicated.

One cannot seriously study statistical regressions of beans and beasts within the universe of words starting with b. Repeating published exhortations, I emphasize that within the gross interference of crudely estimated rigidity, one should profit of given buildings, with permissive settlements, as settlement profiles in each *Floor Support*, each building as one fixed universe of (partial differential) interest. Moreover, the city of Santos, Brazil, presents an unrivalled universe of more than 1500 very similar buildings that have settled between 50 and 250 cm, with many distortions of 1:50 or more, *Without a Single Structural Failure* (even nominal) *(Conservatism V)*. Figure 18 merely hints at the treasure of revisitation data, by summarizing data on just one typical case, published in the Paris 1961 International

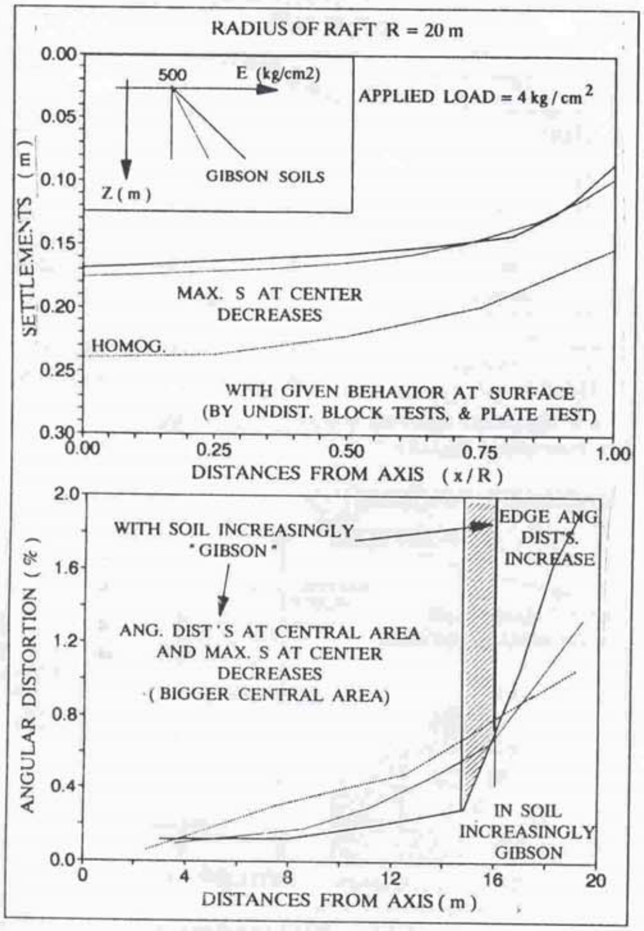

Fig.18 - Sample case of building in Santos calling for updating and revisitations.

Conference and cited in the State-of-the-art report of Burland, Broms and de Mello (1977).

8. THE FERTILITY OF ODDITIES

This over-extended message had to be limited to a couple of topics, of presumed priority. But it should be evident that oddities abound in almost every topic, and principally, like weeds, take roots in the paths erstwhile considered secondary. Geotechnique must be alerted, because secondary paths have tended to become primary, and the tacit acceptance of a well-rooted weed as desirably planted is an innate danger in the culture of the *Word*. We must learn to take ourselves with a jovial pinch of salt.

A few examples may be briefly cited, to close this effort in tune with its keynote:

a) There is the case of the introduction of a pseudo-parameter $C_c/1+e_o$ by somebody who was lazy at seven seconds of seven-year old arithmetic. Conventionally, in a given clay the C_c was reasonably constant and correlatable: which permitted developing memorized experience. And, ipso facto, in any respectably thick stratum e_o has to be varying with depth (idealized equation deducible): thus the profiling of $C_c/1+e_o$ presents inevitable variations with depth. Further confusion appears by pre-consolidations leading to more than one e_o at any constant C_c and σ'_p. Does anybody wish to adhere to the elusive pseudo-parameter ?

b) As the faith in the in situ vane test undrained shear strengths began to grow rapidly, the need was laudably recognized of some coefficients of adjustment to presumed reality, principally considering effects of destructuration and anisotropy. *Sic* (Bjerrum, 1972) "case records were reviewed and... corrections were derived which may be used to bring the results of in-situ vane tests into agreement with the shear strength mobilized at failure of the embankments. By plotting these correction factors against the plasticity index of the clay, the correlation... was established". Apparently the tantalizing question "to apply, or not to apply" such a correction has persisted, for 20 years, principally because it would generally be small. But maybe very big could be the surprises at the composition of oddities. From the side of analyses of embankment failures we have seen how very crude is the profession's capacity. Much more important, moreover, is the rejection of the presumption that all failures back-analysed correspond to a condition of FS = 1.0: this was a justifiable conventional corollary of the "statics of sliding equilibrium", but is obviously a mistaken and harmful hypothesis. We have switched from *Stability Analyses to Analyses of Destabilization*. Failing corresponds to *Passing Through FS* = 1.0, and *Not Being at FS* = 1.0: such recent considerations would make even more questionable the side of the offered correction graph generated by failed embankments.

However, the greater oddity yet arises from the other side of the graph. The plasticity index results from a difference (greater error probability) of two index tests on the absolutely remoulded condition of a clay quality. How could it possibly expect any remote association with the intact in-situ structural and anisotropic condition of the clay deposit?

c) At rest earth pressure coefficient K'_0. Profiting of this important podium I begin by appealing that consistent with adopted ISSMFE symbols, we adopt the K'_0 to emphasize that it is a coefficient of effective stress. In my experience a high proportion of professional accidents has been due to confusing the K'_0 as applicable to total stress.

For decades there was almost no interest in K'_0. Yet the very laudable analytic interest in deducing the *"At Rest"* condition had to be vented. And it was a time of rigid-plastic theorization when the only parameter available for defining and distinguishing "sands" was the failure angle of *Repose!* Roughly equivalent to the friction angle ϕ', in infant simplicity. And the mathematical idealization had to establish "at rest conditions", and even "at birth conditions" based on mathematically mobilized proportions of the single parameter available.

The irony of the silently developed fifty-year old situation suggested joking with analogy to the art-science of fossil studies, and with an extreme form of inverse astrology. Are not the living features of prehistoric animals postulated from the imprints (Kerisel, 1985, and the discipline of ichnology) or the only other available parameter which is their remains at final rest within mudstones? However, if you believe in astrology, you may give some furtive credence to the men that depending on the day and hour of your birth ("at rest" starting condition) your day's behaviour and prospects may have been predetermined. Yet, very few, if any, astrologists will go to the point of predicting a predetermined manner and time of death, as death is, after all, subject to Extreme Value Statistics. Now, what would such believers say of the astrological hypothesis of establishing the "at rest" conditions of birth and life, as predetermined by the fixed parameter that describes your condition of death? What is ϕ' except a parameter of definitive failure, assumed constant, a soil property defining the soil's death?

d) Should we not concur that very broad dispersions in such examples as cited in (b) and (c) above are quite justified and insuperable under the present approaches? That they still persist because our conservatisms make them inconsequential? That the only hope for improvement might lie in stopping for a hearty laugh, and starting off towards new approaches, with lateral thinking using the greater number of parameters presently available?

9. CONCLUSION

The world needs engineering, and economical engineering, more hastily than additional glitter of science. Such was the prime life example of Odair Grillo. Civil and geotechnical engineering are challenging and exhilerating pursuits on their own merit, and question their false lovers who really woo Ph.D theses and publications in geosciences. It is not by the perspiration and midnight oil of Ph.D theses but by the sweat and blood of on-site professional decisions, taken, suffered, and corrected, that civil-geotechnical engineering practice is anointed. In a period when the stock of written knowledge and collective undiscriminating memories are multiplied, recorded, and diffused as never before, selective forgetting becomes more than ever a prerequisite for sanity. If we look and see correctly, the book of executed foundations is much greater and more revealing (in the scales of what really matters) than the prolific writings that have dominated the field and us: wherever necessary we can fit-in estimated parameters and data, checkable by parametric variations, to support our reading between the lines.

For better setting our line of sight, it is imperative that we keep revisiting our origins and reappraising our goals of service to society. We move imperceptibly from finding adequate solutions to significant problems, to seeking illusory refinements of solutions, to finding problems in solutions, and to seeking problems in problems. *Quo Vadis, Geotecnica* ? As has been ably affirmed, "throughout the history of development, the illusion of knowledge has been a greater obstacle than ignorance, and the feeling of knowing, more appealing than knowledge". The rate of change of physical solutions (investigations, foundations,instrumentation, etc.) has been so much greater than the rate of digestion of their effectively applicable results, that most net effects are the undermining of adequate analytical solutions and Babel. Let us watch for time irretrievable and haste unpardonable.

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