REVISITING OUR ORIGINS
REVENANT A NOS ORIGINES

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SYNOPSIS : A retrospective analysis of geotechnique is attempted, with reference to Terzaghi's principal exhortations, and emphasizing differences between scientific developments and engineering decisions. The latter must be based on dispersions affecting statistical evaluations of risks, and of costs to Society. Predictions vs. performance challenges are analyzed in this light. For illustration, two current professional problems are used, as embankments on soft clays, and building foundations, the latter concentrated on piles. Disadvantages from the plethora of dispersive publications are cited, in calling for repeated revisitations of WHAT IS ALREADY THERE, in respect for time irretrievable and haste unpardonable. In practice let us preach prediction, prescription, probabilities of problems, prudence, painstaking perception of performance, pardoning the price of pride and playful prodding of premises (purposeful alliteration alike raga recurrence).

RÉSUMÉ: On propose une analyse retrospective, en prenant pour référence les exhortations principales de Terzaghi, où l'on met en relief la distance entre les progrès de la Science d'une part et les décisions de l'Ingénierie de l'autre. Celles-ci doivent prendre en compte les dispersions qui affectent les évaluations de risques, et les coûts pour la Société. C'est ainsi qu'on analyse les défis des comparaisons des prévisions et des comportements constatés. A titre d'exemple on examine deux problémes courants, les remblais sur sols mous et les fondations d'édifices, y comprises particulièrement, les fondations sur pieux. On cite les inconvénients dus à la pléthore de publications éparses, tout en incitant a que l'on en revienne maintes fois a la magnificence de CE QUI EST DÉJÀ LA, par respect pour le temps irrécupérable et la hâte impardonnable. Dans la pratique, prêchons la prédiction, la prescription, les probabilités des problémes, la prudence, la perception pointilleuse des performances, tout en pardonnant le prix du prestige et en nous plaisant à pousser les prémisses.

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

(Alfred, Lord Tennyson)

When I accepted the honour of delivering this address, many deep and conflicting emotions confronted me within the responsibility and the challenges. To all of us it is an occasion suggestive of historic meanings, as we gather once again in the tropical geotechnical area, but this time in a setting of respected ancient and different cultural challenges to us as Civil Engineers. Moreover, I need hardly mention the more personal significance of this occasion on returning after half a century to the areas whence, from junior college, my chariot of destiny took me across the oceans. It was in craving for knowledge and action, to find, fight, fail, fall and fell; and it was specific to civil engineering as embodying decisions despite doubts within the zest of intended optimized service to Society. All of this has caused me to reflect on the imprinted past and forging present.

In the world of technological advances, spurred by doctrines of certainty transmitted by the "WORD* revealed" and the notions of ONENESS, it seems both sobering and refreshing that geotechnique maturing should sip also of the doctrines of the MAYBE, of the dazzling wondrous VARIETY of creation sensed by SEEING WHAT IS ALREADY THERE, wherein is denied any hierarchy, and any comfort of certainty, or of, the intention of being right being ASSURED OF THE RIGHT TO BEING RIGHT**. My two predecessors (T.W. Lambe and Kaare Hoeg) at this quadriennal address rightly described the guaranteed success of tackling major projects in a systematic manner. I venture to REEXAMINE OUR ORIGINS AND COURSE, in the light of my MAYBE VISION, attempting to assess the high costs imposed on fast-growing societies of the developing world by our presumed professional obligation to be right despite ignorances multitudinous.The opposing aims are maximized safety and technical prowess, at minimized cost. Appropriately for Engineering "in media virtus" may be a lie, as expressed by G.K. Chesterton in "Orthodoxy": "in media mediocritas"; the virtus derives from well-balanced compensations of the extremes.

KARL TERZAGHI, 1925

Roughly 70 years have elapsed, essentially 3 generations, since the gestation of Terzaghi's book "Erdbaumechanic auf bodenphysikalisher Grundlage" (Vienna, Deuticke, 1925). He was annoyed at the ineffective qualitative GEOLOGY of that time, and eager for action and responsibility within a context of deterministic physics and mechanics. At the same time he was guarded by persistent self-questioning and patient-pertinacious personal experimentation (that is the label of humility in greatness) in primitive laboratories ([1]). He gave two fundamental contributions to Civil Engineering that have catalyzed much of mankind's modern development and have influenced many other fields:

a) The Principle of the Effective Stress.
b) The life example of incessant zest at facing new challenges as an ENGINEER of practical decisions for PROBLEMS OF SIGNIFICANCE (which is quite relative across the world of civil engineering, dependent on the user's priority needs).

I dare emphasize the important partnership of the two in their attraction-repulsion cooperation. The first, a DEFINITION, has the potential for sterile fixity of DOGMA, expressly in order to open the doors to multitudes of test

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* *"In the beginning was the Word, and the Word was with God, and the Word was God"; " I am Jehovah (Yahveh) i.e. HE WHO IS"*

** From the Rig-Veda "Hymn of Creation" (taken from Daniel Boorstin's "The Creators")

"He, who surveys it all from highest heaven
He knows - or maybe even he does not know".

([1]) Two points merit emphasis: (1.1) the brain's capacity at extracting the essence out of a set of rough loosely-interrelated results is often such as to dispense with second-order sophistications, and even to do better without them, because their dispersions might cause distractions; (1.2) No matter how modern and "precise" an innovative laboratory, its results always require experience adjustment from the user, and for the innovator the achieved level always results insufficiently satisfying, exacting continual revision, thwarting experience accumulation.

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trials of provable usefulness within the desired level of SIGNIFICANCE ([A]). As one of several well-known analogies we could say that Dalton's Atomic Theory (1810) established an irrefutable NOMINAL BASIS for chemistry's early big strides, tested-proved with avidity and encouraging success: fortunately, however, there occurred the follow-ups of Mendeleev's Atomic Chart and the continuing incremental fertility of the Divisible-Atom, pursuing the interplay of science-engineering, without destroying the building founded on Dalton's behavioral Indivisible-Atom chemistry.

Figs.1(a) and 1(b) intend to illustrate schematically three important facts. Firstly the conclusion concerning continually increasing dispersions both deriving from "scientific" laboratory investigations (1a) and from field in situ tests (1b) that have introduced new instruments and procedures at rapid and accelerating rates. Secondly, the fact that the urgency of publishing partial results can only generate confusion, and scatter of professional knowledge and decision. Thirdly, the fact that almost never do publications report on PROBABILISTIC CONFIDENCE BANDS (CB, of dispersions) which are indispensable for engineering decisions "on the safe side, to some judicious degree" as expatiated in Fig. 2. In LABORATORY INVESTIGATIONS, more and more parameters of potential interference are established for clear cause-effect relations, under the scientific principles of PARTIAL DIFFERENTIAL EQUATIONS. Thereby, to the keen reader conditioned by professional prudence, in ignorance of which of such parameters do/dont occur in successive strata in Nature, for his problem, the inevitable effect must be of incorporating all corresponding dispersions of which he is aware. In FIELD INVESTIGATIONS the misdirected focus has been on comparing one parameter vs. another, both analogously lame. While the primordial clearly-defined relationships between strains % and calculable σ', FOR THE PROJECT, remains as the only basic link in our engineering aim, we must shudder at the undesired trend towards widening the gap between perfectioning knowledge, and engineered decision: apparently only the neurological computer programmed under "experience and judgment" (always subjective, principally connected with only one or another of the available procedures) can effect the needed integrations with moderate success.

Within my present aim of REVISITING OUR ORIGINS I wish to concentrate on a third and most important guidelight owed to Terzaghi. Because of the trends imposed by the obsolescence-sociology of first-world industrial development, this is possibly the least recognized of Terzaghi's three guidelights.

c) The call for developing the CYCLE OF EXPERIENCE by SUCCESSIVE ADJUSTMENT OF PREDICTION, PRESCRIPTIONS, DESIGN AND PERFORMANCE OF THE PRODUCT offered by us professionals to our customer oriented society.

In true professionalism the problem has to be assessed from alpha to zeta, from geology to sociological and environmental impact. Compartmentalizations must be critically tempered in our art-technology, and only used to the extent that Society may be benefited and not harmed. Obviously because of changing data-bases and aims, the results of such periodic revisitations will be forever temporary and relative.

It is instructive to subdivide this third guidelight into two parts, because they establish the vectors for my technical message: Terzaghi's early error of professional diagnosis; Terzaghi's exhortations for well observed and digested CASE HISTORY OBSERVATION and analysis.

The above error is little known, and seldom, if ever, mentioned. I refer to Terzaghi's early very significant misinterpretation of the settlements of the M.I.T. building, as reported in the paper by Aldrich and Seeler, 1981, at the M.I.T. Seminar or "Past, Present, and Future of Geotechnical Engineering". I do so in order to pay tribute to his undaunted ability to "rise from falls" ([2]).

The following quotes, extracted as published, reveal the facts and thinking. "Terzaghi determined that the natural water content of the clay **increases**

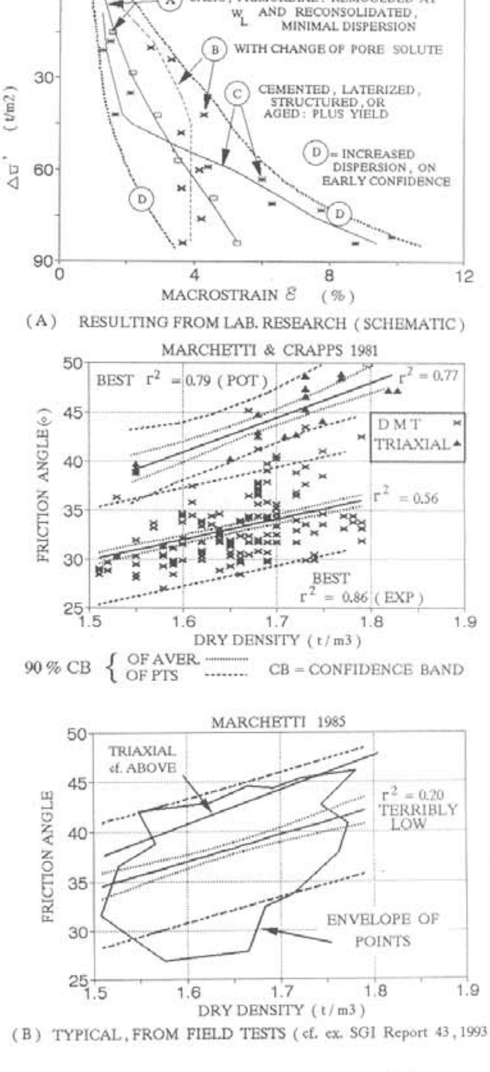

Fig. 1. Progress and rapidly increasing dispersions in geotechnique.

([A]) see in Annex.

([2]) As stated by Popper "Theories are only provisional: they can never be proved but only disproved. Scientists can approach the truth only by elimination; they can never say what the world is, but only what it is not". Cognizance of the continuum requires perception of discontinuity. Since failures are discontinuities in Civil Engineering trajectory, everybody concurs that we learn from failures. Quoting from R.F. Scott's 1987 Rankine Lecture, "Failure", "To substitute an ill-understood model of the world for the ill-understood world is not progress" (Boyd & Richerson, 1986), and "increasing resolution does not bring with it increasing understanding". Emphasizing the importance of cost, benefit/cost, and statistical determination of "fact", I question: what have we learnt from the doubly costly failure to learn from pre-failure indications and failures?

with depth, meaning that 'the percentage of excess water increases considerably with depth', and assumed that 'consolidation is still going on as a **geological** process' and, of the three causes for unequal settlements of the M.I.T. buildings since Dec.1916 (having reached 1.4-6" by 1923, and 2-9" by Dec.1963) Terzaghi concluded that "the settlements are essentially due to **lateral flow**, at fairly constant water content", "Because of sampling and test errors and imprecisions, he admitted extremely slow consolidation..., concluding that in 1929 it had not even started, when in fact by 1930 the primary phase had already finished". The interference of concept stands out with regard to the pile foundation design of the ENGINEERS, despite a test program of driving 91 piles and running 25 load tests, from which "the working values have been taken at about 1/16 in. settlement... It is believed that the effect of a difference in settlement of 1/16 in. in the foundation can be safely ignored". "A settlement of 1/4 in. was considered to be the limit of usefulness of a pile, and it was assumed that greater settlements than 1/4 in. might create conditions which would cause very unsatisfactory results. It was also considered necessary that the piling have a safety factor of not less than 2.5 based on the limit of 1/4 in. settlement".

Both serviceability and failure-safe criteria were clearly recognized, but using what data, theories and predictable performance? Have the M.I.T. buildings been in any manner impaired by their settlements which have grown to the order of 25cm ?

The obvious lesson stands out of the interplay between data and theory, both dependent on systematic and erratic errors, and subjective while singular: observation depends on the view of the observer, and on the resolution of the test data.

My intent is to emphasize that:

1) if a physical INGENIOUS ENGINEERING SOLUTION to a problem chances forth, it requires theoretical cross-examination, as well as adjusted confirmation in practice;
2) truths are nominal and variable, only validated within Confidence Bands by statistical analyses of sufficient numbers of similar cases, i.e. WITHIN FIXED UNIVERSES (the presumed fixity being largely subjective);
3) the goal continues to be the PROJECT and its economy-serviceability and benefit/cost ratio;
4) it could be argued that blatant fallacious dogmatizations have accumulated a much greater burden to Society by systematic conservatism, than would obtain from somewhat more pre-failure observations of significance, followed by the failures, and associated complete backanalyses; such richly documented case histories being thereafter repeatedly reanalysed.

In his Opening Address at the London 1957 ICSMFE (Vol.III p.55-58) Terzaghi expatiated on the central theme of his professional philosophy (already mentioned briefly in the Zurich 1953 ICSMFE, Vol.III p.76, in the anecdote about the old southern negro foreman who had learned the facts of life the hard way, "If you ain't got no college education, you sure do have to use yo' head"): sic., "the importance of never missing an opportunity to find out by direct observation, the difference between forecasts and the real developments... The prospects of a graduate to rise above the level of a technician are nil, irrespective of his innate qualifications, unless the following additional condition is satisfied. His subsequent professional activities must give him ample opportunities to COMPARE HIS DESIGN ASSUMPTIONS AND FORECASTS WITH THE REAL CONDITIONS AS DISCLOSED by the subsequent construction operations and the performance of the completed structures. Otherwise he lives, without knowing it, in a fictitious world".

We are living, not in a fictitious world, but in a world made ever more unattainable and expensive to our fellowmen. MANY OPPORTUNITIES MIGHT BE GIVEN OR RECEIVED; BUT MOSTLY THEY MUST BE SOUGHT AND TAKEN.

TRUTHS USEFUL TO GEOTECHNICAL ENGINEERING.

Do we make mistakes? Systematic, for some time, until corrected? And also episodic? Of course we do.

And are we inexorably subject to some degree of erratic error (statistical dispersion) within any fixed line of action (investigation, test, analysis, decision, execution)? Of course we are.

And will we ever rid ourselves of these realities? Despite subconscious hopes and illusions to the contrary (the "philosopher's stone" complex, and the still pervading deterministic science context) I emphasize the hopeful belief that indeed we will never reach such culmination. Thus my fundamental conviction is that in civil-geotechnical engineering we would make more systematic and efficient progress if we drastically reduce our judgments of cases under the prism of DISCREPANCIES TO BE ELIMINATED, and forever adopt the attitude of seeking COEFFICIENTS OF ADJUSTMENT to be used in temporarily validated working hypotheses: thereupon such COEFFICIENTS call for progressive improvement, principally VIA DECREASING DISPERSIONS associated with developing full-cycle methodologies.

Thereupon it becomes important to emphasize the significant difference to geotechnical (and Civil) Engineering between progressive improvement of the predictability of AVERAGE behaviours, and the progressive narrowing of the CONFIDENCE bands referred to statistical dispersions around the average. And the worst mistake of all, unpardonable, is not to tie our novel experimentation to the past, as a relay-racer who would sprint off without gripping the baton from his team predecessor.

Since our engineering actions aim at avoiding "UNACCEPTED RISKS", that is, choosing decisions on low PROBABILITIES of being insufficiently right regarding safety, Fig.2 has been prepared to emphasize that we do not necessarily benefit by improving definition of the AVERAGE (for PREDICTION of probable behaviour). If such improved average truth is accompanied by the disadvantage of having GREATER DISPERSION (because of more novel intervening parameters and too few data) we may really be penalizing the PROFESSION even while benefiting the scientific pursuit.

The truths, always nominal, that we have to use are statistical; this presupposes a "fixed universe" (mental, observational, and of design-construction); also, reasonable cause-effect relations of parameters considered; and, indispensably, a satisfactory number of relevant analogous data-cases.

Merely as an example consider the case of FILTER CRITERIA (FC) used for assuring the safety of dams against piping failure. Let us emphasize that it involves a problem of great concern to society, and ourselves as professionals. Piping failures are serious to catastrophic because of greatest probability of occurring under full reservoir conditions; and they continue to figure as one of the most frequent types of failure (B).

Using a set of research-test data more recently published under very authoritative authorship (Fig.3, Sherard 1984) I excuse myself for submitting it as no more than one out of many, irrespective of any justifiable questionings on both the concept and the conduct of sundry filter tests.

The intent is to show how inferences may result widely different by use of a different mental model, both regarding method of analysis, and as conditioned by intent. There are many details to be reconsidered about the tests themselves, their aim and conduct: but that is not within the scope of this presentation. My point is that having accepted the data we should analyse them together with the broadest possible data bank, and in the light of minimum statistical principles. The publication tabulated the data, and in principle, limited itself to concluding, possibly under the conditioning intent, that "all" the data preserved the classical Bertram-Terzaghi (1940) filter criteria as continuing to be satisfactory and sufficient. Fig.3 however synthesizing the same test data in one possible graphical form would already lead to some MAYBE obligatory curiosity.

The test data referred to clear failable vs. non-failable boundary (in the specific test procedure) of the D_{15F} / d_{85B} INDEX of the F=filter and B=base grainsize curves. For a first try these values are plotted in Fig.3 vs. the respective d_{85B} diameters. The striking first observation is that there is too broad a dispersion of results (possibly due to an oversimplified single index) for a PRESCRIPTION on so serious a FAILURE PROBLEM. For instance,at a d_{85B} grain diameter of 0,07mm the "well-defined boundary" developed under FC indices as widely different as 10 and 55. Moreover, this unpretentious graph would insinuate a trend of the FC index being increasingly unsatisfactory with increase of d_{85B}, base material grain diameter to be filtered. Seemingly contrary to "intuition" (dictated by what mental model ?) this should call for investigation-explanation. Did the

(B) see in Annex

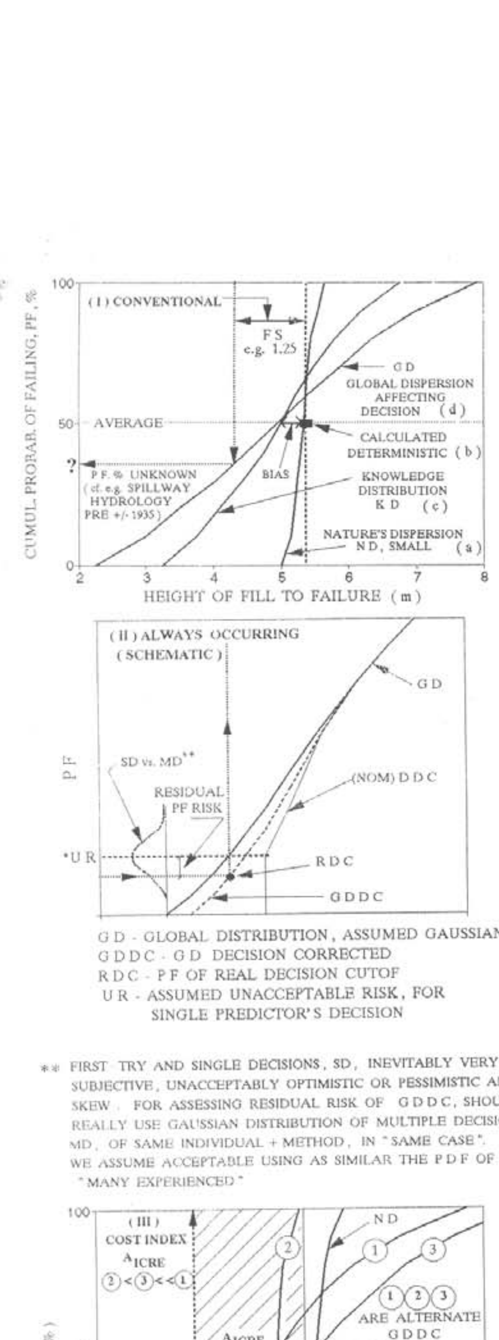

Fig. 2. Example of engineering decision viewed statistically

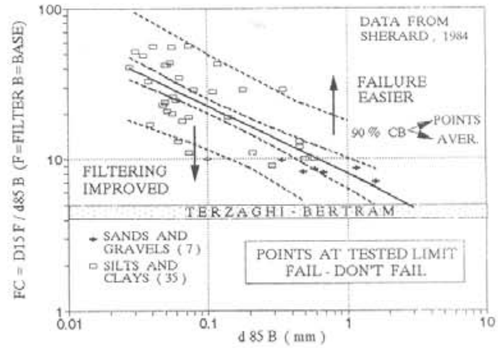

Fig. 3 . Seepage tests on filter criteria viewed graphically & statistically.

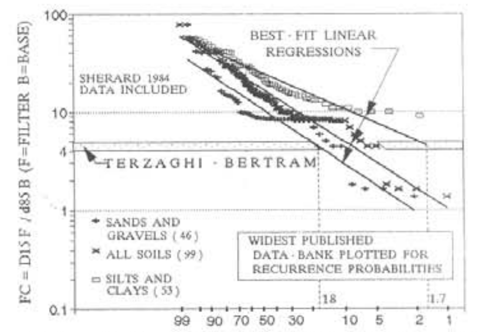

Fig. 4. Seepage test data on filter criteria viewed probabilistically. Shocking indications?

research close the serious problem, or did it really open it all the more to any inquisitive MAYBE mind?

Fig.4 incorporates the same test data together with the broadest possible data-bank of analogous published test data (including, for instance, Ramganga Dam, 1972). The "failure condition data" were separated as pertaining to sand-gravels and silts-clays, and are plotted in Gumbel extreme-value probability paper of maximum flood recurrences; data assembled irrespective of different testing criteria and conditions. The postulation of piping being a phenomenon of extreme-value statistics is my underlying mental model: subject to check and accept/reject. The trends and regressions that result seem of interest and impact: the conclusions would derive that Bertram-Terzaghi prescribed criteria would involve 18% and 1.7% probabilities of piping failures in sand-gravels, and in silt-clays respectively.

The analysis of those lauded research efforts could be pursued much further, but that is not the present intent and scope. We recognize that besides our "truths" being conditional, nominal and statistical, all design is dependent on PRESCRIPTIONS, of what is anticipated to be PROBABLY AVERTED, and not on the (average) predictable probabilistic truth of what will happen. On so serious a problem as catastrophic flooding failures of dams due to piping, we cannot rest satisfied with such low probabilistic guarantees (further increased by the dispersions on the regressions) on a phenomenon logically understood to be dependent on other parameters besides the FC INDEX of 1940. Further along we shall assess the absurdly different COST-OF-RISK criteria that have become generally implicit in such other routine sample problems as EMBANKMENTS ON SOFT CLAYS, and FOUNDATIONS.

DESIGN REQUIREMENTS, AND QUEST FOR KNOWLEDGE. PREDICTIONS AND PERFORMANCE IN FOUNDATION ENGINEERING.

Very rough estimates taking into account growths of population and, principally, of irreversible social requirements, have suggested that the annual expenditures in foundations in the world might well reach figures of the order of US$(100-400)$10^9$. It is, indeed, an industry in which investment well directed should result in great savings, all the more desired because costs are buried unperceived. There is a significant well-recognized bias in decisions, of much higher preference for avoiding loss than for seeking gain. Without any effort of imagination one can estimate that this bias becomes much greater when the loss is buried and distributed among millions unaware, than when it is blatantly concentrated on singular cases, and exposed to criticisms and highly punitive law suits. It would stand to reason, therefore, that Foundation Engineering Practice should have grown progressively more conservative and expensive (buried, generalized, unnecessary incremental costs) in proportion to the singular cases of courageous design, and the consequent doubly singular cases of blatant failures. The thousands of uneconomically and conservatively conducted routine cases establish a chronic epidemic while deriving a vicarious pride and prestige from the occasional publicized over-meticulously conducted big projects... much as in many a society and religion the dearths of the multitude are sublimated in the ostentatious wealth of the leader.

Such trends could only be aggravated by the complex of disparaging comparison between concepts of successful research and development in Academia and the synthetic Industries, versus the risks of Engineering in Civil and Geotechnical interaction with Nature. The exaltation of KNOWLEDGE as static-deterministic-scientific-mathematical pushes towards indefinite search for "knowledge of Nature" in her micro-tendencies (despite the admission that we never know the "status quo" but only alterations or differences thereof). If knowledge is rightly exalted, why should knowledge of ENGINEERING (i.e. the artisan pursuit of economic moulding of Nature) be any less prestigious than the infinite search for the infinitesimal behavior trends in IN SITU INTACT SOIL ELEMENTS? The atrophy of the very concept of Engineering beckons us to reevaluate some major milestones past, and their undercurrents, occasionally misdirected.

Foremost among these milestones are the rare and expensive prototype tests and Prediction vs. Performance Challenges, which merit summary cross examination.

T.W. Lambe's Rankine Lecture, 1973, rightly emphasized the preferences for Type A Predictions, and raised some possible (and all-too-frequent) suspicions against types B (really the basis of the Observational Method of design adjustments) and C_1 ("one must be suspicious when an author uses type C_1 predictions to 'prove' that any prediction technique is correct"). Systematic regretable simplifications and misunderstanding of those proposals, together with the psychology of seeking laurels at a professional Olympiad, have done a great and growing harm to our profession, which relies entirely on a patient progressive adjustment of estimates TOWARDS NARROW-DISPERSION REALITY, at MINIMIZED INCREMENTAL COST AND WASTE, by Bayesian prior to posterior probability adjustments.

Any type C condition can be re-established as a renewed type A case, merely by making the existing case anonymous, with all identifying characteristics well altered (without altering the essentials of the geotechnical data), and with the known end-result kept secret.

Moreover, if we are honestly seeking systematic advance of our technology, there are irrefutable arguments for REVISITING OVER AND OVER AGAIN the type-C field cases, transformed by disguise and anonymity into periodically repeated type A prediction and Design-test cases on the SELF-SAME DOCUMENTED NATURAL BEHAVIOUR.

In any process of adjusting ourselves to a goal (by skew-Bayesian successive adjustments of prior and posterior probabilities of improving the aim at the target-center as well as narrowing the dispersion around the dead-center) the starting obligation is to maintain the WELL-DEFINED GOAL FIXED, IDENTICAL. In principle in the face of such cases there are 4 principal tests involved: (1) NATURE'S BEHAVIOUR, indelible, an asset invaluable as a single crown jewel, the HOPE DIAMOND, not only because of high costs already spent, but much more, because of time irretrievable; (2) Our capacity to investigate and observe; (3) Our capacity to analyse, forecast, and decide, with justifiable confidence in our consequent results and decisions; (4) Our capacity to educate ourselves, measurable by systematic evolution of improved procedures, ever more widely applicable and convincedly accepted. It is indeed a slur on us that in a profession most deprived of the conveniences of adequate-size model and prototype testing, and in a world dominated for over 50 years by the "cybernetics" of rapid yes-no refining of choices, we have not absorbed "into our groins" the lesson of such Bayesian evolution of experience.

In fact we are obliged to conclude that by having failed to draw the psychological and sociological lessons from such Type A field trials, which obviously had to give frustrating and disperse results, the net effect has been unfavourable, and detrimental to Engineering's service to Society. The incentive to search for the scientific "philosopher's stone" solution, the EUREKA COMPLEX, has only been spurred by the inabilities disclosed. Easier and more attention-attracting than to work at gradually improving our existing instruments, parameters and methods, has been to hasten to open more novel proposals, each and all inevitably born naked.

By references to Fig.2 we should emphasize that every major field test trial should be used not merely as a Prediction Challenge case (ability to hit the Average Predicted into equivalence with Performance Reality, within a minimal dispersion) but even more as a check on our benefit/cost Design Decision ability. For the latter the decrease of Dispersion is much more profitable than the improvement of the Average: inasfar as possible the data from the Prediction Challenges discussed below exemplify pointedly how our geotechnical engineering has foregone the dominant obligation of concentrating on both technical and economic improvement to Society.

EMBANKMENTS ON SOFT CLAYS.

This is a very significant technical and economic problem, with mankind mostly settled around water: it has been faced since early geotechnical history as requiring our concentrated developmental attention. Sundry directions have been, and can be, taken in this discussion: among them the many reinforcing treatments, and the different serviceability criteria on tolerable settlements and displacements. I choose to concentrate on the oldest geotechnical field test, Väsby, Sweden (Terzaghi, Jan.16, 1946), the 2 internationally publicised Prediction vs. Performance "challenges" (1974, 1989) on the limit height causing failure, and the vanguard investigations conducted at the Bothkennar Test Site, U.K. (Geotechnique, June 1992). Failure has always been abrupt, and undisputably observed: therefore the ENGINEERING AIM of minimally averting failure can be well defined whereas most other serviceability and computational parameters are rather undesirably intangible for this presentation's purpose. Meanwhile the soft clay generally has such low ACTIVELY EFFECTIVE STRENGTH, so close to zero, that one can pardoningly justify the historic concentration of interest only on the clay: yet, on revisiting the issues one should correct this understandable error, because under scientific-technological principles, well-proportioned attention should be given both to the CAUSATIVE FACTOR (the fill) and to the AFFECTED/REACTIVE CONTRIBUTOR (the clay), all the more so since both are obliged to behave within their ranges of wide statistical dispersions at close to zero.

Väsby, Sweden, 1946.

Terzaghi's report recommended the field tests at Väsby "in such a manner and on such a scale that they will inform us on all the factors which determine the behaviour of soft clay under the influence of temporary and permanent surcharges. Foremost among them is the secondary time effect. Once this knowledge is available the preliminary investigations for the construction of a flying field on soft clay in any part of the country can be reduced to routine soil tests which can be performed for a short time". Rather deterministic and confident regarding "all the factors", "any part of the country", the credence to "routine soil tests", and the important professional problem of extrapolating from short-term to long-term behaviours to be predicted. Every such point quite understandable in historic retrospect.

However, on revisiting this milestone effort after 47 years of the possible infinite benefit/cost ratio, because of being the singular case and because of the elapsed time irretrievable, what do we find? Shall we repeat the prototype test to be authentically Type A, and await till the year 2040 to be in a better position? How depressingly unscientific to repeat the starry-eyed belief that

NOW, yes, WE do have the right to claim a grip on "all the factors". Moreover, in the oblivion gradually sentenced to the secondary time effect, declared as "FOREMOST among... factors..." (by the father of primary consolidation theory) how bitter to reflect that academia cannot devote interest to really long-term problems, while design professionals on their side can defend themselves all the better from liability suits and guilty consciences behind the mysticism curtain of collective ignorance.

The Väsby test fill is eloquent in proclaiming the obligation to repeated revisiting. A careful examination of the records serves as a most eloquent lesson on three facets: the importance of viewing our endeavours historically; time irretrievable in prototype observation; the great cost and value of Nature's behaviours well evidenced and remaining available for successive reanalyses while our methods undergo changes. During the recorded trajectory every single revisitation has taught something technical, but, above all, it should have taught the message of our need to return over and over with our erroneous and dispersive visions, to try to improve rational adjustment to the crystal-clear course of Nature's behaviour. Some of the most illustrious institutions and geotechnical leaders have been involved, both in the initial effort, and in two important revisitations, around 1966-'69 (after 20 years) and 1979-81 (35 years), besides a partial one on sampling, 1985; and it behoves us to emphasize the obvious and MAYBE reasons why we have to continue correcting ourselves. Facts and questionings require being proclaimed aloud: the persons behind them dispense identification, as mere laudable instruments of our cumulative service to an unfathomable destiny.

The initial program envisaged simultaneously two very distinct purposes: the practical engineering purpose of observing long-term settlements (as subject to viable accelerated anticipation and control, or not); the theoretical purpose of interpreting the SETTLEMENT BEHAVIOUR via the original idealized consolidation theory, or a generalizable revision thereof.

It is impossible to recount herein the series of insufficiencies and deficiencies reported, as resulted in the 20 year and 35 year Revisitations. Many are the lapses of investigational logic, associated mostly with the vicious circle of begging the question (lifting oneself by one's shoe laces) under wishful thinking. For instance, the interpretations on the presumed separation between primary and secondary consolidation are tied to the historic first-order pragmatic procedures of Taylor and Casagrande graphical interpretations in oedometer tests without pore pressure monitoring.

In short, and principally, as is inevitable, there is always a lack of superabundant redundancy of tests and/or instrumentation-monitoring, to establish statistical dispersions. And there is always a lack of superposition, at the same moment (same operators etc.) of the HISTORIC vs. PRESENT optimized sampling-testing-interpreting.

If the theory of "self-induced primary consolidation process" has been firmly hypothesized, and "this process is likely to continue until the clay structure re-established itself", the controlled experimental avenue should have been promptly followed. The theory is much more profitably confirmed under precise laboratory control.

Meanwhile, in the field the theory postulated a process likely to continue at so significant a constant rate as 6mm settlement per 100 days (!) until geologic re-establishment of stability, one is forced into incredulity; discrepancies are to be noted, inasfar as:

(1) the natural ground is concluded to be stable (N.B. the 120-day observation would be unconvincing in terms of geologic or "secular" time);

(2) the 30cm undrained fill is monitored to have "experienced no settlement during the life of the load test" (22 years, June 1946 - Sept. 1968);

(3) but regarding geologic dating, only the "lower clay's bottom" is dated, as 7900 B.C., while the more relevant dating attributable to the "upper clay", which is merely described as "post-glacial... relatively young", should be indispensable, and easily obtainable.

The fact is that after installing new highest precision modern piezometers, duly calibrated as to controlled responses IN SITU AS INSTALLED (by external tubes permitting injection or extraction of water at cell-tip), it should be highly profitable to add a thickness of fill on top of the existing one, to check on incremental behaviour, now better documented, regarding fill pressure and the developed excess pore pressure.

In retrospect we find ourselves most surprised at how little research data was collected on the fill, and on the theoretical vehicle for interpretation of secondary compression, which is the excess pore pressure: the explanation would seem to lie in the expectation that the field test could be based merely on comparison of analogous fills for "drained" vs. "undrained" behaviours of the clay, and on the observation of the end-result of settlement rates after fulfilled theoretically anticipated "total consolidation dissipation" of excess pore pressure. Research dominated by confident deterministic expectations could be pardoned in the infancy of the profession, but should have been recognized and corrected by now, in appropriate revisitations.

Having defaulted on some fundamental principles of progressive investigative adjustment of our transitory methods to the observed SIGNIFICANT REALITY, and the research having been temporarily finalized with an unusual theoretical conclusion classifiable as a THEORY OF A SINGULAR CASE, this field test presently stands at the extreme of a ZERO benefit/cost ratio, instead of being deservedly taken to the very high benefit/cost ratio corresponding to its AGE IRRETRIEVABLE. I dare pronounce that the many internal inconsistencies call for redress and for one or more additional revisitations.

As a startling beginning I venture a suspicion that the very poor definition of the quality of the gravel fill, coupled with a MAYBE wrong intuition on changes of stresses accompanying the increasing settlement, can implode the very basis of the published hypothesis. The entire interpretation arises from an assumed calculation that as the fill settles below groundwater level (taken as fixed) the "submergence" would progressively REDUCE the applied (would-be) effective stresses causing excess pore pressure and ulterior settlements. The intuitions regarding the principles of Archimedes are ingrained: who would stop to reconsider the specific case, in face of so undisputable and elementary a calculation?

Are we not compelled to such reconsideration in the face of so very important a theoretical and professional problem as the secondary or "secular" compression in field conditions?

In the published Report (1981) the "causative factor", the gravel fill loading is too minimally described: "The western half of the fill was placed by free dumping without compaction, while the eastern half was compacted after dumping. As a result of the method of placement, the western half of the fill was slightly higher than the eastern half. However, the magnitude of the load on the whole area was believed to be the same. The unit weight of the gravel fill in its uncompressed state was determined to be 1.7 t/m³". From the thesis that generated the Report one does not extract additional significant information.

I would conclude that presumably not only the gravel fill was rather loose, but understandably almost dry. Let us assume percent saturation and water content of the order of $Sr = 15$ and 3.75. Obviously as such a fill submerges, its Sr would increase to about 95%: thus the unit weight of the submerged thickness would increase to 2.01 t/m³. Note that the intuition on decreasing pressure with submergence arises from compacted clayey fills that start at Sr $\approx$ 95% and would hardly increase in Sr at all, or not more than 2-3% (cf. the need for back-pressure saturation).

Thereupon we refer to Fig.5 and go back to first principles of "prospective" effective stresses as total stresses minus pore pressures. With a constant ground-water level, at any depth z of a soil element the pore pressure remains constant. Assuming "no" lateral displacement of the clay above a given point, as compression occurs the total stress due to the clay remains constant because of the increase compensating the ΔH compression. As far as concerns the gravel, repeating for times $t = 0$ and T, correponding to X settlement, the applied total stress only increases linearly until the entire gravel fill is submerged (2.5 m settlement). The comparative profiles (A) and (B) of Fig.5 should clarify the reasoning, and the graph (C) indicates the changing total stress with settlement assuming a constant groundwater level.

Incidentally, the same MAYBE REVISITATIONS would apply also to the invaluable Skä-Edeby test fill, 1957. One would conclude that, to begin with, the data on the fill should be greatly incremented and improved; a very easy task. Moreover, considering the but modestly successful piezometric data of the past, and the enormously improved modern instrumentation, I vouch that Terzaghi must be asking that we should add another meter or so of fill, to confirm or dispell the maybe theories.

The M.I.T. 1974 "Foundation Deformation Prediction Symposium".

It may seem unfair and sterile to return after 20 years to that milestone case, but it is from such markers of the past, freely reanalysed, that we must develop our collective Experience, especially when, as in any first-try, there

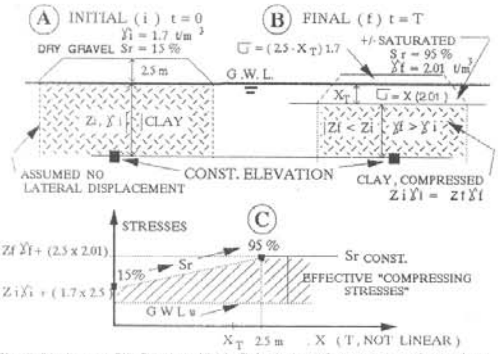

Fig. 5. Väsby test fill, Sweden, 1946. Calculation of stress increasing and not decreasing with time

is the greatest tendency to misjudged orientations. Those were the days of concentrated faith and effort on effective stress analyses, computational modelling, finite elements, normalized behaviour generalizations and constitutive equations, greatly improved testing and instrumentation precisions, and the PROJECT SERVICEABILITY AIMS focussing on deformation. In fact, the Symposium's name was Prediction of Foundation DEFORMATION, although inevitably the most salient feature shifted to being the neat FAILURE, the only significant and clear-cut behaviour.

Once again the oft-mentioned clear description of perfectly defined "brittle" FAILURE stood out as the fly for a sharp-shooter's marksmanship (3). It is clear that in this case of homogeneous clay deposits Nature's behaviour is "theoretically" crystalline as regards failure, whereupon any discrepancy or dispersion in our prediction lies squarely and only on our shoulders, and not on the oft-slandered geologic erraticities. In fact, as was shown on Fig.2, Nature's behavioral dispersion is very much smaller than our capacity to quantify it; our task is both to approach the Average reality of PREDICTION = PERFORMANCE, and, for economic design decisions, to decrease our much wider dispersions.

Meanwhile, both the aims and the conduct of the field test were too broadly-embracing and undefined as regards "performance of the foundation during and after construction": scientifically one should ever remember the partial-differential-equation principle, of aiming at one target at a time, and significant; professionally one tunes in on experience at what matters, which would be, in a nutshell, end-of-construction transitory unstabilization potential, and/or long-term after-construction deformations. One should avoid a confusing mixture of the two, that can only hint at a field test aimed at matching an idealized theoretical thesis, with but left-handed attention to the typical professional engineering problem and the need to tie back to digested experience.

For the present purpose of submitting how very much was lost in that case and could still be progressively regained by revisitations, the results summarized in Fig.6 and 7 should suffice. Some striking facts of importance to ENGINEERING DECISIONS (the accept-reject prior cutoff in the knowledge distribution, cf. Fig.2) may be summarized:

a) The 10 learned predictors (more documented than, say, 98% of typical similar professional cases (4)) used widely different personalized theoretical approaches, none of them adjusted to practice via case histories, and essentially all with such deterministic unfounded bias (optimism or pessimism) that mostly they did not individually straddle across the average or the observed result. (Fig.6 A).

If the client had decided to pay 10 times the (rather exceptional) design cost, and to average the 10 recommendations, by fluke he should have ended up with a good project.

As shown in Fig.6 B, a cheaper design, of equivalent average and lesser dispersion, would have resulted from a few hours of "feel" by all the 26 members of the audience: strictly speaking, however, this should also be recognized as another fluke, because of other factors, some important and singular.

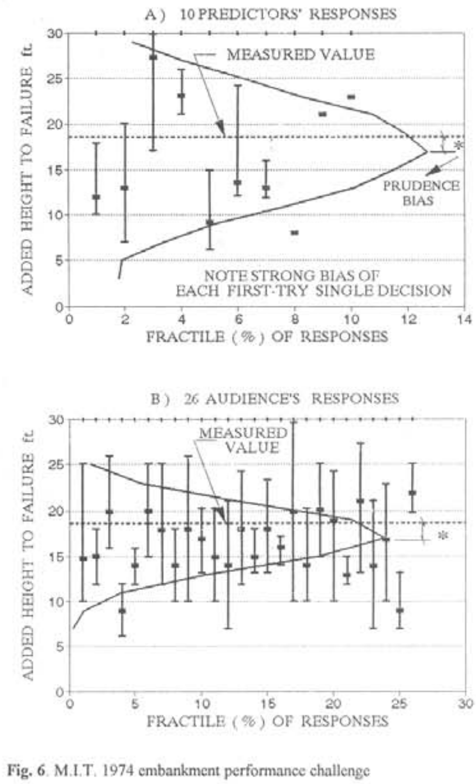

Fig. 6. M.I.T. 1974 embankment performance challenge

b) As regards prior professional experience, it should be noted that the proposed case was quite NOVEL. It would not appear that any previous (or ulterior) embankment on soft clays had been designed on any basis other than FS with respect to FAILURE. No "end-of-construction deformations" had ever been of interest (in comparison with long-term settlements, secondary compression, maintenance etc., cf. Väsby). No designer had ever considered monitoring construction-period deformations and piezometers, to accompany pre-failure indications. The Type A prediction was thus a challenge on untested and unadjusted theoretical presumptions, suggesting acceptance of "data" as factual, at stationary face value, stripped of historical transience.

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(3) "Early in the morning... a failure of extraordinary proportions occurred. Within minutes... crest to drop about 30 feet and the sides to heave as much as 14 feet. ...No surface cracking was noticed the previous day, nor was a clear indication of impending failure obtained from the field instrumentation. ...Failure occurred to both sides..."

(4) One opening statement was "The major cause of inaccurate predictions is faulty and insufficient data". Faulty they always are, to greater or lesser degrees, and intimacy and experience are called to compensate. In the face of professional practice, "determinedly misdirected" might be a more realistic qualification than "insufficient".

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c) Regarding such acceptance of test data (e.g. undrained strengths) at face value, Fig.7 summarizes two extreme graphs of heterogeneities quite beyond

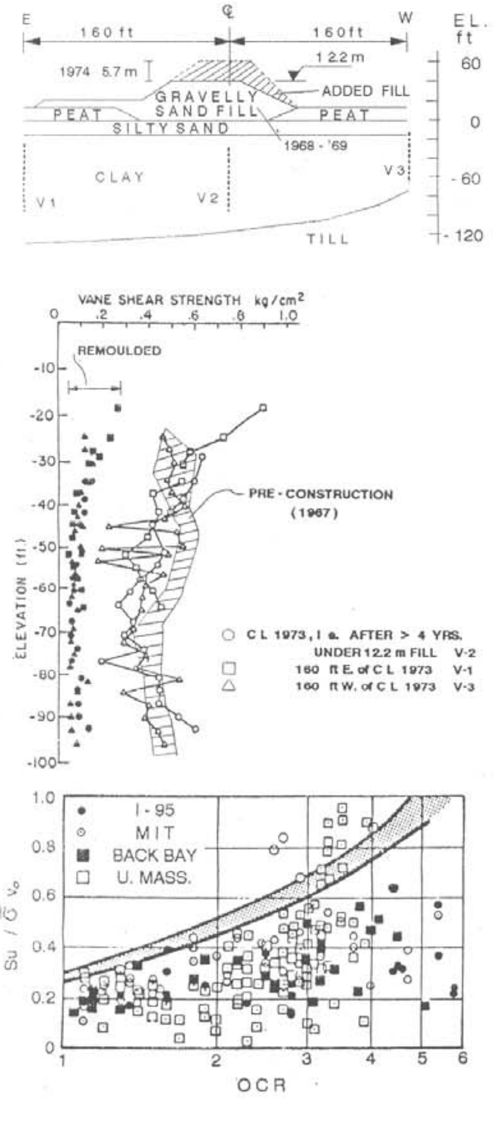

Fig. 7. M.I.T. 1974 tests. Examples of extreme erraticity of data, some contrary to logic. Interferences of equipment, procedures, personnel, to unusual degree

reason or acceptability. One notes the lack of any consistent attempts to "correct" for Sensitivity-remolding, boring-sampling-handling disturbances, sample and specimen quality as reflected in stress-strain curves etc.. In qualifying a sample merely as a (e.g.) "5-inch diameter undisturbed sample" the concern for such historic dictates as in Hvorslev "Subsurface exploration and sampling of soils for civil engineering purposes" (1940, ASCE) were neglected.Incidentally, the predictors did not express advance complaints, or desire for the conventional samples-tests (however poorer) to which their experience would have been adjusted.

d) The 2-step embankment filling, firstly of 12.2m height (Apr. 1968 to May 1969, with winter interruption Nov.15-Apr.15), and finally, five years later, of the 5.7 m increment in "late summer 1974" (to failure, 20/Sept/1974) constituted another unusual complicating factor, obviating any "model-to-prototype" Bayesian adjustments. Moreover such adjustments could only be viable if the monitored parameters were significant, and pursued the same "laws" of phenomena in model-to-prototype evidenced behavior.

e) From an engineering standpoint the most striking fact was the absolute lack of attention to the fill itself, both as the basic causative factor, as having reached a thickness of up to 17.9 m, and as having nevralgic strength and "brittle stress-strain" behaviors at overburden stresses close to zero, poorly quantifiable except in UU "quick" tests.

The 8 (only!?) field density tests varied between 1.74 and 2.20 t/m³, a ± 11% variation around the mean, leading to the same variation in applied pressure: however, the denser conditions are coincident with much higher strengths (at low stresses). And the fill's strength testing was limited to six CD(!?) triaxial tests, with possibly nominal effective stresses depending on suctions. Many more points may be made, calling for profitable reassessments (not all of them criticizable as of hindsight) of this case in which Nature's behavior was so definitive, and ours so very poor, and passable by fluke. It would be unfortunate if different "schools" should pursue their separate paths, heedless of each other's comparative advantages, and, especially most regrettably, heedless of the need to adjust to the only valid test, which is to improve technical-economically on the design solution for Society.

Kuala Lumpur K.L. 1989 trial embankments.

The type A prediction challenge in this case was better oriented with regard to typical design decisions. Firstly, the limiting height to failure, necessary for a cutoff decision on PF%. Secondly, for the situations considered beyond the acceptable height with its risk, the challenge to specialized ground treatment organizations (consultants, specialist contractors, and suppliers of proprietory products) to design and conduct alternative treatments to meet well-defined performance criteria of magnitudes and rates of settlement avoiding expressway surface regulation more than twice a year (by pavement experience the limit set of 100 mm settlement over 2 years after commissioning).

Specially praiseworthy is the fact that COSTS are submitted, the indispensable second leg of ENGINEERING besides TECHNICAL EXPERTISE. In passing I submit my doubt that in my intense worldwide coverage of geotechnical papers over the past 40 years, more than 2 or 3 papers per thousand ever mention costs: a disparaging observation.

The treatment included: electrochemical injection; sand sandwich; preloading, geogrid reinforcement and prefabricated vertical drains (two different enterprises); well-point preloading; electrosmosis; prestressed spun piles; sand compaction piles; vacuum preloading and prefabricated vertical drains; preloading and prefabricated vertical drains. No further mention will be made herein on these treatments except that (1) dispersions and rushed novelties abounding are suffering, and taking from Society, the inevitable much higher toll of more frequent failures and disparaging comparisons; (2) more than 50% of the cases incurred in failure during the construction sequence or were abandoned (5); (3) the cost data permit shockingly revealing

(5) This should be recognized as unusual in the face of the dictum that generally a good creative solution should be superabundant in its achievement in order to be noticed and increasingly used. The explanation for the exception is simple: on the one hand the solutions hovered around the indeterminancies "close to zero", in the aim for economic competitiveness; on the other hand, they were solutions subconsciously pushed by vested interests.

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comparisons. At any rate, despite the insufficiencies and failures that occurred, in order to avoid increased complexities and confusions, in my present purpose I adopt the reinforcement treatments as "perfect, no risk", and each at its minimum cost as published in the Proceedings.

In Fig.8 we present the comparative probability distribution curves and bar diagrams of predicted/observed failure heights as ratios, for comparison. From the best-fit Gaussian distributions there appears to have been in the 15-year interval a slight improvement both in the academic aim of the median coinciding with 1.0, and also in any typical design decision cutoff (e.g. 20% cumulative probability risk of failing). This impression needs correction, however.

The results of this additional geotechnical milestone have already been ably summarized and discussed. For my purpose of viewing the advances for the profession deriving from the historic ties and reappraisals, the geotechnical comments are minimized, while the cost implications to Society call for emphasis:

a) The fill's field density (given as associated with percent compactions of 9l-100%) merited more attention: 365 tests averaged 2.04 t/m^3, still with a dispersion of roughly $\pm$ 9%.The fill's conditioning strength parameters were yet offered in terms of effective stresses, notwithstanding the very low stress range and the sandy-clay CH soil of $16 \leq h_{opt} \leq 18\%$ and max.$1.75 \leq \gamma_d \leq 1.83$ t/m^3. Predictors were cautioned as to discrepancies and low credibility of the strength parameters although determined from block samples.

b) Once again, essentially no comment on greatly disperse sample qualities, sensitivities, stress-strain curves, etc., the test results being taken at face value. Incidentally, with the baptismally-blessed stationary thin wall samples we should reexamine if, when used to great lengths, the intent of sampling with MINIMAL STRESS AND STRAIN DISTURBANCES is not being disguised under the index-robe of automatic control of length changes, under compensating internal changes of stresses and strains. The attitude of accepting "data" at face value extends to the piezometric records on unexplained hydrogeology, and essentially all parameters. A far cry from the indispensable approach that all data are always wrong, possibly to different degrees, and to estimable values of bias and dispersion. For instance, it is difficult to reason on "average" in situ strength profiles when most determinations only tend to deteriorate sensitive strengths: the remoulding effects occur in concomitant logical trends in each sample, affecting sensitivity S_t, s_u, % at peak, and preconsolidation σ'_p.

A special item must be devoted to Quirks and Queries on Logic, regarding many a practice well sired and firmly rooted that is unredeemable by statistical adjustments.

c) One notes that a fair proportion of the analyses emphasizes the importance of "cohesion" strength of the fill, up to one extreme postulation that beyond a certain fill height the FS remains constant because each incremental thickness incorporates exactly the additional resisting force to compensate the unstabilizing increment. The cracking of the fill is also mentioned. The added layer's cohesion is not acquired by fairy wand.

The principal conclusion derived from the analyses submitted is the confirmation of the trend (schematically postulated in Fig.1) of increasing dispersions of methods and parameters that have spread across the world, even in so continually repeated a professional problem. Just as opposite examples one notes that in one case preference is given to unconfined compression strengths (the $\pm$ 1945 practice, but with what sampling-handling?) whereas in another, success is hinged on the ever-elusive in situ K'_0 parameter.

It is not surprising that once again the Knowledge Probability Distribution was somewhat pessimistic-prudent, and very dispersed, whereas Nature's behavior repeated (at the position of the test) the essentially clear-cut failure condition, almost deterministic, with but some longitudinal cracking the previous day. Incorporating some inevitable small dispersion (unknown, in any part of the world, because of prevailing single deterministic fail-don't fail approach, which is most unfortunate for engineering progress) along the longitudinal, and adopting the construction reality of a fill rising layer by layer, we now proceed to the key lesson to be extracted by revisiting this case. Figs.9 and 10 have been prepared based on the published costs, not to be discussed, but accepted as nominal and quantitatively comparative. The method of analysis refers back to Fig.2.

For the sake of simplicity (6) in the comparative nominal cost computations

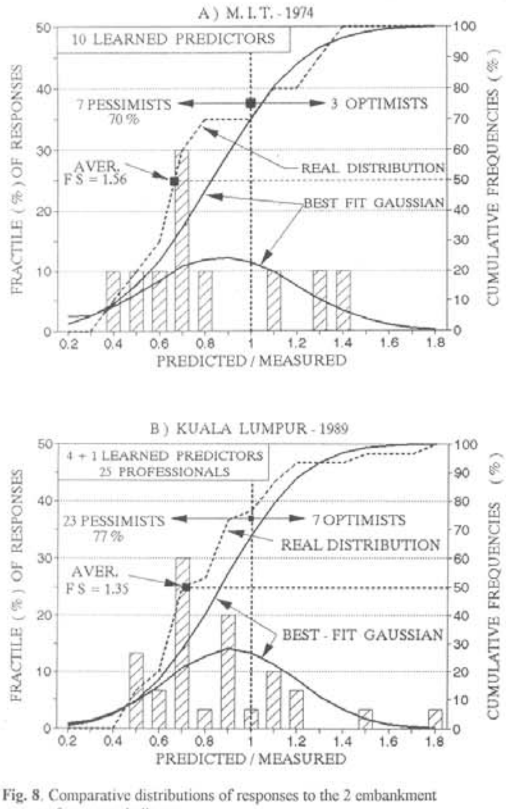

Fig. 8. Comparative distributions of responses to the 2 embankment performance challenges

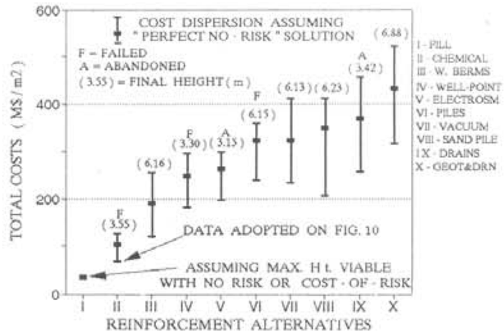

Fig. 9. Kuala Lumpur 1989 Embankment challenge: summary information

(6) The more complicated situations are quite as straightforward, but lengthy, detracting from this presentation's purpose of emphasizing principles.

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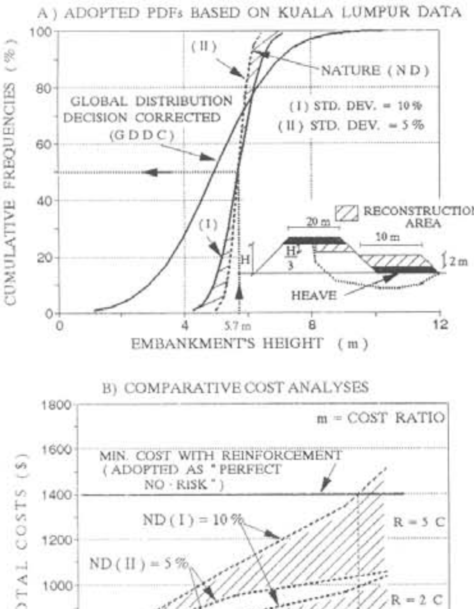

Fig. 10. Bases for comparative cost analyses, showing great advantage of optimist and repeated corrections of failed sections

we adopt the hypothesis that any specific reinforcement is "perfect, no-risk": the same is applied, much more justifiably, to the hypothesis of reconstituting any failed pure embankment section by additional fill, as much as necessary as a berm, and the rest to get back to fill height.

The increase of prudent pessimists from 70% in 1974 to 77% in 1989 represents an increased cost to Society (each project employs one designer only, that is, one decision, not the average of 30 opinions). If one designer has concluded that the failure height is 3.5m (say), he would really use a FS (say 1.25) limiting his design to acceptance of 2.8m without reinforcement; all the remaining length of higher embankment, is forced to use some reinforcement, more expensive (Fig.9). However, for simplicity and on the conservative side we can assume that similar Design decisions would arise from a subparallel Decision Distribution Curve at FS=1.0, which is analogous to the distribution curve reached by the 30 predictions (7) aiming at the bull's-eye of coincident average failure PREDICTION = REALITY.

Along a long embankment of gradually increasing grade elevation, the lengths of stretches reinforced or not will vary from designer to designer. However, for the present we are well documented to imagine a case of a long (say 1000 m stretch) of constant 6m height of embankment, for which the costs, for presumed perfect no-risk reinforcement solutions, derived from the

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(7) In fact we are discussing an utopian condition of collective decision probabilities of our worldwide community. In unfortunate reality, since each client tends to rely on only one designer at a time, and each designer has his bias plus dispersion (the former much more dominating because of lack of repetitive cases for tuning-in) the most uneconomical project would result from the most prudent pessimist.

conjunction of the varied pessimism (greater intensity of reinforcement) plus costs of the specialized services.

While we have concentrated on site and component-issue of methods a, b, c etc. vs. k, l, m, n etc. what we have failed to realize is that the most important information of all, which is Nature's Distribution curve (in this problem) is WHAT WE DO NOT HAVE (but the "experienced designer" with many repetitive cases begins to feel, if developmental academia will permit using the same method over and over). THE MOST IMPORTANT EMBANKMENT TEST WOULD BE JUST TO FACE A LONG PROJECT WITH OPTIMISM (or repeated Type C-DISGUISED trials). Let us imagine such a trial, assuming a reasonable ND curve as shown in Fig.10 A, and a different dispersion on it.

If we are dealing with an optimist over the 1000 m length of 6 m embankment, on curve ND I of STD.DEV 10%, we would have 5, 20%, 30% etc. cumulative probabilities of failure on reaching heights of 4.7, 5.2, 5.4 m respectively. IT MUST BE EMPHASIZED THAT THIS RISK IS INSTANTANEOUS, WELL WORTH TAKING, BECAUSE STABILITY ONLY IMPROVES THENCEFORTH WITH TIME (8). The real failure data of the K.L. 1989 test were of a failure on reaching the 5.7 m height over essentially the entire short length of embankment. This was taken as reasonably indicating an average (≈ 50%) probability of failure. The fill having been of too short a length, this failure probability could have been lower, but such an assumption would be on the conservative side for our conclusions.

For the sake of simple cost comparisons we assume that: (a) the fill rises by 0.2 m lifts simultaneously over the entire 1000 m length; (b) the physically viable failure lengths are ≥ 50 m; (c) the drop of the crest, will be (1/3)H; (d) the volumes for reconstituting any failed section include completing the heave to become a 2 m thick berm, plus going back to grade; (e) a reconstituted failed section is risk-free for the required additional height; (f) the ND data continue to apply to the remaining still unfailed lengths; (g) the cost per cubic meter of fill for reconstituting failed sections is between 5 and 2 times the initial cost of fill.

The cost of such a "shameless" non-Bayesian embankment-construction test is represented in Fig.10 B. The conclusion should be absolutely startling, but irrefutable: the acceptance of up to 45-60% probability of failure roughly matches in cost with the cheapest of the perfect no-risk reinforcing treatments. In other words, are we not really failing to optimize engineering for society, while really minimizing cost of our prestige, at considerable expense to society? (9).

The value of such a physical test (as above mentalized) to determine ND is absolutely inestimable, and at very low cost. Above all, along the kms of foundation clay reasonably adopted as uniform (fixed statistical universe), no matter how much sophistication is incrementally introduced for the progress of geotechnical science, the starting principle is that the gross of the investigation must be logical, simple and very repetitively usable, and the "monitoring" basically of facts flagrant in the engineering scale.

Bothkennar soft clay test site, U.K. 1992.

To mention this remarkable additional MILESTONE still in the making can only be envisaged as a MAYBE CONTRIBUTION in the line of the present thesis. Much more than another trial embankment on soft clays, the farsighted and noble intent of the U.K. Science and Engineering Research Council (SERC) has been set towards developing one soft clay engineering research site for uninterrupted long-term research. And, besides counting on the greatest specialists in geotechnique, an earnest call has been put out for enhancing it as an international test bed site, by promoting joint research in

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(8) Consider, in comparison, the short-term risk that any dam engineer HAS TO ACCEPT in a cofferdam and diversion, and ponder on how we have been betraying the principles of Civil Engineering.

(9) Of course it must be recognized that prestige does have its fundamental "value" to be preserved, for the very sake of society also. There should be a concerted effort of educational communication to lead society to recognize ingrainedly that engineering is not deterministic right-wrong, and that in such problems of cost of risk close to nil, radical changes of attitude must be implanted into clients, media, and society.

which outside bodies would collaborate with the U.K. group(s). The call for a worldwide cooperative effort prods me to use this international podium to extend the suggestions and appeal, because, as will be expatiated, much of the input for broader professional applicabilities will have to be contributed by the distinct past participants in less ambitious field tests across geography and time.

Engineering must straddle judiciously between singular sophisticated cases, and multitudinous roughly assessed similarities/variabilities. With regard to indispensable historic ties surely we need not remind ourselves of the feeling that each SINGLE MILESTONE , too widely separated for direct vision of others, could be seduced into fancying itself as the ultimate NIRVANA. Would one need to be guarded against "scorning the base degrees, By which he did ascend" i.e. forgetting that for all mortals there must be a ROAD (evolving practice) already spotmarked by other milestones, of which there will be more forthcoming, of course progressively altering course ?. A ROAD and GOAL are real, while the arrival is illusive: such is the concept of "uninterrupted long-term research" into past and future.

For instance, could it be that secondary compression testing and the milestone of Väsby have been consummately explored or have deservedly lost interest? So also such widely used indices as unconfined compression strengths and Sensitivity (as conventionally defined) etc., in statistical comparisons with ulterior "improved" substitutes?

The scientific conscience is markedly evidenced. Merely as an example I pick on the question of sampling and sample quality, to employ the researchers' own logic in benefit of worldwide "routine practices" and of tying-in with historic experience. The aim "to use the sampling and testing techniques that were regarded as the best available in current practice" is stated, and is meritorious for a spearhead. But, were not past efforts admissibly intentioned in like fashion? And what percentage of professional cases is (or will be, in foreseeable future) able to use similar spearhead practices?

Meanwhile, in the effort to preserve the legacy of past evidences of Nature's behaviour, two avenues are available, and in at least some significant cases should be used in complement. One is to repeat, for past cases, the current "best available practices" for due comparison. Despite the transience of such past involvements in analogous efforts by important institutions across the world, an earnest call must be made along this avenue, because it is the only way to add some statistical credence to the Bothkennar single-clay findings. The other, more feasible immediately, is to repeat in the case under current study some of the dominant practices associated with the past cases, so that, assuming moderate similarity, some adjustment factors may be quantified for present parameter estimates vs. the erstwhile adopted ones.

The very significant differences signalled (cf. examples summarized in Fig.11) between key results as obtained from the three "presently ideal samples" should reinforce our recognition of the need to compare also the results of the WIDELY VARIED SAMPLING AND TESTING TECHNIQUES that were spread across the world, and are still in duly respectful use by good disciples and acolytes. When results were poor, tending towards significant disturbance-remoulding, obviously the differences had been greatly attenuated, FAVOURING A COMMON LANGUAGE AND PRESCRIPTION; but they were made sufficient for each start, inescapably humble.

I would venture the guess that due to lags in time, geography, economics, and composite factors, surely more than 98% of geotechnical past-and-present experience and judgment is tied to much cruder sampling-handling-testing-interpreting practices than used in the Bothkennar research publications. It cannot escape notice that peak strength results differ by as much as 45%, and preconsolidation pressures determined by as much as 200% ! If we change (under the best and most laudable scientific intentions) our MEANS so very significantly, should it not automatically require proportionally significant adjustments of our EXPERIENCE-ADJUSTMENT COEFFICIENT towards the only point that is, in the final judgment, the PURPOSE of geotechnical engineering RESULTS?

Can we countenance disregard for the price paid for countless past field tests, and the immensity of project evidence of over-spending in totally non-misbehaved cases, spot-marked by failures questioningly analysed? And can we also disregard the vast majority of endeavours across the world that are still (and will always inevitably be) out of phase with any single spearhead of development ? Surely not : and that is where a concerted worldwide effort,

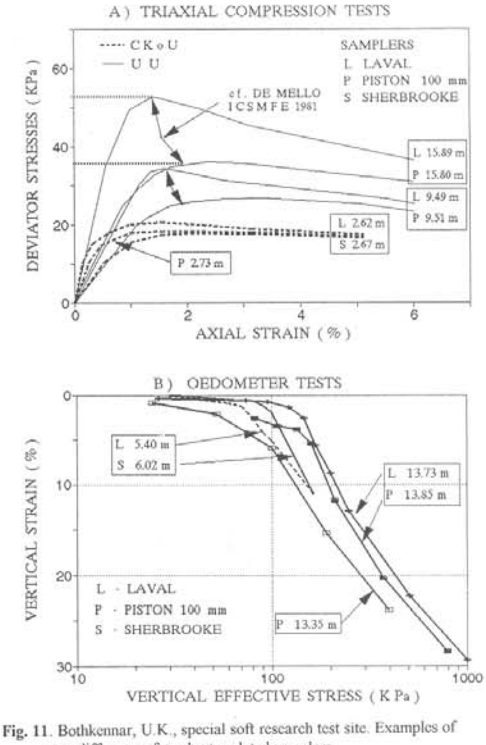

Fig. 11. Bothkennar, U.K., special soft research test site. Examples of great differences from best updated samplers

technical and financial, must be mustered around these presently final sprinters, to hand them the batons from across geography and time.

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, Sao 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

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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.

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 Sao 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 kg/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 2 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 diam. vs. 0.3 x 0.3m plate, associated with THE SAME 10mm SETTLEMENT CRITERION, already incorporated a roughly 200-230% (C) greater margin of conservatism than the presumed experience embodied in the Code derived from Boston's buildings pre-1945.

Fig. 12 summarizes the data of reconnaissance borings, with SPT blowcounts, with which ~ 98% of design decisions have been taken in Sao 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 Fig.13. 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 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

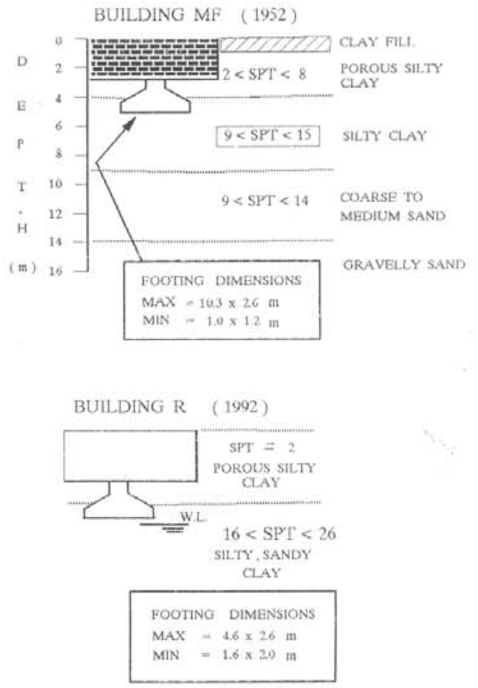

Fig. 12. Basic data, comparative spread foundation cases - 1952 & 1992

(C) see in Annex.

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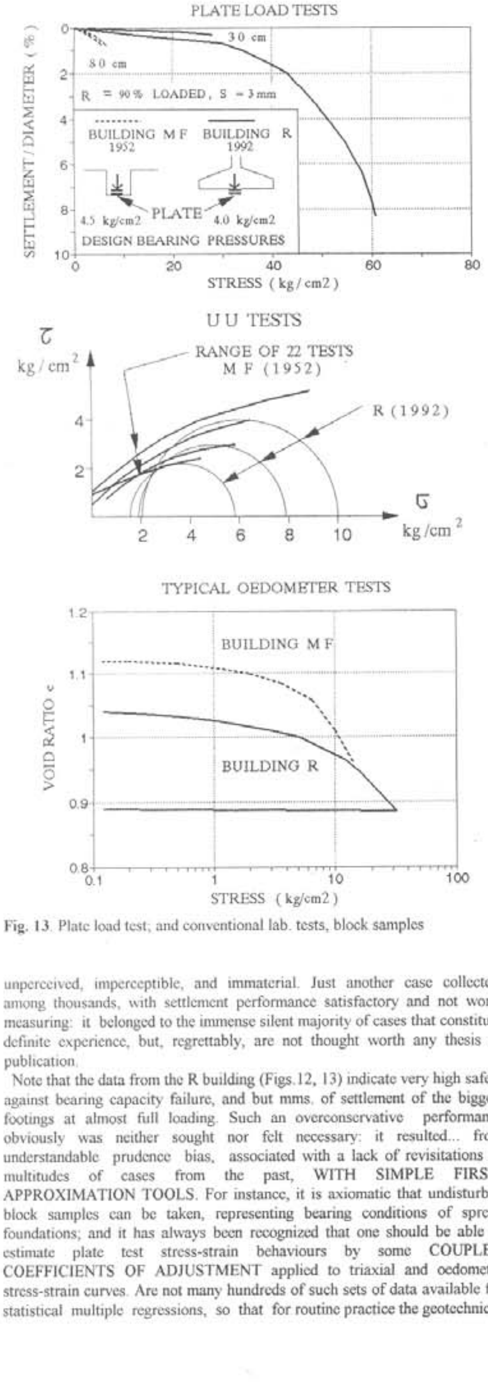


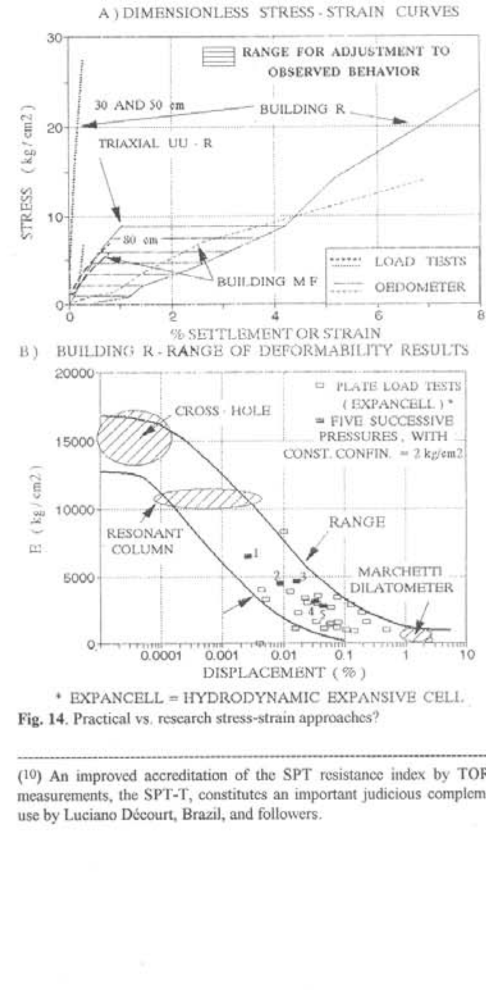
Fig. 13. Plate load test; and conventional lab. tests, block samples

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.12, 13) 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/ft², there was also the statement "compression tests should always be made on the spoon samples" (Terzaghi & Peck, pg. 300, 1948 Ed., repeated pgs. 346-7, 1967 Ed.)[10]? 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 totally new embryo backbones, or fracturing the existing one except for judicious reconstitution.

Fig.14 summarizes data, for Building R, from much additional testing of types currently promoted. Do such data directly favour design decision for the



* EXPANCELL = HYDRODYNAMIC EXPANSIVE CELL

Fig. 14. Practical vs. research stress-strain approaches?

[10] 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.

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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 tó 10 have been frequent over more than four decades, and have not changed perceptibly by changing in situ or laboratory tests.

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.

Fig. 15 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 Fig.15 that extract data from the driven point-bearing concrete piles for the TANK-T structure of Fig.16. Published prescriptions by Aoki-Velloso (1975) and Décourt-Quaresma (1978) have been very much used in Brazil. They were based on uncorrected SPT values from routine reconnaissance borings, and were aimed at predicting the driven pile lengths L, that would guarantee satisfactory load capacities as per static load tests and the FOUNDATION CODE (settlements at failure and allowable loads set at 15 and 10 mm respectively). Moreover, most of the piles were of small 25-40 cm diameters. It can be seen that both methods give good results for the purposes: (a) similar predicted Load Capacities (often taken as 1.5x the maximum load reached, a frequent limitation because of cost, and the vicious circle of dimensioning the test reaction load to 1.5x the desired/predicted pile design load); (b) good prediction of lengths of driven piles; (c) because of microstrain conditions at play in the pile data, and the tremendous available rate of change $\Delta Q/\Delta L$, the stage was set for easy success by routine overdesign of driven pile lengths, always self-justified in vicious-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

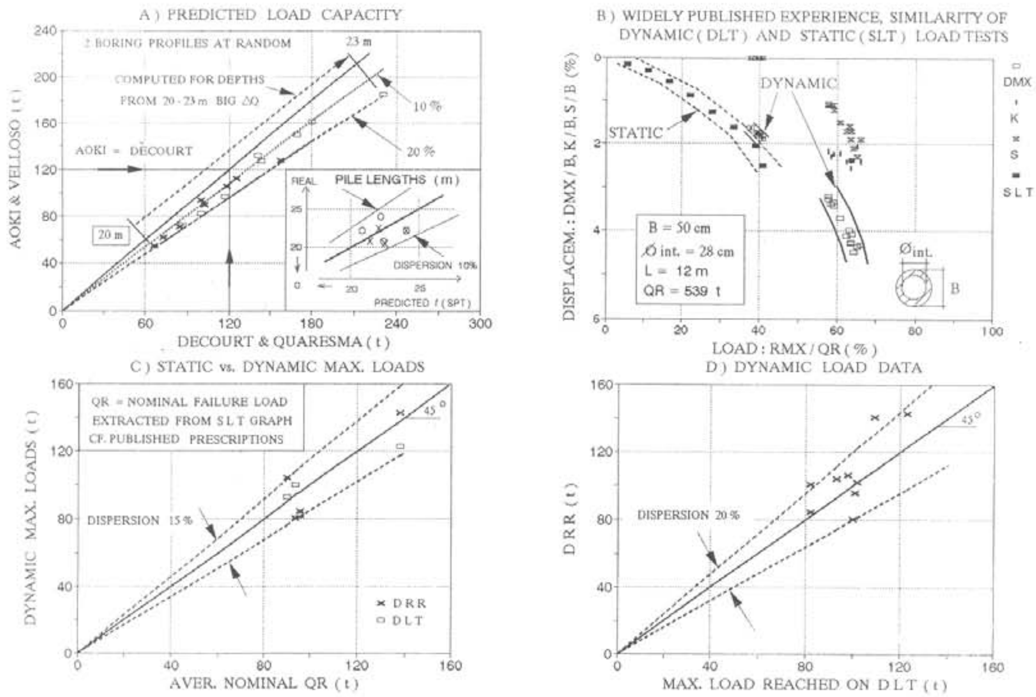

Fig. 15 Driven pile data validating SPT - index correlations, and dynamic rebound & load test analyses

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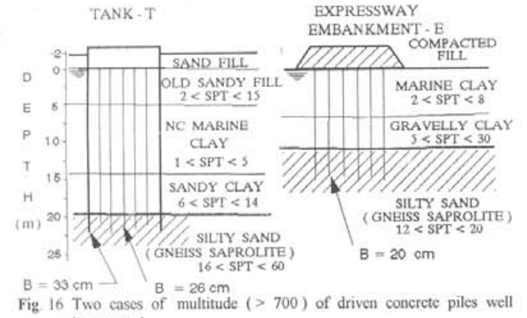


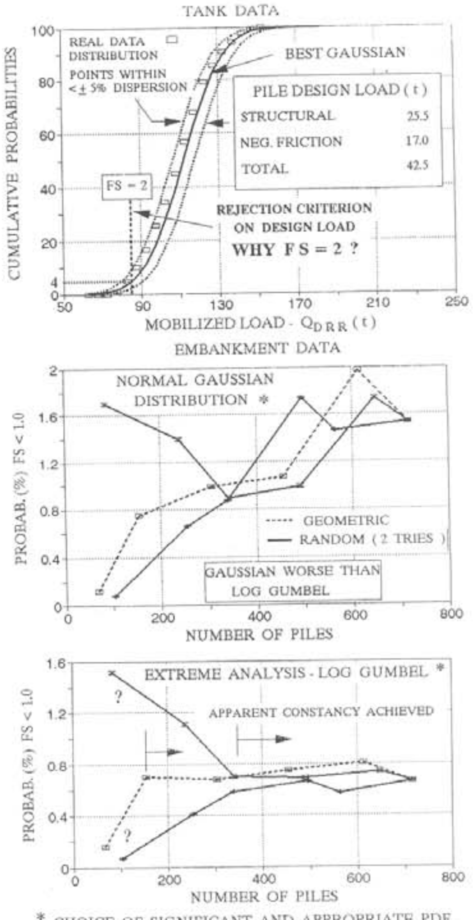
Fig. 16 Two cases of multitude (> 700) of driven concrete piles well documented.

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 have 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 R_d (only 15% higher than the mobilized load at final driving "set"). The data identified as DRR are such dynamic QR values (Fig.15c).

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.15b, c, d). Recapitulating: in the classical routine of pile-driving control, the weight and fall (energy) has been kept constant, under the convinced fear that since dynamic ≠ static (conventional dogma), it was fundamental to respect avoiding any conscious differences. More recently, however, it was reasoned that if a given (arbitrary) Energy E_1 gives the unequivocal dynamic failure load $QR = \alpha (R_d)$, α traditionally taken as 1,0 , then any energy E_2, E_3, ... E_n should also give the same failure load. Curiosity, and the principle that no two things are ever equivalent, led to questioning this would-be coincidence of the theoretical idealizations: and it was discovered that, quite to the contrary, on USING PROGRESSIVELY INCREASED ENERGIES E_1,... E_n at the point of final set the CAPWAP analyses lead to data quite similar to those of loads-displacements of SLT (Fig.15b). 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.16) . 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.17b 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



* CHOICE OF SIGNIFICANT AND APPROPRIATE PDF (HYPOTHETICAL MATHEMATICAL IDEALIZATION)

PSEUDO - BAYESIAN SUCCESSIVE ANALYSES FAVOURING DECISIONS ON DISPERSIONS + ACTION

Fig. 17 Tank piling. Sample statistical analyses and conclusions.

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-Grumbel 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 Fig.18, 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 (or nominal failure load) and, a fortiori, the smallest values of that universe should be most appropriately adjustable to EXTREME VALUE

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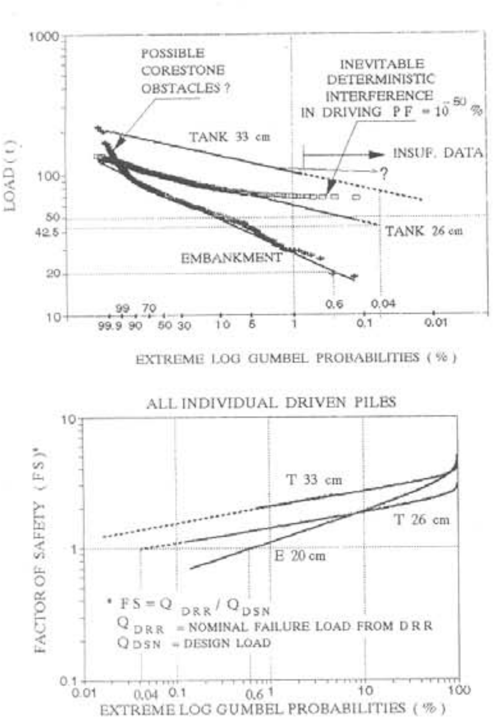


Fig. 18 Driven piles, single ; Probabilities of nominal failure

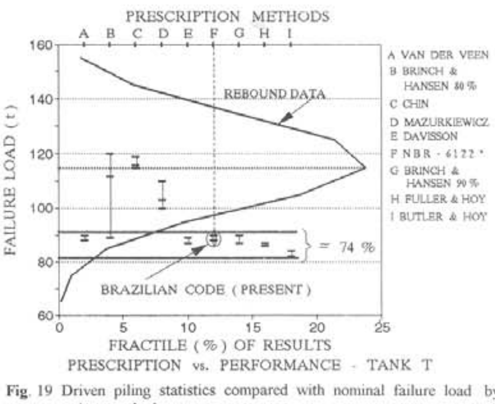
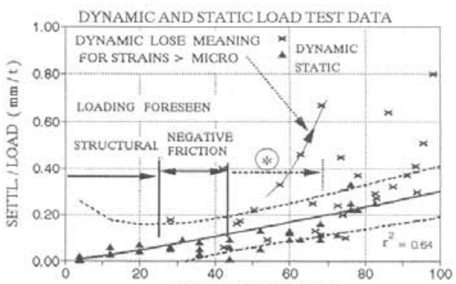


Fig. 19 Driven piling statistics compared with nominal failure load by various methods.



(*) IF STRUCTURAL LOAD ON PILES WERE DOUBLED (CONCRETE CHECKED NOT TO "FAIL") REDUCING TO ≈ 60 % OF PILES, THE Δ S ≈ 9 mm ...

WHEN , WHAT CONSEQUENCE ?

Fig. 20 Analysis of consequence if nominal load on pile is exceeded.

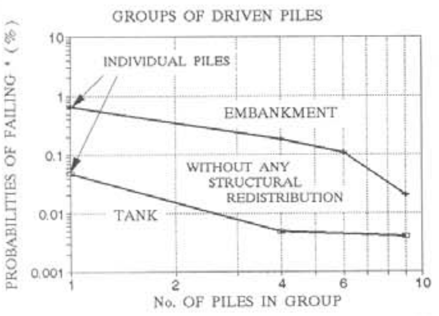
DISTRIBUTIONS. The two graphs are convincing enough, and reveal logical trends, and astounding magnitudes. We can forego pointing to the obvious trends: the Embankment piles are quite logically a little less exigent than the Tank piles, but both are far more exigent than should be required by judicious safety and serviceability criteria.

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

Do the learned writers of prescriptions and codes realize how much and how 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.

Fig.19 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. Fig.20 shows what would be the physical consequences BEYOND THE NOMINAL FAILURE postulated. Cases of brittle failure emphatically excluded, all that happens if the condition of FS < 1.0 begins to set-in for the INDIVIDUAL PILE, is that a minimal inconsequential rate of INCREMENTAL SETTLEMENT of 0.1 mm per 35 tons would begin to force some redistributions of loading (cf. Fig.21). Regarding savings in the foundations it is seen that if the total number of piles were reduced to 60% of the designed array, an increase of (flexible, first-load) settlement of 9 mm would be the only result. Finally, by using the individual DRR data of contiguous piles and, without any structural redistribution, merely considering the arithmetic average QR of groups of 2 x 2, 2 x 3, and 3 x 3 contiguous piles, the PF% of FS = 1.0 drops to about one-tenth of the corresponding PF% established for the individual pile. Barring geotechnical disturbances of one pile to others nearby (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 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



* ARITH. AVER. OF LOAD ON GROUP OF ADJACENT PILES, COMPARED WITH AVER. FAILURE LOAD FROM THEIR Q_{DRR}

Fig. 21 Driven piling. Reduction of probability of failure in groups vs. single.

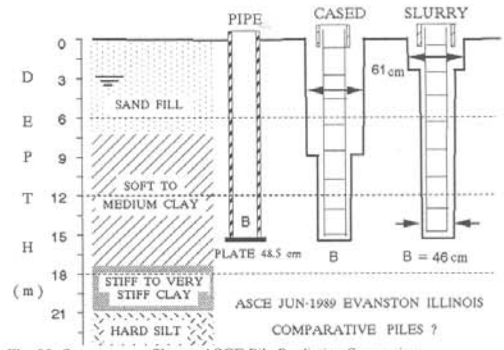


Fig. 22 Summary profiles on ASCE Pile Prediction Symposium.

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?

Broader range of pile types. Continually refining subsoil knowledge vs. predicting different pile types. ABEF, Sao Paulo, 1989 Experimental Site, and ASCE, 1989 Pile Prediction Symposium.

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 has been exponentially increased in the opposite direction.

The ASCE Pile Prediction Symposium, 1989, offers data of interest for a revisitation. Fig.22 summarizes the minimal data on the subsoil profile, and the three pile types chosen for our presentation. The routine and special profiling tests 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 Fig. 22 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 Figs. 23 (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 effects are extracted from the ABEF 1989 Experimental Site in a gneissic saprolite profile. In conection 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.

Fig. 24 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 chosen are (a) auger-bored, and/or bentonite-stabilized-bored (b) precast concrete driven (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

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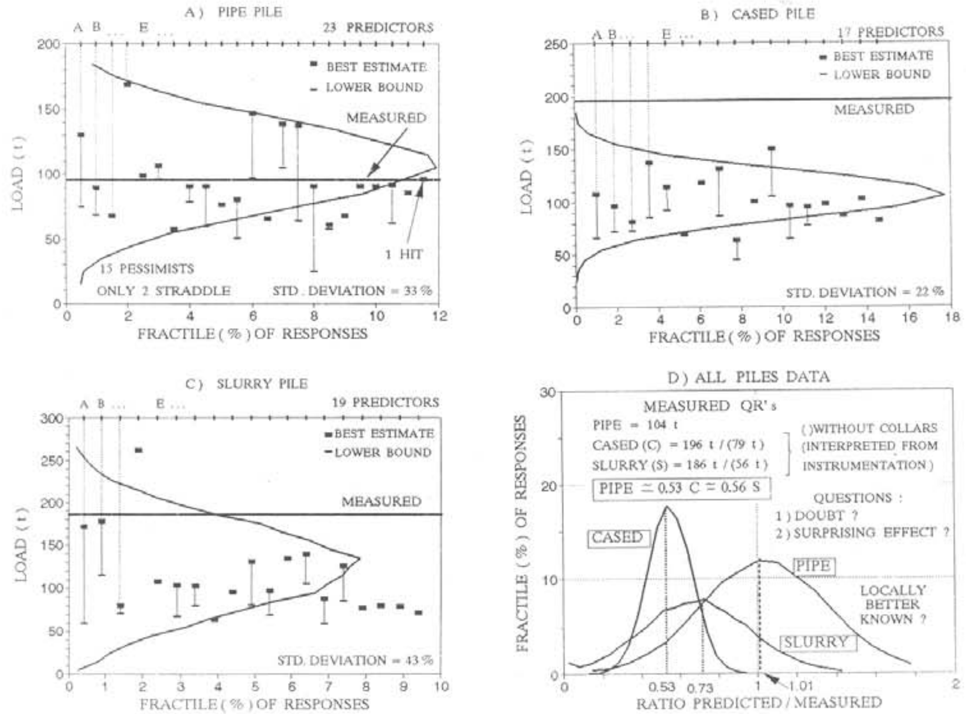

Fig. 23 Statistical analyses of predictions.

base. The tremendous differences, of necessary Coefficients of Adjustment (herein ranging from 40% to 260%) regarding execution effects, in

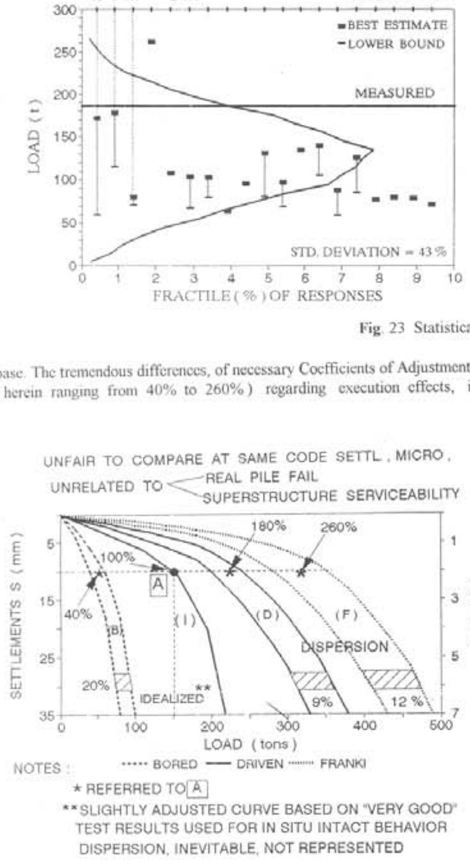

Fig. 24 Impressive pile execution effects in gneiss saprolite, ABEF experimental site, 1989. Use of intact parameters thwarted.

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. London Large Bored Piles, 1960), 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.

Fig. 25 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) Fig. 26 illustrates the unfair consequences to bigger diameter piles and piers, and particularly to bored vs. 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 in the lateral friction. But why forego partial knowledge, when all knowledge is forever partial?

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-McDonald 1956, Bjerrum 1963, De Mello 1969, Grant-Christian-Vanmarcke 1974, Burland-Broms-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.

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

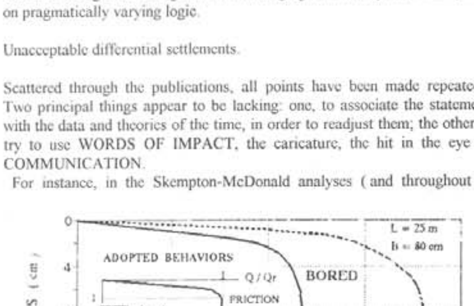

Fig. 25 Updated idealized composition of a load-settlement curve.

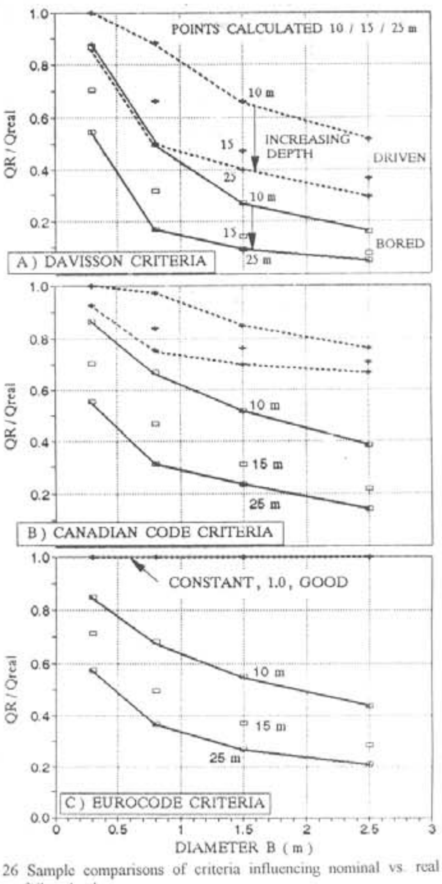

Fig. 26 Sample comparisons of criteria influencing nominal vs. real pile failure loads.

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 improved Carrier-Christian soil model (Geotechnique 1974,'73) should be an obligatory adjustment of revisitation since 20 years ago. Fig. 27 shows the trends of some changes of conclusions. 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

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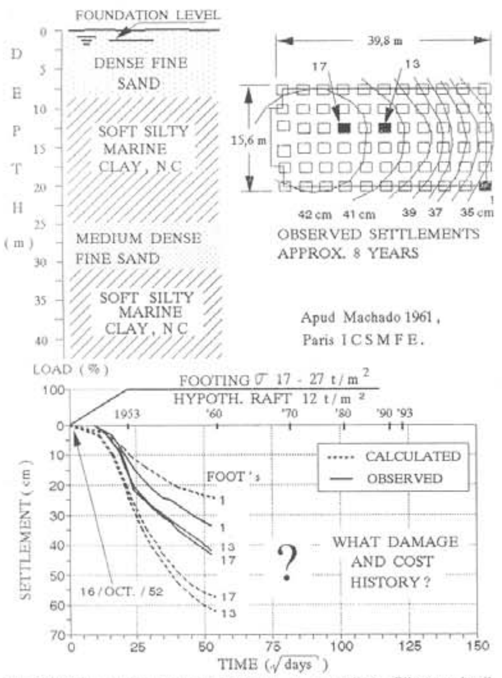

Fig. 27 Change from idealized constant E to realistic Gibson subsoil. Changed influence of raft diameter on settlements.

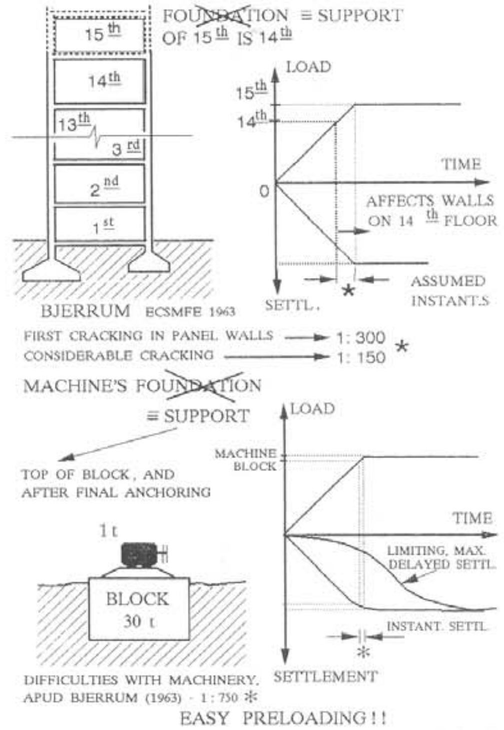

Fig. 28 Recommended use of "SUPPORT" instead of "FOUNDATION" for improved logic and great savings on many cases.

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 Figs. 28 (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). 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). Fig. 29 merely hints at the treasure of revisitation data, by summarizing data on just one typical case, published in the Paris 1961 ICSMFE, and cited in the State-of-the-art report of Burland-Broms-de Mello, Tokyo 1977 ICSMFE.

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 $C_c / 1+e_0$ 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_0 has to be varying with depth (idealized equation deducible): thus the profiling of $C_c / 1+e_0$ presents inevitable variations with depth. Further confusion appears by pre-

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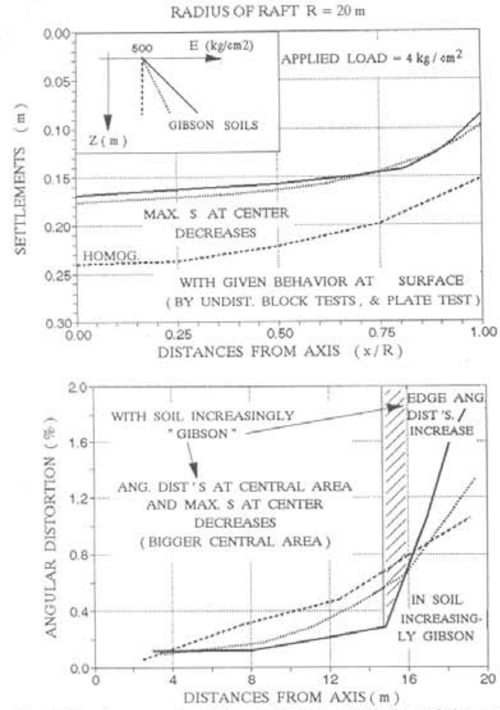

Fig. 29 Sample case of building in Santos calling for updating and revisitations

consolidations leading to more than one e_0 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, Purdue Conf. ASCE 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 international 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 (cf. Kerisel, San Francisco ICSMFE, 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?

CONCLUSION

The world needs engineering, and economical engineering, more hastily than additional glitter of science. 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. Such examples as the fire of Rome A.D 64 and Nero's aesthetic megalomania affecting Western architecture, and analogously Chicago's catastrophic fire of 1871 offering American architects a similar well-seized opportunity, serve as reminders of man's endless capacity to make catastrophe the catalyst of creativity: burning must be metaphorical. As far back as the famous Ptolemy (90-168 A.D.) of Alexandria, refurbisher of the library burned by Caesar (48 B.C.) it was emphasized that he should "draw on the best observations made before him, and exercise the need for repeated, increasingly precise observation".

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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WE PROFESSIONALS BEG LESS RAPID NOVELTIES, MORE RENEWED REVIEWINGS OF WHAT IS ALREADY THERE.

" ' tis pleasant through the loopholes of retreat to peep at such a world ; to ***see the stir*** *of the great Babel,* ***and not feel the crowd*** *".*
(William Cowper, 1731-1800)

" Father Mackenzie writing the ***words*** *of a sermon* ***that no one will hear*** *... ... all the* ***lonely people, where do they all belong ?*** *"*
(John Lennon / Paul McCartney)

ACKNOWLEDGMENTS

Firstly in dedication to the memory of Maria Luiza Soares de Mello, whose dedication is reflected in every work and word that chance wove into my destiny; and to our children Luiz Guilherme de Mello and Lucia Beatriz de Mello Alessio for their shares of support. To Profs. John Burland and M. Jamiolkowski for immeasurable brotherly help and advice. To Kaare Höeg, in representation of countless friendly colleagues of the highest international rung across the world, for warm and stimulating interaction. To my office colleagues Engs. Antonio C. Sobral, Victor M. Soto and Guilherme O. T. Dutra, and the dedicated staff, Dora N. Piscino and Lucilene R. Marinho, for bearing with every emergency and obstinacy. To Eng. Romildo J. dos Santos F° of CBPO- Contractors and colleagues for the trusting call to unravel another emergency that confirmed my maybe conclusions, while turbulently reshuffling their digestion.

ANNEX

(A) Since civil engineering relies on building functional shapes ("forms") of her materials, her dominant concern is with DEFORMATIONS; thus the obvious priority concern with stress-strain, well covered by the nominal, but EFFECTIVE, stresses, within the typical ranges of significance to erstwhile problems. It has long since been recognized that many other causes, time, chemical, electrical, thermal, etc., also intervene besides "mechanical stresses"; and some of these effects become significant at the extremes of very low and very high nominal stress levels. Take for instance at the very low stress level the very important behaviors (chemical, colloid-chemical, secondary compression, etc.) of embankments on very Sensitive or Quick clays; and at the high level, in point-bearing piles on sands, or in very high rockfills, the point-contact crushing of grains by INTERGRANULAR STRESSES, manifold higher than the nominal effective stress, and greatly varying with grainsize distribution, grain shape, and grain mineralogy, etc.. Thus, VERY IMPORTANT ENGINEERING WORKS call for a revision of the original equation. Ironically, but quite possibly interpretable under subconscious needs, some of the most important PREDICTION VS. PERFORMANCE challenges of the past 20 years have struck these very heels of Achilles of our profession.

Mathematically, if the strain were yet soon to be formulated as a function of the many undisputable parameters, a, b, c, d, etc., we would acquire conscience that it is the PARTIAL DIFFERENTIAL $\partial\varepsilon/\partial\sigma'$ that should be well expressable through Terzaghi's equation, within practical range of interest. Respected and illustrious efforts have been made under the aura of Terzaghi's EFFECTIVE STRESS EQUATION being a pseudo-scientific truth. I submit that, MAYBE, the greater than desirable scatter of behaviors under such special conditions of extremes (including unsaturated and collapsive and "problem" soils, saprolites and laterites, of growing concern to vast regions of the world) will continue to frustrate engineering behavior predictions, for DESIGN PRESCRIPTIONS, unless some more generalized mathematical formulation appears, out of which the different partial differential equations may be statistically adjusted to the welter of experimentally established behaviors, within the narrower dispersions reasonably imposed.

(B) For appropriate evaluation of parameters effectively interfering in geotechnical engineering problems it has been emphasized that we must begin by assessing for each case the SUI (Significant Unit of Influence) and the EHU (Equivalent Homogeneous Units). Doubtless one of the most pervading and pernicious failures in the interaction between research-development and professional practice, has been the total disregard to these fundamental needs. Thus, for instance, the exaggerated zig-zagging of the point resistance q_c of CPT sounding tends to be affected by precisions too high and localized for the SUI or EHU of a pile, and much more so for the SUI and EHU of a footing or enlarged caisson base. At the other extreme, for a piping failure the SUI in any dam, no matter how large, may be as small as a meter in diameter, making the problem very much a case of indeterminate extreme value statistics.

(C) 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, 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 1 (progressively we shall note the sum of conservatisms).

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