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PROBLEMS AND CHALLENGES OF GEOMECHANICS REVISITED

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Over the past 40 years the most marked fact in technology has been the exponential evolution of INDUSTRIAL MULTIPLES; and consequent changing in background thinking. For instance, take computers and computation: the millions of high-speed YES-NO choices (as summarized in Plate 1) lead to PROGRESSIVE PRECISION... leading to the realization of "as far as necessary", and, thereupon, "necessary why", and "necessary for what". Typically engineering thinking: which decreed the death of DETERMINISM and of the ANALYTIC SOLUTION.

In such an ambience, for us Civil and Geomechanical Engineers, does it SHAME us, or does it DELIGHT and STIMULATE us to be ever SINGULAR, DIFFERENT? "The old order changeth; yielding place to new; And God fulfills himself in many ways; lest one good custom should corrupt the world" (Alfred, Lord Tennyson).

The analytic avenue that seemed imperative and seductive was, indeed, mathematically valid for the full range of $0 < x < x$; but behind the curtain was the fact that reality was IDEALIZED. The

IN THE EVOLUTIONARY CYCLE OF THE TECHNOLOGY OF COMPUTER CHIPS, AS THE SPEED OF PROCESSING INFORMATION INCREASED, THE COMPUTER PRICES DECREASED.

YEAR	EQUIPMENT	A) INSTRUCTIONS PER SECOND	B) PRICE US$	RATIO A / B
1975	IBM MAINFRAME	10 000 000	10 000 000	1 / 1
1976	CRAY 1	160 000 000	20 000 000	8 / 1
NEED OF TIME-SHARING EMPHASIZED				
1979	DIGITAL VAX	1 000 000	200 000	5 / 1
1981	IBM PC	250 000	3 000	83 / 1
PROFESSION OF PROGRAMMING OPTIMIZERS IN GREAT DEMAND *				
1984	SUN MICROSYSTEMS 2	1 000 000	10 000	100 / 1
1994	PENTIUM-CHIP PC	66 000 000	3 000	22 000 / 1
1995	SONY PCX VIDEO GAME	500 000 000	500	1 000 000 / 1
1995	MICROUNITY SET-TOP BOX	1 000 000 000	500	2 000 000 / 1

** THE PROFESSION DECLINES MUCH CHEAPER TO USE AN OLD INEFFICIENT PROGRAM THAN TO PAY HOURS OF PROFESSIONAL FEES OF SPECIALIST PROGRAMMER TO OPTIMIZE.

Plate 1 - The Faster the Cheaper

numerical avenue accepts both origin and goal as PSEUDO-REALISTIC, with simplification by DECISION, in order to constitute REASONABLY ANALOGOUS GROUPS i.e. statistical universes. Thereupon, one must recognize and emphasize that PRECISIONS attainable and necessary are completely different in: RESEARCH >> PROFESSIONAL TESTS (Lab. and In Situ) >> ENGINEERING WORKS.

All three components of the global orchestra are separately valid for their purposes, but it dismays to witness how sadly they fail to compose into a symphonic product.

Just 2 recent examples may serve to show some absurdities to which Geomechanics made public has imperceptibly come, even in 1st World Advances. Figure 1 refers to data published in Geotechnique, June 1.992, of tests on the best undisturbed sampling presently imaginable in the world, for the UK Govt (and worldwide contributing) Bothkennar Soft Clay Test Site under the Science and Engineering Research Council SERC). Without any detraction on the fundamental behavior parameters, with regard to the simplest and most precise of all index tests, water content, on which almost all saturated clay behavior hinges, it is most surprising that (a) variations are enormous, around about 50-78%, and (b) significant differences appear from lab to lab even among labs of very highest repute. Are test specimens being taken too small for problem significance, and are they handled differently?

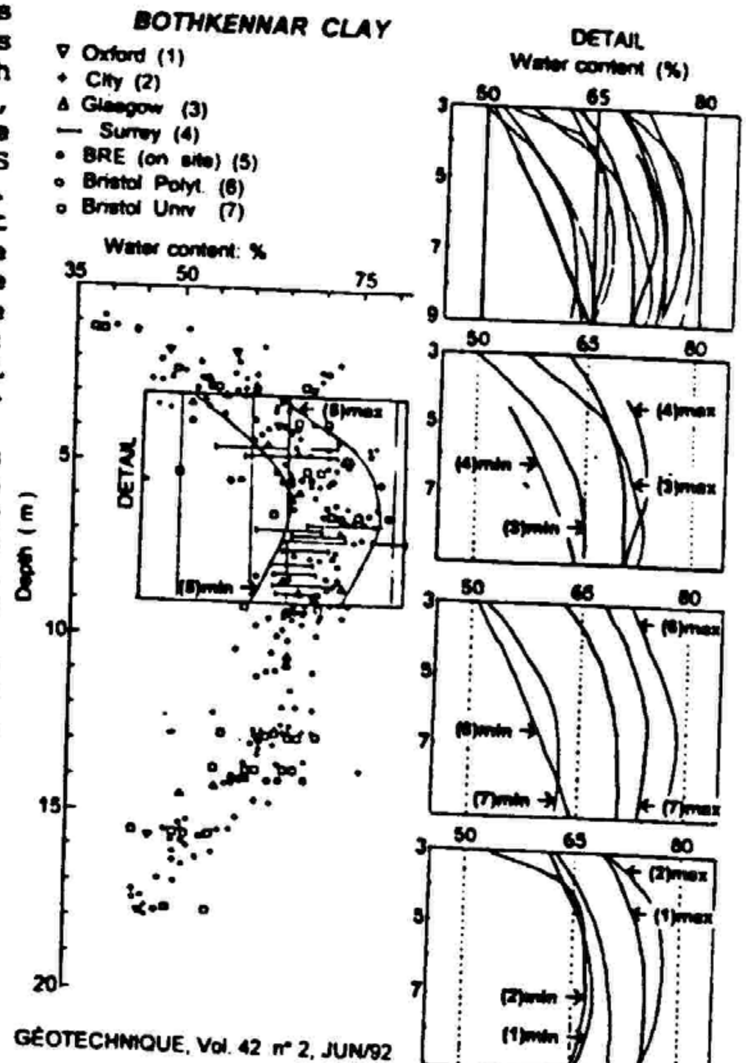


Fig 1. Strange extreme dispersion in simplest index test.

Figure 2 refers to the tragic collapse of the Munich Subway tunneling extension (Oct. 1.994) in hard marl underlying gravels. Could it be that within the civil engineering routine of linear interpolation between borings, the geomorphological fact was forgotten that erosive velocities of marl are not too different from depositional velocities of basal gravels, and erosion gullies in cohesive materials tend to be subvertical, thwarting subhorizontal linear interpolations, and permitting major localized thinning of the important cover for tunnelling?

The point is that we must question and challenge constantly, everything being wrong (despite having been useful) or insufficiently right to different degrees; and the needed degrees of rightness have increased with time, compounding with increased dispersions of testing and thinking, favoured by mass communication and unchallenged conventions.

Figs. 3a and 3b exemplify the progressive increase of dispersions generated from academic research, recognizedly well-meaning, and also from premature publication of innovative field test results. In Fig 3a (schematic) the erstwhile empirical ''law'' of behavior (curve (A)) was well established by good tests on specimens of idealized conditions. Subsequent research added indications of the interferences due to each of 3 different parameters, curves (B), (C), (D). Thereupon, the better professional, conditioned by desirable continual learning alongside with prudence, can only adopt decisions based on the outer band of the disperse added empirical pseudo-relations, because he is rarely documented to the point of confidently excluding from his professional case the presumed complicating parameter. In Fig. 3b I reproduce, as a mere example, some data from Report 43 of the Swedish Geotechnical Institute, an unquestionably top-notch source for any of us: methods of interpretation published under almost the same respected authorship, four years apart in 1981 and 1985, offer widely different statistical regressions Moreover, the intended improved AVERAGE correlation of 1985 is seen to be accompanied by a much worse CORRELATION COEFFICIENT (r^2), i.e. much wider dispersion.

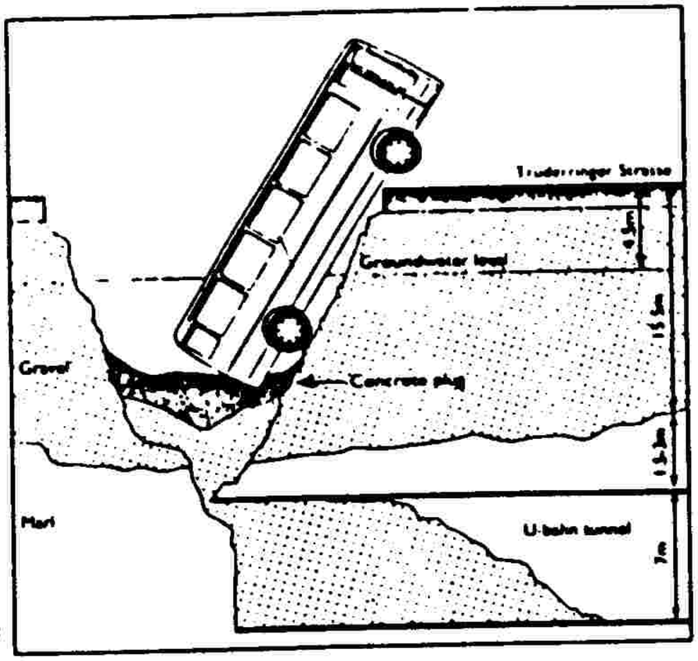
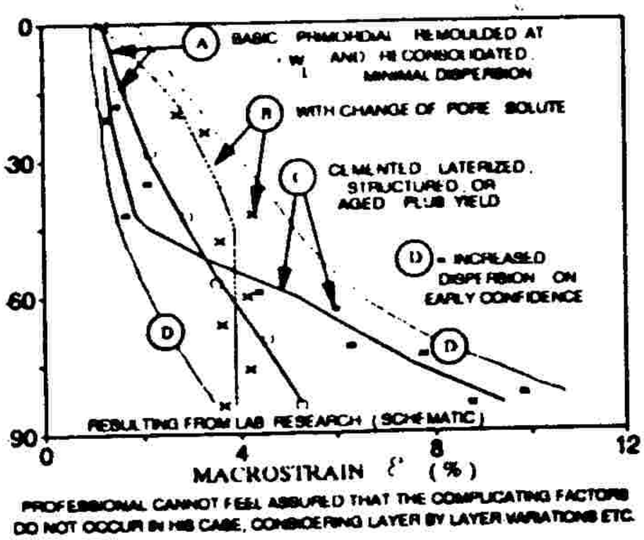


Fig 2. Collapse of Munich Metro (Oct/94).



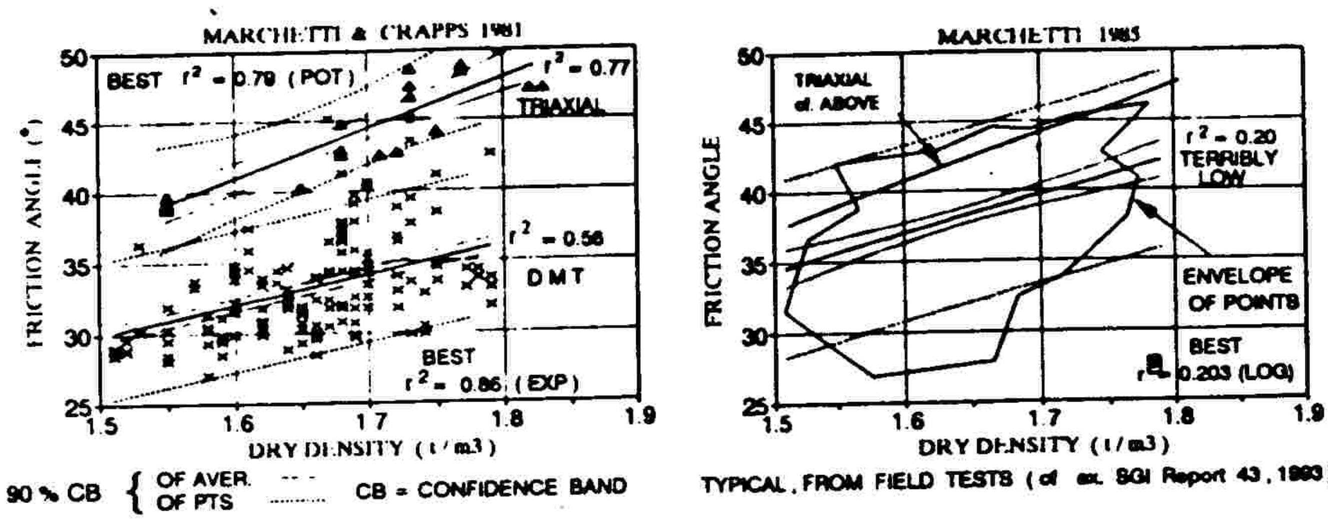


Fig. 3. Progress and rapidly increasing dispersions in geotechnique..

What are the chances that the same professionals who adhered to the 1981 recommendation would take note, with due confidence, of the revision recommended in 1985?

Similar disconcerting graphs abound, for all other important tests and parameters, not only in the same Report, but throughout our recent technical litterature. A 10° (+/-) dispersion in the friction angle attributable to a sand is much greater than would have been assigned ''by feel'' 40 years ago. What, then, is the benefit/cost ratio of the increased triaxial and Marchetti dilatometer (DMT) test programs?

It is fundamental to recall that:- (I) Wrong data, and erroneous analysis have occurred, and will continue to occur, blurring diagnosis by greatest of mentors (cf. 2 cases cited further down), and justifying CRITICAL (and respectful) REANALYSES, progressively; (II) our engineering decisions are NOT based on average parameters, but on UPPER OR LOWER LIMITS thereof (for assuredness, to avoid the blunter term, safety); (III) systematic errors (proliferated by mass communication) are much more damaging, cannot be attenuated (as are erratic errors) by tightened Confidence Bands, CB, derived from increasing testing; (IV) depending on how the behaviors of ''elements''accumulate (or not) into the prototype-size body-behavior, it is very different to consider Percent CBs on AVERAGES (e.g. compressibility/deformability) or on INDIVIDUAL POINTS (e.g. brittle shear or tension failures); (V) ENGINEERING, however, CANNOT BE DIVESTED OF A PRIORITY DECISION ON DEGREE OF SIGNIFICANCE, of the quantified parameter, to the PROJECT ACHIEVED, and not to research or theory presumed involved. That is where the practice of engineering has dismally failed to provide quantifying supports for the continual neurological Bayesian exercise of EXPERIENCE. We have sadly failed two generations of eager followers.

Fig. 4 submits data from a recent very meticulous driven pile foundation for a residential highrise building in Sao Paulo. The geotechnical SPT classification, aided or encumbered by lax pseudo-geology, indicated a ''tertiary''(how many millions of years, and how unimaginatively fettered the possible vagaries of Nature during the period?!) stiff clayey bed as adequately uniform for a pile foundation to garantee minimized settlements, total and differential. Driven concrete piling carefully controlled by the proven PDA technique was reasoned to be doubly guaranteed for homogeneity, because each pile is pre-proven during its micro-

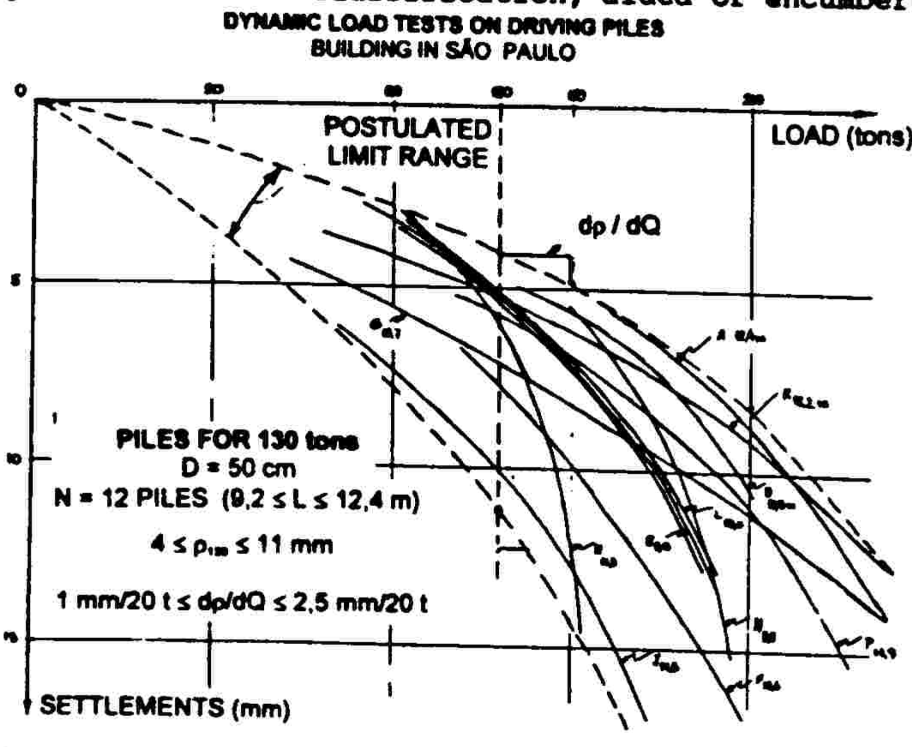


Fig 4. Great apparent dispersion in load-settlement data.

dynamic penetration to desired set (proven, and logically, similar to micro-static load-test penetration).

The first impact of the data is of a surprising, almost shocking, heterogeneity, despite the homogenizing cumulative effect of friction (dominant) plus point contributions. The 12 piles documented by Dynamic Load Testing gave: driven lengths varying 16% about the mean of 10.8 m; short-term "elastic" micro-settlements varying between 4 and 11 mm at pile working load of 130 t; at pile working load, an incremental settlement between 1 mm and 2.5 mm per increment of 20 t (15% of load). [N.B. Such incremental behavior is what matters for damages to the building's cladding, the most sensitive element as per experience]. However, as regards the engineering dispersion, the net effect should be the OPPOSITE to what automatically occurs because of structural engineers' idealization of ZERO DIFFERENTIAL SETTLEMENT, and our own hitoric first-order attempted prescriptions. The difference between 4 and 11 mm settlement at working load is irrelevant to the building's behavior, both meriting equivalent treatment under the traditional label "negligible"; so also the incremental settlement of 1 to 2.5 mm per 15% incremental load. The point is that doubtless all buildings that behaved perfectly well (the multitudinous silent majority of cases not so well documented or monitored) had settlements equivalent or worse, NEVER HAVING BEEN SUBJECTED TO ANALYSES OF STRUCTURAL REDISTRIBUTIONS as if novel and dramatic. The dispersion revealed indeed rightly entices calculating redistributions; but, parametrically differentiated situations (wider probabilistic confidence bands) should be collaterally analyzed, and the ACCEPT/REJECT BOUNDARY should NOT continue to be set at the idealized ZERO, but at some condition JUSTIFIABLY MORE DISPERSE than the present one. Once again I beg to emphasize that absence of evidence cannot be taken as evidence of absence [a most frequent error of concept in technology], merely because of inexorable idealizations, illusions, and historic limitations on micro-quantifications. By falling into the common trap of doing the opposite, attributing to our deterministic idealization the presumption of the start-off truth, we let such cases result in burdening Society immeasurably, deplorably for from using such a engineering progress to benefit Society and our professional experience.

Summarizing, therefore, some of the problems of serious waste of energy have arisen because of a) lack of understanding regarding the very DIFFERENT LEVELS OF SIGNIFICANCE in research, professional testing, and project materialization; b) lack of recognition of the big difference between CORRELATIONS and PRESCRIPTIONS; c) failure to emphasize that engineering decisions depend on SAFE PRESCRIPTIONS, and the latter need improved statistical quantifications of dispersions in order to avoid being forced into increasingly safer and more expensive bounds; d) failure to retroanalyse historic ENGINEERING EVIDENCE with realism liberated from the indispensable erstwhile mathematical idealizations except for support in indicating trends.

To the above failings I must add the blatant lack of distinctive contribution from the 3 key components of GEOMECHANICS, Geology, Soil Mechanics, and Rock Mechanics. Respectful of lack of space I summarize in Plate 2 some well-known key thoughts pertaining to this tripod of geomechanics, so that in the face of each new technical paper, or professional challenge, the well-intended professional be alerted to questioning incisively: how are such key requirements so very frequently set aside as if inexistent?

In calling for REVISITING I must also point to the spattered interference of human error. Do we make mistakes, irrespective of how

1. GEOLOGIC CONTEXT BASIC, INDISPENSABLE

1.1. BACKGROUND VISION
1.2. SEARCH FOR THE DISCONTINUITIES; ANY MISS DEFINES THE CONTINUUM
(cf. children's game of "Naval Battle")
1.3. QUANTIFIED AGE IMPORTANT
(continuous/ discontinuities)
1.4. BIFURCATES { GEOLOGIC ESSENCE
GEOLOGY FOR : REQUIRES { ITERATION WITH USER
(e.g. Mining,Civil, etc) MUCH BETTER QUANTIFICATION

2. GEOTECHNIQUE, MECHANICS OF SOFT CONTINUUM

2.1. INITIALLY: a) WEAK b) COMPRESSIBLE/DEFORMABLE
a) FAILURE b) MACRO-DEFORMATIONS OF MODEST WORKS
2.2. NOW: MACRO-WORKS LIMITED TO MICRO-DEFORMATIONS
EARLY TESTS CRUDE, MANY ILLOGICAL.
EXCEPT IN RESEARCH, ROUTINES KILLED QUALITY + SENSE OF PURPOSE

3. ROCK MECHANICS, MECHANICS OF DISCONTINUUM

ORIGIN IN INVESTIGATING THE MATERIAL, ROCK, $\rightarrow \infty$
GRADUALLY TRAPPED IN RECOGNITION OF SCHIZOPHRENIA
WHAT MATTERS IS NON-MATERIAL ($\rightarrow 0$), CRACK, CAVITY
HOW TO REVERT INVESTIGATING PROCEDURES ?

"MINOR GEOLOGIC DETAILS" (e.g. MALPASSET FAILURE)

Plate 2 - Geomechanics

prestigious the author? Systematic, for some time, until hopefully corrected? And also episodic, justifiable in hindsight? Of course we do; and with but the littlest shame in comparison with that of persisting in the error.

The following 3 examples are purposely extracted with regard to cases and persons meriting my (our) deepest respect, and principally for the educative purpose of distinguishing between criticism of data, hypotheses and interpretations, and criticism of professionals behind them. Firstly, Terzaghi's own early very significant misinterpretation of the settlements of the M.I.T. building, his key professional assignment collateral with starting the course on Soil Mechanics. The following quotes are from the paper by Aldrich and Seeler, 1981, at the M.I.T. Seminar ''Past, Present, and Future of Geotechnical Engineering''. ''Terzaghi determined that the natural water content of the clay INCREASES with depth... and assumed that 'consolidation is still going on as a GEOLOGICAL process': of the three causes for unequal settlements of the M.I.T. buildings since Dec, 1916 (having reached 1.4-6'' by 1923) 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 entire paper should be read, also with regard to other erroneous concepts on pile load tests and settlements, all in a respected historic setting. However, it suffices for me to emphasize the interplay between data and theory, as the stumbling-block to the father of consolidation theory (and soil mechanics) , keen pursaer of geology, aware of glaciation preloading as the geologic process at play.

Secondly let me submit an example that I presume to have been conditioned by wishful thinking of professional sufficiency of PRESCRIPTION , in the face of the difficulty of overcoming lack of evidence under wider variations of conditions. The importance of the subject of FILTER CRITERIA (FC) for assuring the safety of dams against piping failures is unquestionable: they 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. Despite several intervening research efforts, the basic DESIGN PRESCRIPTION continues to be the Bertram-Terzaghi (1940) one, based on primitive lab tests. By using a set of research-test data more recently published under very authoritative authorship (Fig. 5, Sherard,1984, ASCE) I merely emphasize both the eminent opinion enticing revisitation, and the fact that observation and interpretation depend on the view of the observer.

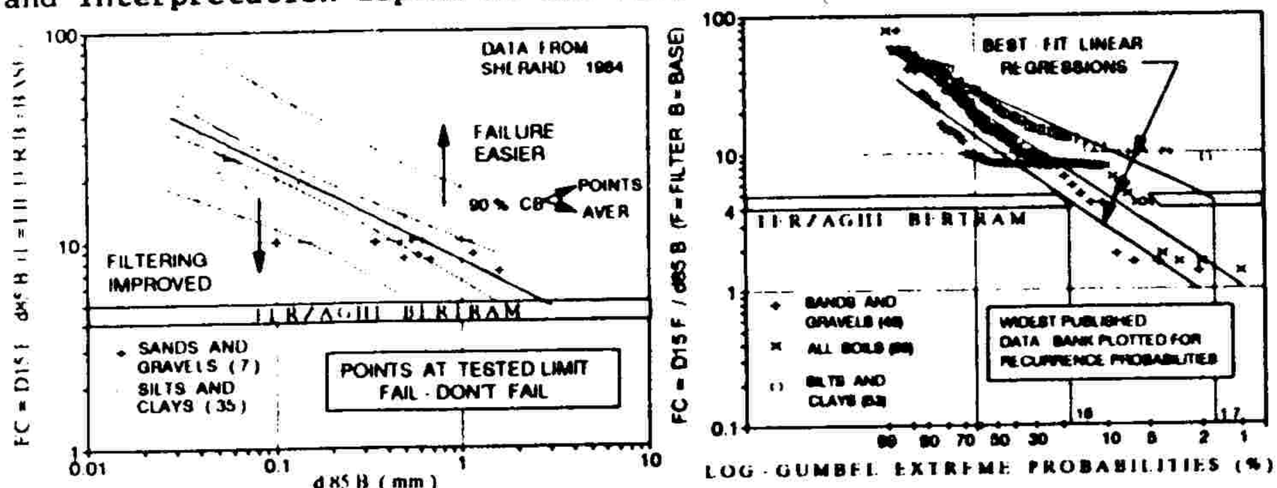


Fig 5. Seepage test data on filter criteria viewed probabilistically. Shocking indications?

There are many details to be reconsidered about the tests themselves, their aim, and conduct: but within my present scope I merely demonstrate that having accepted the data as pertinent, we can reach different conclusions by analysing them with an open mind, together with the broadest possible data bank, and in the light of minimum statistical principles. The publication tabulated the data and limited itself to concluding that "all" the data preserved the classical Bertram-Terzaghi filter criteria as continuing to be satisfactory and sufficient.

In Fig. 5, however, I synthesize the same data points in one possible graphical form that should open some questioning. Is it not important for technology to OPEN QUESTIONINGS FOR THE FUTURE, simultaneously with CLOSING THE TEMPORARY ANSWER for immediate sufficient use? The test data referred to clear failable vs. nonfailable boundary (in the specific test procedure) of the F-filter and B-base grainsize curves. These D_{15F}/d_{85B} INDEX of the F-filter and B-base grainsize curves. These failure-boundary points are plotted in Fig. 5a 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-parameter index) for a PRESCRIPTION on so serious a FAILURE PROBLEM. For instance, at a d_{85B} grain diameter of 0.07 mm the "well-defined boundary" developed under FC indices as widely different as 10 and 55. Indeed, all values high above the established design criterion of 4-5. But, on further examination we see that this unpretentious graph would insinuate a trend of the FC index being increasingly unsatisfactory with increase of d_{85B} : although this would seen contrary to "intuition" (dictated by what mental model?) it should call for investigation-explanation.

Fig. 5b incorporates the same test data together with the broadest possible data-bank of apparently analogous published test data. The ''failure condition data'' were separated as pertaining to sands-gravels and silts-clays, and are plotted in Gumbel extreme-value probability paper of maximum flood recurrences: the data series assembled irrespective of different testing conditions and failure criteria. The postulation of piping being a phenomenon of extreme-value statistics has been my underlying mental model for a score of years: eagerly inviting critical checking and accept/reject. Unless rejected, the trends and regressions that transpire in Fig. 5b seem of interest and impact: the conclusions would result that the Bertram-Terzaghi prescribed design criteria would involve 18% and 1.7% probabilities of piping failures in sands-gravels, and in silts-clays, respectively. On so serious a problem as catastrophic flooding failures of dams due to piping, we cannot rest satisfied with such low probabilistic TEST-CONDITION GUARANTEES (further decreased by the dispersions on the regressions) on a phenomenon logically understood to be dependent on other first-degree parameters besides the FC INDEX of 1940. What criteria of COSTS-OF-RISKS, absurdly varied, have been subconsciously incorporated in these and other design prescriptions?

Thirdly, permit me to resort to a most respected old prototype field test of our profession, regarding EMBANKMENTS ON SOFT CLAYS, the Väsby (Sweden, 1946) case, to demonstrate the possibility of unperceived error in analysis, and consequent imperative need of QUESTIONING REVISITATIONS. Intended for determining the secondary compression time behaviour of soft clay under embankment surcharge, it has become so loaded with technical value because of ELAPSED TIME IRRETRIEVABLE, that on this single historic count the potential benefit/cost ratio becomes incalculably high. Shall we repeat a (now) better-conceived test, and await till the year 2040 to be in a better position?

Space and time limitations restrict my present discussion to 3 points: Terzaghi's 1946 report; one flagrant lack of basic data, easily reconstitutable at any moment, whenever desired (if noticed); and my hypothesis of an error of simple calculation on change of surcharge pressure with settlement/time, altering principally the conclusion on the test's priority aim.

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''. [N.B. 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. Meanwhile, how bitter to reflect that present-day 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, with challenging dialectic. 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 progressive reanalyses while OUR METHODS UNDERGO CHANGES. During the recorded trajectory every single revisitation (e.g. SGI Reports after 20 years, 1966-'69, and after 35 years, 1979-81, involving some of the most illustrious institutions and geotechnical leaders) 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.

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 easily justifiable in a historic context; however, two important ones are of investigational logic, associated with the vicious circle of begging the question under wishful thinking. One would be surprised at how little monitoring focussed on the theoretical vehicle for interpretation of secondary compression, which should be the EXCESS PORE PRESSURE; but the imprecise instrumentation of the time, coupled with confident deterministic expectations, should be pardoned at the infancy of the profession. What cannot be excused is how little test data was collected on the ''CAUSATIVE FACTOR'', the gravel fill loading. As per the 1981 Report ''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/m^3''. From the thesis that generated the Report one does not extract additional information: but if the lack were perceived and felt, additional tests to heart's content could have been incorporated at any time in the interim. I would conclude that presumably not only was the gravel fill rather loose, but understandably ALMOST DRY. Let us assume percent saturation and water content of the order of Sr = 15% and W = 3,75%. Obviously as such a fill submerges (by settling below water table) its Sr would increase to about 95%: Thus the unit weight of the submerged thickness would increase to 2.01 t/m^3. Such simple test facts about physical indices of the gravel fill become very important because the 1985 Report submits a strange theoretical conclusion that exactly as rapidly as pore pressures dissipate (and observed settlement continues at constant rate) there is an EXACTLY CORRESPONDING DESTRUCTURATION of the clay structure, reconstituting the pore pressure. This Theory, classifiable as a THEORY OF A SINGULAR CASE was fundamentally based on 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 causative of excess pore pressure and ulterior settlements.

Finally, my presumed and postulated error of simple calculation. 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? Note that the intuition on decreasing pressure with submergence arises from compacted clayey fills that start at Sr ≅ 95% and would hardly increase in Sr perceptibly (cf. the need for back-pressure saturation in micropore soils). I refer to Fig. 6 and go back to first principles of ''prospective'' effective stresses as TOTAL STRESSES minus PORE PRESSURES. With a constant groundwater 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 increased unit weight compensating the ΔH compression. As far as concerns the gravel, repeating for times t = 0 and T, corresponding 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. 6 should clarify the reasoning, and the graph (C) indicates the changing total stress with settlement. Thereupon, an openly questioning revisitation on the case, to retrieve, its present extreme of a ZERO benefit/cost ratio, might seek easily incremented and improved test data and calculations: the theory of ''self-induced primary consolidation process ... likely to continue until the clay structure reestablished itself'' would implode.

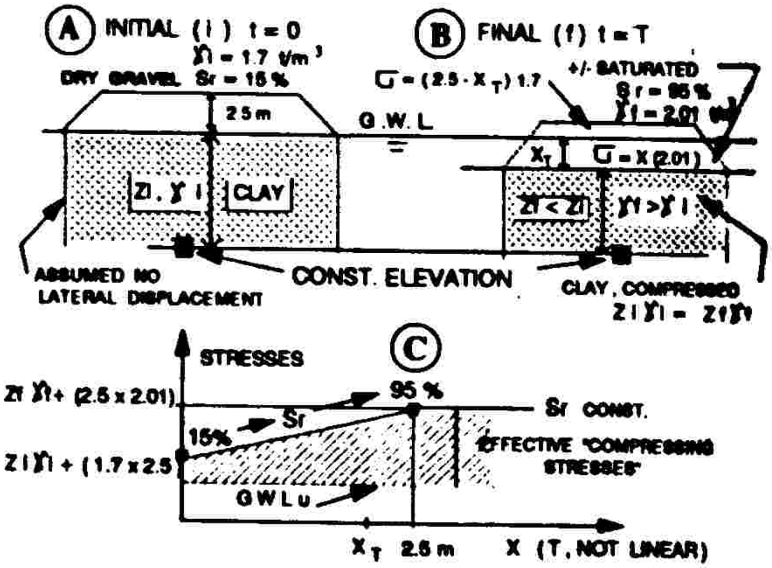


Fig 6. Väsby test fill,1946. Calculation of stress increasing and not decreasing with time.

[N.B. Incidentally, because of atavisms possibly the same MAYBE REVISITATIONS would apply also to the invaluable Skå-Edeby 1957 test fills much used for calculations on long-term settlements of clays with vertical wick drains].

It is the principles of ''certainties'' vs. progressive technological advancing that are at stake, not the specific cases and efforts involved, subject to eternal ulterior critical revision. And persons behind such efforts profitably dispense identification, in order to promote frankly open reevaluation, since all of us in civil geotechnical engineering are merely laudable instruments in the obligation to service through testing the frontiers of ignorance and impunity in the very prototypes.

EAGERNESS FOR RAPID INNOVATION IS DETRIMENTAL.

Is it not natural that at all stages of our trajectory we have had (1) the best intentions (2) some degree of (unjustifiable) confidence, nurtured by the scientific concept, that NOW, INDEED, we have a grip on ALL THE FACTORS THAT MATTER? How else would one explain, and condone with, the gross negligence regarding statistical confidence bands, besides regressions (slowly introduced)?

At the risk of repeating the oft-forgotten obvious, I note that there are very fundamental differences between science and the technological practice of CIVIL GEOTECHNICAL ENGINEERING. Science does not have the principles of SAFETY and SERVICEABILITY CRITERIA, which are the priority essence of engineering. And, further, in distinguishing between such modern epitomes of success as the industrial engineerings (mechanical, chemical, electronic, etc..) in CIVIL GEOTECHNICAL ENGINEERING we have to (1) deal with individually different conditions at every job (2) use the very prototype as a test, working between the frying pan(economic failure)and the fire technical rejection),

With due respect, the most laudable and illustriously sired MILESTONE CONTRIBUTION of the Bothkennar (Geotechnique, 1992) soft clay test site, under the farsighted and noble intent of the UK Science and Engineering Research Council (SERC) may be used for making my point. The invaluable contribution must be gratefully recognized, of the intended international test bed site, with a call for a worldwide cooperative effort, as one soft clay engineering research site for uninterrupted long-term research.

Engineering must straddle judiciously between singular sophisticated cases, and multitudinous roughly assessed similarities/variabilities. On indispensable historic ties we might humbly remind ourselves that each single milestone, too widely separated for direct vision of others, might well be seduced into fancying itself as the ultimate nirvana. There must be a ROAD (involving practice) already sportmarked 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.

The scientific conscience is markedly evidenced. Merely as an example I pick on the question of sampling and sample quality (persisting in the error of employing only METHOD SPECIFICATIONS, without assistance of END-PRODUCT SPECS., whereby with change of depth the quality should suffer an inevitable trend for difference); and I employ the researchers' own logic in benefit of collateral check against worldwide "routine practices" for tying-in with historic experience.The aim "to use the sampling and testing techniques that were regarded as the best available current practice" is stated, and is meritorious for a spearhead. But, were not past efforts admissibly intentioned in like fashion? What lesson of logic from the very fact that the 3 "best current samplers" selected for use gave quite different results? 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: an earnest call to the important institutions across the world involved in such past efforts must be made along this avenue, because it is the ONLY WAY TO ADD STATISTICAL CREDENCE TO THE BOTHKENNAR SINGLE-CLAY FINDINGS. The other, more feasible immediately, is for Bothkennar to repeat 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

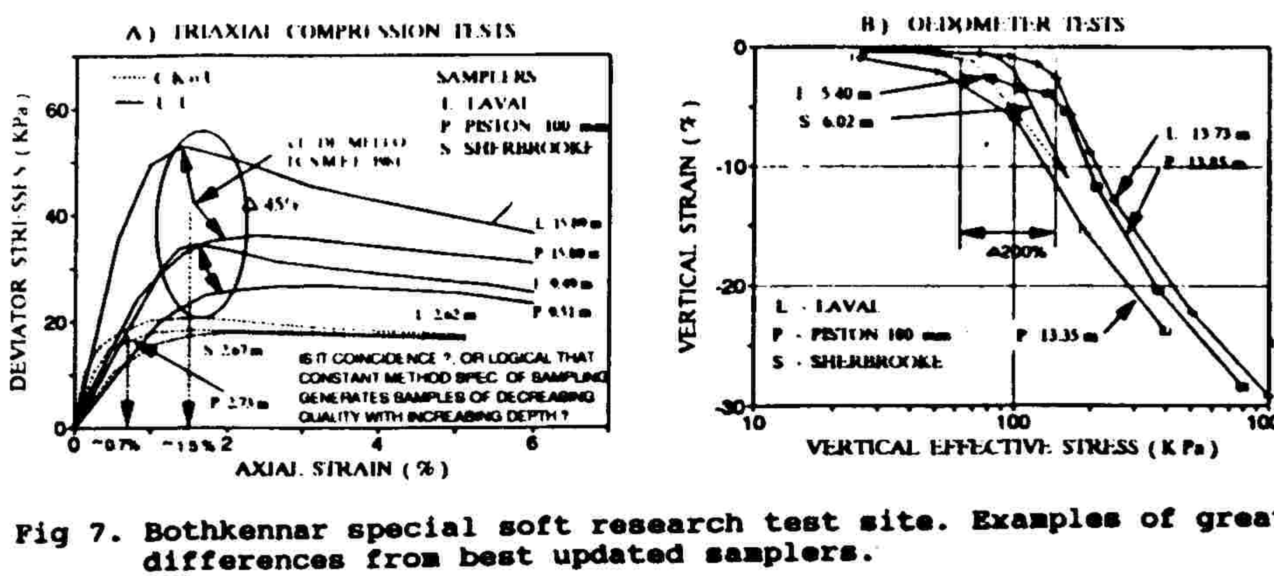


Fig 7. Bothkennar special soft research test site. Examples of great differences from best updated samplers.

summarized in Fig. 7) between key parameters 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 (means, not ends), and also of subsequent calculations and decisions, that were spread across the world, and are still in duly respectful use by good disciples and acolytes. Note that when results were poor, tending towards significant disturbance-remoulding, and timidly adopted parameters, obviously the difference across the world had been greatly attenuated, FAVOURING A COMMON LANGUAGE AND PRESCRIPTION; but they were made sufficient for each start, inescapably humble, but not necessarily too conservative/expensive in final decisions, because of the possible compensations in adopted theories and adjustment factors.

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 judgement 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 judgement, the PURPOSE of geotechnical engineering RESULTS?

We cannot disregard 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 analyzed. We cannot 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. For statistical appraisal, and good benefit/cost results, we must muster evidence across geography and time.

To enhance comparative respect for some of the past, inevitably simpler, I shall limit myself to the following quote from the paper ''Predictions associated with the pile downdrag study at the SERC soft clay at Bothkennar, Scotland ''by Little and Ibrahim, Wroth Memorial Symposium, pg. 809: ''Generally, predictions for down-drag over-estimated that measured in July 1992. In some case the over-prediction was 500-600%. The size and distribution of the downdrag prediction submitted by Rojas and Houlsby (USING THE METHOD OF ZEEVAERT) was considered to be VERY CLOSE TO THE ACTUAL DISTRIBUTION''. Accumulated experience, rather than lure of rapid innovation.

LURES AND PITFALLS OF PREDICTION VS. PERFORMANCE CHALLENGES.

A new era was established when T.W. Lambe (M.I.T. 1967 on) discovered the need to expose the great discrepancies that were occurring in the capacity of predicting the real behaviour of projects in the practice of the profession. Such challenges have proliferated across the world, generally under sponsorship of the most prestigious Academic and Society Institutions. I limit myself herein to revisiting the pioneer case and another analogous case 15-years later, both of embankments on soft clays. A comparable analysis of Foundation cases is broached in the Odair Grillo Lecture, 1993. The global lesson extracted should be depressing indeed, except for the fact that we must take cheer in educating ourselves to rise undaunted from episodic falls, and to rethink our stand in repetitious failings: it is fundamental that we diagnose the principal pitfalls ... they are, again, on concepts of engineering decision, and statistical dispersions.

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 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[1]. 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, 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. 8 and 9 should suffice. Some striking facts of importance to ENGINEERING DECISIONS (the accept-reject prior cutoff in the knowledge distribution), may be summarized;

a) The 10 learned predictors (more documented than, say, 98% of typical similar professional cases[2] used widely different

[1] ''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...''

[2] ... statement was ''The major cause of inaccurate

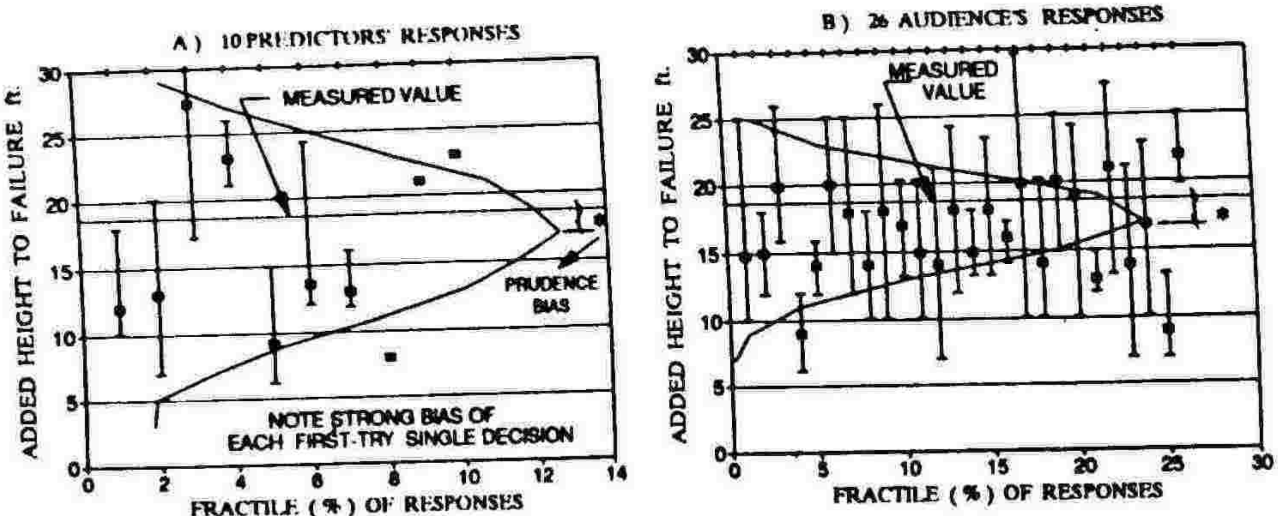


Fig 8. M.I.T. 1974 embankment performance challenge.

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.8a).

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. 8b, 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.

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.

c) Regarding such acceptance of test data (e.g. undrained strengths) at face value, Fig. 9 summarizes two extreme graphs of heterogeneities quite beyond 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.

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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''.

d) The 2-step embankment filling, firstly, of 12,2 m 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 17.9m, 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/m^3, a $\pm$ 11% variation around the mean, leading to the same variation in applied presure: 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.

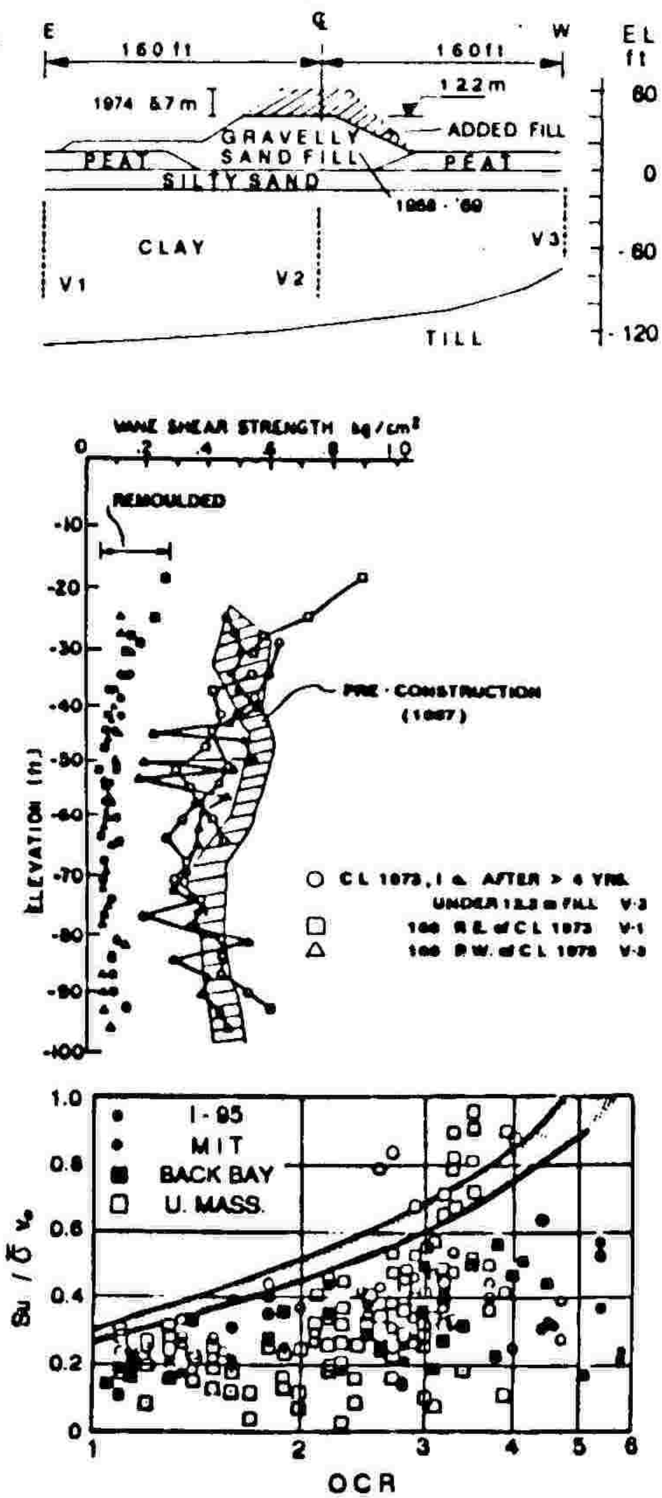

Fig 9. M.I.T. 1974 . Examples of extreme erraticity of data, some contrary to logic. Interferences of equipment, etc. to unusual degree.

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 Probability of Failure 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 proprietary 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 100mm settlement over 2 years after commissioning).

Specially praiseworthy is the fact that COSTS are submitted, the indispensable second 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. 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[3], (3) the cost data permit shockingly revealing 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.

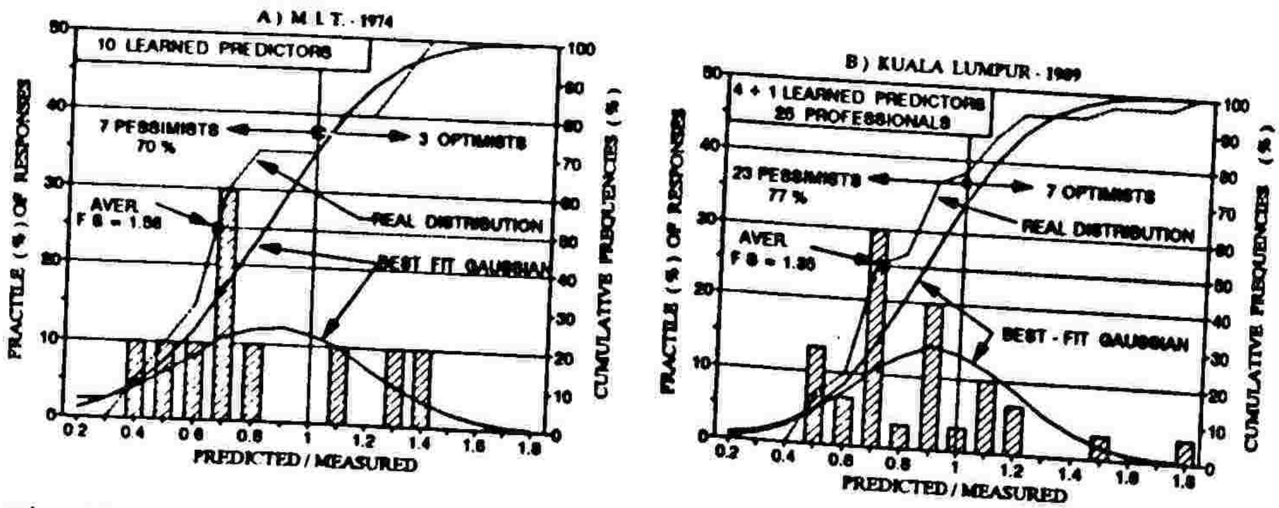

Fig 10. Comparative distributions of responses to the two embankment performance challenges.

In Fig. 10 I present the comparative probability distribution curves and bar diagrams of predicted/observed failure heights as

[3] 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.

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, 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 91-100%) merited more attention: 365 tests averaged 2.04 t/m^3, still with a dispersion of roughly ± 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 MINIUMM STRESS AND STRAIN DISTURBANCES is not being disguised under the index-role 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 additionalresisting 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, occurs slightly retarded with regard to the added weight.

The principal conclusion derived from the analyses submitted is the confirmation of the trend (schematically postulated in Fig. 3) 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 ± 1945 practice, but with what sampling-handling?) whereas in another, success is hinged on the ever-elusive in situ K'_o parameter.

It is not surprising that once again the Knowledge Probability Distribution was somewhat pessimistic-prudent, and very dispersed, whereas Nature's hehavior 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 derives directly from the use of ''cutoffs'' on the probability frequency distribution curve (cf. Dr. Mello, Terzaghi Oration, New Delhi, 1994).

For the sake of simplicity[4] in the comparative nominal cost computations 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 3.5 m (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. 11). 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[5] aiming at the bull's-eye of coincident average failure PREDICTION = REALITY.

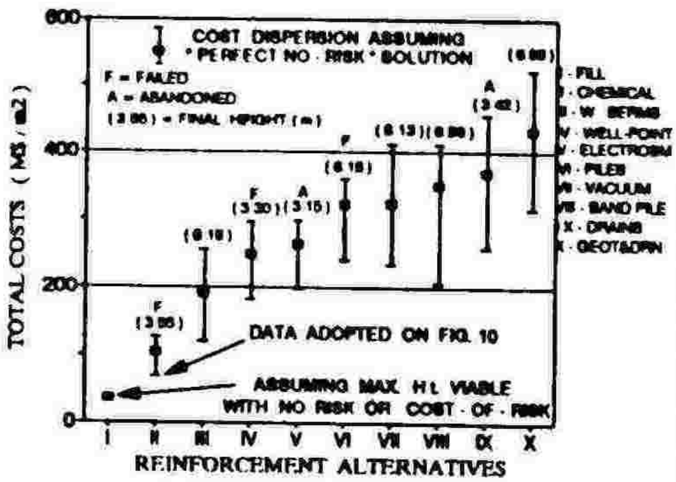

Fig 11. Kuala Lumpur 1989 Embankment challenge: summary information.

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

While we have concentrated on sité 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

[4] The more complicated situations are quite as straightforward, but lengthy, detracting from this presentation's purpose of emphasizing principles.

[5] 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.

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 Normal Distribution ND curve as shown in Fig. 12 A, and a different dispersion on it.

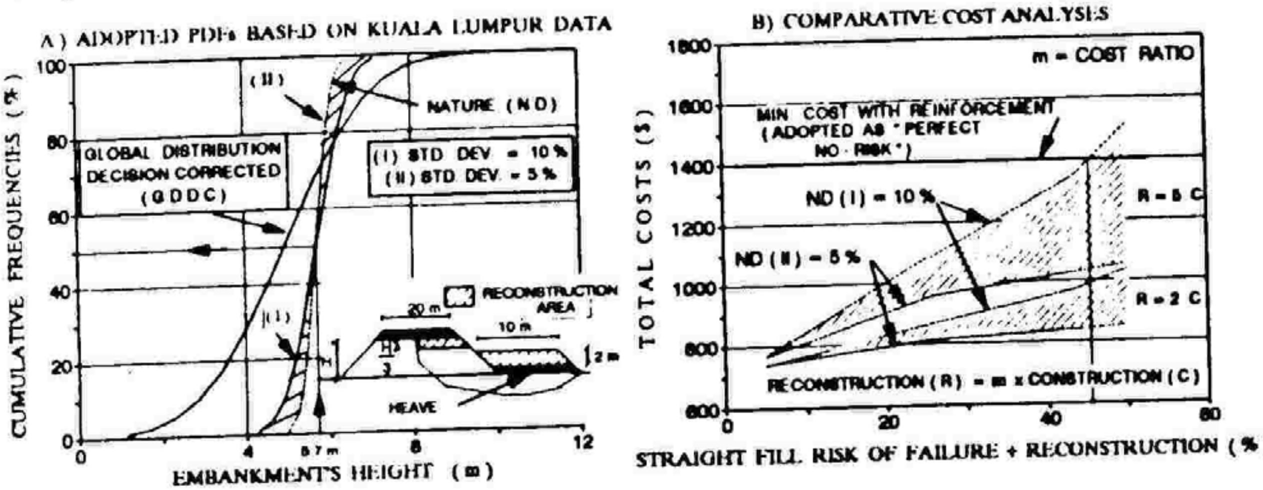

Fig 12. Bases for comparative cost analyses, showing great advantage of optimist and repeated corrections of failed sections.

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*. 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 presented in Fig. 12 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

* 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.

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?[7]

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.

The details summarized on these 2 sample embankment-on-soft-clay cases are found to have been persistently reaffirmed, in sundry foundation prediction challenges also, with regard to some fundamental conceptual lessons. I thus submit that the rare and expensive prototype tests and Prediction vs. Performance Challenges merit radical 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 regrettable 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 because of the different, singular, episodic challenges. Our profession 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 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, so transformed, by disguise and anonymity, into periodically repeated type-A prediction and DESIGN-TEST cases, on the SELF-SAME DOCUMENTED NATURAL BEHAVIOR.

In any process of adjusting ourselves to a goal (by skew-Bayesian successive adjustments of prior to 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 BEHAVIOR, 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, variable; (3) Our capacity to analyze, forecast, and decide, with equivalent justifiable confidence in our consequent results and decisions; likewise variable with time; (4) Our capacity to EDUCATE OURSELVES, measurable by systematic evolution of improved

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[7] 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.

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" (cf. Terzaghi) 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 instruments, parameters and methods, has been to hasten to open MORE NOVEL PROPOSALS, each and all inevitably born naked. 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.

As a conclusion of the above exposé of our frailties, in order to embark on a minimal submission of a local message to my dear colleagues, I synthesize in the following PLATES 3 and 4 some of the keyword thoughts.

SUMMARY VISION OF SOME KEY QUESTIONS AFFECTING BRAZILIAN GEOMECHANICS.

Leaving aside the equally important international scene, how very may priority tasks of great technico-economical importance to Tropical Geomechanics, and to us,call for visitation, revisitation, and possible drastic revision!? Time and space force me to jot down, very briefly, only some points that to me seem to require radical revisitations considering the 50 years elapsed since the promising implant of local geotechnique (e.g. the ground-freezing underpinning of the 26-store CIA. PAULISTA DE SEGUROS building, R. Libero Badaró,cf. Geotechnique Mar. 1956, pp 1-14). Some of the topics on foundations are expatiated in the ODAIR GRILLO Lecture, 1993.

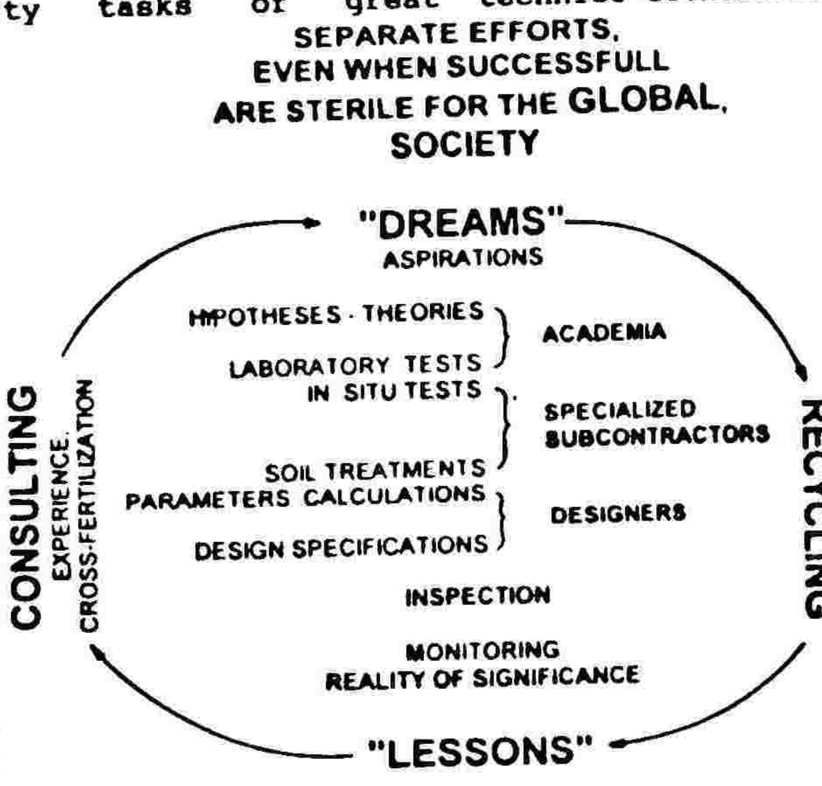

Plate 3

1. GEOLOGY,

AKIN TO... ANTHROPOLOGY ARCHAEOLOGY... ETC. } MODELS OF ADMIRING STUDY OF THE **PAST**, INVALUABLE, REQUIRING **RECONSTITUTIONS**, PASSIVE.

CONTINUED PROGRESSIVE ANALYSES.

2. GEOMECHANICS → ACTIVE PRESENT

CONSTITUTION OF MINI-TERM **FUTURE.**
PRUDENT SETTING WITHIN PAST TRAJECTORY

3. PRESENT RADICALLY REVISED NEEDS

A. STOP EUREKAS OF THEORIES
ABOLISH **INDIVIDUAL** CASE-HISTORIES
(cf. Terzaghi's request, ICSMFE 1953) DESCRIPTIVE

B. USE **WORKSHOPS** ref. **GROUPS** OF CASES

1. "WELL" DOCUMENTED. **ANONYMOUS**
2. FILL-IN MISSING DATA, PARAMETERS, VARIATIONS
3. DEBATE DOUBTS OF ALTERNATIVES
4. PINCH-IN DISPERSIONS REF. THEORIES
5. REVISE PRESCRIBED PRACTICES
6. PRIORITY FOCUS ON **COSTS**

Plate 4 - Perspectives. Message

1) The loosely denominated TERTIARY beds of Sao Paulo have repeatedly demonstrated having been reworked, deeply eroded and redeposited. The very foundation case above-mentioned suffered from a minor similar erraticity. Erosion banks in stiff to hard clays are subvertical, up to 10-30 m (very significant for foundations and tunnels). Moreover, there has been since the very beginning (M. Vargas, 1951) a dire need confirm and improve the SAMPLING-TESTING-DISTURBANCE CORRECTING of the period, and to correct the erroneously idealized-simplified evolution of geologic sedimentation levels based on ODOMETER PRECONSOLIDATION PRESSURES. Preconsolidation pressures by overburden in sloped terrain, and with thick unsaturated upper horizon, have never been challengingly researched. Likewise, assuming strongly meandering streams and rivers, geologic-geomorphological identifications across several millions of years have been sadly deprived of minimal quantifications in dating, rare mineralogy tracers, very-long-term chemical (e.g. limonitization) and secondary effects, etc..

2) Residual and saprolitic-soil geology/geotechnique has not even started, despite repeated appeals. Simple examples may be cited. a) Considering the tens of thousands of borings-samplings across residual soils, saprolites, and bedrock, use of CHOSEN INDEX TESTS and statistical studies could permit notable economies via ANTICIPATION of underlying bedrock petrography etc. based on overlying horizon mineralogies etc.; b) Statistical correlations should be investigated via test-pits, between joint-sets in

saprolites, and the underlying bedrocks, as cored, and as exposed in project excavations; c) in many a profile wherein the top of historically eroded bedrock is identified by coarse basal gravels, by judicious dating the time of ulterior development of saprolite/weathered-rock horizons could be associated with the respective thickness; d) effective permeabilities of weathered rock horizons, as compared with tests in borings (e.g. Lugeon) should be statistically compared with flows measured in projects (ideally modelled through presumed flownets); e) and so on.

3) In defining shear and tensile strength parameters that really intervene in stability problems in saprolites with relict joints, NEVER YET, to my scant knowledge, have there been RESEARCH ATTEMPTS TO DISTINGUISH BETWEEN MASS BEHAVIOR AND PREFERENTIAL BEHAVIORS ALONG THE PLANES (cf. De Mello, Hong Kong 1972). Should be more relevant than anisotropies (imitating foreign academic research).

4) Comparisons of shallow footing foundations of analogous high-rise buildings across 40 years (cf. Odair Grillo Lecture 1993) demonstrate that bearing pressures used then were much higher; and, with settlements (several measured, and, as is inevitable, mostly estimable) of several centimeters, buildings showed no unacceptable distress. Why have the neatly more expensive design-construction practices, and settlements limited to mms. (less than thermal and creep effects) progressively invaded the building construction industry, and at what unchallenged and unexplained costs to Society? How far do maintenance and overhead costs of buildings due to defective HYDRAULIC INSTALLATIONS exponentially surpass those eventually due to foundation-generated distress? Is this global Civil Engineering? Or are we, typically, paying the scapegoat price, merely because we have been far more advanced/honest than collateral branches, and have recognized/mentioned a problem, while others have been unmindful and secretive about theirs, VERY MUCH BIGGER/MORE-OBNOXIOUS?

We must retrieve and digest our historic data, of a world-unrivaled ''park'' of high-rise buildings built in the 4 decades in which, for instance, Sao Paulo's urban population grew by about 12 million, and Brazil's population by the equivalent of more than a France plus Spain put together.

Indeed we must reflect somewhat bitterly on why some such efforts as Doctorate Theses on (ex.)numerical modeling of creep behavior of natural slopes in England/Canada/etc. might have had greater lure of prestigious seduction: and we have to ACT, to REVERT.

5) Driven pile foundations under closer examination, and illustrating important rethinking.

Some of our most promising colleagues armed with 1st World postgraduate studies could profit from the Predictor's vs. Audience's performance on the M.I.T. 1974 Embankment Challenge to rethink their attitudes towards the effective development of our art-technology. There is no such thing a theory VERSUS practice: and, if there is, it is effective practice progressively advancing that we want, because to any theory we should always attach the qualification PSEUDO-theory

In any piling, driven-displacement, or bored, the academic insistence on behavior analyses based on ''perfectly'' undisturbed effective stress parameters is shameful hypocrisy because of INSTALLATION EFFECTS. Until now the timid attempts to incorporate such intuitive complications as excess pore-pressures and remolding-reconsolidation consequences have been far too timid... and quite understandably, because the accumulation of field data on the problem is, and will doubtless remain, a starry-eyed dream. If we reason statistically, over the past 50 years the theoretical intuitions have been known, unchanged: literally millions of piling projects have gone up, each documented with a few borings alongside with hundreds of piles, ... a frequent rough ratio of a hundred to one. How many such cases have had collateral documentation on effective stress parameters and intervening priority parameters? One in hundred-thousand? Why, and what perspectives of the horizon changing? If we really feel in our groins the important statistical attitude towards advancing analysis and practice, is it not obvious (dispensing courage) that we should laughingly set aside the pomposity of ''theoretical solutions'' to the problem? Have we been cornered into forgetting that in truth the crude beginnings of SPT and CPT were conceived as models to the piles as prototypes?

If you have to duel somebody, don't ever choose the weapon at which your opponent is far better: no inferiority complex can ever atone for suicidal decision to be inferior. Academia propose effective stress analyses because, indeed, that is their weapon and singular, they have no access to hundreds of cases RESPECTABLY ANALYZABLE IN PRACTICE, and they know that professionals don't have that weapon. We are the ones that have multitudinous data on SPT (or CPT in few European countries), and on driven piling, in our soils (locally differentiated). Does that make our data LESS, or MORE important? Does that make direct meaningful GROSS LUMPED-PARAMETER statistical correlations LESS VALID than indirect attempts to work through ''theory''? Do we not remember by the theory of errors how exponentially broader become the dispersions when working through a longer sequence of parameters, especially when NONE OF THEM CAN REASONABLY BE CHECKED AND REVISED, since almost universally the only comparison is gross end-results?

Two empirical lumped-parameter correlations between SPT and DRIVEN PILE LENGTHS TO MEET BRAZILIAN-CODE BEARING CAPACITIES have been in successful use: The Aoki-Velloso (1975) and the Décourt-Quaresma (1978) methods. In the O.Grillo Lecture I point to obvious needs and methods of updated revisions, principally because their backup data have been critically examined to have pertained to micro-deformation nominal failure loads as imposed by the old illogical code: it is NOT DIFFICULT, but EXPENSIVE to Society, to be right by being excessively prudent. But the points to make here are: (1) How sad that across 15-20 years, with very much more (and selectively more pertinent) data, those excellent offered efforts have not been supplemented, revised; (2) How revealing of the misoriented unimportance given to the subject, that never has a local WORKSHOP been convened on the question; (3) How sad and revealing that in no other country, of those that use SPT systematically, the hint should not have been taken of DEVELOPING ANALOGOUS EMPIRICAL RELATIONS, especially since over the past 23 years (cf. De Mello 1971) the stature of corrected-improved-promising SPT has been gradually redressed from bastardly to legitimate; 4) How revealing that in the collateral case of analogous but static penetration CPT the theoretical-analysis trend has literally suppressed any attempt at lumped-parameter correlations, everything having gone via confused and confusing theoretical analyses mixing strengths and deformabilities.

The astounding conservatism that is presently resulting is demonstrated (in the O. Grillo Lecture) by the fact that the probability of a ''nominal failure'' single driven-pile load being reached is a low as 0.04%, to be compared with such catastrophic sudden events as a 1:1000 to 10.000 flood risk for a dam and spillway. What respect to we desire or merit?

6) Tolerated and tolerable settlements, total and differential, of buildings.

The Skempton-McDonald (1956) recommendations on limiting distortions for damages to cladding of high-rise buildings etc. (and subsequent Bjerrum, 1963, expatiations) arrived, and were noted, among us considerably after very many buildings in Santos had been designed and built, UNDER THE PRESSURE OF SOCIOLOGICAL NEED, for acceptance of very much higher limits. The Santos "park" of buildings, several hundreds, very closely analogous, has essentially continued with the local practices (so much so that geotechnicians are not even consulted any more, since over a decade or two). Many buildings now have up to 45-50 years of long-term settlements of a meter or more, often with distortions up to 1:50 or so. Should we not recognize that both for ourselves nationally, and for the developing world, this is an INCOMPARABLE STATISTICAL LABORATORY OF PROTOTYPES for reanalyses, in comparison with which the very best of international intents and offers become dwarfed?

To free us from castration by "authority" let me begin by quoting authority vs. authority. Terzaghi's discussion on the Skempton-McDonald welcome MILESTONE included "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 unnecessary incremental costs to Society, our conclusions should risk being OPTIMISTIC, AUDACIOUS, to test and observe, quantify, the pre-failure (pre-unacceptability) limits. Have we OPENED, or CLOSED and FORGOTTEN?

If we openly reexamine the data-bases of those respected pronouncements, two starting objections on logic stand out, besides many other questionings repeatedly mentioned. (1) The word "foundation" automatically evokes "subsoil": let us urgently adopt the substitute "SUPPORT". The stringent requirements attributed to machines only apply to the "top-of-block support", and that only after the machine has been immovably anchored. Regarding buildings, note 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. The "support" of the 15^{th} floor is the completed structure up to the 14^{th} included. (2) One cannot seriously study statistical regressions of beans and beasts within the universe of words starting with b. Within the gross interference of CRUDELY ESTIMATED GLOBAL RIGIDITY (not merely the structural engineer's nominal one, but also that of imbricated wall panels complementing), which is quite specific to COUNTRY-CODE-MARKET, one must profit of given buildings, with permissive settlements, for obtaining settlement profiles in EACH FLOOR SUPPORT, each building as one fixed statistical universe (partial differentials) of investigative interest.

Santos provides unrivalled conditions for statistical recovery of data on both counts, even if settlements have not been repeatedly measured and cracking continually monitored, because the smooth time-developments permit credible reconstitution based on any recent settlement observation, and the noticeable EFFECTS OF CRACKING must be recorded in the Condominium actions on maintenance.

7) Slope stability analyses correctly applied to DESTABILIZATION CONDITIONS.

By revisiting the historic contexts one cannot fail to justify and praise the developed methods of STATICS of slope stability analyses (much debated down to second-order factors). And the methods persist. However, revisitations of collateral developments have long since exposed 4 failings, one very grave, which should have been radically corrected by the means easily available now.

The principal point is that via 2 successive stability analyses, before (FS_1) and after (FS_2) a certain CAUSATIVE FACTOR, what we do is to calculate $\Delta FS = FS_1 - FS_2$, the DESTABILIZING FACTOR introduced. The principles of significant relevance of stress-strain-time trajectories impose such reasoning; moreover, we always evaluate much more precisely WHAT CHANGES ARE WROUGHT, rather than the presumed status quo. The grave point, however, came as the corollary of rigid-body statics vs. Failure: the ABSOLUTELY ILLOGICAL PRESUMPTION that, AT failure, the slope FS is exactly equivalent to 1.00. Very illustrious authoritative papers, of the period when what was at stake was to confirm METHODS OF ANALYSIS, put forth, and undesiredly perpetuated, this absurd numerical equivalence. What I emphasize is that failing corresponds to PASSING THROUGH FS = 1.0, and NOT BEING AT FS = 1.0. Moreover, by intuition it would seem that the speed and dislocation of the failing volume should be associated with the magnitude of the ΔFS generated, and how far below 1.00 the FS_2 reaches. Consistent with my keynote message it is obvious that we shall have to collect data statistically: but, to begin with, one has to look at cases with a proper vision.

The third and fourth points can be secondary in many soils and cases, but in cases of my local experiences have been the almost consistent rule. One is the fact that slope failures tend to be CYCLOIDAL, SUBVERTICAL at top and subhorizontal at base, and NOT CIRCULAR: easily justified by tensile failure (at top, especially in more extended slopes) at neatly lower strains, and with $\varphi = ds/d\sigma$ greatly decreasing with σ. The other factor is associated with the above, and corresponds to rejecting the rigid-body premise of simultaneous failure at peak stress-strains all along the slip surface. The conditions of mud flows vs. calm accommodation of slide volume to a slightly flatter (stable) slope, should be distinguishable by contractive-saturated vs. dilating-unsaturated stress-strain-time behaviors, and must be treated distinctly.

8) The skeletons in the family closet, and our professional conscience.

As a final point I limit myself to showing a few photographs. I have seen these evidences almost daily, over decades: and I wonder why is it that so many of us professionals SEE AND DO NOT PERCEIVE, preferring to read (especially in foreign languages) and render prostate reverence. Slopes resisting by surface limonitization, compared with adjacent soils rapidly eroded. Sliding insinuated by trees vs. sophisticated instrumentation. In the same apparent geology, the comparison between a volume that did slide, and others at the four boundaries that are and continue perfectly stable. Behavior of thick unsaturated ''porous'' horizons, and how to investigate them, both in situ and in the lab., possibly resorting to ''non-wetting'' fluids for ability to measure pore pressures. The effective, real, suctions in the field, and how to measure appropriately their changes, often so devastating in causing landslides and mudflows under light-persistent vs. heavy rain. So many local orphaned topics of great importance: do we not notice them, in order to adopt them as truly ours?

The photos presentable in the paper are regrettably much fewer, and less vivid, than in a rapid succession of colored slides. It is hoped that they will merely serve as a reminder, to prick our professional conscience.

CONCLUSION

The world needs engineering, and economical engineering, more hastily than additional glitter of science. Civil and geotechnical

Frequent evidence, dangerous in urban areas. Are trees statistically valid inclinometers for our soil profiles that improve much with depth? What risk thresholds?

The frequent surface limonitization that develops excellent slope protection: when and how naturally? How to foster artificially?

Marked geochemical differentiation Worth investigating?

How rarely indeed is the slip surface circular? Revising analyses indispensable.

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. 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.

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)