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Embankments on soft clays, a continuing challenge of misspent efforts

Victor F.B.de Mello
São Paulo, Brazil

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ABSTRACT: A few very important cases of embankment test fills are reviewed, and lessons are extracted confirming to a surprising degree the fact that any past effort, no matter how good at the time, inevitably calls for ulterior revisions, both of mistakes and bias, and of dispersions. However, using as mere examples the world-publicised cases of Vasby, Sweden 1946, the M.I.T. Performance vs. Prediction 1974, and the corresponding Kuala Lumpur 1989, one concludes that there is little cause for elation regarding ENGINEERNG, despite the notable advances in geotechnique's scientific bases.

1. INTRODUCTION

In surveying the developments in Geotechnique from Harvard 1936 to New Delhi 1994, we cannot but emphasize two very important points: Firstly, the difference between geotechnical "science" and the dominant routine applications in geotechnical "engineering": thereby, we can perceive somewhat of effective reality within different degrees of shadows, despite the dazzling brilliance of the advances of scientific methods and results. Secondly, the fact that for the needs of ENGINEERING, which is decision and action despite doubts and dispersions, one must respect an intermediate rate of change of available tools, to permit establishing "factors of adjustment" of nominal-truth to reality, and thence the experience and judgement. Too fast a rate of introduction of novelties leads to confusion and sterility; on the other hand, too slow an incorporation of advances must also be rejected as frustrating the very principles of progress and engineering optimization.

2. 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 re-evaluate 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 ("one must be suspicious when an author uses type C 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, which relies entirely on a patient progressive adjustment of estimates TOWARDS 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 Figs. 1, 2, 3, 4 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.

3. 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.

3.1. 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

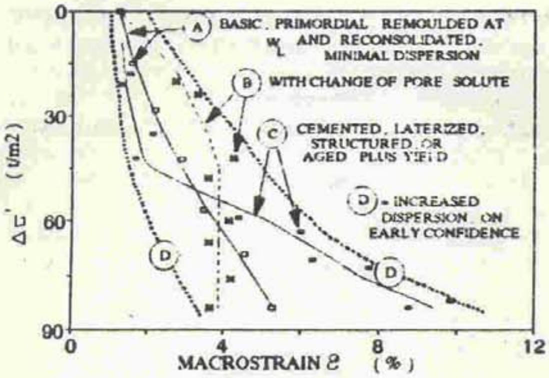
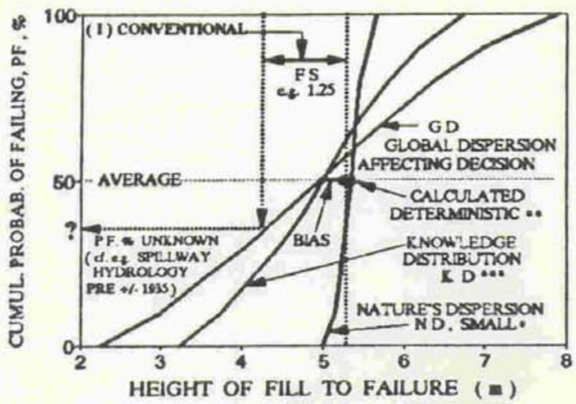


FIG. 1 SYSTEMATIC IMPACT OF LAB. RESEARCH FINDINGS ON THE SPECTRUM OF DISPERSIONS TROUBLING THE PROFESSIONAL (SCHEMATIC)



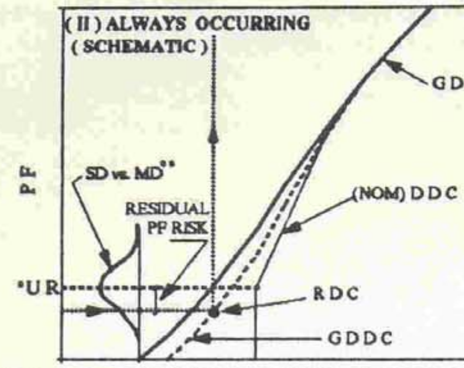
* WITNESS "PERFECT FAILURES"

** RESEARCH DEVELOPMENT AIM AVERAGE = REAL

*** GREATER, BIASSED, SKEW

(I) IGNORANCE OF BIAS, DISTRIBUTIONS, PF s, etc. HINDERS PROGRESS BY EFFICIENT SUCCESSIVE DECISIONS

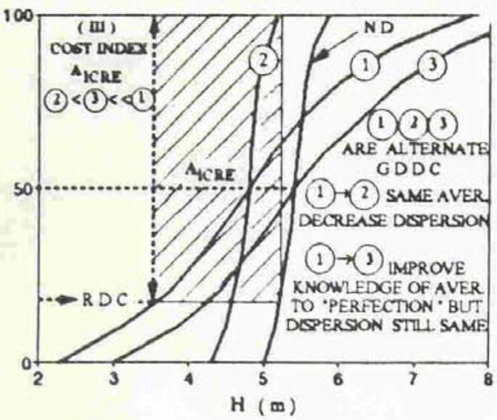
FIG. 2 DIFFERENCES BETWEEN CHALLENGE OF SHARPSHOOTING AT MEDIAN MARK, AND ENGINEERING OF AVOIDING GREATER RISK THAN TOLERATED



** FIRST - TRY AND SINGLE DECISIONS, SD, INEVITABLY VERY SUBJECTIVE, UNACCEPTABLY OPTIMISTIC OR PESSIMISTIC AND SKEW. FOR ASSESSING RESIDUAL RISK OF GDDC, SHOULD REALLY USE GAUSSIAN DISTRIBUTION OF MULTIPLE DECISIONS MD, OF SAME INDIVIDUAL + METHOD, IN "SAME CASE". WE ASSUME ACCEPTABLE USING AS SIMILAR THE PDF OF "MANY EXPERIENCED"

GD - GLOBAL DISTRIBUTION, ASSUMED GAUSSIAN
GDDC - GD DECISION CORRECTED
RDC - PF OF REAL DECISION CUTOF
UR - ASSUMED UNACCEPTABLE RISK, FOR SINGLE PREDICTOR'S DECISION

FIG. 3 RECOGNITION THAT BECAUSE OF PDF, THE SINGLE DECISION CUTOFF AT GIVEN PF ALWAYS IMPLIES SOMEWHAT HIGHER RESIDUAL RISK PF THAN USED



ASSUME GIVEN PHYSICALLY POSSIBLE FAILURE OF 50 m LENGTH, EMBANKMENT OF INCREASING H, 1000 m LONG ∴ ≈ 20 POSSIBLE PHYSICAL FAILURES. FAILURE CASES ARE NOT STATISTICALLY INDEPENDENT, BUT PROGRESSIVELLY INSTRUCTIVE, TO QUICKLY APPLIED EXPERIENCE INDEX.

A_{ICRE} = AREA INDEX OF INITIAL COST OF RISK EXCLUSION. (BY GIVEN DECISION)

ON (1) LOWER AVER. AND GREATER DISPERSION, FOR FILL OF H > 3.4 m REINFORCED FOUNDATION USED BY RDC. HOWEVER, FILL RISING UP TO 5.2 m WOULD NOT REALLY REACH RDC ON ND.
∴ A_{ICRE} GOES FROM RDC TO 100% AND 3.4 m TO 5.2 m CASE OF INCREASING DISPERSION OBVIOUSLY MUCH WORSE

FIG. 4 SCHEMATIC EXAMPLE OF CALCULATION OF COSTS OF RISKS. DEMONSTRATION THAT FOR ENGINEERING, DISPERSION IS MUCH MORE COSTLY, EVEN IF CHALLENGE MEDIAN MARKSMANSHIP ACHIEVES "PERFECTION" 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 three important revisitations, around 1966-69 (after 20 years), and 1979-81 (35 years), and Apr.85, Sept.87 ASCE, 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 reestablished 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 reestablishment 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 th many internal inconsistencies cal for redress and for one or mor additional revisitations.

As a startling beginning venture a suspicion that the ver poor definition of the quality o the gravel fill, coupled with wrong intuition on changes o stresses accompanying the increasing settlement, can implode th very basis of the publishe hypothesis. The entire interpretation arises from an assume calculation that as the fil settles below groundwater leve (taken as fixed) the "submergence would progressively REDUCE th applied (would-be) effectiv stresses causing excess por pressure and ulterior settlements The intuitions regarding th principles of Archimedes ar ingrained: who would stop t reconsider the specific case, i face of so undisputable ar elementary a calculation?

Are we not compelled to suc reconsideration in the face of s very important a theoretical ar professional problem as th secondary or "secular" compressic in field conditions?

In the published Report (1981 the "causative factor", the grav fill loading, is too minimal described: "The western half of th fill was placed by free dumpi without compaction, while th eastern half was compacted aft dumping. As a result of the meth of placement, the western half the fill was slightly higher th the eastern half. However, th magnitude of the load on the who area was believed to be the sam The unit weight of the gravel fi in its uncompressed state w determined to be 1.7 t/m3". Fr the thesis that generated th Report one does not extra additional significant informatio

I would conclude that presumab not only the gravel fill was rath loose, but understandably almo dry. Let us assume percent satur ion and water content of the ord of Sr=15% and 3.75. Obviously such a fill submerges, its Sr wou increase to about 95%: thus t unit weight of the submerg thickness would increase to 2. t/m3. Note that the intuition decreasing pressure with su mergence arises from compacted clayey fills that start at $Sr \simeq 90\%$ 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 $\Delta\gamma$ increase compensating the ΔH compression. As far as concerns the gravel, repeating for times t=o and T, corresponding to x settlement, the applied total stress only increases linearly until the entire gravel fill is submerged (2.5m 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: thereafter the changing tendency to generate compressing effective stress while the hydrostatic pore pressure due to constant ground-water level remains constant. In the 1985-87 revisitations this item was recognized as important, but treated lefthandedly, and left inconclusive and erroneous (?!).

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.

3.2. 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 re-analysed, 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 oftslandered 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 $\equiv$ 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

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(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..."

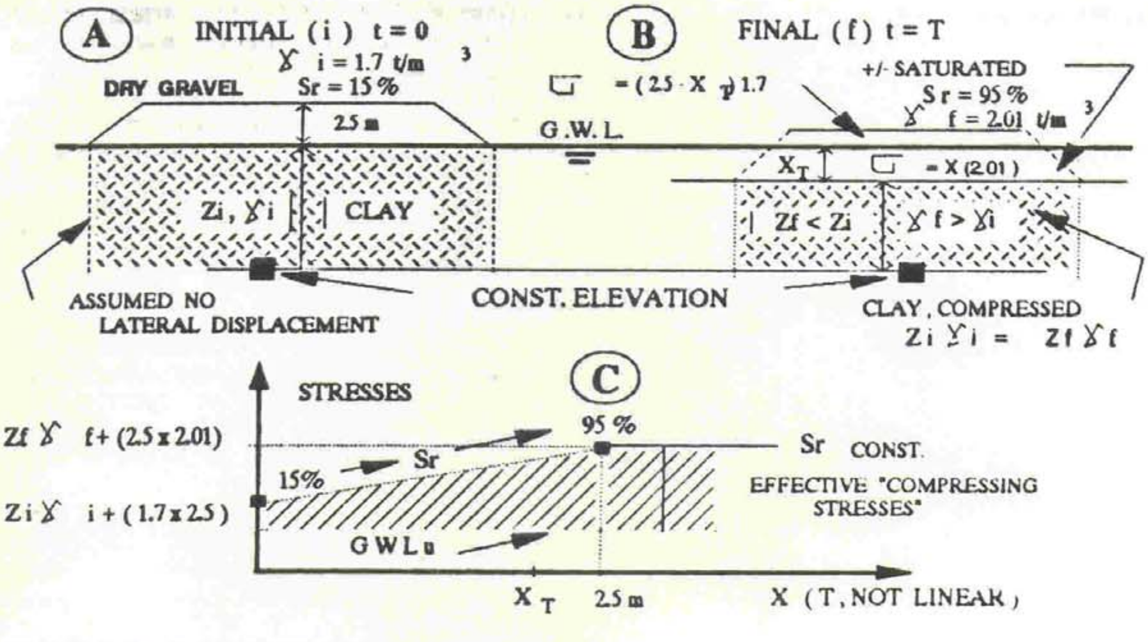

FIG. 5 CHANGE OF EFFECTIVE " COMPRESSING STRESSES WITH SETTLEMENT, GRAVEL FILL.

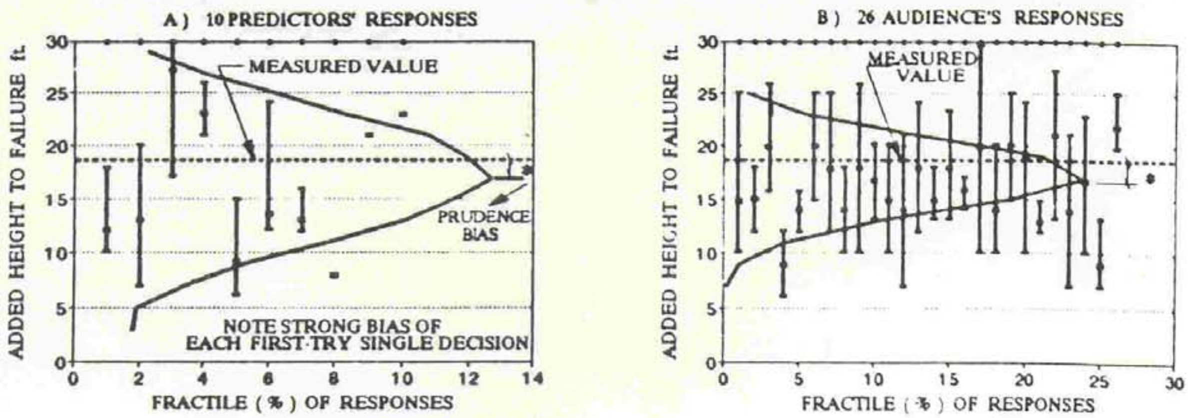

FIG. 6 M. I. T. 1974 CHALLENGE " PREDICTION vs PERFORMANCE "

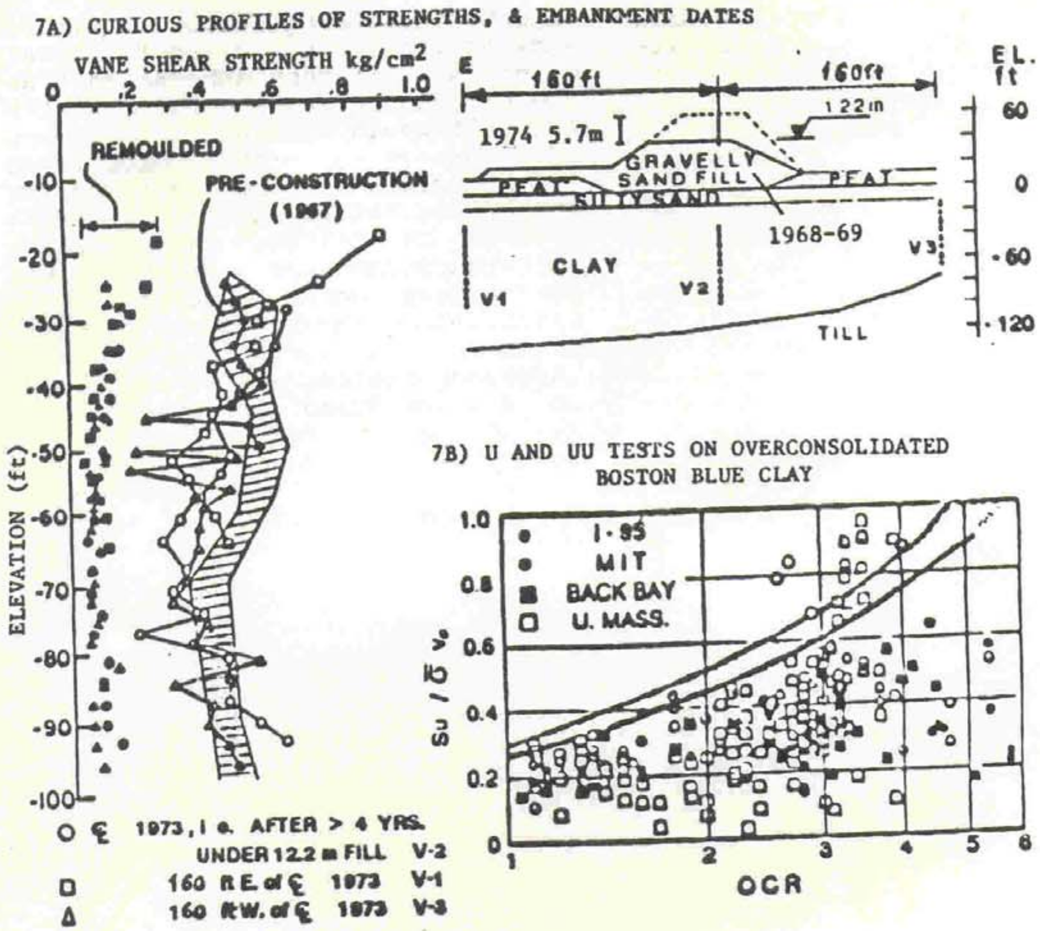

FIG. 7 M.I.T. 1974 TESTS, EXAMPLES OF EXTREME ERRACITY OF DATA, SOME CONTRARY TO LOGIC. INTERFERENCIES OF EQUIPMENT, PROCEDURES, PERSONNEL TO UNUSUAL DEGREE.

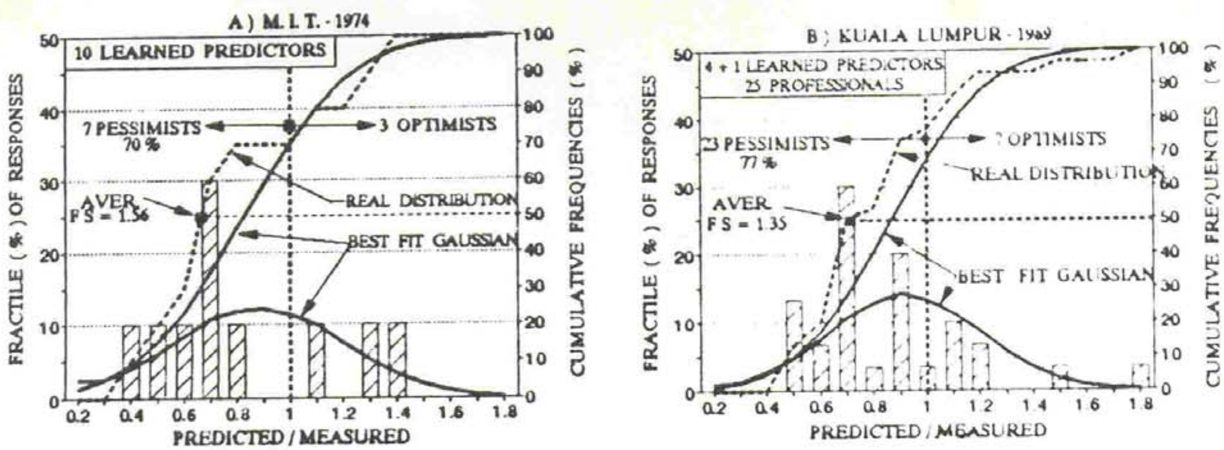

FIG. 8 COMPARATIVE STATISTICAL, ANALYSES OF 1974 AND 1989 CHALLENGES

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 destabilization potential, and/or long-term after-construction deformations, and not 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. Figs. 2, 3, 4) may be summarized:

a) The 10 learned predictors (more documented than, say, 98% of typical similar professional cases (2)) 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.6A).

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.6B, 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, since 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), and 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.7 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.

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 5.7m increment in "late summer 1974" (to failure,20/Sep/1974) constituted

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(2) 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".

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/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.

3.3. 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 100mm 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 (3); (3) the cost data permit shockingly revealing comparisons. At any rate, despite the insufficiencies and

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(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.

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 91-100%) merited more attention: 365 tests averaged 2.04 t/m3, 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/m3. 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 automatically at face value. Incidentally, with the baptismally-blessed "undisturbed stationary-piston thin-wall" samples, simply described as such, and indiscriminately used to great and constant lengths (e.g. 100, 130cm) without judicious adjustments, we should reexamine if the intent of sampling with MINIMAL STRESS AND STRAIN DISTURBANCES AND READJUSTMENTS is not being disguised under the indexrobe of automatic imposed control of length changes, via compensating internal stresses and strains. With disturbance principally controlled by minimized Area Ratio and static penetration, the further principle was classically emphasized that the tendency to expand of each sample due to stress release (obviously increasing with depth of hole) should be essentially controlled at the cutting-edge bevel, and via the inside clearance; the stationary--piston should be but a complement of prudence, the sample's 100%(±) recovery ratio having already been reasonably adjusted by a sampler penetration that balances friction compression vs. tendency to expansion. One notes that the compartmentalized distance between field and office has increased so much that not only are such details not furnished, but neither are they demanded by the predictors.

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 in most conventional testing disturbances only tend to decrease sensitive strengths: decreases of preconsolidation (or "yield") ' are the principal index, the in-situ values having to be along the upper limit profiled; but the concomitant logical trends have to be used also, confirming the decreased nominal C_c and peak s_u, and correspondingly increased strains at failure. Logical trends should condition judicious choices of parameters.

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. Note that the added layer's cohesion is not acquired by fairy wand, the destabilizing weight becoming effective before compaction is completed.

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 ±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 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(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 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(5) 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 1000m 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 site-and component-issue of methods a, b, c vs. k, l, m, n what we have failed to realize is that the most important information of all, which is Nature's Distribution

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(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.

curve (in this problem) is what we do not have (but the "experienced designer" with many repetitive cases begins to feel, if develop-mental 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.

If we are dealing with an optimist over the 1000m length of 6m embankment we would have 5,20%, 30% etc. cumulative probabilities of failure on reaching heights of 4.5, 4.7, 4.9m respectively. IT MUST BE EMPHASIZED THAT THIS RISK IS INSTANTANEOUS, WELL WORTH TAKING, BECAUSE STABILITY ONLY IMPROVES THENCEFORTH WITH TIME(6). The real failure data of the K.L. 1989 test were of a failure over essentially the entire short length of embankment, therefore pin-pointing a roughly 99% probability of failure on reaching the 5.7m height (the test was of too short a length).

For the sake of simple cost comparisons we assume that: (a) the fill rises by 0.2m lifts simultaneously over the entire 1000m length; (b) the physically viable failure lengths are ≥ 50m; (c) the drop of the crest, will be (1/3)H; (d) the volumes for re-constituting any failed section include completing the heave with an added 2m 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 5 times the initial cost of fill.

The cost of such a "shameless" non-Bayesian embankment-construct-ion test is represented in Fig.10B. The conclusion should be absolutely startling, but irrefutable: the acceptance of up to 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? (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.

3.4. 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 geo-technique, an earnest call has

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(6) 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.

(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.

been put out for enhancing it as an international test bed site, by promoting joint research in 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 spot-marked 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

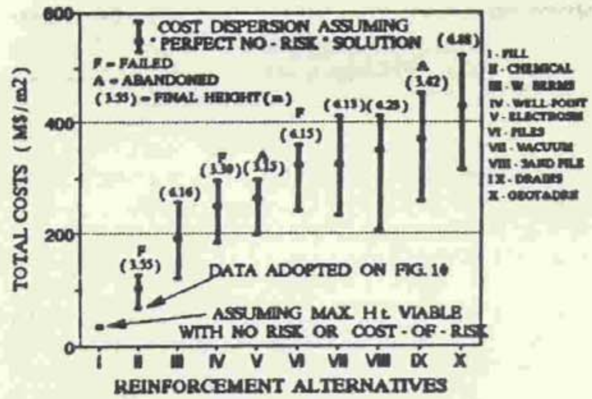

FIG. 9 ESTIMATED COSTS FOR 6 m HIGH EMBANKMENT

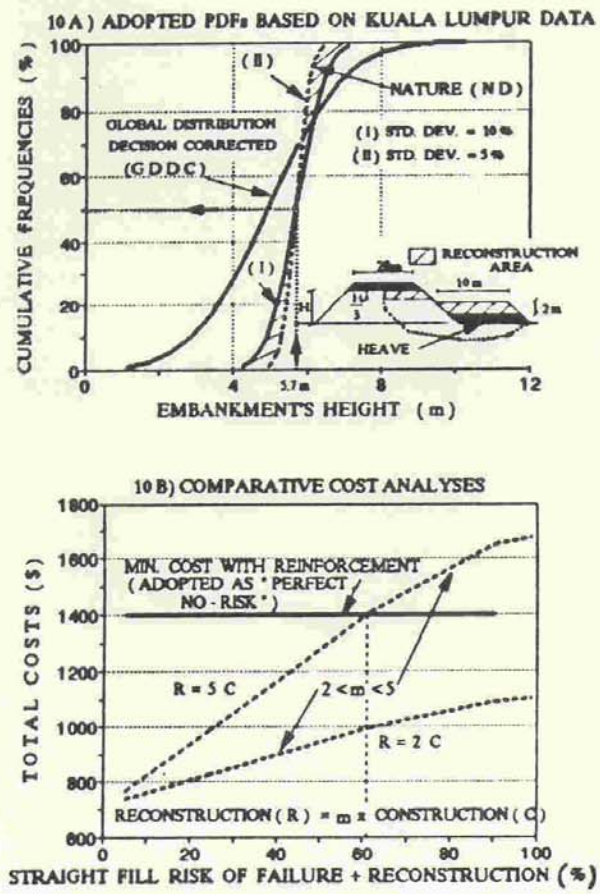

FIG. 10 COMPARATIVE COST ANALYSES OF OPTMIST RISKING FAILURES AND RECONSTRUCTIONS vs. MIN. COSTS OF REINFORCEMENTS FOR PESSIMIST.

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-

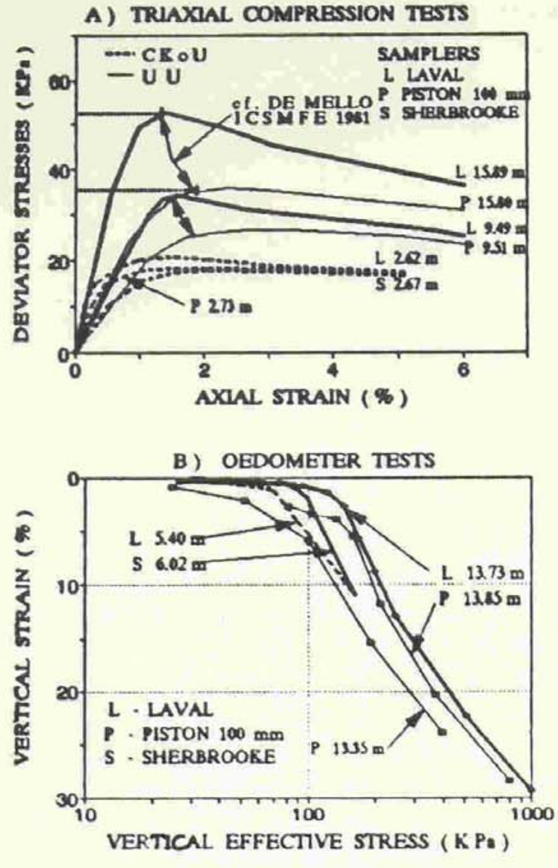

FIG. 11 MARKED DIFFERENCES IN KEY PARAMETERS DUE TO SAMPLERS (cf. BOTHKENNAR 1992)

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

4. CONCLUSION

The classic problem used for these

analyses involves homogeneous, saturated, idealized sediments akin to primeval "text-book cases". It is a problem faced with great frequency in professional life, and has been subjected to over 300 test-fill studies. We have progressed very much indeed in quantifying a number of "additional" parameters, presumed relevant to engineering works. Clients and Society are called to pay much higher costs of investigations and analyses, if induced into most advanced current practices. It would seem, however, that the greatest proportion of professional practices across the world, even in well-developed areas, lags considerably behind the level of advances available for incorporation. Incidentally, in Engineering is there an advance if it is in methods, but with no perceptible advance in net results?

Two fundamental challenges to geotechnical CIVIL ENGINEERING have been neglected under the prestigious avalanche of the published WORD in scientific quantifications. One is the nurturing of past experience of individual cases, never entirely repeatable, but ever judiciously retrievable with estimated quantifications of the new parameters erstwhile disconsidered. The other is the global resulting comparative cost to Society of the constructed facility, with due inclusion of the costs of risk and of discredited professional prestige.

It takes relish of KNOWLEDGE to keep abreast of the most updated geotechnical-science refinements, but even more thirst of the WISDOM not to use recent knowledge any faster than it becomes tempered to PROJECT EXPERIENCE. Since the tasks of Civil Engineering remain in essence the same, with but changes of measures and methods, let it not be justly said of us that "...when he once attains the upmost round, He then unto the ladder turns his back, Looks into the clouds, scorning the base degrees By which he did ascend."

(Shakespeare, Julius Caesar).