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FACING OLD AND NEW CHALLENGES IN SOIL ENGINEERING

by

Victor F.B. de Mello, Consulting Engineer
Victor F.B. de Mello & Assoc.

It is very appropriate that both individually and collectively we should choose special occasions to mark our wayfaring. "Man and his symbols": we need symbols, we need discontinuities to take cognizance of a continuum. Like unto milestones on a road, but preferably, as is the case on this special occasion, obviating the regularity of milestones, which automate themselves into a new drone of a continuum.

I was taken by surprise upon receiving the announcement and the invitation. And I gladly allowed myself to be moved by the poetic call of life's needs: first and foremost I am here to pay homage to a Man, an Institution, and a Period. I was moved to recall the day exactly 35 years ago when I ceased being his student and became his younger colleague: and when we all faced the Brave New World of postwar reconstruction of a dream and a goal, applied to the world of civil, and not military, engineering. I set to work on the "miracle juice" that, sprinkled from a couple of passes of an airplane, would solidify any soil into an acceptable runway surface. Those were the days when such coordinated national efforts as are conjured up by sacrifice and war, led to the boasting certainty of "bigger and better, indefinitely."

Time has tempered and mellowed our rushing in as fools, "where angels fear to tread." Fortunately we are allowed to claim that we have gained experience and that we retain the spirit of youth. In a triad, curiosity-effort-experience, that in varying proportions could define the evolution from youth through adolescence to maturity, we euphemistically comfort the thought of ageing under the laws of nature, through the self-attribution of gaining of experience, duly ignoring the fact that experience is a vector and not a scalar dimensionless abstraction. But, of experience, effort, and curiosity, the greatest is curiosity. And for some privileged spirits, the modern world favors keeping perennial the flame of curiosity, of youth. If we recognize youth as a period when we face a disproportionately high ratio of things unknown and new, to things already mastered, the one fortunate fact of the exponential aggression of the technological world is that it can keep us all perennially childlike. (cf. Memo A)

It is to this spirit that I dedicate my message. As Mark Twain remarked, "My interest is in the future, because I'm going to spend the rest of my life there." But it is impossible to practice futurology without some presumed analysis of historical context. And since soil engineering serves civil engineering, and the latter cannot but serve Society, it is inevitable that our technical predictions be conditioned by presumptions of sociological analyses and prognostications.

It has been remarked that "The new students know not the old lessons." And I may add, the old students (for we are all students) have bred for the old problems the contempt of intimacy. Quo vadis, Atlas? The Atlas of Soil Engineering that supports Civil Engineering, which in turn stands under Man's worldly needs? Could it be that after we understand, we have to jerk ourselves out from under, so that out of rebellion we might beget better progress than out of the self-satisfying thought-content crystallized into the synonyms "comprehend, apprehend"? The fact is that it does take a flash of dazzling light at midday, and a fall from horseback, to convert a Saul into a Paul! As we forge progress, should we not remind ourselves that there are possibly as many persons now living in conditions of 1000 BC or 1500 AD as there were living in the world in those very days? (Fig. 1)

Will trends continue as have been irrevocably accumulating through integrations of infinitesimals? Or will there be quantum rebellions versus continua? What will be the relative proportions of ingenious engineering versus "engine" engineering? Is it in developing countries or in developed countries that we most need and favor development? How do forthcoming perspectives compare regarding global vs. tribal (technological) Man, with due consideration given to the fantastic factor of communications? In a nutshell, could one state that soil engineering practitioning bred soil mechanics, and then disperse communications of soil mechanics across geography and time have bred new soil engineering practices prevalent side by side? And so could the sequence repeat: from practices to unified theory to disperse practices? To what extent is there a distinction between practitioning and practice? Is it akin to the distinction between rebellion and revolution? (cf. Memo B)

It is my impression that we are entering a period of challenge and discredit of what has appeared dominant in many a place for many a year. The EUREKA COMPLEX, the REFUGE SYNDROME, and the SOPHISTICATION TOTEM, have generated and been defended by the UMBRELLA SOLUTION: and the increasingly huge world of the poor cannot afford umbrella solutions. Let us give credit to the Man who heralded the oncoming period through publicizing discredit of his own and our collective capacity of Prediction. And bear with my right to make fallible predictions, especially when they are intended to generate antibodies.

What might be forthcoming steps in soil engineering depends on the past and present realities, efforts and errors: i.e., depends on Soil Engineering history. And History, as Churchill asserted, is made by great men and chance. Meanwhile great men are made by chance and effort. Thus could it be that we should aim at unleashing chance somewhat more? Who will accept the risks of correcting in Society's mind the age-old impression that Engineering is an "exact science" and that avoiding failures is a question of certainty and not of probabilities? Notice how differently various professions are "accepted." Doctors are recognized as mere postponers of death inexorable: any hit is a run. Economic planners are jovially accepted to be always wrong, but without affecting the ability to survive of the ones that talk and count. Lawyers are presumed to deal with 50-50 chances of having the client judged guilty or not guilty; and there are many phases of appeals and rejudging. Singularly, the engineer is supposed to build to stand up under any and every risk: who bred and catered to that image? Society, or the Engineer himself? Or was it innate in the recognition that although necessity is the mother of invention, inventions come forth by leaps, incorporating the impact of great affluence of the ratio possibilities/requirements, and forging progress by events that were statistical outliers? (cf. Memo C)

1. Firstly a REASSESSMENT AND REVIVAL

There has been essentially no confrontation or competition in soil engineering, such as takes one industry into bankruptcy to be swallowed up by another. Thus our dissatisfactions with poor predictions have only led us to more and more conservatism and disregard for economy. However, the world will not pardon us such a trend, as belts tighten inevitably and as mankind has become the great predator of surface geology. We need to reflect and recognize that many of the "base degrees by which he (Soil Mechanics) did ascend" are incompatible with today's needs. The start of a revival is the MEA CULPA recognition, for beckoning to any prospective convert. And in order to permit minimal accreditation of any recognition of errors or frustrated expectations and efforts, it is necessary that such confession be either by the very proponent(s) of the earlier hypothesis or theory, or by a consensus of a technical committee's state-of-the-art report. We join T. Carlyle in his "I do not believe in collective wisdom of individual ignorance," and recognize, a *fortiori*, the ponderous wisdom of collective recognition of *individual* ignorance.

1.1 Investigation, identification, classification

Planning subsoil investigations must be with regard to geology, geologic time, and the cause-effect relationships (with their dispersions) implicit in "formation." Appearance of silts and sands adjacent to open-work gravel is not random, but quite definitely deterministic, and an open-work gravel "lens" seen in a plane cross-section may well be continuous in a sinuous cross section along a historic buried meander (Fig. 2). Whereas the meander would increase the seepage path by about 40 to 50% in comparison with a straight line, the differences of permeabilities of a coarse gravel compared with a silt-sand is exponential (e.g., one-hundred thousand times) and so there would be absolutely no relevance to a lack of continuity in the straight upstream-downstream dimension. In such a situation, obviously a cut-off treatment would be absolutely necessary in comparison with the lengthening of the seepage path by a blanket. The important point, however, is to recognize the mental model implicit in linear, geometric, interpolations and extrapolations. Histograms of continua should be substituted for geometric arrays justified in reasonings of homogeneous strata and dichotomic distinctions between clays-sands, compressible-incompressible, etc. We have come far from the dictates of Terzaghi 1940 (Purdue Conf., p. 151).

> . . . in order to apply a theory to a soil problem we are compelled to replace the real soil profiles by simplified ones consisting of a small number of homogeneous layers, and to assign to each one of these layers a single set of soil constants to be derived from the results of soil tests by means of some process of averaging . . .

How right for first cognizance, but how sterile for continued progress!

In order to identify and classify we well recognize that at present no amount of testing satisfies in comparison with a minute's visual-tactile assessment by an experienced geotechnician. Culturally, we collect a great wealth of information by visual-tactile identification, but unfortunately our cultural development of the ability to communicate is infinitely smaller (witness our capacity to recognize different faces, and comparative incompetence at describing to a stranger the face of as near a relative as a brother or wife). Compare the informational impact of a good colour photograph of a rock core-box, with a profile drawing such as Fig. 3. And further, how much more we gather from the graphical data of the right-hand side of such a log than from the arbitrary attempts to classify rock qualities I, II, III, etc., in a central column (16) of the boring log. As another example I submit that many a case of piping through gap-graded or skip-graded granular materials may have been unwittingly associated with the difficulty of assessing visually a condition of gap-grading through the exclusive adoption of the routine curves of cumulative percentages of grain sizes. (Fig. 4)

The basic challenge is to use as seeing-eyes the perceptive possibilities unleashed by electronic devices of the most diverse types, and to resort to multiple profiling, and information digestion and storage. A soil profile may well be described by varying proportions of reaction to excitations A,B,C,D,...Z (or more), and soil behaviors as we now know them may be related to other proportions of the same excitations. Thus statistically correlated identifications, classifications, and attribution of behaviors may be developed without employing the highly restrictive medium of word language at all. Geotechnicians would do well to emulate some of the fantastic modern techniques of "non-destructive excitations" and comparative identification that are in continual development and successful use in the field of medicine and bioengineering. Incidentally, one need not apply restrictive reasoning as to what excitations to try out; one may well gain by play leading to discovery and discovery to invention.

Regarding standardized classification tests, everybody recognizes that both the grainsize analysis and the most current liquid and plastic limit tests are very crude. Teachers of the 1940's soil mechanics testing have become a solid obstacle to the acceptance of revisions that have long since suggested themselves. In soils of significant geologic age the grainsize characterization must respect the in situ nucleations and/or the weathering and crushability of coarser grains. In highly sensitive clay soils (Scandinavian quick clays) and in ocean sediments, the consistency limits must respect the salt and ion content of the porewater, and so on.

In short, one must ever seek a careful balance between the extremes of inventive play on the one hand, and standardization on the other, for storage and communication of the cultural advance. Standardization at its worst can be no more than a strut to the lame and a chain to the challenger. In scientifically understood self-discipline there is absolutely no need for standardization, because anyone should feel "in his groin" that to investigate $\partial G/\partial M$ one must keep all other factors (known) constant, while "unknown" intervening factors of a given time and place appear as statistical dispersions.

To begin with, one may but emphasize the persistent observation that routine tests as simple as those of sample preparation, and of identification, were standardized without specific interest and/or care for uniformity or consistency. In Fig. 5, reference is made to grainsize fractions that are separated before running different tests on any given soil. To emphasize the differences possible, the grainsize fraction excluded from a given test is represented by a wide band estimated as the limits to which the percentages of fines, or the coarse fraction of filler, could interfere in a given soil test without being permitted to participate in another test on the same soil. How then could correlations between identification tests obviate wide dispersions?

Another well recognized problem concerns poorly chosen indices. (cf. Memo F)

1.2 Quality of sampling

After the early distinction of undisturbed vs. disturbed (or fully remolded) samples, despite the recognition of the tremendous importance of remolding on compressibility, stress-strain-strength, and permeability, there has been absolutely no systematic reporting on the quality of samples as they affect all published test data on would-be undisturbed samples, to represent in situ elements. At best, in a few instances, indications on sampling have been given via "method specifications" and not, as should be, via "end-product specifications." Four distinguished schools have devoted fruitful research effort to comparing stress-strain-strength behaviors of Intact (or Field) Elements, and Perfect, Undisturbed, Partially Disturbed, and fully Remolded samples. The Sensitivity index s_u (und.)/s_u (rem.) is always a Partial Sensitivity index, from which we must definitely try to infer a likely Intact condition. (See the problem of responsibilities, under item 3.1.) Schmertmann (1954) and Bromham (1971) resorted to oedometer curves for such evaluation of disturbance indices and intact behavior, but less than 1% of good publications ever mention the sensitivity or the sample quality. (cf. Memo D, and Fig. 8)

In Fig. 6, I have reproduced the results of a simple analysis used a long time ago in an attempt to refer UU strengths to a presumed common reference of "undisturbed quality." Around 1953-56 I had opportunities to sample and test a significant volume of shelby samples of foundation clays, and obviously noticed the relationship between percent strain at failure and the degree of disturbance, as indicated by so-called partial sensitivities St_p. The test data were analysed statistically, assuming regressions variable with nominal St of a presumed minimally disturbed specimen. Thereupon the resulting coefficients and regressions were used repeatedly to estimate a presumed "perfectly undisturbed" specimen's behavior as corresponding to a failure peak at 1% strain. These were candid working hypotheses which served a purpose, and may yet continue to serve, without a presumption of "research truth." The surprising fact, however, is that even in clays of moderate to high sensitivities, all strength results are most commonly lumped together without any attempt to refer them to a common data base with regard to partial sensitivities and disturbances.

1.3 In situ testing and multiple profiling. Quality quantification of such testing

I shall not expatiate on the well-known fact that considerable effort has been expended on in situ testing, both because of a desire to identify in situ conditions and to assess model-prototype conditions, and to obviate the disturbance associated with sampling and handling. The dynamic spoon penetration testing (SPT), the static cone penetrometer (CPT) and its developments (including local side friction, LF, for identification, and especially the CPTU as a multiple profiler), the recent Marchetti dilatometer, the vane shear test, the pressuremeter (pressiometre) with multiple applications, the K_o' profiling (e.g., camkometer), the in situ permeability testing by pumping-in and pump-out techniques, and finally load-deformation tests, are a day-to-day array of expedients upon which our designs are based. Oceanographic subsoil investigations have employed much more multiple profiling, and could open much greater promise if they recognized the errors, consistent and erratic, of conventional soil mechanics tests.

All of these were developed under rational prognostications, but, as was inevitable, under so simplified a theoretical basis that only gradually have the illusions been exposed. The great problem we face is to develop methods for assessing quantifiably the qualities of the work. The early association of disturbance with samples and therefore sampling, led to the search for in situ testing under the wishful thinking of illogical associations:- (cf. Memo E) Spurious logic:

samples $\rightarrow$ disturbance

$\therefore$ non-sampling $\equiv\rightarrow$ non-disturbance

$\because$ in situ testing does not sample

$\therefore$ in situ testing $\equiv$ non-disturbance $\therefore$ unquestionable.

Acceptability of in situ testing results has been discussed on the basis of the complex end result of the constructed project. But, no two cases are alike, dispersions have been great, and there are too many intervening steps and factors that may introduce compensations and/or magnifications of errors of initial investigations.

I do not know of any jobs or research work in which a given in situ test (e.g., CPT or CPTU) has been repeated several times side by side at distances on the order of a couple of meters, for assessment of dispersions: neither have there been reports of clusters of such in situ tests compared side by side. In comparison with laboratory tests, the principal present failing of in situ tests is of never having been applied before and after a given loading, to check on their ability to reflect changes of conditions.

1.4 Revisions of badly chosen indices

Many indices defined for soil engineering need to be revised, substituted, or abandoned altogether, principally because they were defined and proposed piecemeal, generally under the impact of dichotomic observation, and without concern for interrelationships with other indices. Some are unsatisfactory even for present-day deterministic soil engineering, others can be foreseen to develop into stumbling blocks for our necessary advance towards histograms of behaviors as quite independent from the decision of truncation at chosen levels of allowable-unacceptable. Such truncation cannot be a-prioristic, such as to discuss settlements on compressible-incompressible subsoil profiles; it must vary according to the value systems.

It is fully recognized and repeatedly commented that our abilities at computation have advanced at least a century or more relative to data input and to the use of output, the ability to employ the computed results in meaningful and economical engineering decisions. The ability to apply results cannot begin to flourish before the roots are fertilized. As regards the roots of data input, is not our present dearth principally because the earliest efforts were at quantification of indices on data, and those early dynamic challengers have long since become authorities, and to challenge their indices would be as uninviting as challenging the territorial imperatives of dominants? We must urgently recognize that as the rate of progress of a technology becomes incompatible with the animal life-span, it may become unavoidable that authority sterilizes, and that new trends may sprout best proportionally to the square of the distance from the centers of progress of a score of years earlier. (cf. Memo B) Just as examples I mention three indices, each as theoretically and empirically obvious as intuition itself. The $\bar{B}$ or r_u coefficient $u/\gamma z$; the colloidal activity index of a clay (Skempton) $AC = I_P/\% < 0.002$mm; and the earth pressure coefficient "at rest" $K_o = \sigma'_h/\sigma'_v$. What happens to any of the three when we get to the mathematical indefinition that accompanies the denominator tending to zero? What happens with u negative? Should we not investigate $\partial u/\partial(\gamma z)$? What significance is there in AC when I_P is determined on fully remolded conditions with no regard to porewater ions, and the % clay fraction is determined on soil with maximized deflocculation? What right have we to take for granted that K_o should be constant (a soil property) down the profile of a given "homogeneous" stratum, when it is inexorable that all things are different unless proved reasonably alike? (cf. Memo F)

As a fourth example one might mention the Density Index I_D (previously relative density) of sands $(e_{max} - e)/(e_{max} - e_{min})$, which has been the focus of considerable concern because of investigations on liquefaction potential. It has suffered from so many criticisms that one is surprised at geotechnicians continuing to flog a dead horse: why doesn't somebody develop a substitute index? Unlike the Consistency Index I_C of clays that was fortunately content with arbitrary limits of the scale at W_L and W_P, for I_D there is the presumption of seeking real experimental maximum and minimum values: moreover, the recommended test methods are not applicable to any but relatively uniform sands because of segregations: finally, it is too crude an index to reflect parameters of definite influence on liquefaction, such as the difference between normally compressed and precompressed conditions of the same sand.

Finally, as a most important example of some of the confusions to be expurgated, one may mention that generated by mere word associations when a word is vaguely defined. (cf. Memo G) It has been considered most important to seek the attribute of "plastic behavior" in compacted clay cores of earth-rock dams. What is really desired is the ability to undergo large strains without "fissuring," that is, "cracking open, in tension." As a first questionable word association one finds this requirement transformed into that of large strains to shear failure in triaxial testing: questionable, but somewhat acceptable because in "brittle" vs. "plastic" stress-strain curves it is in the first that one tends to find open fissuring. It is in the next step that the shocking confusion arises because plasticity behavior is confused with plasticity index. The latter represents a potentiality, an essence of being, a range of water contents over which a soil exhibits a "plastic state." Meanwhile the aim regards a plastic behavior at a given condition (temporary), say as compacted at a given water content, say the Proctor optimum compaction water content. It so happens that soils of high I_p have to be compacted at water contents below the plastic limit W_p because of problems inherent to compaction. In my experience (de Mello, ICOLD, Madrid 1973) it is only for intermediate I_p values (approx. $7 < I_p < 22\%$) that the Proctor compaction water content happens to be wetter than W_p. There is, at any rate, no logic in a word association between plasticity index and plastic stress-strain behavior as compacted.

1.5 Questioning the validity of some routinely quoted correlations

Soil mechanics practice is full of quoted correlations that have been of great use for some design estimates. However grateful we must indeed be for those correlations offered, a wealth of collateral theorization and data would force us to recognize the long-felt need to revise them. Brief mention may be made of some.

(a) The correlation $C_c \cong 0.009\ (W_L - 10)$ proposed for "field consolidation" of "ordinary clays of medium or low sensitivity" by a simple multiplication of the $C_c \cong 0.007\ (W_L - 10)$ on remolded clays, should need revision. It has been used indiscriminately. It is just inconceivable that for behavior of undisturbed clays, a single remolded index should suffice, and then for all clays, irrespective of "structure" (sensitivity), stress-strain-time history, a single factor of adjustment of 1.3 should be generally applicable. To begin with, considering that for a given W_L there is a wide range of I_p values occurring in nature, it is incredible that the remolded clay C_c should not reflect some interference of I_p as a minimum second parameter. Moreover, as a matter of discipline regarding undisturbed clays, all such correlations in the future should be adjusted to reflect as closely as possible the intact behavior: otherwise there is no common ground for building up experience. Secondly, in order to reduce dispersions, it should be important to incorporate into the statistical correlation the factors likely to reflect influences of "structure" and time (Leonards-Bjerrum quasi-preconsolidation effect). The great mass of geotechnicians and routine geotechnical applications cannot afford to do without correlations, but correlations cannot retain credibility if they remain so grossly disrespectful of accepted theorization. Many improved statistical correlations have been put forth, introducing such obvious intervening parameters in a given stratum as the initial void ratio e_0 and the preconsolidation pressure p_c, etc. In Fig. 7, I have reproduced one such simple investigation of Santos marine clays (de Mello, ICASP, Sydney 1979), in which further parameters assessed included the recompression index C_r, and the sample's in situ effective stress $\gamma' Z$.

(b) Another classic correlation reflects undrained strength (cohesion) as a function of I_p and preconsolidation pressure p_c: for instance, $c = p_c\ (0.00343\ I_p + 0.115)$. One should recall that in early days, c was taken directly from unconfined compression tests, and R_c, merely on modest shelby samples, and that no adjustments have since been made regarding better undisturbed sampling and use of UU or CU test results for determining undrained c. The very odd thing, however, is that such a correlation is quite contrary to theoretical expectation insofar as ϕ' decreases in clays of higher plasticity, and the other significant intervening parameters, u_f, and swelling, should further reduce s_u with increased I_p. (cf. Figs. 8, 11, 12)

Once again, it is impossible to forego the need to incorporate both indices I_p and W_L on remolded behavior, and, most especially, it is unacceptable that in a formulation of undisturbed undrained strength the Sensitivity S_t should not appear! The criticism again lies primarily in the fact that unless intact behavior is sought to be represented, there will be utter confusion.

(c) A third example concerns the suggestion that in "typical" normally consolidated clays, the conventional $K_o' \approx 1 - \sin\phi'$ be substituted by a linear regression $K_o' \approx 0.44 + (0.42)\ I_p/100$ for $20 < I_p < 80$, and this essentially irrespective of being "disturbed" or "undisturbed." (Fig. 9)

The above Figure is adapted from Massarsch, 7 ECSMFE, Brighton 1979, vol. 2, p. 245. Firstly we should desire to substitute the dichotomy disturbed-undisturbed by values of S_t. Next, are we saying that $\phi'_{und} \approx \phi'_{rem}$ (at least within $20 < I_p < 80$) and therefore $s_{und} \neq s_{rem}$ only because of differences of c' and u_f? At any rate, adopting exponential exhaustion relationships for ϕ' vs. I_p as is intuitively accepted (Fig. 11), we see that the data continue to plot very satisfactorily with reference to non-linear regressions. We must respect the evidences of extreme values of K_o' approximately corresponding to $\phi' \approx 30°$ for $I_p \approx 5$ and $\phi' \approx 5°$ for $I_p \approx 350$ (sodium-bentonite), as well as the asymptotic trend $K_o' \to 1.0$ as $\phi' \to 0°$. We should not sacrifice the intrinsic recognition of K_o' as generated as a function of shear stress, and thus limited by shear strength. Is it not better to use regressions that respect theoretical trends, even for data considered crudely empirical?

In Fig. 13, I summarize a hint of a practising professional's methods of advancing working hypotheses on the presumed body of accepted theorization and some minimal pragmatic observation. At the top are the equations repeatedly quoted in textbooks. A direct comparison of the simplified K_o' expression with the Mohr-Coulomb failure criterion suggests that K_o' conditions prevail at a factor of safety $FS \approx 1 + \sin \phi$. Incidentally, it seems reasonable that K_o' conditions be assumed to prevail up to the limit of "elastic behavior," and we often find that a soil of $\phi' \approx 30°$ exhibits linear stress-strain behavior up to about 2/3 of the peak failure deviator stress. Thereupon, if a complete strength envelope is assumed for a soil, including an overconsolidated stretch with cohesion, and if we arbitrarily maintain constant the FS ratio of elastic to failure stress envelopes, we could determine trigonometrically the band of K_o' stress ratios possible through much of the overconsolidated range. Will research aim at cross-examining such working hypotheses?

(d) The important exercise is to question and debate, and to work towards systematically reducing the dispersions around our reasonably founded regressions. As a final heresy I submit but one example of a concern of world-wide influence. We owe it to no less an admirable and dear friend than Bjerrum himself (e.g., VIII ICSMFE, Moscow, Vol. 3, p.111) (Fig. 10). Since a correction of s_{vane} is required for stability computations, it was declared and recognized to be imposed by effects of "structure," progressive failure, rate effects, anisotropy, differences between undisturbed and remolded stress-strain, etc. If such a correction is presumed adjusted to I_p, are we saying that an index derived from two rudimentary tests on fully remolded material (with different ion contents of pore water) is satisfactory for reflecting undisturbed in situ behavior? Are we saying that S_t, differences of permeabilities of undisturbed vs. remolded, different stress-strain mobilization of cohesion vs. friction, and so on, are directly related to I_p? Obviously very doubtful, except under a strictly local or regional coincidence.

2. The THREE WISDOMS in discredit

Since it is not easy for teachers to look at themselves in the mirror and to thump their chests MEA CULPA in acknowledging that they have not been changing the diapers of baby soil mechanics often enough, professionals had turned to the production line of publications and conference papers, both in the donor and receptor capacities. I don't know how many of the donors have started to take themselves with a pinch of salt. I fear, however, that there is a generalized discredit of the production line by receptors. (cf. Memo H)

Three principal trends have proliferated, and have so cuddled themselves in mutual admiration societies, that few of the practitioners have stopped to heed how little distills into practice.

2.1 The EUREKA COMPLEX is apparently dominant. As soon as any new fact or behavior is detected satisfactorily enough for description and dissemination, one must rush at the game, bingo. The net result is a welter of separate correlations, and tit bits of information that practising professionals have difficulty digesting for use in the job context. Since the findings have been few at which one would exclaim "for heaven's sake, that is really surprising. I would never have thought that to be the trend," the value of the publication has to be measured in terms of value of data, equations, and dispersions, for use in optimizing designs. However, generally correlations are between single parameters and with no statistical treatment. If treated at all statistically, regressions tend to be linear, statistics at random, with no interaction with equations that should be inferred on the basis of various cross-linkages of theory. My impression is that the roads of professional practice and of the published production under the eureka complex are strictly parallel, and by definition will only meet at the infinity of God's special grace.

2.2 The REFUGE SYNDROME is a little less evident, but may be detected in some erudite production along lines that cannot be checked. Theoretical developments may be quite sound, but if they concern episodes of failure and probabilities of catastrophe, it is difficult to confirm or reject them. Will a given volume of sand tend to liquefy under the worst likely earthquake attributable to the project, or will a clay core crack in tension? The trend of professionals is to join the elite club as rapidly as possible lest one be dubbed backward. The net result is increased costs to the client at a cost/benefit ratio of infinity. Few would have the stature to join M. Twain in saying that "civilization is a limitless multiplication of unnecessary necessities."

2.3 The SOPHISTICATION TOTEM holds a double attraction for the brainier young: firstly, they can work at something dazzling far beyond the drab engineering of dirt; secondly, they can stake out territorial dominance without even approaching any of the old dominants! Why not develop a three-dimensional analysis of a visco-elasto-plastic rheology with strain softening? It is easy to understand why computational ability is so far ahead of parameter formulations and post-computational decisions. I only wonder why as soon as a fully competent program has been developed, the authors do not go back to dozens of projects of the past 50 years to apply the program to recomputing those projects to check what would have changed either towards safety or towards economy. The standard complaint is that the old projects did not record sufficient data: however, civil engineering has always stood firm on dubious ground of data, and a person should have no difficulty assuming simple data after having assumed a visco-elasto-plastic rheology!

In short, the net impression is that there has been considerable sophistication for sophistication's sake, and too little demonstration of the economic advantages that could ensue.

3. UMBRELLA SOLUTIONS

Firstly, there has to be protection of prestige. Failure conditions are difficult to quantify with reasonable precision, and Factors of Safety are under much debate, but failure is anathema and must be kept at arm's length. And we honestly do not acquire quantifiable statistical experience from failures, and from poorly understood nominal Factors of Safety.

Thus designs tend to be overdesigned. Moreover, for any design computation consciences are only eased if several methods of computation are used, even if little is the solace, because neither parameters nor decisions are helped. As a net fact, design engineering services have become noticeably expensive by all measures. In the Society of Services that was foreseen to follow the consumer and waste Society, the learned prophecy forgot that professions become so expensive that only Corporations can afford them. But Corporations do not accept unsatisfactory behavior as meek individuals might: so professionals go about being expensive at giving safer and more expensive solutions than if they had not been engaged.

In fact, in a sense we have to rejoice at a modern trend towards investing in construction inventiveness and developments. However, if we analyse such a trend more closely, does it not hide a practical defeat of Soil Mechanics, in the sense that such inventions are effectively to dispense with any closer knowledge of the soil? Would that be a corollary of too high a cost/benefit ratio of geotechnical engineering services, principally for the exaggerated requirements and precisions imposed by modern technologies on the superstructure?

Three principal categories may be listed (cf. Memo I):

3.1 The burden of heavy equipment

The trend during the past 35 years has been of such exponential increases in weights and capacities of construction equipment that it could not fail to exert considerable influence on several aspects of geotechnical engineering. I do not wish to repeat the obvious: that geotechnical man has literally moved mountains, and scarred the face of the earth. My interest is in examining some of the psychology behind such endeavors. Whereas our mentors, such as Terzaghi, Taylor, Casagrande, Skempton, and Peck, nurtured a passionate love and respect for the delicate frailty of soils, the modern tendency is to brutally disrespect soils as a nuisance that one can do without. Some earthwork engineering and foundation solutions are superabundant to the point of achieving the desired equal-to or better-than behavior irrespective of the soil. "When in doubt, grout: if still in doubt, grout throughout," exemplifies jestingly a frequent reality. At what cost, we shall not ask; why is the world becoming unbearably expensive for everybody, everywhere? Development of big capacity for the tackling of the mammoth projects was unquestionable: the problem lies in designing and building medium-size and small projects as if they were mammoth jobs dwarfed. (cf. Fig. 8)

More than ever it becomes important to set out clearly what are the distinct functions and responsibilities of the designer and the contractor, because "execution effects" tend to be overwhelming. The present (hopefully temporary) phase is of confusion, and therefore utter irresponsibility. It used to be that "execution disturbance effects" of boring-sampling-testing were higher and those of construction equipment and activities more modest, and therefore early geomechanics was seduced into the attempt to establish direct adjustment coefficients between test results and construction activities (sterile correlation synthesis-to-synthesis of lumped parameters). The two activities have developed intensely in absolutely opposite directions: testing has gone off to minimize interferences, and construction has progressed towards ever greater scales and consequent disturbances. Since all our knowledge depends on closing the EXPERIENCE CYCLE and then re-cycling, are we making our very perspectives hopeless and fluid, because each effort to improve any link automatically upsets the chain? (Fig. 14)

3.2 Inventive engineering products and procedures

The past decade or so has been fertile in bringing forth a series of solutions somewhat more inventive than potent; but these have opened important new avenues to subsoil and earthwork engineering. The Franki pile was doubtless a very inventive idea of the early days of foundation construction. Electroosmosis and vacuum preloading of saturated compressible sites were two highly inventive developments which, however, were not fully marketed by their enthusiasts. Bentonite-stabilized diaphragm walls and bored piles constituted another inventive leap. Then came the selective grouting of alluvial foundations of dams about 20 years ago, and a recent publication on the behavior of Serre Ponçon dam indicates excellent performance, improved and not deteriorated with time.

In rapid succession we have had such additional creations as gabions, reinforced earth, geotextiles, fiber vertical drains, stone-column and lime-column stabilization, root-piles, CCP piles, deep compaction, and so forth. We cannot but praise these developments since ingenious engineering is of the essence. However, in an attempt at analysing the trend and its significance, could we venture some speculations? Necessity may be the mother of invention, and so there may be some inferences to be made from attributing the origins of many such developments to Italy, France, Sweden, etc. Besides cultural factors, could it be that the greatest fertility for such production is associated with regions faced with the needs of keeping abreast with bigness, and somewhat less favored with economic abundance? I prefer to recall Dr. Land's affirmation, when he described the invention of the Land camera (1948), that the two components of an invention are, first, "to give free bridle to your wishful dreams," and then, "to work hard to make them come true."

However, side-by-side with the civil engineering euphoria at such creativity, what presage should we cull for conventional soil mechanics? Is it not further evidence that the more and more exacting demands on geotechnicians, and the relative dissatisfactions with insufficiently precise, safe, and economic solutions, have suggested recourse to solutions that essentially dispense with detailed concern for the soil's personality and whims?

3.3 Systematic improvement of equipment capabilities

From some areas there have been remarkable systematic improvements of equipment capabilities, both for investigation, and directly for construction. When we stop to think of the exponentially exponential developments in electronics, and in most industrial developments, it is easy to realize what can be achieved by concerted collective efforts at development.

Many centers may be mentioned, but the prime example is conceded to be Japan.

In attempting to draw a lesson from such trends and potentialities, one may again note that the emphasis has changed somewhat disconcertingly: one ceases to direct prior interest to knowing the soil, or even to knowing what to do with it, and one shifts attention to what to do to it, or even despite it. (cf. Memo I)

Man in developing civilization cannot resist acting against Nature, to mould her to his desires.

4. Nature's Razor's-Edge Equilibrium at FS = 1.00

If on the one hand we can rejoice at our abilities to dominate Nature, on the other hand there has been a growing consciousness of the need to be wary of the difference between winning battles and winning the war. Ecologists are not the only ones to be heeded, but our own common sense, as well. From the exaggerated solutions of one generation arise the plagues of the next. Nature has no commitment to prestige measured with respect to preserving the status quo: on the contrary, her prestige is the fantastic ability of natural selection on the brink of FS = 1.00. The most remarkable lesson of the recent Stockholm conference was a chance one -- the film of the quick clay slide in Norway, triggered by a mere excavation of foundations for a barn, and quickly extended to involving rapid flow of hundreds of thousands of cubic meters of mud with village houses floating on it.

The fact is that despite our proud structures that call attention to themselves, the vast majority of populations live close to Nature's equilibrium of no-greater than necessary. And unwanted behaviors are accumulated or triggered continually. Not merely in the liquefaction of Scandinavian quick clays and the avalanche sliding of residual soil slopes in Hong Kong or the massive mud-flows of bouldery colluvia in the Andes, but also in the expensive slow deteriorations of cities settling by the oceans, or of factories, buildings and dams requiring expensive monitoring and maintenance.

If activities of big construction can dispense with soil mechanics finesse in investigation and design refinement, is it not at a heavy cost, too heavy to permit reducing the cost of living? Industrial output can cater to and absorb costly sophistication because of the exponential multiplications of identical items; but in geotechnical engineering at FS close to 1.00, each case is individual, and the cost of sophistication cannot be diluted.

For all such situations, what is it that we need, today more than ever? Is it not the fundamental requirement of civil engineering to be economic, to be no more than just better than good enough? Is it asking too much of us civil engineers, who earn more when engineering is sophisticated and expensive, and who have everything to lose and nothing to gain but our solitary self-respect if works are made less conservative; is it asking too much of us, that we ourselves should advocate a cheaper, more daring engineering?

5. What We Need.

This session is aimed at prognostications on Research and Practice. As regards practice we may set aside computational ability. Thereupon, regarding both research and practice, the question is how to direct our efforts most fruitfully. Obviously and fortunately there are a great many and varied opinions and ideas. It would be disastrous if more than a few learned colleagues had the same opinions on what is presumed unknown; it is cheerfully difficult enough to find many agreeing on what is presumed known. In research and in life's challenges we have learned to cherish differences. That is why I venture to offer my very personal impression, already expressed on other occasions.

Initially let me explain that to me the industrial product of civil engineering education, and collateral research and development activity, should be proudly recognized as ENGINEERING DESIGN AND CONSTRUCTION. Research and publication are really means to that end, and one regrets to note that often such a pragmatic aim has been forgotten, through the very zest of academic pursuit for its own sake.

5.1 In soil mechanics, I propose that the principal new parameters for us to investigate more intensely will be porosimetry, air-pores, and suction. They affect a large proportion of the world geography and geology, and air pores are what matters for compaction and many a soil treatment. We could say that in an early period, soil mechanics was principally concerned with solids: then came a period of almost total dedication to research on the liquid phase; it stands to reason that it should now be the turn of the gaseous phase. (cf. Memo J)

5.2 Regarding geotechnical engineering I would suggest a three-pronged approach to meaningful observation. In a nutshell it may be described as representing the decisions (a) to stop paying too much attention to "analyses" of "failures," which are news items or even newspaper headlines, and to systematize data collection on the "silent majority of cases of which civil engineering is made," that do not invite attention or publication, and (b) to collect data on physical universes that are really constant or similar, for the sake of realistic assessments of incremental actions vs. incremental effects.

The proposal is recognizedly uninviting because there will be a protracted period of data gathering, and essentially no glamour. But we used to be admonished to take care of the pence and not the pounds!

The three key avenues necessary concomitantly are:

5.2.1 Revised definitions of nominal safety factors. There are many fruitful discussions on the meanings of factors of safety, but everybody recognizes that they are and will continue to be nominal. We cannot avoid the psychological need for calculating factors of safety. I have emphasized that in civil engineering design of projects of great responsibility and consequence, one desirable principle to observe is the pretest principle, that is, subjecting the soil elements during the construction period to tensions at least slightly greater than those that may be predicted to occur under the critical operational conditions. Thereupon the need arises to recognize a distinction between the conventional Factor of Safety and a nominal Factor of Guarantee. In fact, in a crude first approximation I have proposed recognizing the distinction between at least three nominal Factors, that of Safety (conventional) and those of Guarantee and of Insurance. Unless in all our data collection we distinguish between these, in subsequent correlations with behavior we shall be generating dispersions and confusion.

In Fig. 15, I postulate that when resistances are known to be higher than some pretested value (truncated histogram), the ratio of resistance to predicted stress is no longer a Factor of Safety but a Factor of Guarantee. In Fig. 16, I schematically summarize the cases of jacked or driven piles to "refusal" as a condition in which the favourable histogram truncation on resistances establishes such a Factor of Guarantee FG in comparison with the routinely defined FS = (Resistance ± error) / (Stress ± error). Moreover, at the other extreme there are situations wherein the histogram of strengths can only be less than a certain ideal value (e.g., the Intact sample's): thereupon, the routine FS is changed into a Factor of Insurance FI. In Fig. 16, situations are schematically indicated suggesting that bored piles and shield tunneling problems are often related to values of FI instead of FS.

For obvious innate psychological reasons our data collection of allowable vs. unacceptable behaviors will continue to require association with nominal Factors of "how distant the critical predicted condition will lie from the limit."

5.2.2 Histograms of non-failure behaviors. We must clearly recognize the two-step distinction, first, of establishing the histogram of the continuum of behaviors gradually worsening, and second, of applying the yes-no decision of truncation of such histograms according to individual value systems (inexorably varying). We have wasted too much effort in the childlike quest of the "bang and fireworks" of sudden failure: it is comprehensible, but "when I was a child I spake as a child....", and it is time that we grew up into adult attitudes. For instance, if we want to investigate embankments on soft clays we should observe the varying behavior as the fill height (over a constant soft clay) gradually increases: and we should monitor the increasing fever of the patient, the gradually varying blood-count, or what have you. We must really choose what to monitor, be it deformations, or micro-acoustic emissions, etc., so that it is significant, opens an easily discernible wide-spectrum, and is preferably easy and cheap.

For instance, in discussing allowable (or unacceptable) differential settlements in buildings, rather than the "first crack" (which is obviously chimerical), what we should observe is the rate of change of cracking with change of differential settlement and distortion, as I shall discuss below. It is very cheap and significant to observe the evolution of a crack after it has signified where it is; and since distortions due to differential settlements of two adjacent columns inevitably attenuate from floor to floor, a significant statistical universe to analyse is the several floors of the same building. After all, the 10th floor reference level acts as a "foundation" for the 11th floor in the same manner as the (buried) foundation acts as the support for the ground floor. And if we want to be honest, different buildings in Hong Kong, Chicago, Sao Paulo, and London, cannot be lumped into a single statistical universe merely because they all merit the name "building." What would become of zoology if all bipeds were statistically analyzed as a single universe?

Two examples may suffice. The list is long; in fact, in almost all projects we have lumped together significantly different conditions in single universes merely because of the cloaks of similar names. Why is it so difficult to correct such absurdity? Because of both Engineers and Clients. How difficult it is to design and build a long dam with the same slope varying longitudinally, say from 1:2 to 1:2.2, to 1:2.5, to 1:2.8 at every hundred meters or so, just for the purpose of collecting conscious data on varying non-failure deformation behavior, to prod a little and push a little our definitions of the frontiers of impunity! The EXACT SCIENCE complex, the CERTAINTY complex, the RIGHT-WRONG dichotomy complex are difficult to uproot.

5.2.3 Observations on incremental actions vs. consequences. One of the most common mistakes in experimental and observational technology is not recognizing the errors of observations close to zero. Many are the inexorable causes. I may summarize it by recalling Byron's beautiful sentence that won an essay contest on the topic of the miracle of turning water into wine at Canaan. Against dozens of pages of prose, the winning statement was poetically concise: "The water saw her Lord and blushed." The moment we decide to instrument, the instrumented point has been singled out, has become singular, and "blushes." Close to zero of any parameter, dispersion and errors abound. What we have to do is to concentrate our efforts on observing Δ behavior vs. Δ action, and then extrapolate towards zero if we wish. Just to exemplify, I shall return to the problem of cracking of buildings.

If we set aside interest in the beginning of the first crack, which implies organizing an extensive alert and monitoring system for catching the bingo, without any real inkling of where it would arise, we would very cheaply organize to let the cracking begin. All the junior office workers or residents become our monitoring system for free . . . everybody is interested in the appearance of a crack, or can easily be invited to such pleasant cooperation. Right after the crack (associable to differential settlements) appears, we can instrument to observe its rate of growth; concomitantly we can instrument to monitor the settlements of the two adjacent columns. Moreover, we can monitor differential settlements at the levels of many floors above and below the said occurrence, and can be alert for similar cracking developing on other floors. Such are meaningful observations of $\partial C/\partial(\Delta\rho)$ where C = crack and $\Delta\rho$ = differential settlement, under conditions of as nearly the same physical universe as possible.

Laboratory research has led to very fruitful conclusions because it always respected the need to investigate two parameters at a time, all others maintained constant; and it early recognized the need to correct for "seating or installation errors" close to zero.

In the really important laboratory of prototype observation, the laws of technological research have been regrettably disregarded, but they should be heeded. I see the greatest promise for civil and geotechnical engineering through a concerted effort following such principles.

My candid estimate of futurology in geotechnical engineering? What is the benefit/cost ratio of inventiveness? What is the benefit/cost ratio of inviting Nature's cooperation? What is humanity's greatest need but to solve the age-old challenges by new inventive and economic methods? Besides the new frontiers of the ocean bottom, of icy or arid deserts, and of equatorial forests, is not the principal frontier for hundreds of millions that of living in the more liveable world we already occupy?

(cf. KEY-WORD MEMO A)

WE NEED DISCONTINUITIES TO TAKE COGNIZANCE OF A CONTINUUM

CURIOSITY - EFFORT - EXPERIENCE
≡ ≡ ≡
YOUTH → ADOLESCENCE → MATURITY
(cf. BIBLE'S : FAITH, HOPE, CHARITY)
GREATEST OF THESE IS CURIOSITY

"THE NEW STUDENTS KNOW NOT THE OLD LESSONS" (B. RUSSELL)
THE OLD STUDENTS (FOR WE ARE ALL STUDENTS) HAVE BRED,
FOR THE OLD PROBLEMS, THE CONTEMPT OF INTIMACY?

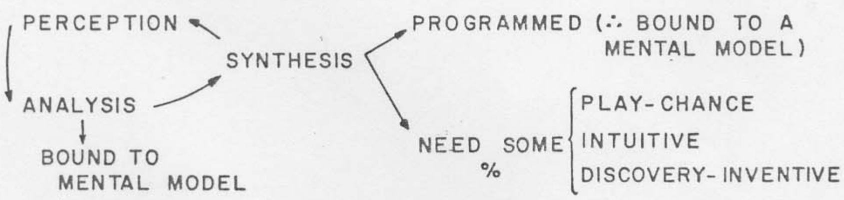
$\int_1^n$ (STEADY TRENDS) VS. QUANTUM REBELLIONS ?

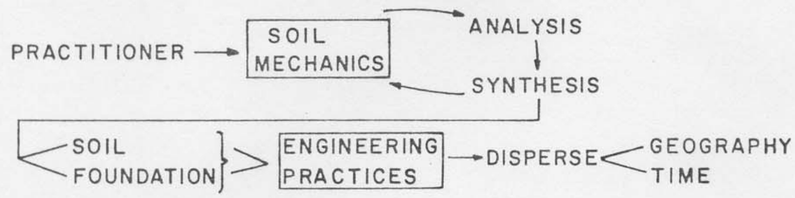
INGENIOUS ENGINEERING VS. "ENGINE" ENGINEERING ?

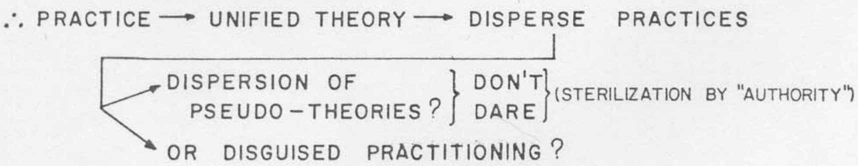
DEVELOPING COUNTRIES OR DEVELOPED COUNTRIES } MOST < NEED / FAVOR > DEVELOPMENT?

GLOBAL VS. TRIBAL (TECHNOLOGICAL) MAN ?
BECAUSE OF FANTASTIC COMMUNICATIONS ?

MAN'S ANIMAL INSTINCT SHRINKS BACK THE DIAMETER OF HIS TERRITORIAL IMPERATIVE ?







(MEMO B)

FUTURE = f (SOIL ENGINEERING HISTORY)

HISTORY = f (Σ GREAT MEN + CHANCE)

GREAT MEN = f (CHANCE + EFFORT)

∴ SHOULD AIM AT UNLEASHING CHANCE SOMEWHAT MORE ?

<u>DISSATISFACTIONS</u> WITH POOR PREDICTIONS

→ MORE CONSERVATISM

→ DISREGARD FOR ECONOMY

NEED REASSESSMENT + REVIVAL

(MEMO C)

QUALITY OF SAMPLING.

QUANTIFICATION OF QUALITY ?

INTACT (OR FIELD) PERFECT "UNDISTURBED" PARTIALLY DISTURBED FULLY REMOLDED	SAMPLES + SOIL ELEMENTS

NO SYSTEMATIC REPORTING IN PAPERS

AT BEST "METHOD SPECS" MENTIONED
AND NOT "END-PRODUCT SPECS", AS INDISPENSABLE

IN STATE-OF-THE-ART ASSESSMENTS, ALL SAMPLES OF OVER 30 YRS. ETC. LUMPED ACCORDING TO DESIGNATION "UNDISTURBED" ALTHOUGH EFFORTS HAVE BEEN TO IMPROVE QUALITY CONTINUALLY

(MEMO D)

IN SITU TESTING

MANY EARLY (?) DELUSIONS OF ADEQUACY INTUITIVE
ASSUMPTIONS OF CRUDE MODEL-PROTOTYPE ANALOGIES

PRESENTLY UNDER VERY PROMISING CROSS-EXAMINATION.

IN SITU TESTS NOT YET APPLIED TO COMPARE CHANGE
OF CONDITIONS, BEFORE ⟶ AFTER LOADING ?

NOTE: ABSENCE OF EVIDENCE IS NOT EVIDENCE OF ABSENCE
(IN PRESUMED CORRELATIONS)

(MEMO E)

REVISIONS OF INDICES POORLY CHOSEN (MEMO F)

e.g. 1) $\bar{B}$ OR $r_u = u/\gamma z$

NOT LINEAR VARIATION $u = f(\gamma z)$

$\bar{B}$ NOT CONSTANT.

CONDITION OF u − VE ?

NUMERICAL VALUE OF $\bar{B}$ AS $\gamma z \longrightarrow 0$? DISPERSIONS ?

$\bar{B} \longrightarrow \infty$?

2) $AC = \dfrac{Ip\,\%}{<0.002mm}$ * ← UTMOST DEFLOCCULATED (VARIATIONS OF IONS)

* Ip QUITE CRUDE.

UTMOST PHYSICAL DISINTEGRATION AND PLASTIFICATION

NO WORRY ON IONS OF POREWATER, AND VARIATIONS OF IONS

3) $K'_o = \sigma'h/\sigma'_v$? CONSTANT FOR A STRATUM ?

INEXORABLE PRINCIPLE THAT ALL THINGS DIFFERENT UNLESS ACCEPTABLY PROVED OTHERWISE.

3.1) SEDIMENTATION (OCEAN BOTTOM INVESTIGATIONS, TAILINGS, ETC.)

IF $K'_o \approx 1 - \sin\varphi'$ (VIRGIN CONSOLIDATION)

HOW DOES φ' CHANGE FROM 0° (LIQUID) TO "SOIL CONSTANT" ?

WHAT ESTIMATIVE FOR $\partial\sigma'h/\partial\sigma'v$ AT $\sigma' \approx 0$?

3.2) OVERCONSOLIDATED STRATUM, CONSTANT − $\gamma'z$ OVERBURDEN REMOVED.

OCR IPSO FACTO GREATLY VARYING WITH Z

$\because K'_o \approx f(OCR)$? $\therefore K'_o$ SIGNIFICANTLY VARYING.

EXAMPLE OF IRRATIONAL WORD ASSOCIATIONS (MEMO G)

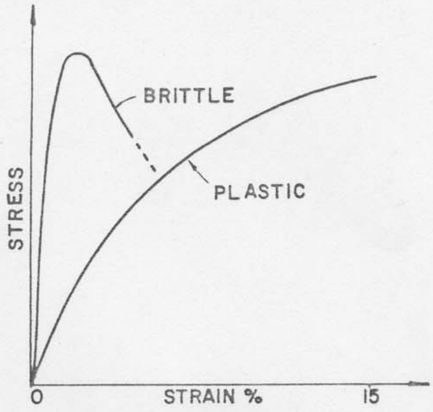
PLASTIC BEHAVIOR
IN COMPACTED CLAY DAMS

↓

∴ HIGH STRESS-STRAIN
PLASTICITY

↓

∴ HIGH PLASTICITY INDEX ?

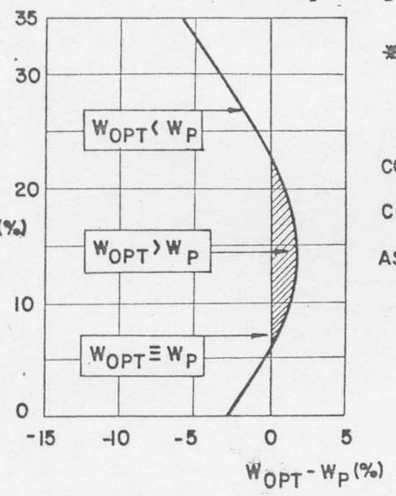


i.e. HIGH $W_L - W_P \equiv$ PROPERTY OF TYPE OF CLAY

$\equiv$ RANGE OF W FOR POTENTIAL PLASTICITY

NEED PLASTICITY AT GIVEN CONDITION, AT $W_{COMPACTION}$

DIFFERENCE
- TO BE [SER]* ESSENCE
- TO BE [ESTAR]* CONDITION, TEMPORARY



* HISPANO-PORTUGUESE DISTINCTION OF VERBS NON-EXISTENT IN ENGLISH

CONDITION AT OPT. COMPACTION W, USED AS $1^{ST.}$ APPROX. INDEX

THE THREE WISDOMS IN DISCREDIT (MEMO H)

THE PRACTISING PROFESSIONALS FACING THE DIVERSION OF
A 1000-YR. FLOOD OF PAPERS

THE EUREKA COMPLEX

RUSH EVERY TIT BIT OF INFORMATION TO PRESS

THE REFUGE SYNDROME

PRODUCTION ALONG LINES THAT CANNOT BE CHECKED

THE SOPHISTICATION TOTEM

SOPHISTICATION FOR SOPHISTICATION'S SAKE

NET RESULT: UMBRELLA SOLUTIONS

PROTECTION OF PRESTIGE

DESIGNS OVERDESIGNED
MULTIPLICITY OF COMPUTATIONS } ∴ ENGINEERING SERVICES EXPENSIVE, AT MAKING

PROJECTS ALL THE MORE EXPENSIVE

MODERN TREND. (MEMO I)

IN CONSTRUCTION PROJECTS,

INVESTING IN CONSTRUCTION INVENTIVENESS

3 MAIN CATEGORIES:

(1) THE BURDEN OF HEAVY EQUIPMENT

(2) INVENTIVE ENGINEERING PRODUCTS + PROCEDURES

(3) SYSTEMATIC IMPROVEMENT OF EQUIPMENT CAPABILITIES

NEGATIVE SOIL MECHANICS PSYCHOLOGY:

DISPENSE WITH SOIL, BRUTALLY DISREGARD IT

NEITHER WHAT TO DO WITH IT,

NOR _ _ _ _ _ _ _ _ _ _ _ _ TO IT:

RATHER _ _ _ _ _ _ _ DESPITE IT.

WHAT WE NEED ? (MEMO J)

FOR HIGHEST BENEFIT/COST POTENTIALITY

1. IN SOIL MECHANICS ,

POROSIMETRY, AIR-PORES, SUCTION.

SOLIDS — THE GOOD GUY, INVESTIGATED FIRST

LIQUIDS — THE BANDIT, STILL PURSUED

GASEOUS PHASE — THE MEDIATOR. NEXT INTEREST.

2. IN GEOTECHNICAL ENGINEERING

2.1 REVISED DEFINITIONS OF FACTORS OF SAFETY

2.2 HISTOGRAMS OF NON-FAILURE BEHAVIORS

ACCEPTANCE-REJECTION TRUNCATIONS ARE SUBSEQUENT, INDEPENDENT, VARIABLE AT WILL.

2.3 CHEAPER SYSTEMATIC PROTOTYPE OBSERVATIONS

ON INCREMENTAL ACTIONS VS. CONSEQUENCES.

AVOID OBSERVATIONS AND ERRORS CLOSE TO (0,0).

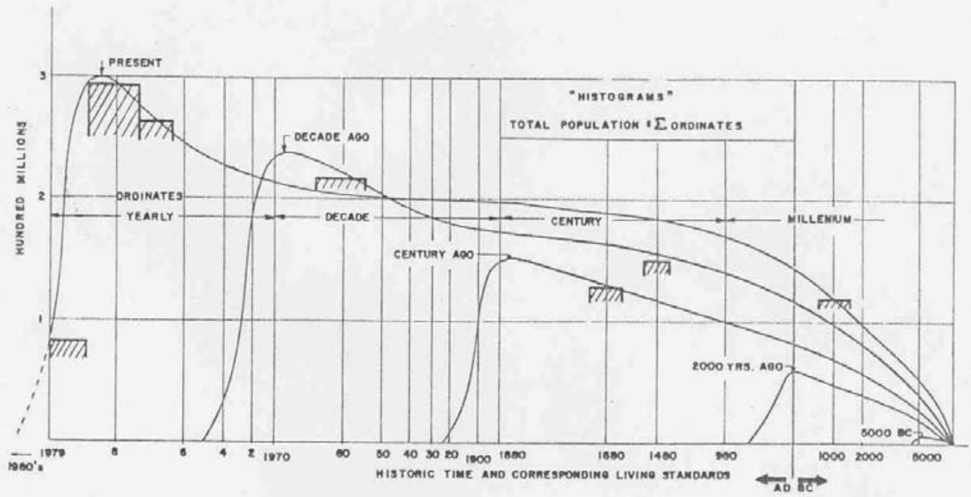


FIG. I SCHEMATIC IMPRESSION (PESSIMISTIC) OF DISPERSIONS OF WORLD LIVING CONDITIONS, AS PROGRESS, POPULATIONS AND NEEDS INCREASE.

INVESTIGATION, IDENTIFICATION, CLASSIFICATION

GEOLOGIC ORIENTATION, NOT GEOMETRIC, NEITHER RANDOM

GEOMETRIC PRESUPPOSES < HOMOGENEOUS / RIGID

RANDOM ≡ DISPERSION WITHIN GIVEN PHYSICAL STATISTICAL UNIVERSE

e.g. CONTINUITY OF BIG GRAVEL DEPOSITIONS IN RIVER ALLUVIA? LENSES?

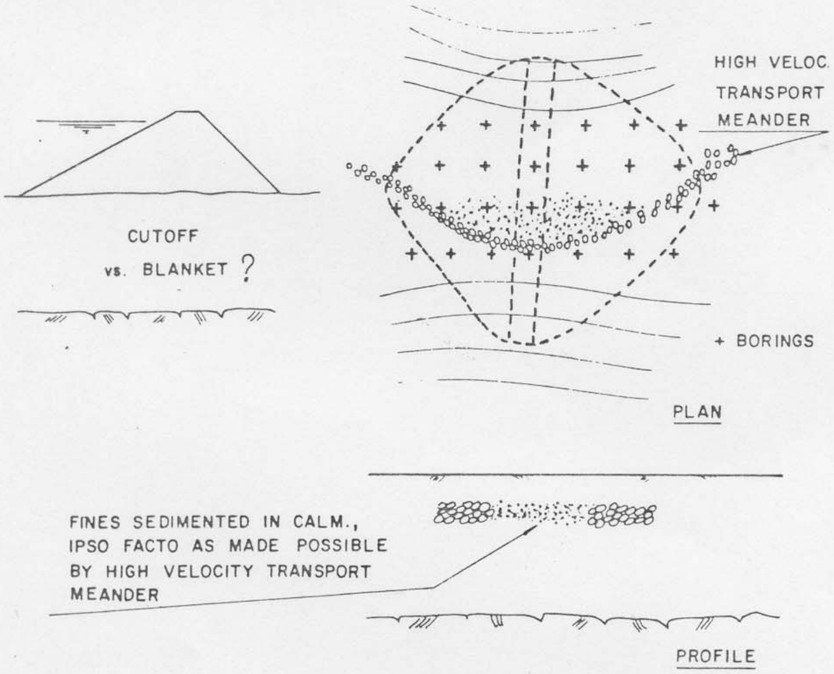


FIG. 2 SCHEMATIC. COMMON ERROR IN PLANNING AND INTERPRETING SUBSOIL INVESTIGATIONS UNDER LINEAR GEOMETRIC REASONING

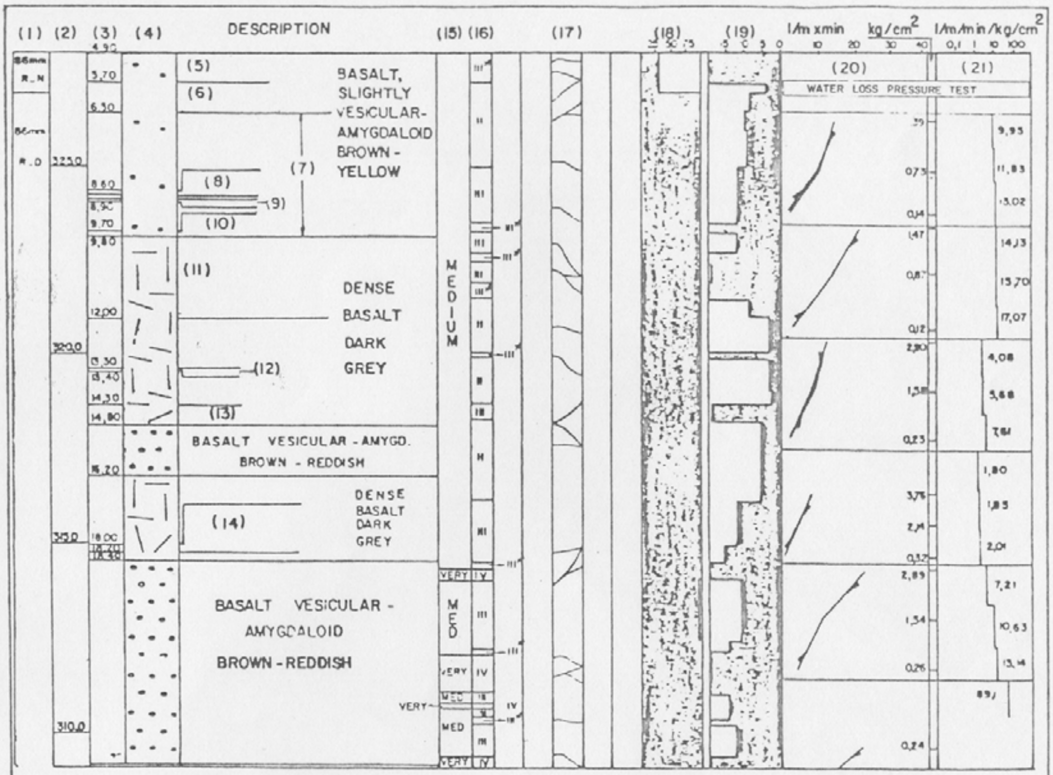
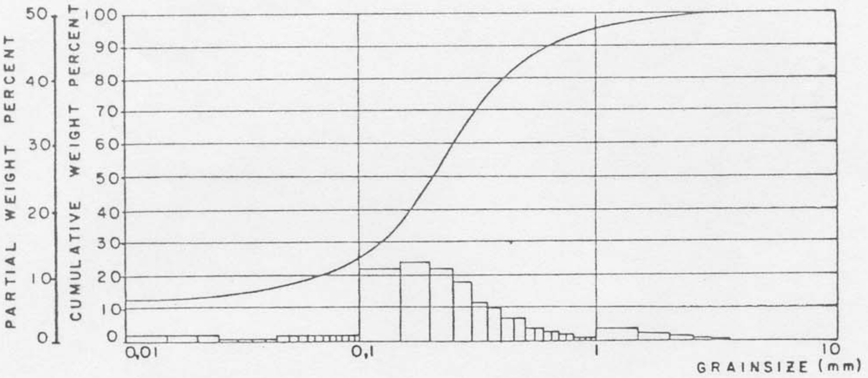
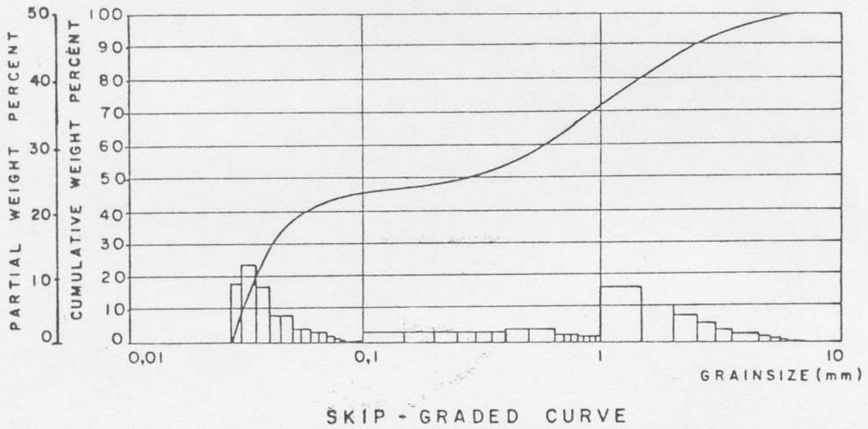


FIG. 3 TYPICAL FULLY DOCUMENTED CORE BORING PROFILE

COMPARISON BETWEEN DIFFERENT GRAINSIZE CURVES PRESENTED AS HISTOGRAMS



USUAL GRAINSIZE CURVE



SKIP - GRADED CURVE

FIG. 4 ROUTINE CUMULATIVE % GRAINSIZE CURVES MAY NOT EVIDENCE SO WELL A DANGEROUS GAPGRADING AS BY DRAWING HISTOGRAMS OF SIZES

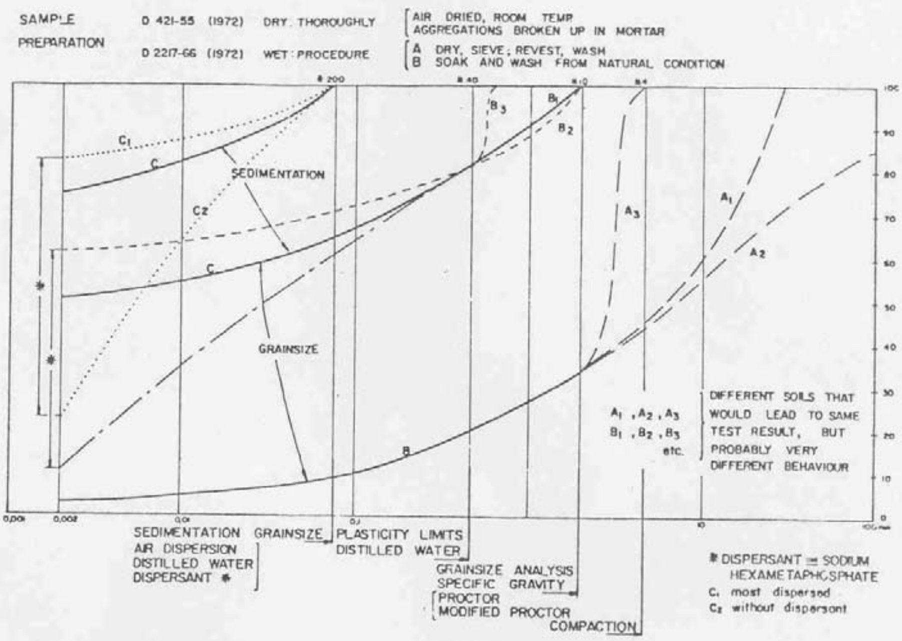


FIG.5 DIFFERENTIATIONS IMPOSED ON SOIL IN ROUTINE TESTS ; ASTM STANDARDS

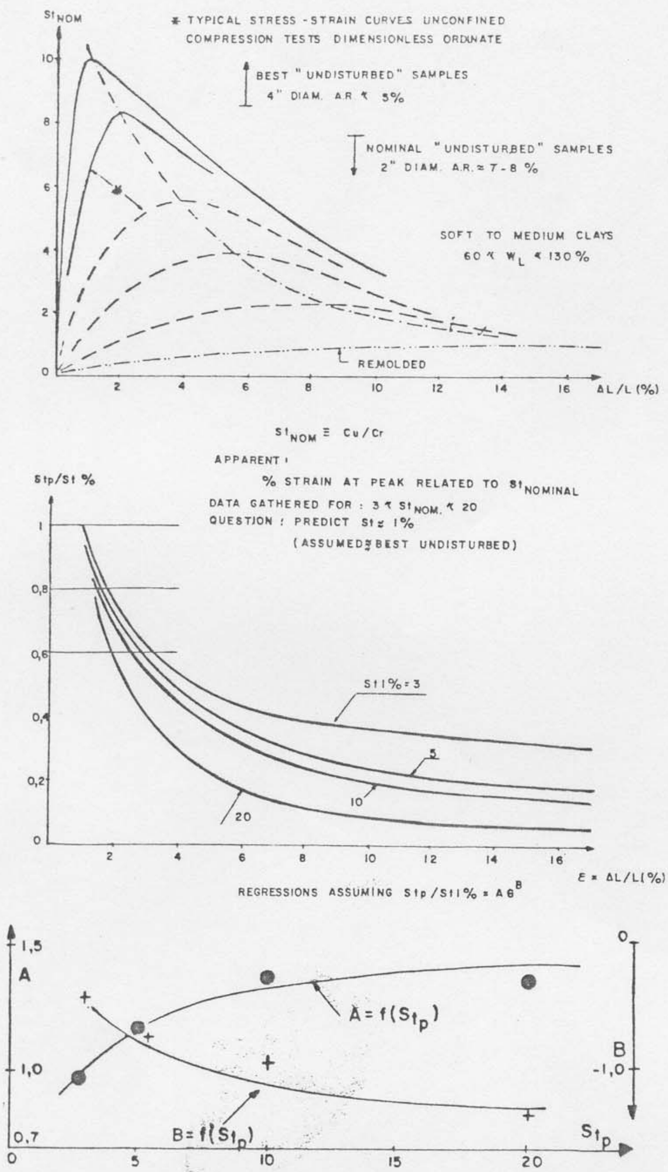
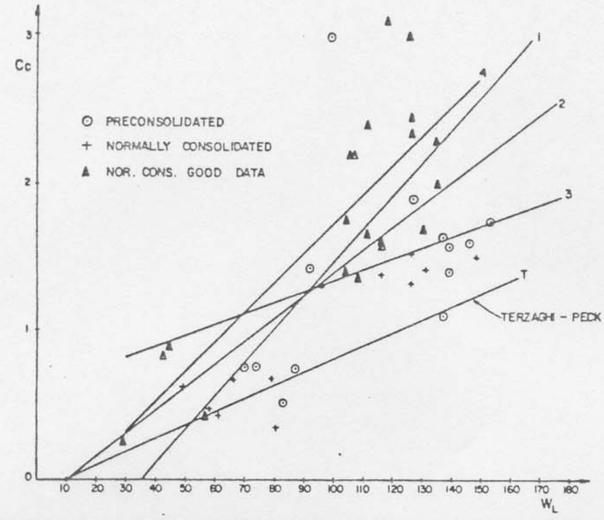


FIG.6 QUALITY OF "UNDISTURBED" SAMPLING REFLECTED IN PARTIAL St



	A - CLAY TYPE	r	*	B - STRESS CONDITIONS / C - PHYSICAL PROPERTIES (C_R)		r	*	A - B - C		r	*
NORM. CONS n = 32 + Δ	0,0182(WL - 19,05) CORRELATION 1	0,74	3º	γ'z	0,3289 + 0,01688 WL - 1,0817 (γ'z)	0,80	1º	γ'z	0,1902 + 0,0026 WL - 0,1700 (γ'z)	0,71	4º
				Pc	- 0,1232 + 0,01822 WL - 0,3674 (Pc)	0,75	2º	Pc	0,09659 + 0,00276 WL - 0,0206 (Pc)	0,65	5º
				e₀	CR = 0,00275 (WL + 30,58)	0,65	5º				
Precons n = 13 ⊙	0,007356(WL + 81,74) CORRELATION 3	0,36	3º	γ'z	1,3185 + 0,004054 WL - 1,115 (γ'z)	0,51	1º	γ'z	0,358 + 0,002567 WL - 0,1081 (γ'z)	0,29	4º
				Pc	0,7553 + 0,007444 WL - 0,1855 (Pc)	0,42	2º	Pc	0,3001 + 0,0005872 WL - 0,02224 (Pc)	0,25	5º
				e₀	CR = 0,0005767 (WL + 488,30)	0,19	6º				
ALL DATA n = 45	0,01426(WL - 1,842) CORRELATION 2	0,63	2º	γ'z	0,4268 + 0,01272 WL - 0,6561 (γ'z)	0,66	1º	γ'z	0,2044 + 0,001798 WL - 0,06214 (γ'z)	0,54	3º
				e₀	CR = 0,00199 (WL + 74,20)	0,52	4º				
** n = 20 ▲	0,02025(WL - 14,63) CORRELATION 4	0,77	2º	γ'z	0,2513 + 0,01809 WL - 0,7954 (γ'z)	0,80	1º	γ'z	0,1953 + 0,002616 WL - 0,1203 (γ'z)	0,71	3º
				e₀	CR = 0,002943 (WL + 38,22)	0,68	4º				

* ORDER OF MERIT CORRELATION.

** NORM. CONS. RECENT SPECIALLY CAREFULL DATA FROM 32 TESTS OF CORREL. I

FIG.7 - BRIEF INVESTIGATION OF C_C, C_R CORRELATIONS IN COMPARISON WITH THEORETICAL EXPECTATIONS.

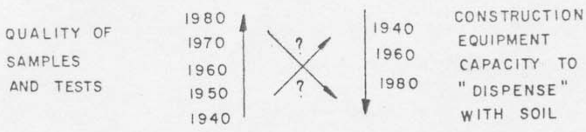


FIG.8 PROBLEMS OF CONSCIOUS VARIATIONS IN TIME AFFECTING THE CLOSING OF THE "EXPERIENCE CYCLE"

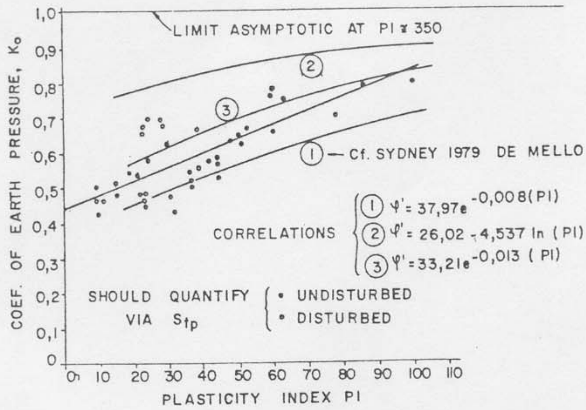


FIG. 9 APUD MASSARSCH 1979 ADAPTED

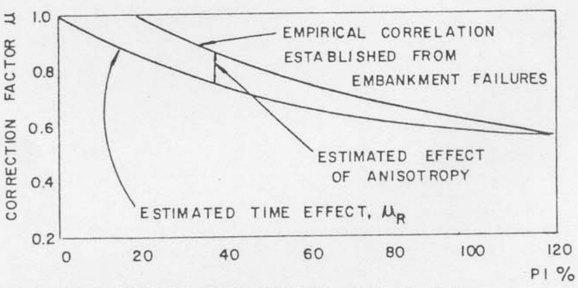


FIG.10 APUD BJERRUM, VIII ICSMFE, VOL.3 P.124

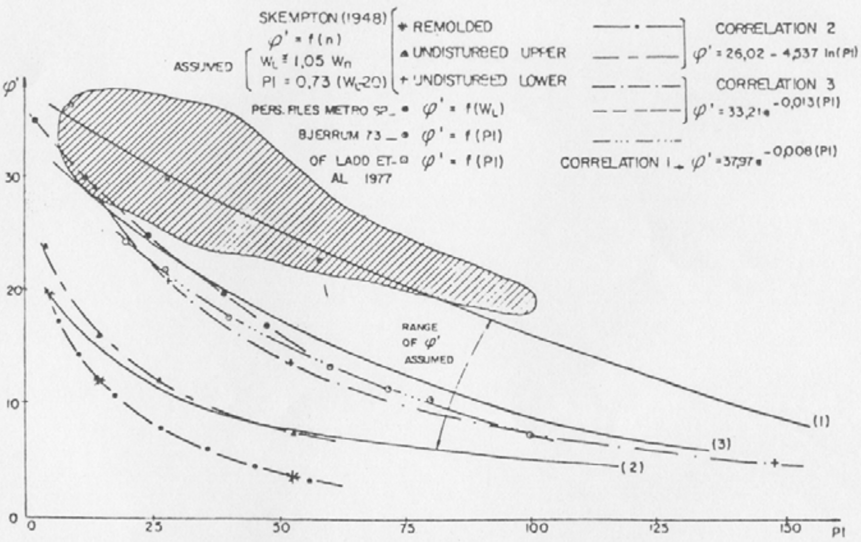


FIG. 11 - BASIC DATA FOUND ON φ'= f (PI)

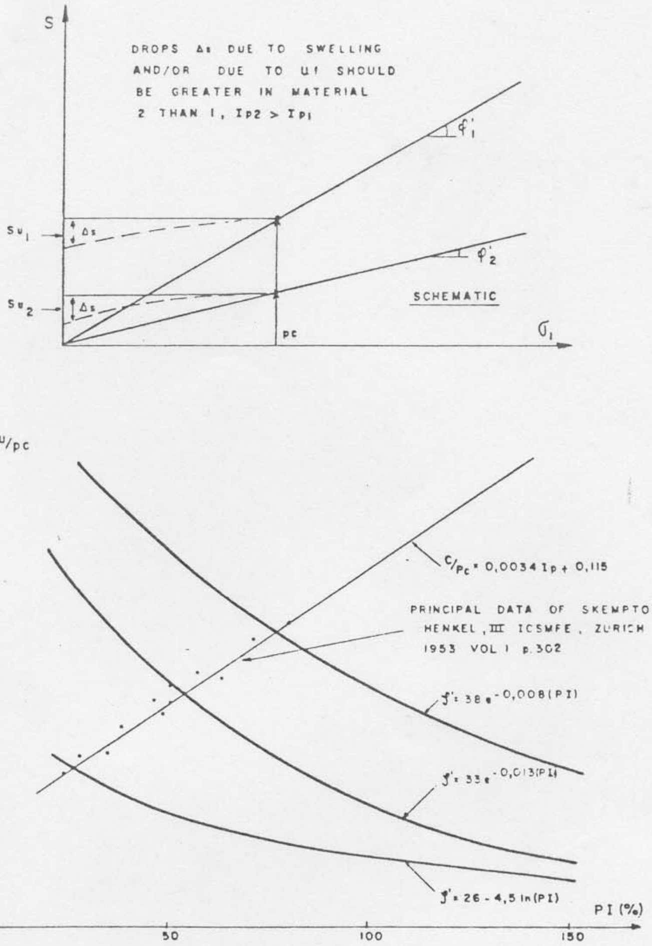


FIG.12 QUESTIONABLE CORRELATION FOR $C_u = f(pc, Ip)$

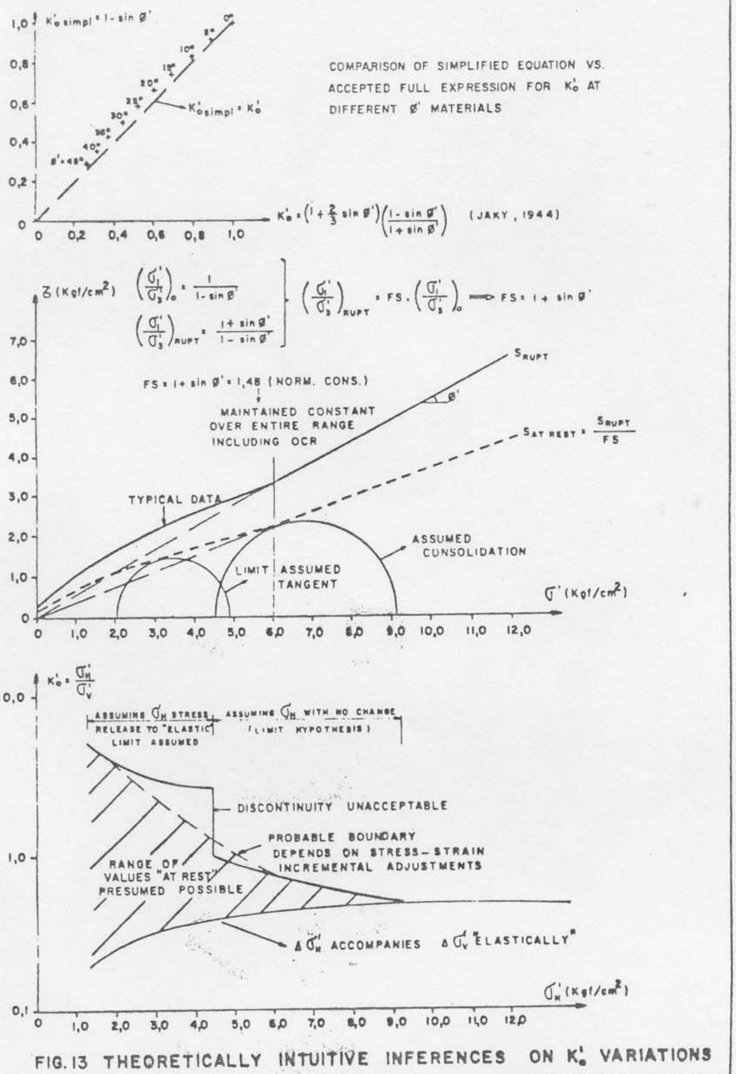


FIG. 13 THEORETICALLY INTUITIVE INFERENCES ON K_o' VARIATIONS

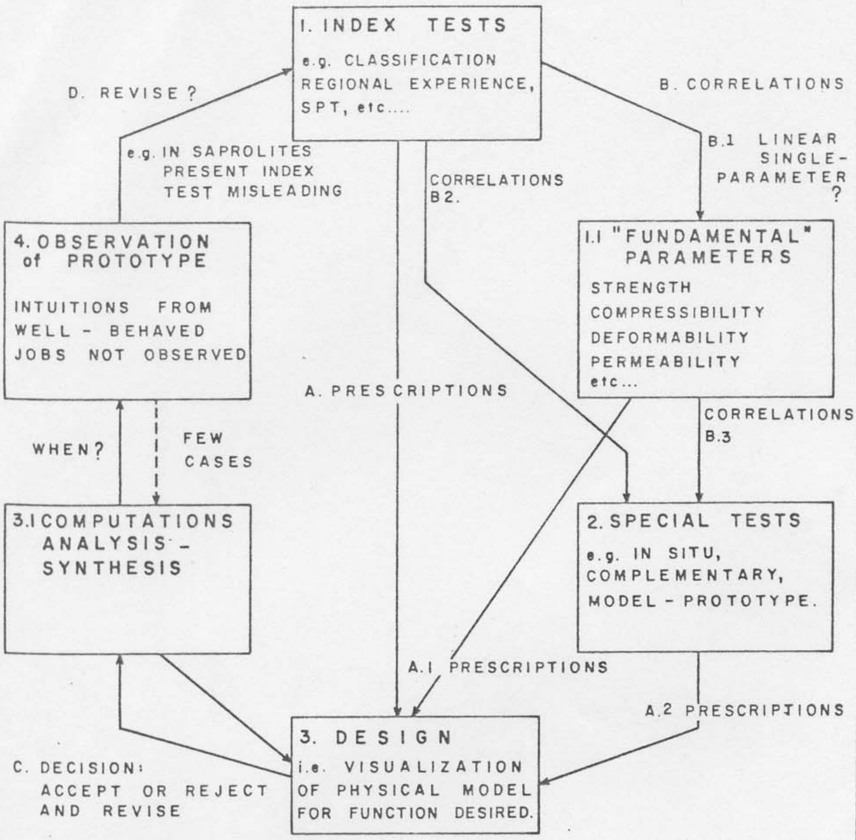
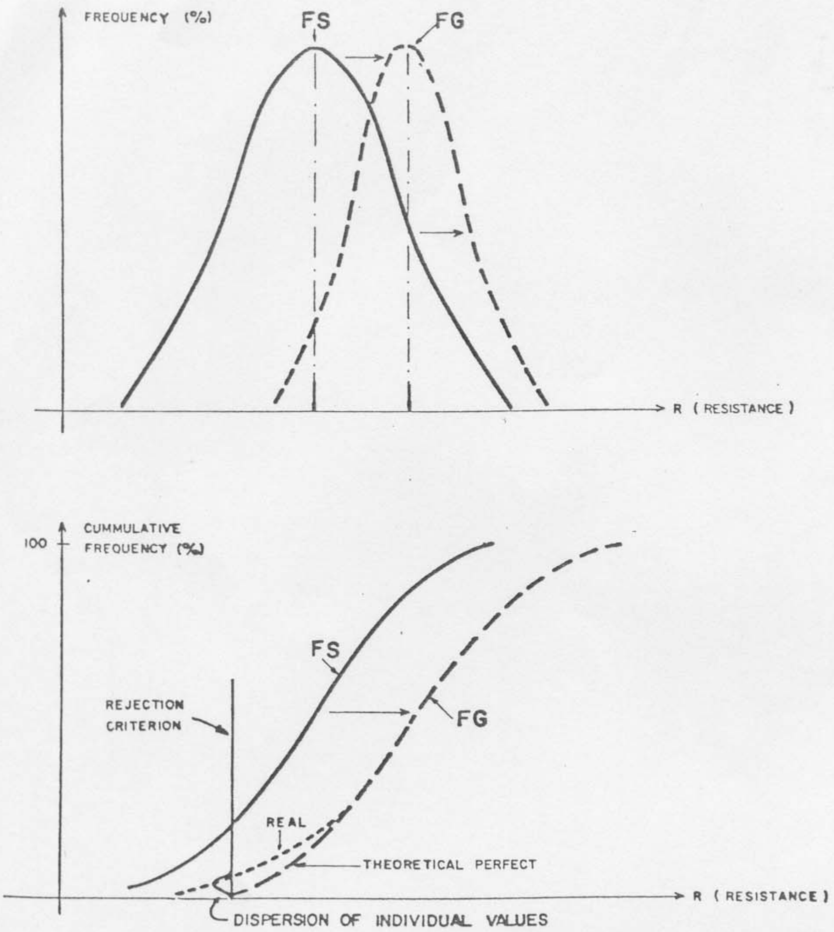


FIG.14 EXPERIENCE CYCLE IN CIVIL ENGINEERING

FIG. 15 PROPOSED DISTINCTION BETWEEN FACTOR OF SAFETY (FS) AND FACTOR OF GUARANTEE (FG)



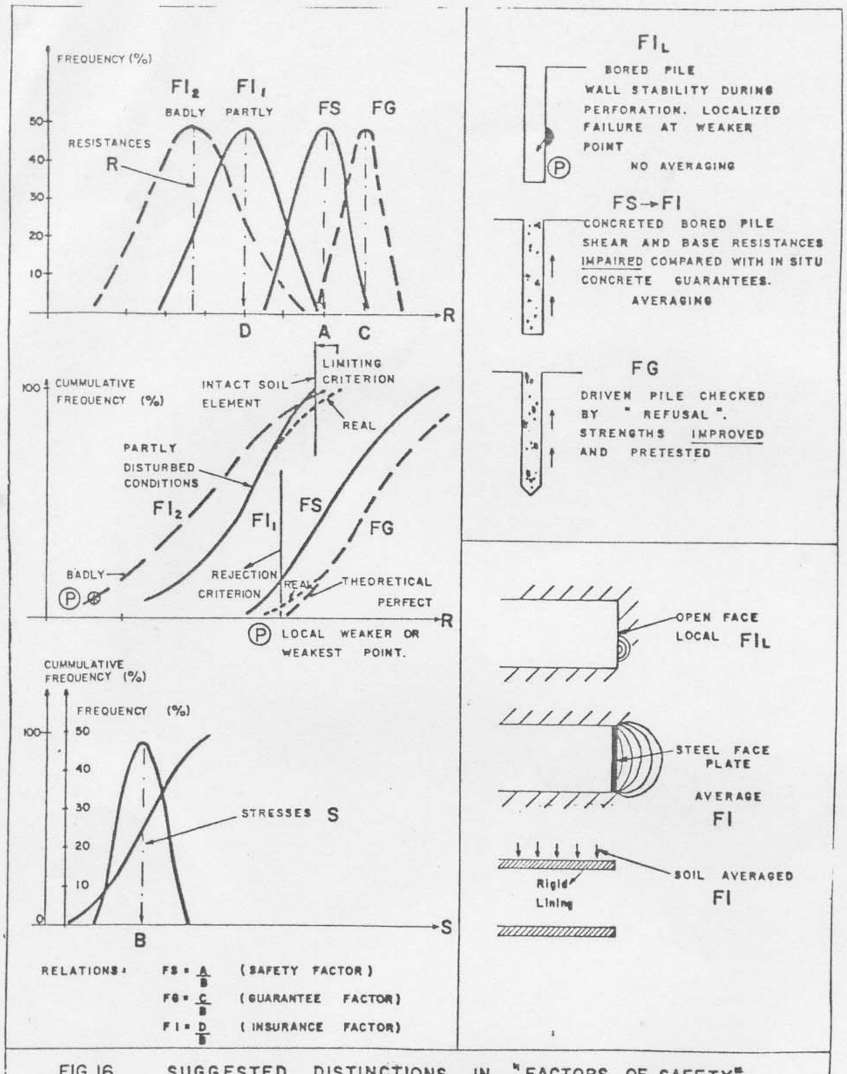


FIG. 16 SUGGESTED DISTINCTIONS IN "FACTORS OF SAFETY"