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ATTEMPTING AN HISTORICAL APPRAISAL OF SOIL MECHANICS AND GEOTECHNICAL ENGINEERING

CONFERENCIA DE CLAUSURA

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1. INTRODUCTION

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.

The special occasion now chosen is the 25th. anniversary of the formal founding of the Mexican Soil Mechanics Society, one of the strongest and most active of the Societies in the international geotechnical community. If one considers the official birth of this Society, one must recognize it as an infant-prodigy, because when it was barely about two years of age it was already very successfully sponsoring the first Panamerican Conference of Soil Mechanics, and when it was merely twelve years of age it was hosting the remarkable 7th. International Conference. And, indeed, prodigious has been the trajectory of the Mexican endeavours in soil mechanics and foundation engineering. But one must recognize that there is always something very arbitrary in any appraisal of history, because our sense of history is bound to, and bounded by, formal events as discontinuities; and, above all, one must remark that is is intrinsic to the very nature of the remarkable that it be subjective.

Inevitably subjetive must be any appraisal of the past, present and presumed future of the professional field we have embraced with love and zest. Moreover, it should be all the more pardonable as inexorable that the requested critical appraisal of such a trajectory should be yet more subjective. We do not merely live in an age of uncertainty, but we forge it, through simultaneous promotion of multitudinous ideas, facts and discoveries. Indeed, through the wonders of technological communication we have been brought once again to the kind of bewilderment that must have accompanied our forefathers through most of their historical attempts to face the complexities of Nature. It seems to me that probably the period of certainties (and determinisms and positivisms) must have been very short in the trajectory of human societies, since a certainty requires a very peculiar ratio of dominant idea to the ability of spreading it convincingly. Such a peculiarly selective ratio can be too easily upset either by changes in the numbers of ideas amenable to dominance, or by changes of their ability to spread and take root.

Thus it is my belief and message that rather than seek the illusory comforts of homogenization, we must learn to draw special pleasures and rewards from heterogeneities and from our honestly recognized differences. May we ever enhance idea-fertility and cross-fertilization, as well as the kind of natural selection leading to the equivalently fit multiplicity that is the test and proof of reality in anything connected with Nature. May each of you see in my present personal exercise nothing but the stimulation for your using a similar priviledge, differently conditioned and directed, because imbued with the same intent.

Before embarking on my challenging technical task, I should clarify my position regarding terms, and terms of reference. The question concerns the distinctions between engineering, engineering science, analytical pursuits and ability, computational ability within a given theory or working hypothesis, and the practice of engineering tasks within socio-economic restrictions. There has been increasing confusion regarding these distinctions. Society has wrought requirements of vast numbers of engineering workers as organized performers of tasks defined, conducted and finalized under routines temporarily accepted unquestioned. But the numbers dominating Society's temporary needs should not overwhelm us into the confusion. All the above different facets have equivalent collateral importance, like different organs sustaining a living body; and the proportions of different organs and activities must be appropriately balanced. Possibly in most minds it would be expected that I direct attention forthwith to the so-called conventional analysis-synthesis Soil Mechanics; but I do feel bound to respect the order I consider significant, which is <u>a</u>. inventive or ingenious engineering, <u>b</u>. engineering by pres-

WE NEED D I S C O N T I N U I T I E S

TO TAKE COGNIZANCE

OF A C O N T I N U U M

WE LIVE IN AN A G E OF U N C E R T A I N T Y

AND WE F O R G E IT

MULTITUDINOUS I D E A S, F A C T S, D I S C O V E R I E S

BEWILDERMENT AT COMPLEXITIES

END OF D E T E R M I N I S M S

END OF ILLUSORY COMFORTS OF H O M O G E N I Z A T I O N

SPECIAL PLEASURES AND REWARDS

IDEA - FERTILITY

CROSS - FERTILIZATION

E N G I N E E R I N G ≡ ACT OF DECISION DESPITE DOUBTS

ENGINEERING SCIENCE

ANALYTICAL PURSUITS & ABILITY

COMPUTATIONAL ABILITY W I T H I N W O R K I N G H Y P O T H E S I S

PRACTICE OF ENG'G TASKS

UNDER SOCIO-ECONOMO-LEGAL CONSTRAINTS

O R D E R O F S I G N I F I C A N C E

1. INVENTIVE OR INGENIOUS ENGINEERING
2. ENGINEERING BY PRESCRIPTIONS
3. THEORIZATION & ENG'G ANALYSIS - SYNTHESIS

criptions, c. theorization and engineering by analysis-synthesis.

2. INVENTIVE OR INGENIOUS ENGINEERING, FOUNDATIONS AND EARTHWORK

Our discussions of the history of soil mechanics and soil engineering almost without exception start with Terzaghi, circa 1923. That in itself would seem to emphasize the role of analytical work, with some disregard for the truly important place of engineering creativity.

Of course, we must begin by conceding some validity to the proverb that reminds us that when you are inside a forest you do not see the forest but the tree trunks. We are in the Terzaghi era: that is our first reality.

But there is somewhat more to be extracted from the observation. There are important reasons why inventive engineering is set aside. Principally we are concerned with the great numbers of engineering workers to be put to their tasks, and we are dominated by the needs of communication, of theories, procedures and rules, for others to apply unquestioningly. Thus we are subconsciously influenced in our assessment of the profession by the prevalence of tasks pertaining to academic circles. Soil engineering becomes what can be taught and learnt, and not what can be done. And we must further recognize that whereas professionals prior to around 1940 were sent our to fend for themselves with relatively little subsequent subjugation to academic production, in the recent past the rate of production of additional information and the intensity of technological development and communication has greatly increased, and perennialized throughout professional life, the subconscious dominance by academic activity. We are eternal students; but nowadays, much less so of life than of the flood of writings of teachers. We have imperceptibly allowed processes of information to occupy the biggest space of education and of professions, to the detriment of formation. Without any undue emphasis, may we remind ourselves that when we understand we may do nought but stand-under; or slightly less pungently, when we comprehend, we are fettered together.

Creativity is not created in frequency, and is not generally taught. It is difficult to institutionalize an academic structure whereby creative students are instigated to question, challenge, disagree, and propose other solutions, presumably more elegant. Yet we cannot deny the preeminence of engineering creativity as a physical visualization of a solution that so elegantly and superabundantly sets aside or dominates a set of problems, that calculation and analysis most frequently becomes quite dispensable.

In the past the engineering endeavours have been accompanied by a relative affluence of a ratio possibilities/requirements, doubtless because "requirements" had always been quite modest. Thereupon progress was always forged by a "breakthrough", statistically well ahead of the routines, that was tried, and achieved success; and thereupon the eminently imitative animal man stored the cultural gain through the "copying of success". Noticeable success to be imitated was always conservative in the sense that it was much better than necessary to meet the immediate requirements. Inventive progress is intrinsically by steps or leaps, each development opening a possibility that takes a considerable time to be used up by increasing demands. It is thus that good engineering, in design or construction, avoids being cornered, from its position of affluence of ingenious ideas into being better calculation or more conscientious engineering labour.

2. WE ARE IN THE <u>TERZAGHI ERA</u> :
THAT IS OUR FIRST REALITY

<u>SOIL ENGINEERING</u>
— WHAT CAN BE TAUGHT AND LEARNT *
— NOT WHAT CAN BE DONE

* GREAT NRS. OF ENG'G. <u>WORKERS</u> TO BE PUT TO <u>TASKS</u>

<u>DOMINANCE OF ACADEMIC ACTIVITY</u>
<u>EXTENDED THROUGH PROFESSIONAL LIFE</u>

WE ARE ETERNAL STUDENTS
— NOWADAYS — MUCH LESS SO OF LIFE
— THAN OF FLOOD OF WRITINGS

<u>CREATIVITY</u> — NOT CREATED IN FREQUENCY
— NOT GENERALLY TAUGHT

ENG'G "<u>BREAKTHROUGH</u>" = <u>REMARKABLY</u> BETTER
THAN JUST NECESSARY

∴ (1) IMITATED, BECAUSE <u>REMARKED</u>
(2) LEAP AHEAD, OPENS POSSIBILITY THAT TAKES TIME TO BE USED UP,
BY <u>INCREASING DEMANDS</u>

2.1. <u>HISTORICAL PERSPECTIVE</u>
MANY ELEGANT INVENTION INHERITED,
<u>TAKEN FOR GRANTED</u>

2.1. Historical Perspective

Many an elegant invention inherited from the past tends to be taken for granted with a -- gross underestimation of the degree of creativity involved at the time. For instance, we tend to exult in the recent developments related to the use of geotextile tension --- reinforcement (simultaneous with drainage) - at the bottom of embankments founded on soft clays, but the use of bamboo and sticks as - fascine is age-old. In a sense, the use of driven piles as a support was a remarkable - anonymous invention of foundation engineering that we take for granted, while architects and structural engineers recognize the -- significance of the invention of the masonry arch and dome for compression, and of suspension bridges for tensile materials. Recent documentary evidence shows that the Romans - used a most elegant offshore foundation for a lighthouse, still standing: they filled a boat with the hydraulic cements of the time, floated it out to position and sank it. In much more recent times the concept of the -- use of compressed air for working "in the -- dry" must rank as quite remarkable. So also in the matter of optimizing the benefits of a driven pile and recoverable casing, with - those of cast-in-situ concrete, Mr. Edgard - Frankignoul's (Belgium) invention of the --- Franki pile must be recognized as ingenious.

When was the concept of the floating foundation first used for buildings? Was it by a geotechnical engineer, and was such ingenious engineering dependent on the teaching of conventional Soil Mechanics? You well know, better than fellow professionals in any city in the world, how much our studies of soil - mechanics contributed thenceforth to the refinements of the application: but none better than yourselves to recognize the intrinsic worth of the inventive idea to begin --- with.

2.2. Modern inventive engineering products and procedures

The past decade or so has been fertile in -- bringing forth a series of solutions some -- what more inventive and potent than the produce of systematic analysis-synthesis of conventional soil engineering. Some have opened important new avenues to subsoil and --- earthwork engineering.

2.2. MODERN INVENTIVE ENGINEERING PRODUCTS AND PROCEDURES

EX. INTERNATIONAL COMPETITION OF SOLUTIONS FOR TOWER OF PISA

EXAMPLES OF DIFFERENT PHYSICAL SOLUTIONS

- ELECTROSMOSIS
- VACUUM PRELOADING
- BENTONITE-STABILIZED DIAPHRAGM ETC.
- SELECTIVE CHEMICAL GROUTING (e.g. SERRE PONÇON DAM)
- REINFORCED EARTH
- GEOTEXTILES
- FIBER VERTICAL DRAINS
- STONE-COLUMN STABILIZATION
- LIME-COLUMN STABILIZATION
- ROOT PILES
- CCP PILES
- DEEP COMPACTION

ETC. ETC.

DR. LAND'S COMPONENTS TO INVENTION
- FREE DREAMS
- HARD DIRECTIVE WORK

MEXICAN EXAMPLES

FLOATING FOUNDATION

CARRILLO'S "TENSION CENTER" + PUMPING-IN ETC.

PILOTES CONTROL

ETC. ETC.

In justification of my assessment of the relative potency of the two facets of engineering activity, it may be of interest to mention the case of the international competition held about four years ago for a possible design-construction turn-key project to solve the problem of the leaning tower of Pisa. Of course, only the biggest and best supported international civil engineering companies, aided by the topmost geotechnical consulting services, participated. Unfortunately the contract was not awarded, and the different solutions have not been divulged; a lecture on the comparative solutions, even schematic, would constitute a fantastic object lesson on civil engineering. In the face of a serious problem, even though more fully and carefully documented than any that can be imagined, there were essentially as many different physical solutions as there were contestants (about 15-20). When faced with a problem of high ratio of responsability/feasibility, it is not in better analytical work that engineers seek solutions, but rather in different physical solutions, different statistical universes that are meant to set aside quite definitely the possible histogram of degrees of unwanted behavior.

Electrosmosis and vacuum preloading of saturated compressible sites were two highly inventive developments which, however, were not fully marketed by their enthusiasts. The bentonite-stabilized diaphragm walls and bored piles constituted another inventive leap, that has recently been extended to the bentonite-shield for tunneling. The selective chemical grouting of alluvial foundations of dams was employed with confidence in making feasible the construction of the major Serre Ponçon dam about 25 years ago, and a recent publication on 20 years of behaviour carefully monitored indicates the 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 drawn 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 need 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".

It is not demeaning to "engine engineering" efforts of soil mechanics to give first priority to intuitive ingenious engineering in most such developments. Some remarkable examples to the contrary might only serve as the exceptions that prove the rule, or as examples of creative breakthroughs based on existing theorization. Nabor Carrillo's early brilliant mathematical solution to the problem of subsidences generated by pumping-out, through analysis of stress-strain changes in a pseudo-elastic medium (Subsidence in the Long Beach-San Pedro, Cal. Area: the effect of a tension center". 1949) ranks as one outstanding example; the highly profitable engineering follow-up of judiciously employing pumping-in (recharging wells etc.) for allaying subsidence (and, in the case of oil wells, continuing to optimize oil production) can thus fall into the category of fertile interaction between existing theoretical tools and intuitive breakthroughs.

My own early attempt at inventive work (1946 1948) embodied in my doctorate thesis (and a joint patent of invention) was conceived under theoretical reasonings that "solidification" of clays could best be achieved by base-exchange with an appropriate monomeric cation, and subsequent polymerization, thus achieving the strengthening of the linkages between clay particles via adsorbed cation and polymerized chain. In a sense, mine was the Acrylic Monomer No. 1, AM-1, a calcium acrylate, and successive developments led to much work at M.I.T. and the present solution grouting product AM-9, used in especially difficult conditions. On looking back I am rather happy that I resisted the seduction of novelty, sensing the problems of costs and modest prospects of practicality, and moved out of the project. On closer analysis one might even observe that the benefits of the stabilization procedure are dominantly those of polymerization of the intersticial solution, with little complement from the theoretically anticipated base-exchange links.

Another theoretically oriented attempt at inventive development that would seem highly profitable, technically and economically, suggests itself as the development of monomeric solutions that might be catalysed into selective polymerization in function of seepage velocities. Possibly through some electrophoretic action. The basic question is that in dams and other hydraulic structures the use of grouting from arrays of holes embodies one valid principle (where water might find its preferential paths, so should hopefully, another liquid that can be induced to solidify), but accompanied by two factors of inefficiency and cost: firstly, the series of perforations attempting to find the future preferential paths, and secondly the pressure injection outwards from holes, quite different from that of impounded water. The sealing action of silting caulked-in by the very pressure of seepage stresses is well known to be efficiently selective, and cheap.

Polymerization could be induced to generate a selective growth of "silting" sizes to improved matching with crack sizes. Apparently developments are being promoted along such --- lines.

Such examples are merely cited as cases of -- Dr. Land's type of oriented inventive activity, interplaying between ingenious and engine engineering. In foundation engineering a remarkable example is the development of Pilotes Control, another local demonstration of - how daring can be the solution sired by ingenious engineering when necessity is the mo -- ther.

2.3. Concept of inventive engineering in geotechnique, and presumed future

Side-by-side with the civil engineering euphoria at such creativity, what reflections --- should we extract therefrom with regard to -- conventional soil mechanics? It seems to me - that we have to be very wise and alert in order to avoid being railroaded off to a siding by two factors of everincreasing intervenience: one is what I choose to denominate "the - burden of heavy and special equipment", the - other is the "excessively exacting demands" - from modern society of geotechnicians.

2.3. PSYCHOLOGICAL TREND OF CONSEQUENCE

FROM INVENTIVE ENG'G. IN

GEOTECHNIQUE: -

BURDEN OF HEAVY & SPECIAL EQUIPMENT

EXCESSIVELY EXACTING DEMANDS FROM SOCIETY

PAST: LOVE AND RESPECT OF FRAILTIES UN-KNOWN OF SOILS

TREND: BRUTALLY DISRESPECT SOILS AS

NUISANCE, TO DISPENSE WITH.

SOLUTIONS DESPITE SUBSOIL CONDITIONS

MAN, IN DEVELOPING CIVILIZATION,

GOES AGAINST NATURE

AT WHAT $\left\{\begin{array}{l}\text{SOCIAL}\\ \text{ECOLOGICAL}\end{array}\right\}$ COST?

3. PRESCRIPTIONS AND

WORKING HYPOTHESES

4. CORRELATIONS SUBSTITUTING

FOR PRESCRIPTIONS

FALLACIES

PRESCRIPTIONS ≠ CORRELATIONS, EQUATIONS, LAWS

CORRELATIONS — SPURIOUS STATISTICS,

FRUSTRATING DISPERSIONS

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 engi -- neering. 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 psy - chology 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 behaviour 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 pro -- jects was unquestionable: the problem lies in designing and building medium-size and small-projects as if they were mammoth jobs dwarf - ed.

Moreover, from some areas there have been remarkable systematic improvements of equip -- ment capabilities, some of them fortunately-channeled directly into civil engineering -- construction. When we stop to think of the-exponentially exponential developments in -- electronics, and in most industrial developments, we are readily carried away into pride at what can be achieved by concerted co - llective efforts at development: many cen -- ters may be mentioned, but the prime example is conceded to be Japan. It is quite clear-possibilities of creation in synthetic, in - dustrialized fields dealing with materials - subject to manipulation of high benefit/cost ratio, will trend toward exponentially gro - wing proportions in comparison with the mo - dest manipulations ob subsoil conditions.

Thus increasing proportions of problems mi - ght be approached from the viewpoint of so - lutions despite subsoil conditions. 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 it, or even despite it. In a vicious circle we presently ride. Highly developed in-dustrial production and its quality control-offers fantastic possibilities, but places - more and more exacting demands on geotechnicians. We are required to guarantee foundations that will not settle more than a cou - ple of millimeters despite unusual combina - tions of loads, temperatures, vibrations, -- etc.; we are required to guarantee against - risk of cracking under hypothetical risks of seismic events. And so on. And without confessing our relative dissatisfaction with -- our available conventional solutions, insufficiently precise, guaranteed, and economic, - we have found recourse in solutions that --- essentially dispense with detailed concern - for the soil's personality and whims. The - self-same industrial output gives us the --- means.

Man in developing civilization cannot resist being against Nature, to mould her to his desires. At what social and ecological cost?

3. PRESCRIPTIONS AND WORKING HIPOTHESES

In my estimation, in a critical analysis of-the development of geotechnical engineering, the second place of importance must be given to PRESCRIPTIONS, both for design and cons - truction and for such prior and subsequent - activities as investigations, testing, specifications, and so forth. No other fundamental tool of our technological chain and rationale meets with so much incomprehension and -- misrepresentation; PRESCRIPTIONS are most often not recognized as such, being either promoted to the levels of dogmas, principles -- and theories, or paired with CORRELATIONS, - or even derided as the practitioner's "fudge factors". Yet is is by PRESCRIPTIONS, ma -- nuals, codes, standards, etc., that the vast majority of our efforts are conducted. And-by a satisfactory prescription we simultaneosly take a step forward in our practice, -- and retard immensely the stimuli for the dynamics of revision.

Doubtless every geotechnician recognizes --- that the use of the CBR criteria for pave -- ment design is a sheer prescription. But -- how many would concede that almost every design and construction practice is similarly-nought but a prescription or working hypothesis?

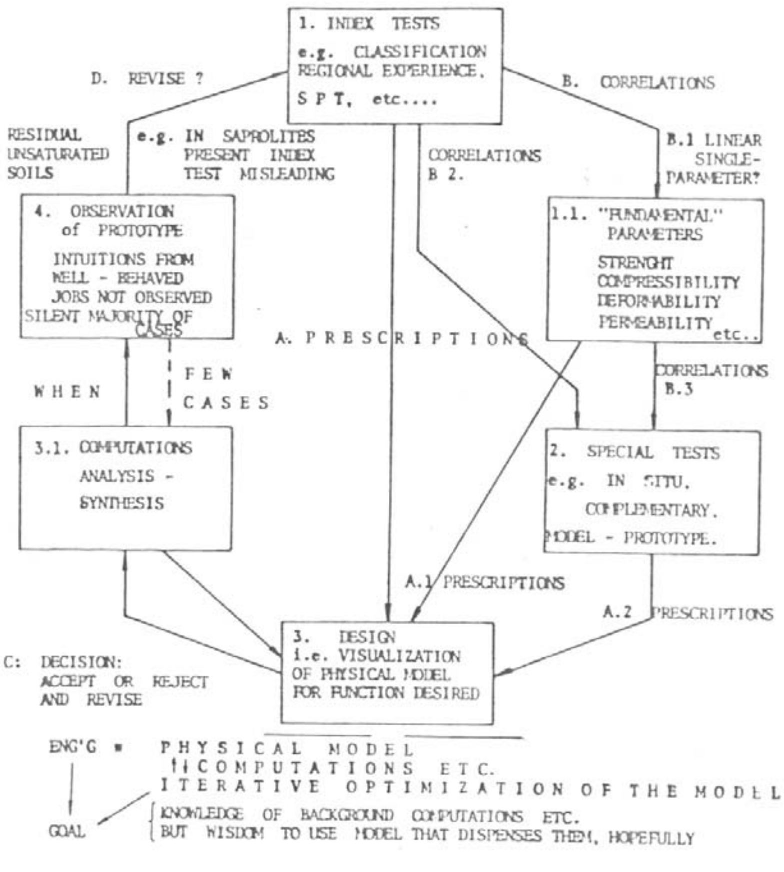
Well, let us begin by the general statement-that most of our works are designed either - for some Factor of Safety FS against failure or for some limiting allowable deformation:- is there anything but PRESCRIPTION to back - up the adoption of FS $\gtreqqless$ 1.5 (say) or an allowable deformation at $\gtrless$ x mm or 1: $\gtreqqless$ y distortion? If these final yes-no decision criteria are nought but prescriptions, everything leading up to them cannot be much different-or better.

Thereupon, as a second step, we could list - specific items pertaining to our principal - works, limiting ourselves to the most significant design items in order to avoid extending the list until it includes every single-design item:

e.g. Dams:

Grouting and drainage treatments of founda - tions.
Disposition of filter-drainage features within the dam body.
Acceptable seepage losses.
Criteria for filters and transition mate --- rials.
External slopes, stability and deformability.
Compaction criteria and field vs.lab. proce-dures.
Acceptable plasticity of core material.
Deformations conducive to cracking, tolera - ble limits.
Liquefaction criteria, seismic behavior and-risks.

E X P E R I E N C E   C Y C L E   I N   C I V I L   E N G I N E E R I N G



Foundations:

Choice of foundation type regarding feasibilities, preferences, risks of defects, damages, deteriorations.
Allowable bearing pressures on footings.
Settlement computations using oedometer data.
Settlement estimates from plate load tests and extrapolation to footing size.
Allowable bearing loads on piers.
Pile working loads, pile driving and final set.
Pile working and failure loads based on static formulae and/or penetration tests.
Bored piles: contributions from adhesion and base loading.

Deep excavations:

Diagrams of earth pressures on strutted or anchored facings.
Comparative diagrams on diaphragm walls.
Construction stability and deformations of diaphragm wall excavations with slurries.
Deformations of supported mass and foundations thereon.
Bottom heave, in general (c',$\emptyset'$) soils.
Choice of groundwater lowering, feasibilities, preferences, risks, consequent deformations.
Soil treatment (grouting etc.) and benefits therefrom.

Machine foundations:

Design for attenuation of vibrations or impacts.
Estimates of behaviour due to vibrations.
Estimates of transmitted vibration behaviour.

Tunneling:

Face stability.
Settlement trough at surface.
Influence of settlement trough on adjacent foundations.

And so on.

As an example, let us consider in slightly greater detail the first item listed. Analogous minimal discussion could and should be applied to any and all items.

When we accept that fractured rocks giving water losses greater than 1 Lugeon (how tested? how computed? how interpreted?) should be grouted, is that anything but the crudest prescription? Are we able to predict anything of the behaviour of the said rock foundation if (a) we did not grout (b) if the rock was characterized by 0.1 Lugeon or 10 Lugeons? (c) what criteria exist, if any, for distributing drilled drainage or relief holes within the rock, or how would the criteria change upon use of grouting or not? And so on. As we well know, we are far from being able to answer any rational cause-effect predictions on comparative treatments: we have accepted the publicized practices by prescription. And, if it is difficult enough to spread the use of a given prescription, how much more difficult is it to revoke its use after epidemic wave spreads, if we find need to correct or improve!

As we shall expatiate under item 4, in most of such items we may even find a considerable body of published papers indicating (under some hypotheses) how to analyse the uplift pressures, seepage flows, seepage gradients, etc., in the said foundation. But, as every conscientious junior engineer will complain, after all the computations (often by several distinct procedures) have been completed,

a) neither is he remotely confident of the realism of his computation,

b) nor is he at all aided in his judgement and decision as to how to use the result.

"Their's not to reason why,

Their's but to do and die"(Charge of the Light Brigade, Tennyson)

Such is the nature of a PRESCRIPTION, and it is in the nature of the patient to use the remedy in patient trust.

CURIOSITY - EFFORT - EXPERIENCE

YOUTH - ADOLESCENCE - MATURITY

GREATEST OF THESE IS CURIOSITY

"THE NEW STUDENTS KNOW NOT
THE OLD LESSONS"

THE OLD STUDENTS HAVE BRED FOR THE
OLD PROBLEMS THE CONTEMPT
OF INTIMACY?

RESPECT FOR THE PAST

ALLEGIANCE TO THE PRESENT

I N T E R E S T } IS IN THE FUTURE

COMMITMENT

In a triad curiosity-effort-experience, that in varying proportions could define the evolution from youth through adolescence to maturity, both in persons and in the technologies they handle, all three being indispensable to progress, we obviously recognize that prescriptions unwisely used unfortunately -- numb all three. They should be intended merely to minimize (the costs of) effort, especially unsuccessful effort. But regrettably what they most achieve is:

a) to cloud the conditions for acquisition - of experience, because PRESCRIPTIONS are- UMBRELLA SOLUTIONS,

b) to kill curiosity. That is indeed the -- most damaging consequence in practice, -- because, of experience, effort and curiosity, the greatest is curiosity. For some privileged spirits, the modern world - favors keeping perennial the flame of curiosity, of youth. Research is not an activity, it is an attitude that can pervade any occupation. 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; and -- none can deny that in the world of geo -- technical engineering our humble but exhilerating position as children is greater- than in most other domains.

Finally, the integrated effect derives from the truism that experience is gained at the- activity exercised. If the activity is of - curiosity-stifled and effort-less resort to- Prescriptions, the experience vector merely- consecrates the unwanted byproduct of an --- otherwise indispensable engineering working- tool. In using a successful prescription we may be using so big an umbrella that there - is gross and frequent overdesign. Not only- does Society thereby pay an inmediately high price: the higher hidden price accumulates - with time. There has to be protection of --- prestige. Failure conditions are difficult- to quantify with reasonable precision, and - Factors of Safety are much under debate but- failure is anathema and must be kept at --- arm's length: we honestly do not acquire --- quantifiable statistical experience from -- failures, and from poorly understood nominal Factors of Safety.

If a histogram of behaviors under a given -- prescription does not at least occasionally- cross the boundaries of the presumed desirable-undesirable, we forego the possibility - of gaining experience for one of the Design- Principles that I consider (and proposed, cf. Rankine Lecture, 1977) as fundamental, i.e.:

Design Principle No. 5: "For every behavior- desired and assumed, check what happens, of- consequence, if it is not successful."

How does one arrive at an unsuccessful umbrella solution of gross overdesign except in - the consequent cost, and cost-of-living? Note that the bane of such trends is worst if- PRESCRIPTIONS and UMBRELLA SOLUTIONS in geotechnical engineering fail to be specific to local conditions, as is the enslaving trend, especially through well-meaning internatio - nal spread of communication and authoritative books.

All is not grey however. To have a solution even if only by PRESCRIPTION, means that we- recognize the problem also. That is already two steps forward: a big one, knowing the -- problem; a stepping-stone one, to know at -- least one temporary acceptable solution.

4. THEORIZATION AND ANALYSIS-SYNTHESIS

Whereas creativity happens, and prescrip --- tions achieve engineering doing, hopefully - engineering science accumulates. Therein -- lies our present interest and concern.

I endeavour to furnish my interpretation, -- despite the risk and certainty of colliding- with other interpretations, equally valid. - After we have been on a road for a long trajectory, what matters is the incremental advance with incremental effort along some direction, along our own individual assess -- ments of presumed directions. What should - be avoided is Brownian movement.

4.1. Personal interpretation of landmarks

Our conventional Soil Mechanics owed its --- first steps of success to cutting the Gordian knot from the complexities and vague qualifications of the geology conducted as one- of the natural sciences of the time, and --- assuming the fertile mental model of deterministic quantitativism based on judicious - testing and accompanying mathematical analysis. The oedometer test and its use in settlement calculations basically represented a model-prototype idealization. Soil mecha -- nics theorization was rational on the basis- of single parameter associations. Soil classification was determined by the dominant -- phase, the solids. The interference of wa - ter ("neutral" pressures) had to be separa - ted, subtracted. Most parameters and tests- created and in use were consciously or sub - consciously towards being dominant, dichotomic: cohesive, cohesionless; (c,ϕ) as $(c,0°)$ or (o t/m2,Ø); pervious, impervious; compressible, incompressible; plastic, non-plastic;

static, dynamic; active, passive pressures;- and so on. To some extent we can sense that the yet pervading pseudo-dichotomy of failure problems as distinct from settlement problems, and undisturbed vs. remolded (forgetting the inexorable adjetives, partially undisturbed vs. fully remolded) were inevitable outcomes of the pervading conscience of the time.

In short, to the benefit of rapid early progress in rationalizations in soil mechanics, direct experimentation was employed on idealized homogeneous soils, and essentially on each individual cause-effect problem referred to a single pair of parameters.

In comparing with the attitudes of collateral natural sciences, health and sanitation, etc., one might postulate that the engineer-(structural) brought into early Soil Mechanics a greater proportion of the attitude of doing, the solution syndrome, plus the cause-effect testing context of Strength of Materials, plus the priority preoccupation with-failure (and the collateral directive to investigate by destructive tests). Meanwhile-the fields of natural sciences and even that of health and sanitation, of great practical importance to progress of our society, developed very noticeably despite restriction to observation and non-destructive testing, destructive testing being essentially impossible in geologic settings and taboo in the -- biological fields. Under the imposed continued observation of thousands of units of -- the statistical universe of minute multiple-causes and effects simultaneously interfering, aided, no doubt, by the back-analyses - of the multitudinous case-histories of ultimate failure (death inexorable), the fields-of biological technologies resorted to more-intense application of the statistical tools of multiple parameter regressions, multi -- variate analyses, factor analysis, grouped - observations in regression theory, etc.

The comparative rates of social and research investment in the two approaches would merit assessment, and correspondingly the comparative benefit/cost rations of the two technologies serving society through civil and sanito-medical engineering. The fact is that - in situ geotechnique is much more akin to -- the conditions of Nature, of many simultaneous small influences, and, in some respects-the euphoria of the successes of the dominant doer-engineer with a deterministic approach and the subsequent single-parameters-correlations (frequently pseudo-correlations of statistics at random) may now pay the price of frustrations in the face of heterogeneities. The phase of respectful recognition of the sensitivities of natural conditions,- and of innate difficulties in each individual case as distinct from all others, came into-early soil mechanics as a sequel of the --- first rapid advances, possibly as problems - of consulting engineering over vicissitudes-increased in relevance and proportion: we -- may term in the phase of the heterogeneous-problem-vision of soil engineering.

It emphasized experience, which was excellent and inevitable; but it left a mute feeling that the roads to gaining and asserting-experience were poorly mappable.

Having postulated the above two early trends I submit that the classification of soils on the basis of fully-disintegrated grainsize - curves was obvious, considering the accepted dominance of solids (grains) and the interest in recent sediments. The initial successes later retarded recognition of the importance of shapes of grainsize curves and of - grain shapes etc., which have yet to be rationally measured, classified, and related - to behaviors. Moreover, in the great land - masses of tropically-weathered and unsaturated, indurated, and partly-cemented soils the inability to test and define a "significant-size of grain-cluster and structure" for -- appropriate classification of soil behavior, has become one of the starting difficulties-to adjustment of conventional soil mechanics to engineering.

The recognition that in fine silt-clay sizes the plasticity behaviors took over preeminence in the classification of soils was -- another early significant step. The index -- tests (Atterberg-Casagrande) on behaviors of plastic soils spread far and wide because of their simplicity, and have served considerably: but criticisms have steadily accumulated, partly because the tests are on fully -- plasticized and remolded soil, and partly -- because of the relatively crude tests standardized and solidly entrenched. Some interesting research studies in the 1958-70 period offered seductive rationalizations, referred to mineralogy, clay-fraction Activity Index, suction, undrained shear strengths, - etc.; they belong to the period of search - for understanding of behaviors of ideal synthetic remolded materials. An elegant theorization on the liquid limit and plastic limit indices as worth revising into two simple index tests of undrained shear strengths (of - the order of 0,17 kg/cm2 and about 100 times higher) was proposed (e.g. Schofield and --- Wroth 1968, Wroth and Wood, 1978, etc.) based on the CRITICAL STATE LINE of remolded - soils. In our further discussions of CORRELATIONS we shall comment on the very slow -- progress of the proposed partial rationalization.

Great significance must thereupon be attributed to the recognition of Structure and Sensivity of clays: in transplanting the laboratory findings to "undisturbed" in situ soil-elements, four automatic consequences were:- a. the start of efforts towards "undisturbed" sampling and research on effects of disturbance/remolding; b. the collateral effort in the direction of in-situ testing; c. the emphasis on "triaxial testing" presumed to aim at stress-path investigation of stress-strain-strength behavior; d. the budding consciousness of varying K'o conditions for defining in-situ states of stress of soil elements.

A. CONVENTIONAL SOIL MECHANICS

1. TERZAGHI: FROM THEORY TO PRACTICE

1.1. ABANDONED QUALIFICATIONS AND COMPLEXITIES OF GEOLOGY

1.2. DETERMINISTIC QUANTITATIVISM
- JUDICIOUS TESTING
- MATHEMATICAL ANALYSIS

1.3. SINGLE-PARAMETER CAUSE-EFFECT RATIONALIZATIONS

1.4. SOIL CLASSIFICATION = f(SOLIDS, ULTIMATE PARTICLES)

1.5. INTERFERENCE OF WATER ("NEUTRAL PRESSURES") TO BE SEPARATED, i.e. SUBTRACTED

"EFFECTIVE" STRESS EQUATION

1.6. DICHOTOMY: 100% - 0%, NO VISION OF DISPERSION HISTOGRAM

COHESIVE, COHESIONLESS

FAILURE, SETTLEMENT

PLASTIC, NON-PLASTIC

DRAINED, UNDRAINED

"UNDISTURBED" (PARTIALLY), "REMOLDED" (FULLY)

ETC.

SOLUTION VISION

FAILURE ∴ DESTRUCTIVE TESTING & F S

2. PERIOD OF DISPERSE DISCIPLESHIP & CONSOLIDATION, 1936 -'48 (ROTTERDAM) - WIDE SPECTRUM

3. 1948 TAYLOR - FUNDAMENTALS: PROBLEMS AND QUESTIONINGS

TERZAGHI-PECK - SOIL ENGINEERING PRACTICE, PRESCRIPTIONS

4. PERIOD 1945-'60

4.1. RESPECTFUL RECOGNITION OF SENSIVITIES OF NATURAL CONDITIONS CONSULTING

4.2. HETEROGENEOUS-PROBLEM-VISION

4.3. PLASTICITY BEHAVIORS, CLAY- FRACTION ACTIVITY

4.4. STRUCTURE & SENSITIVITY ... SAMPLING

4.5. INITIAL IN-SITU TESTING

4.6. TRIAXIAL TESTING - PRESUMED STRESS-PATH

4.7. BUDDING CONSCIOUSNESS OF VARYING K'o

5. PERIOD 1950-'60

5.1. COLLOID CHEMICAL EFFECTS, MINERALOGY, TRACE ELEMENT STABILIZATION, etc... — RESEARCHER'S IMPROVED OWN UNDERSTANDING

5.2. 1957 LONDON:

WET VS. DRY COMPACTION

φ' VS. φ' AND $c' = 0$ VS. c_u

5.3. BOULDER SHEAR RESEARCH CONFERENCE, 1960
EFFECTIVE STRESS STABILITY ANALYSES

5.4. MALPASSET 1959 --→ ROCK MECHANICS: DISCONTINUITY.

6. PARIS, 1961

6.1. PILES, DEMISE OF DEEP FOUNDATION BEARING CAPACITY RIGID-PLASTIC EQUATIONS, COEFFICIENTS.

6.2. DEFORMATIONS INTERFERING.

7. PERIOD > 1966

7.1. DEFORMATIONS AS DOMINANT PREOCCUPATION.

7.2. LONDON LARGE BORED PILE: DEFORMATIONS.

7.3. FINITE ELEMENT ANALYSES.

7.4. CRITICAL STATE SOIL MECHANICS. CULMINATION OF MENTAL MODEL ON REMOLDED CLAY ETC.

7.5. FRUSTRATION WITH TESTS
- LABORATORY: STRESS-PATH IN PRACTICE, QUESTIONED: INITIAL CONDITIONS.
- IN SITU. FIRST DISILLUSIONS

7.6. LIQUEFACTION UMBRELLA SOLUTIONS.

8. PERIOD > 1970

8.1. UNPREDICTABILITY AND STATISTICS.

8.2. COMEBACK OF INVENTION AND DOMINANCE OF EQUIPMENT & PROCEDURES.

The phase of research respect for the sensitive frailties of clays generated a protracted period of efforts along lines of clay mineralogy, colloid chemical effects, thixotropy, minute trace effects, trace element soil improvement, influence on soil structure. Although contributing to the researcher's own deeper understanding of intrinsic behaviors, to the readers of the publications the effects may have been quite varied because of the many assumptions (in series) in simplified correlations, and the idealized conditions; the net effect to practice can be assessed as of minimal benefit/cost ratio.

The engineering concepts and solutions that set aside for about one generation the problems of piping and sand liquefaction (filters and filter criteria on the one hand, and critical void ratios on the other) were among the most important early landmarks.

Spurred by the London Clay investigations important developments were established in fundamentals of shear strength behavior of overconsolidated clays and fissured clays. But, while stress-path triaxial testing was being steadily promoted, the principal landmark is interpreted to be the recognition of K'_o in-situ stresses justifiably different from the assumed $k'_o \cong 1-\sin \emptyset'$ (pertaining to normally consolidated conditions).

Doubtless the Boulder Colorado ASCE Shear Research Conference, 1960, is one of the principal landmarks of the maturing of soil mechanics. Failure criteria (Mohr, effective principal stress ratio vs. deviator stress), predominantly strain-controlled testing, and effective stress (vs. total stress) analyses gained ground so convincingly, that possibly the pendulum might swing back somewhat, for instance, in special cases of collapsive behavior (suggesting stress-controlled, soft-load, stressing, and total stress analyses). Surely, however, the adjustments of observed slope failures to FS = 1,00 in the slip-circle analyses was a deterministic exaggeration that is still transmitting somewhat undamped undesirable influences in geotechnical thinking and practice.

In a collateral line we must note the shocking case histories of Malpasset Dam (1959) and Vajont Reservoir (1963), and the consciousness of Rock Mechanics and of the weak-discontinuity.

In shallow foundation design the consciousness of deformations as the principal preoccupation had been camouflaged under the reduced bearing capacity coefficients (Terzaghi, etc.) of "local failure in compressible materials". Gradually however the practice fell by the wayside, and all attentions concentrated on more realistic settlement computations, to be compared with PRESCRIPTIONS of proposed limiting allowable deformations.

In deep foundations one special landmark might be the London Conference on Large Bored Piles wherein the differentiated load-settlement coparticipations of adhesion and base were emphasized, and again, settlement-criteria came to the fore in comparison with bearing capacity limit-analysis formulations. The most significant turning point would probable be conceded to be the Paris 1961 presentations of the IRABA Chevreuse station prototype-scale pile load tests showing the significant limitation on theoretical rigid-plastic formulations of increased bearing capacity with depth.

Finite element analyses, and a good array of analytical solutions for elastic and elasto-plastic behaviors of soil masses and soil-structure interactions need not be mentioned as the well-recognized dominant crop of the past 15 years. Computational ability for stresses and deformations may be estimated to be a few decades ahead of the capabilities to supply bonafide input data, and to profit of resulting outputs for judicious decisions. In the wake of these very rapid advances have come the proposals for constitutive equations.

Throughout the roughly three decades of world efforts to apply conventional soil mechanics, there have been very significant: a. developments of in-situ testing (especially emphasized as undisturbed-sampling-plus-laboratory-testing came under greater questioning); b. listing and reporting on peculiar soils, unsaturated, indurated, fissured expansive, collapsive (loesses etc.), saprolitic, lateritic, quick clays, etc., beckoning more generalized theorization.

Within research and testing efforts over the past score of years there has been a slow growth of simple statistics to cover heterogeneities. One must note the diminishing of sheer effort feeding questionable or spurious statistics.

Finally, special mention must be made of field observations and case histories. Terzaghi early began to emphasize the importance of field observations, but if seems as if the case-histories were meant to constitute a warning of vagaries exemplifying the importance of "experience", more than documentation for a histogram of natural quantifiable trends from which experience is acquired. In efforts towards PREDICTION of behavior, another significant landmark, the frustrations have been repeatedly exposed over the past dozen years. Unpredictability has been a keynote in the wake of hopefully meticulous stress-path testing and sophisticated computations. Some of the frustations have been assigned to questions on in-situ states of stress, destructuration of specimens under typical sampling-testing, vitiation of strains and small strains even if specimen failure conditions remain relatively unaffected, and so on.

A wide open door has been opened to probabilistic prognostications, and applications of decision theory. It must be noted that in many such pioneering applications the intents are much more commendable than either the methods, the results, or especially the claims.

The most remarkable recent line of development has been connected with observational instrumentation. The first aim has been towards confirming theories and designs, and therefore has been aided but also somewhat straight-jacketed. But the sensorial possibilities are incalculable: for instance, there have already been some successfull trends towards forewarning on damage thresholds by sophisticated recording of microacoustic generation, and so on.

4.2. Dominant first-approximation correlations.

There have been repeated admonitions that most of the correlations established in early Soil Mechanics to aid the practising geotechnician served the purpose of sorting out some perceptible interrelationships, but are neither satisfying in concept nor sufficiently useful in practise for quantifying estimations.

The main criticisms are that, having been extracted from idealized laboratory experimentation, they were a. single parameter correlations, generally without a minimum recognition of even a second significant interfering parameter; b. generally established by visual fitting of best presumed straight lines, with no consideration of the significance of dispersions; c. based on remolded specimen testing, with no hints at natural effects of structure, time, cementations, secondary compressions, etc...

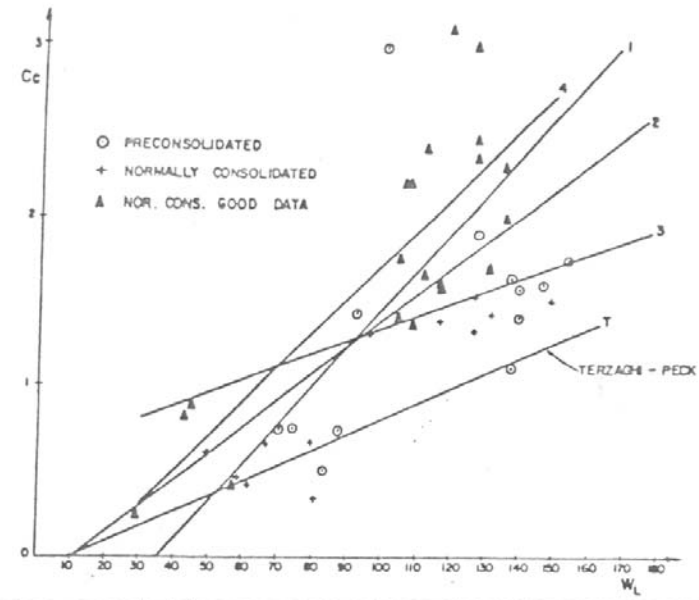
Truly, however, at the back of these criticisms lies a pervading one of concept, whereby not the least effort was made to cross-link with other correlations and data involving the same or related parameters and theoretical implications. The reason must have been the deterministic psychology of single pairs of dominant cause-effect relationships; and probably there was the psychological pressure for urgency in "publish or perish", the EUREKA COMPLEX.

Let us consider separately some examples related to remolded clays, since at least on these there should be close reproducibility of tests, and dispersions should be heeded as signifying definite trends, requiring multiple regressions, etc...

a. Virgin compressibility. Remolded clays.

The very useful simple correlation $Cc = 0{,}007 (W_L - 10)$ should need adjustments. Dispersions around it must have identifiable and correlatable justifications. For instance, to begin with, considering that for a given W_L there is a wide range of Ip values possible in soils of different compositions, it is incredible that the remolded clay Cc should not be expressed to reflect some interference of Ip as a minimum second parameter, even though the Plasticity Chart classification of clayey soils has emphasized such dual interference. There is an intuition that at a given W_L the soils with higher Ip should give a noticeably higher Cc: is that proven, and what correlation $Cc = f(W_L, Ip)$ can be offered as corrective?

Moreover, it is quite likely that there might also be some influences of grainsize (filler contents) and initial void ratio, since it is not reasonable to expect that these obviously influential physical parameters should influence in an exactly similar manner both the Plasticity Index tests, and the virgin compressibility. (Fig. 1)



	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 - 19,08) 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 ○	0,007358(WL + 81,74) CORRELATION 3	0,36	3º	γ'z	1,385 + 0,004054 WL - 1,115 (γ'z)	0,51	1º	γ'z	0,354 + 0,0002567 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 - 18,42) 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 - 14,53) CORRELATION 4	0,77	2º	γ'z	0,2513 + 0,01809 WL - 0,7954 (γ'z)	0,80	1º	γ'z	0,1953 + 0,002646 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. 1

FIG 1 - BRIEF INVESTIGATION OF C_c, C_R CORRELATIONS IN COMPARISON WITH THEORETICAL EXPECTATIONS

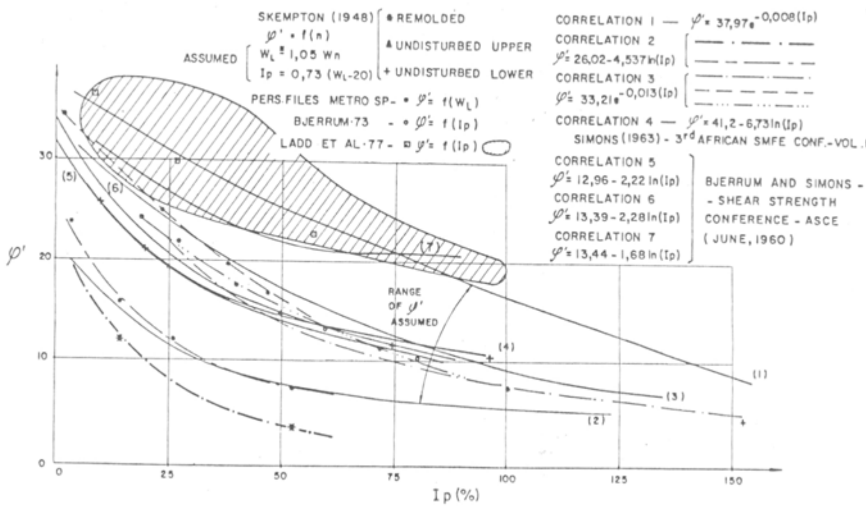


FIG. 2 BASIC DATA FOUND ON $\varphi' = f(Ip)$

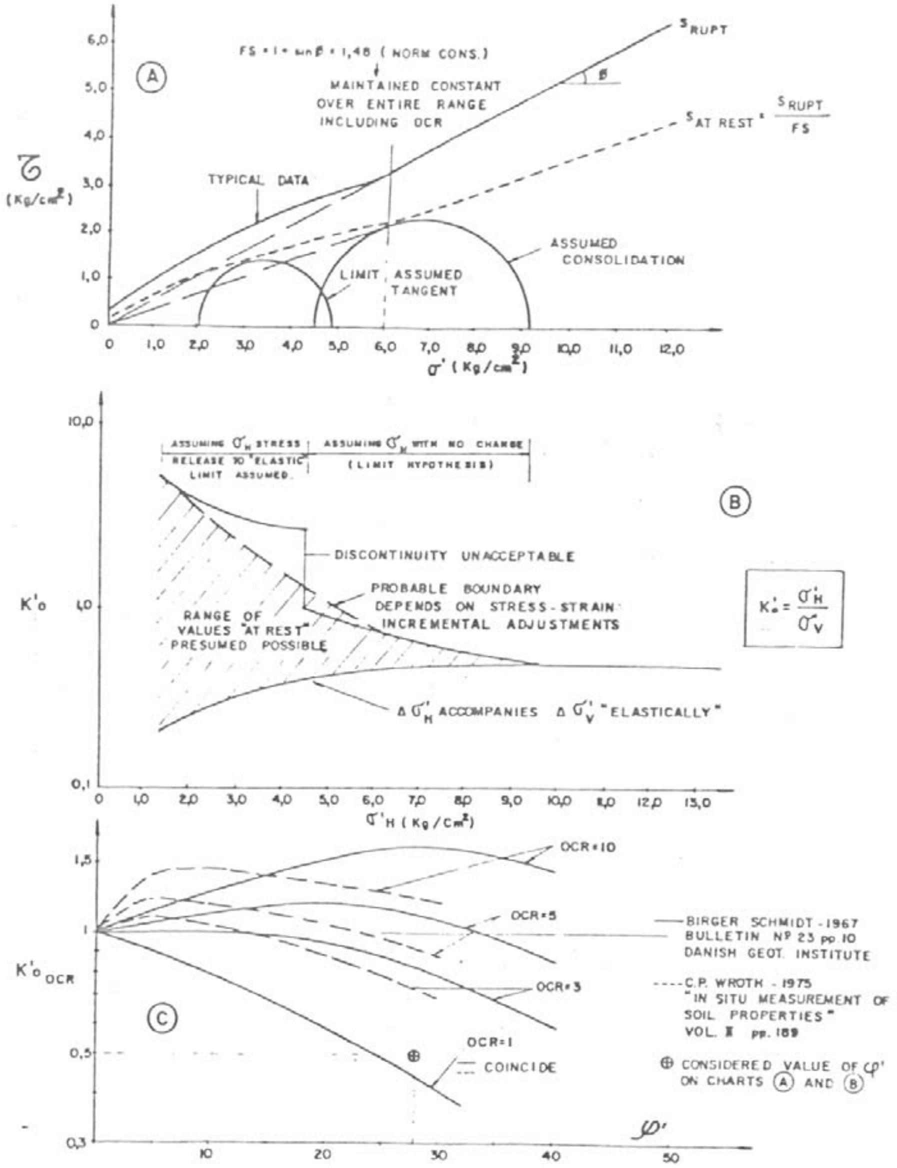


FIG. 3 SUGGESTED DERIVABLE K'o "ELASTIC" VALUES, OCR CLAYS

Let us note, therefore, the changes of attitude required. In nature everything is different unless proved acceptably similar, and all factors intervene, unless proved sufficiently insignificant (function of the problem). Meanwhile for the pioneers it was important to be able to concentrate on the major single issue, so as not to lose themselves in dispersions. The trouble is that " the new students know not the old lessons" and the old students (for we are all perennial students) have bred for the old problems the contempt of intimacy. The spread of geotechnical analysis-synthesis has reached circles relatively insensitive to the fundamental behaviors and the conventional simplifications. Our great mentors of the early days of soil engineering faced the humbling complexities of the unquantified problems, and made an effort to achieve conventional correlations, that they well recognized to be conventional, idealized, and simplified; thus, when applying a simplification they carried with them the full benefit of the wisdom of those wh. start from the bewilderment of reality and painfully reach the ability to distill it to the essences of simplicity required to solve the problem. A new generation of geotechnicians has been taught the simplified solutions as if the equations were reality and the dispersions possible errors, generally without sufficient emphasis on hypotheses, and so the rational simplicity of rationalizations has seduced, and suppressed all humility towards Nature.

b. At rest lateral pressure coefficient K'o, remolded and undisturbed clay.

A second example concerns the suggestion that in "typical" normally-consolidated clays, the conventional $K'_o \simeq 1 - \sin \varnothing'$ be substituted by a linear regression $K'_o \simeq 0,44 + (0,42)$ Ip/100 for 20<Ip<80, and this essentially irrespective of being "disturbed" or "undisturbed". (Fig. 4B)

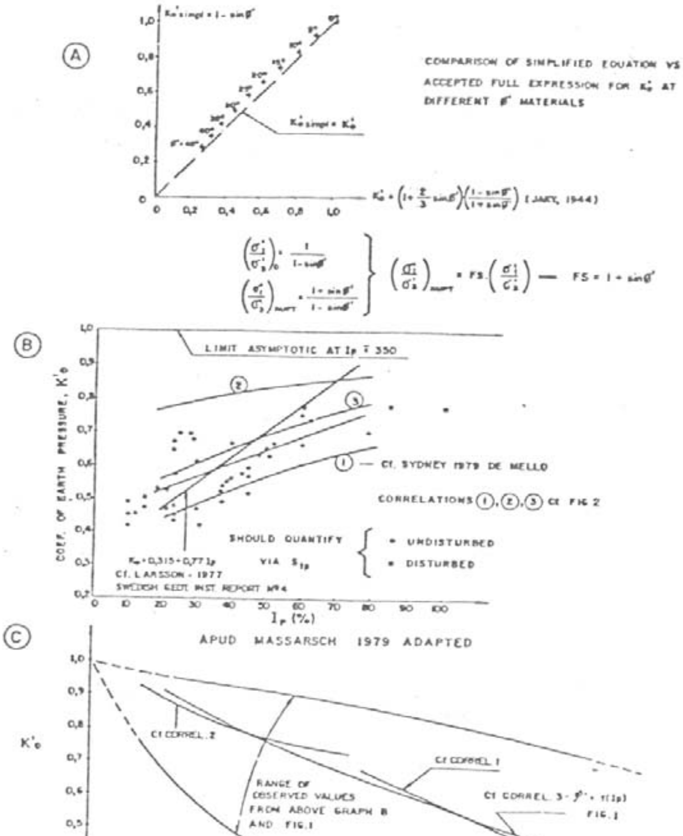


FIG. 4 NORM. CONS. K'_o POSSIBLE FOR DIFFERENT Ip CLAYS

Firstly we should like to substitute the --- dichotomy disturbed-undisturbed by values of partial Sensitivities S_{tp} and, if possible, adjust for varying qualities of sampling-testing by some form of extrapolation to what --- could possibly be the intact soil element behavior. (Fig. 14)

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"
NOT "END PRODUCT SPECS"

IN STATE-OF-THE-ART ASSESSMENTS, ALL SAMPLES OF OVER 30 YRS. ETC. LUMPED ACCORDING TO DESIGNATION "UNDISTURBED"

αC_u

EX. FOR α VALUES FOR PILES C_u IN 1948 = $R_{c/2}$

MODERN s_{VANE} etc. ?

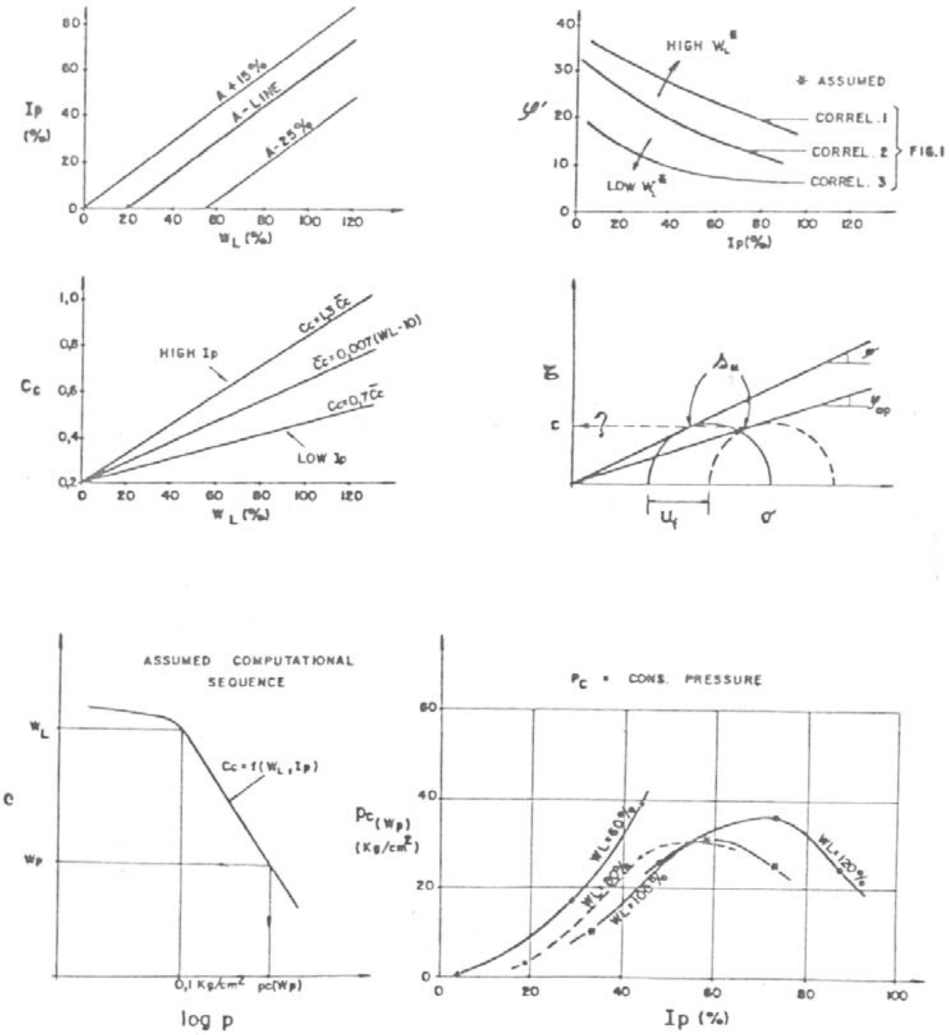


FIG 5 ATTEMPTED JUDICIOUS INSERTION OF SIMULTANEOUS (W_L , pc) FOR DERIVATION OF EQUIVALENT CONS. PRESSURE FOR W_P.

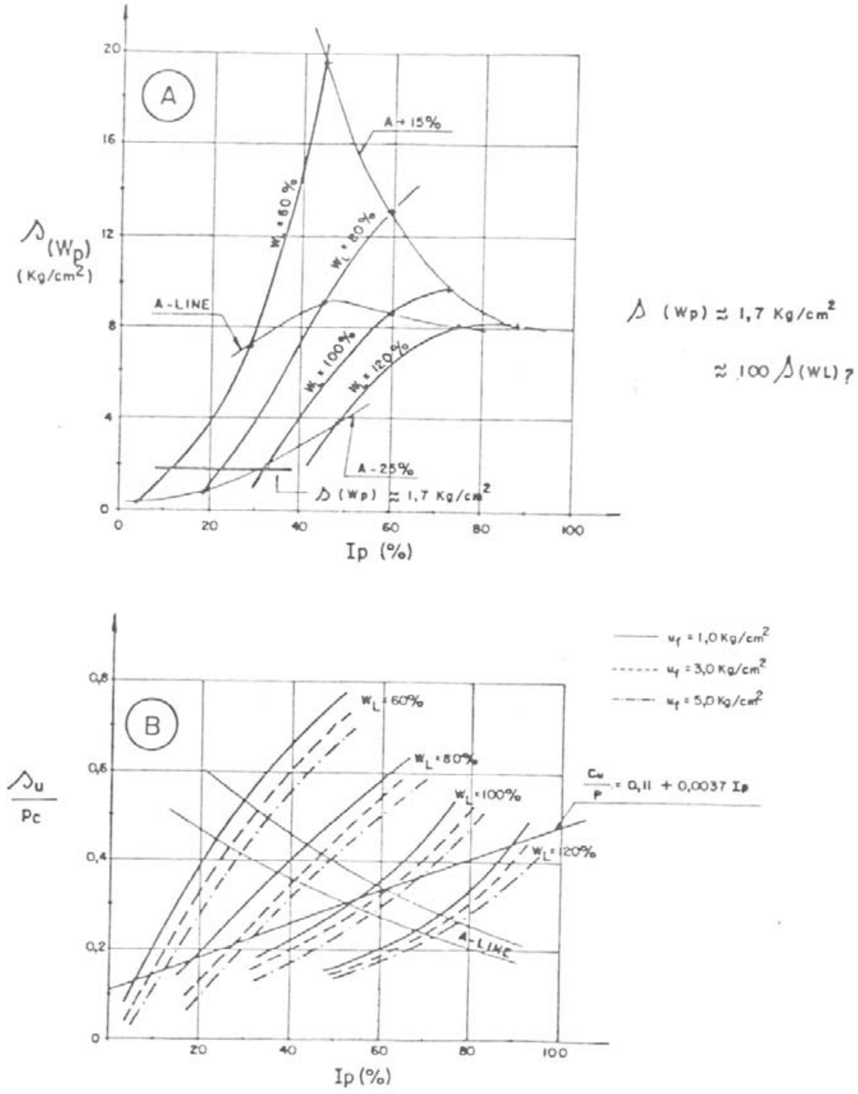


FIG. 6 ATTEMPTED DERIVATION OF $\rho u_{(W_p)} = f(W_L, Ip, u_f)$ AND RANGES DEMONSTRATABLY LIKELY

The importance of intact soil element behavior at small strains is presently meriting increasing attention because of the recognized frustrations with predictions of small deformations. It must be recognized, moreover, that in our use of "experience" from past projects, we should make every effort to adjust quoted parameters from sampling-testing qualities of the different periods and regions. There has been a systematic and relentless effort to improve undisturbed sampling and testing: thus, one of the unacceptable errors of judgement is to presume that the quoted strength and deformability values of a given (e.g. London) clay project in 1952 may be lumped together in the same statistical universe with those of an adjacent project in 1982. In unloading (or active pressure) conditions we may be on the conservative side by using job conclusions of the 1950's and 1960's, but quite on the contrary in loading (or passive pressure): in the latter the association of behaviors with erroneously low strengths and high deformabilities may presently promote a cycle of unsuccessful designs.

At any rate, there have been repeated indications that we should not blindly accept $\emptyset'$ und $\simeq \emptyset'$rem, and most other factors significantly affected by shearing compressibility (u_f, etc.) are well recognized to be markedly different in the undisturbed and remolded states. Thereupon, should we not find it most strange that an in-situ undisturbed (at rest) parameter be correlated with a strictly empirical remolded index (Ip) and further be postulated as unaffected by the radical differentiation "undisturbed vs. remolded" (even in very sensitive clays)? (Fig. 4B)

The question is not academic, but of utmost importance: quoting Wroth, 1975, State-of-the art report "In situ measurement of initial stresses and deformation characteristics" ASCE Conference, "attention is focussed on the uncertainty of any laboratory measurement of K'_o (the coefficient of earth pressure at rest) and the difficulty of making accurate measurements in the field". However, let us meekly apply ourselves merely to remolded clays.

There are, by now, many suggested analytical solutions as well as strictly empirical equations deduced, some of the deductions employing also a free mixing of analytical equations and current single-parameter correlations. The additional suggestion herein offered (Fig. 4B) would attempt to show that the same data quoted would continue to plot very satisfactorily with reference to non-linear regressions believed to be more atuned to theoretical trends. We begin by adopting exponential exhaustion relationships for $\emptyset'$ vs Ip as is intuitively accepted (Fig. 2) and corroborated experimentally, notwithstanding the comprehensible broad scatters. We further attempt not to transgress the evidences of the extreme values of normally-consolidated K'_o approximately corresponding to $\emptyset' \simeq 30°$ for Ip $\simeq$ 5 and $\emptyset' \simeq 5°$ for Ip $\simeq$ 350 (sodium-bentonite), as well as the asymptotic trend $K'_o \rightarrow 1,0$ as $\emptyset' \rightarrow 0°$. The basic thought is that we should not sacrifice the intrinsic recognition of K'_o (normally-consolidated, at rest, presumed respecting elasticity conditions) as generated as a function of shear stress, and thus embodying a factor of safety with respect to shear strength limits.

Thus in Fig. 3, 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 $K'_{o(nc)}$ expression with the Mohr-Coulomb failure criterion suggests that K'_o conditions prevail at a factor of safety of FS = 1 + sin $\emptyset'$. Thus the variation of K'_o(nc) with Ip should be correlated with that of $\emptyset' = f(Ip)$. Incidentally, it seems reasonably that K'_o conditions be assumed to prevail up to a FS = 1,5 for a material of $\emptyset' = 30°$ since it is frequent in such materials to observe a linear stress-strain behavior up to 2/3 of the peak deviator stress: however, for materials of low $\emptyset'$ there would be a disconcerting conclusion of "at rest" behavior up to much lower factors of safety. (Figs. 3A, 3B y 4A)

We might play a little further along the same line with regard to $K'_{o(ocr)}$ values under different OCR conditions. In an over-consolidated clay, if the complete strength envelope is assumed, including the stretch with cohesion, and if we arbitrarily maintain constant the FS ratio of at-rest "elastic" to failure stress envelopes, we could determine trigonometrically the band of $K'_{o(ocr)}$ stress ratios possible through much of the overconsolidated range. Will research aim at cross-examining such working hypotheses? Once again, to compare different clay soils, we would reapply similar reasonings to the varying $\emptyset'$ values as function of Ip. Obviously other parameters and reasonings will interfere as more dominant. But, how can we rest satisfied without testing out the method in our madness? (Figs. 3c, 4c).

c. UNDRAINED SHEAR STRENGTH, COHESION.

One of the index parameters of great interest to early soil mechanics was the cohesion of clays. Highly clayey materials were automatically associated with high cohesion: cohesion was roughly obtained as one-half the unconfined compression strength. Then came the UU (or Q) and CU (or R) triaxial tests, to recover some of the cohesion that was recognized as inexorably lost in sampling and testing, by (a) release of total stress (b) release of (pre)consolidation pressure.

Inevitably came the advances of triaxial shear research, associating undrained shear strength directly with (pre)consolidation. Meanwhile a strictly empirical "correlation" was proposed, and often repeated thereafter, for Cu vs. Pc, the single parameter correla--

tion having been associated with Ip. Clays were automatically related to plasticity, therefore the quantification of clayeyness should be reflected by the plasticity index (indicator of plasticity). Curiously the equations are such (cf. Fig. 6B) that the higher the Ip the higher should be the cohesion for a given consolidation pressure.

Many a geotechnician has dedicated some questioning to the trend, that by conventional theorization would seem directed opposite to the anticipated trend: among other, Bjerrum and Simons, 1960 Boulder Shear Research Conference, must be cited. The anticipated trends according to conventional theorization are reflected in Figs. 2, 5 and 6. What is the explanations for the discrepancy?

The first suspicions and questions would be with regard to the test values of Cu and pc used, especially if they arose from would-be "undisturbed samples". The question lies dormant although the empirical correlation finds frequent use. For a given value of W_L there is a wide range of Ip values possible (Plasticity chart). The simplistic derivations shown in Figs. 5 and 6 are meant to show the importance of investigating regressions of su/Pc vs. the pair of plasticity parameters (W_L, Ip) The derivations assume that we might intuitively attribute trends for the probable interference of the second parameter, not hitherto included in the currently quoted correlations, C_c=f (W_L) and Ø'=F (Ip).

In graph Fig. 6A we would conclude that around the A-line the undrained strength su at Wp would vary around 8Kg/cm2 (apparently too high according to generalized feel of experience), and that low values such as Su (Wp) ≐ 1.7 Kg/cm2 (Wroth and Wood 1978) could only be compatible with clays of very low plasticity, well below the A-line. Meanwhile, in Fig 6B there would be but a small range of coincidence of Su/pc with Cu/pc, with A-line clays around (W_L, Ip) of about (100, 60); for most of the viable combinations of (W_L, Ip) there would be a very significant difference between Cu/pc and the simplified idealized values of Cu/pc.

Why are clays above the A-line "fat" and "tough" clays? The suspicion is that the reason why the presumed theoretical trend results inverted may lie in the fact that Cu is more influenced by "internal porewater tensions" that we imagine from our physical model. Besides the capillary tension (negative pore pressure) there might be an interference of clay mineralogical intercolloidal attractions and repulsions in helping retain the compression energy. Possibly a measure of such trends could be insinuated by the hysteresis loop between each material's C_c and C_e. As we presume, the area of such a hysteresis does not increase steadily in the direction of increasing C_c (therefore W_L and/or Ip), but seems to exhibit a dish-shape, going through a minimum with moderately clayey-silty conditions.

How long will it take to investigate and clarify such questions?

d) Proposed simplified-unified theory for plasticity indices.

Concepts pertaining to Critical State Soil Mechanics have been used to propose a basis of theorization for the significance of the liquid and plastic limit water contents of (remolded) clays (e.g. Wroth and Wood 1978). Indeed, since the Atterberg limit tests have generally been considered crude empirical index tests, it does capture the imagination to find the two absolutely independent values roughly associatable by a unified theory. One thus finds the proposal that "the index properties (be) logically redefined simply and directly in terms of the undrained strength of the soil", and that "the rationale for redefining the plastic limit as that water content that gives a 100- fold increase in shear strength over that at the liquid limit..........soon be adopted".

How wonderful that the intuitions of so long ago, Atterberg 1911, should find support in sheer logic of shearing strength, of as modern a theoretical model as the Cam-clay critical state theorization Yet, for purposes of everyday engineering, are we advancing practice by proposing the supremacy of a single-parameter logic as a substitute for the "classification" tests of plasticity?

If we examine more carefully, we find that the logical derivations depend heavily on assumed simplifications and average condition (N.B. the A-line was initially a proposed average relationship of Ip=f (W_L), cf. de Mello, Sydney 1979), and also on the desire of a unified-behavior theory. Is the undrained shear strength s_{WL} of clays really "constant"? Definitely not (Youssef et al, 1965, Wroth and Wood 1978); variation from 25 to 13 g/cm2 for 30<W_L< 180% is very small in resistances, but not so in proportions thereof; and it is presumably consistent. Are the shear strengths measured at W_L and W_P really nothing but the conventional undrained shear strengths at different ϵ and Pc values Does not a silty clay suffer from some dynamic effects in the liquid limit "slope instability problem"? Would not a sodium-bentonite reasonably evidence an opposite effect of higher "impact" shear strength?

Many such questions may be raised before the geotechnical professionals could feel confident that a reasonably full range of conditions has been covered by the elegant young theory, so as to decide to pass the baton in the relay-race of competing theories.

But, the main point I could raise regards -- specifically the intent. Do we not recog -- nize that the "plasticity personality" (even remolded) of clayey soils is represented by- a wide range and number of taxa? Is not --- identification and classification an intent- to make salient the differentiated taxa? Is not the demonstration that a single mathema- tical simplified relationship could depict - "all clays" a desire diametrically opposite- to that of identification and classification of differences?

Quite definitely we should want to improve - test techniques to decrease erratic errors;- but not to suppress consistent differences,- however small! My quest and complaint (cf.- Sydney 1979) about the Plasticity Chart as a photograph of differentiable soils is that - the graph is badly conceived, because it com presses all soils into too tight a frame.

Once again, we cannot but emphasize how much room there is for work and development, even in so basic a problem.

4.3. Pseudo-statistical correlations, and en gineering needs.

The place of CORRELATIONS is very important- in engineering as a sequel to the use of --- PRESCRIPTIONS for working solutions. PRES - CRIPTIONS provide broad UMBRELLA SOLUTIONS,- on the conservative side, so that we can -- exercise engineering decision and action by- guaranteeing that the solution is better --- than the "minimum necessary": thereby I have emphasized that in Civil and Geotechnical En gineering, experience is predominantly accu- mulated from the "silent majority of cases"- that do not cater to any publication paper - at all; thus we need not be too disheartened inability to predict what will or should hap pen, because it is generally sufficient to - predict what will not happen. However, eco- nomy in Civil Engineering, and especially in Geotechnical Engineering, is of crucial im - portance to Society and its cost of living:- what matters most is first costs, and buried first costs, that act as the first insurance premium on everything thereon and thencefor- th supported.

Thus arises the importance of CORRELATIONS:- correlations should help us get closer to -- the limits of impunity, by improving our abi lity to predict what will probably happen.

Obviously correlations have to be statisti - cal. Soil mechanics and soil engineering ha- ve gradually and very slowly risen to such - recognition. But are we deriving and employ ing statistical correlations in a satisfying manner?

The most general answer is a resounding NO.- Aplications hitherto fail to satisfy either- the men of experience who are frequently --- able to estimate "prior probabilities" (Baye sian) and also "posterior probabilities" -- (the experienced Observational Method) of -- significant parameters and results within -- narrower bands of uncertainties than the pu- blications and "data" suggest; they also -- fail to satisfy the practicing geotechnical- engineer who would be at a loss to have to - decide on projects of responsibility under - such broad dispersions.

Recent publications teem with statistical re gression equations and graphs such as the -- ones selected at random for reproduction in- Figs 7,8,9 just to illustrate a few points of discussion. The following four points may be emphasized on most of these "single-parame - ter regressions at random".

a. In many a case the dispersions are much - greater due to the test data than would --- occur in reality. "Natura non facit saltus" Nature's erraticities generally are not radi cal, they tend to follow moderately smooth - trends of variation. (Incidentally, however when geology does present an abrupt disconti nuity, it is not random, not a dispersion, - but a definite effect of a deterministic --- cause - even if we may not have suspected or known it). On the other hand, because of -- the very small scale of most geotechnical -- tests, and because of the destructive abili- ty of men and machines, tests tend to suffer and reflect variations more erratic than fi- nally observed in prototypes.

As an illustration of such experience one -- might refer to data reproduced from outstand ing publications, and quite representative - of dispersions of behaviors of footings on - sands (Fig. 10) and/or of parameters of bor- ed and driven piles in thoroughly investigat ed conditions (Figs. 11-12). Dispersions -- appear disheartening. However, the silent ma jority of successful foundations designed un der much less meticulous studies would not - confirm the probabilities of significant di- fferential settlements.

b. An impressive number of publications fur- nish the regression equations for the corre- lation merely between the average values of- X vs. Y. The least that could and should be done as a complement is to furnish the % con fidence bands astride the average. A PRES - CRIPTION can only be interpreted to be an up per or lower bound recommendation, conserva- tive: therefore, if we wish to substitute a- CORRELATION for a PRESCRIPTION, in fairness- we must use an equation of an upper or lower bound percent-confidence-band.

Moreover, it is important to distinguish in- concept between such confidence bands around averages, as compared with those on single - events. For an engineer building 1000 popu- lar houses for subsequent sale, it may be -- quite appropriate to work with confidence -- bands on averages: alas, however, for the en gineer building a single house for a speci - fic client it would be rather unfair to dis- cuss anything but probabilities of a single- event.

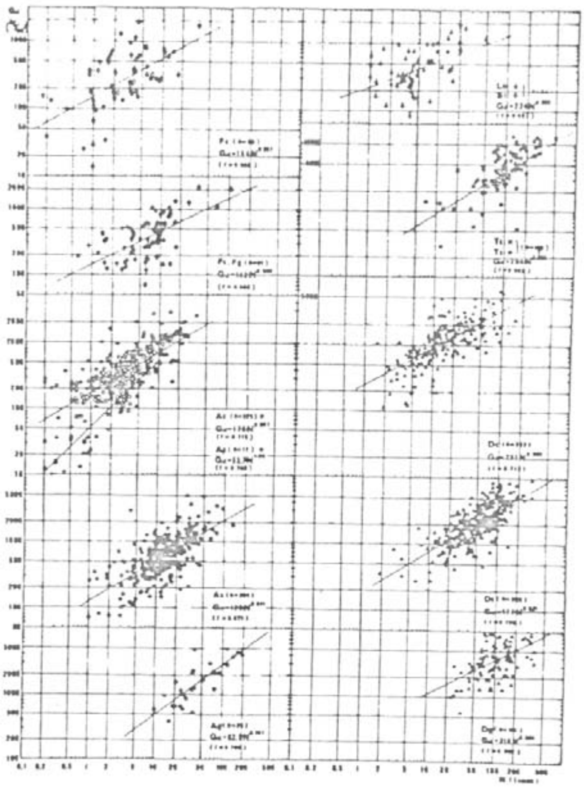


FIG. 7 - STATISTICAL CORRELATIONS OF SHEAR MODULUS Vs SPT

(DIFFERENT SOILS)

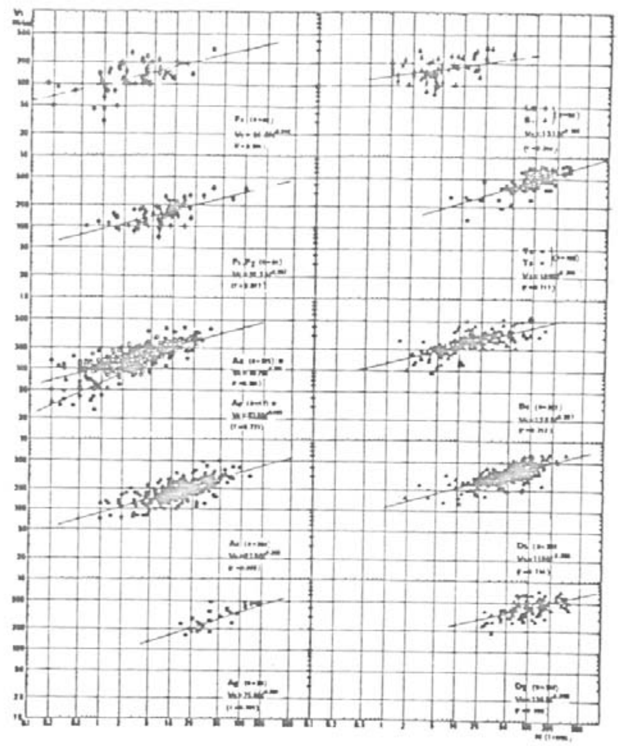


FIG. 8 - STATISTICAL CORRELATIONS OF S-WAVE VELOCITIES Vs. SPT

(DIFFERENT SOILS)

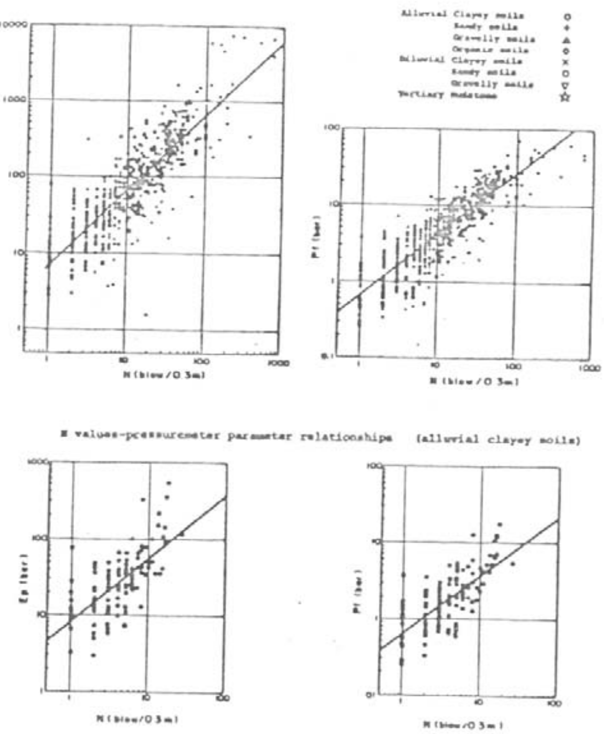


FIG. 9 - CORRELATIONS OF SPT Vs. PRESSUREMETER PARAMETERS

c. Because of many factors, including the above, it is comprehensible that in the hope of improving correlations, a number of especially dedicated workers have turned to collecting vast numbers of data. If the statistical universe were assuredly the same, the considerable increase in data would help, but principally with regard to averages and confidence bands around averages. But it is utopian to expect statistical universes based on but a pair of parameters not to embody additional significant parameters that from site to site would make the universe so different as to detract from any meaningful correlation. For instance, if we try to correlate CPT or SPT results vs. plate load tests, we should tend to find that the interference of precompressions (OCR and varying K'_0 etc.) from site to site would add to the scatter of individual points around the mean regression rather than decrease it.

d. Finally, it must be noted that a site-specific working correlation inevitably tends to become spurious when transplanted to other sites because of the impossibility of inserting adjustments to compensate for the many other parameters of relative significance that are not explicited. Therein lies the most frequent source of error and frustration in present geotechnical engineering. If an author has demonstrated that a reasonable correlation $X = f(Y)$ has been found in some sites and soils, other geotechnicians might well profit from the indication of the type of correlation offered (if justifiable), but should not proceed to use the specific equation (etc.) without some attempt(s) to insert adjustment factors, hopefully reasonable. Unfortunately the more earnest the geotechnician, the more he stands likely to be instrumental to the zealous importation of unadapted and unadaptable equations.

Need one comment on the seduction of log-log plots for linearizing regressions and for disguising the true widths of dispersions? (Figs. 7,8 y 9).

5. APPRAISAL OF SOME "PRACTICAL ENGINEERING SOLUTIONS"

Since geotechnical engineering is our main concern, in assessing the status achieved and the real need for candid revisions, I am going to limit myself to but a few examples of dominant dicta in current practice.

5.1. Clay-core dams, PLASTICITY OF CORE

It has been widely recognized and emphasized that one of the great concerns of high earth-rock dams lies in the possibility of transverse cracking of the core due to differential settlements, distortions. Although but scattered references signify that the only concern is with tensile cracking (that can only occur near the top) since shear-plane displacements tend to make the plane more impervious and not the opposite, let us accept the problem as known. The standard qualitative requirement to obviate the problem is a "plastic core". Herein lies an important example of some of the confusions to be expurgated, generated by mere irrational word associations when a word is vaguely defined. (Fig. 10)

What is really desired is the "plastic behavior" under low confining stresses, that 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 former that open fissuring tends to occur.

EXAMPLE OF IRRATIONAL WORD ASSOCIATIONS

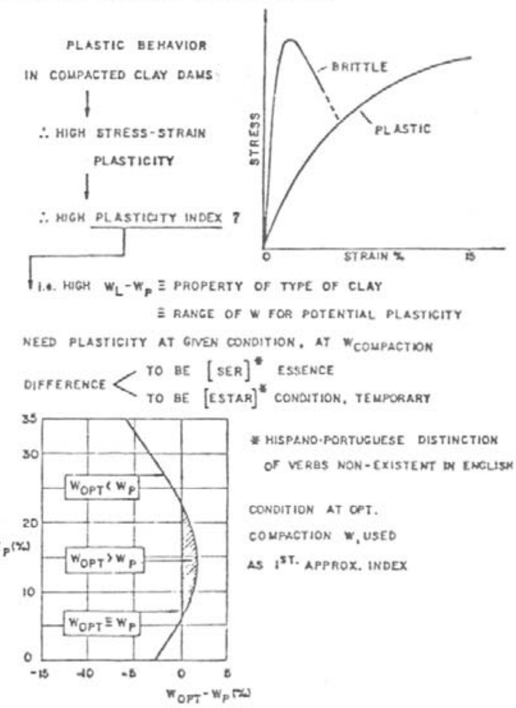


FIG.10 - PLASTICITY OF COMPACTED CORE Vs. CRACKING

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 Ip have to be compacted at water contents below the plastic limit Wp because of problems inherent to compaction. In my experience (de Mello, ICOLD, Madrid 1973) it is only for intermediate Ip values (approx $7 < Ip < 22\%$) that the Proctor compaction water content happens to be wetter than Wp. There is, at any rate, no logic a word association between plasticity index and plastic stress-strain behavior at the water content as compacted. (Fig. 10)

5.2. Footing foundations, sands

Although settlements in sands are generally recognized to be far smaller than in clayey materials, the recognition has spread that in shallow foundations (footings, rafts) on sands it is the problem of differential settlements that governs design. Sands are frequently associated with more turbulent, therefore more erratic, depositions, and therefore differential settlements do not differ much from maximum total settlements.

Foundation engineering has long struggled with the two steps of the problem: a. to correlate index tests with "model footings", plate load tests; b. to establish methods of extrapolating from plates to full-size footings.

Fig. 11 (apud D'Appolonia et al., 1968, 1970) summarizes the state-of-the-art that can be claimed as about the best offered by soil mechanics to the practising professional. Can one be satisfied with such broad ranges of dispersions?

We know that a preloaded sand practically does not rebound: therefore preloading should have a very noticeable effect in reducing settlement and differential settlement. Obviously, however, the minute incremental densities of the dx. dy. dz. soil element do not cater to noticeable influences on resistance. Any wonder, therefore, that a resistance index cannot easily reflect improvements in incompressibility? If two influential unknowns (initial packing reflected in friction, plus preloading OCR) are at play, can we hope to solve two unknowns with a single equation?

Should we not try to improve the means for design predictions via differential profiling, so as to employ more simultaneous equations, of higher sensitivity, to solve for the necessary unknowns?

5.3. Pile foundations

The problems of dispersions in design predictions are not lighter in the case of many a pile foundation. As an example, Figs. 12 and 13 are reproduced from a magnificently documented CIRIA report on piles in chalk.

Obviously once again a significant part of the dispersion belongs to the tests; and, to some extent, in prototype foundation behavior group averaging and "factors of safety" account for the plentiful cases of success.

5.4. Soft-ground Tunneling

Engineering progress may be typified by the statement: "we do, then we begin to explain and understand, and gradually we can and must quantify".

As regards tunneling design there were some remarkable simplifications of earlier times which should have been recognized but were clouded, and thereupon one could state that an intermediate step was temporarily thwarted and, as often happens, the physical perceptions, categorized and simplified, were clouded by the very fact that for some time a pseudotheoretical prescription diverted attention.

The problem were "cohesion" under lateral stress release, seepage, and "stand-up time". Strangely the emphasis of soil mechanics theorization, related to soft saturated clays under "quick" (undrained) loading (c. 1942-'60) dominated the picture so heavily that we could almost claim that for practical tunnel engineering (Peck 1969, almost to-date) it quite forgot the really dominant factors of stress release, seepage, stand-up time.

Fig. 15 presents schematically in the form of hypothetical subsoil profiles the parameters of cognizance recognized in the two arbitrarily quoted periods (c. 1946 and c. 1969) that represent reference milestones. In comparison a present-day profile, shown side-by-side, would emphasize many obvious fundamental parameters of need. Foremost among developments of the past twenty years (post Boulder Shear Research Conference 1960 etc...) have been the emphasis on effective stress analyses and pore pressures (flownet u plus Δu due to shearing ΔV), appropriate stress-path testing, recognition of the importance of pore-air (S%), recognition of the range of variation and importance of K'_0, and, finally, at the crest and in the wake of the computational wave, the "elasticity" parameters (E, μ), and so on.

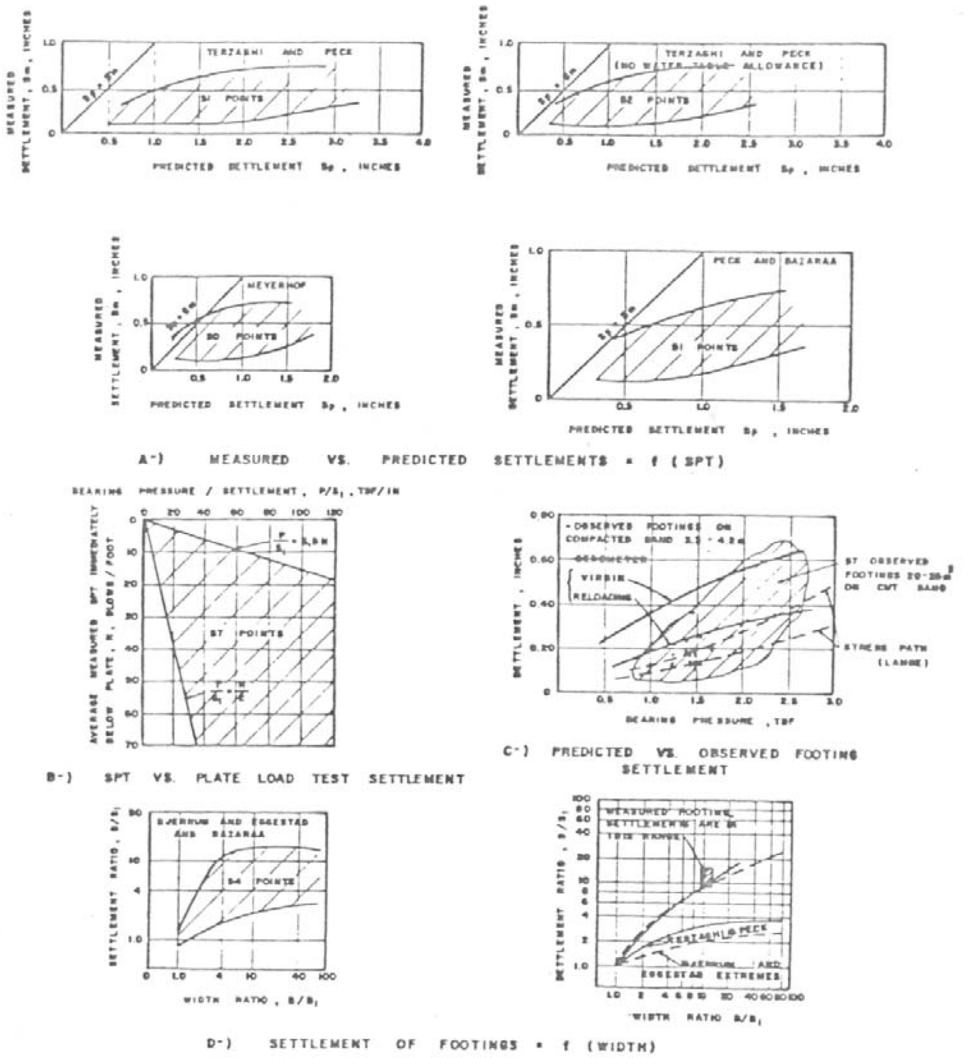


FIG. 11 DISPERSIONS OF PREDICTIONS, FOOTINGS ON SANDS

(APUD D. D'APPOLONIA ET AL., JOURNAL ASCE SMFD VOL 94 SM3 1968, VOL 96 SM2 1970)

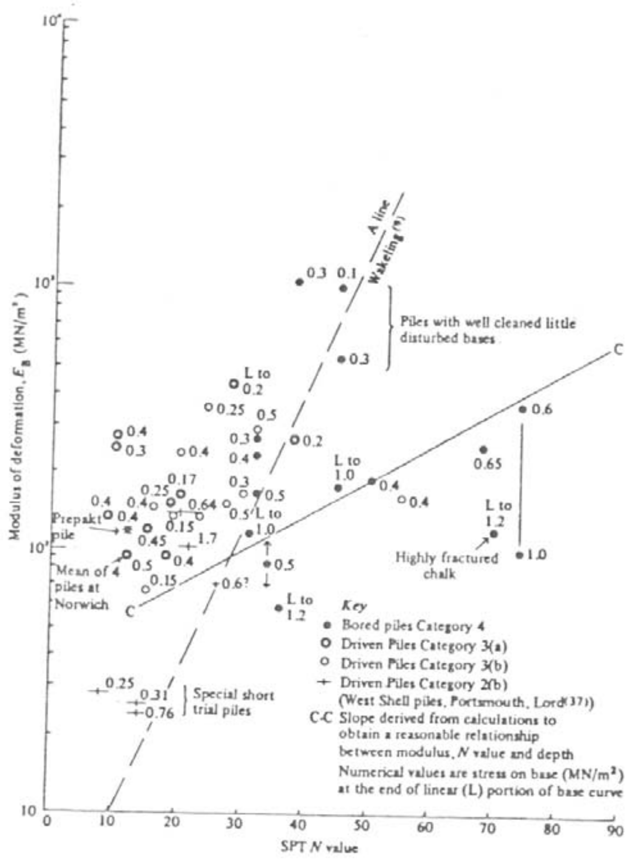


FIG. 12 - PLOT OF BASE MODULUS OF PILES AGAINST SPT N VALUE AT THE BASE (FOR THE STRESSES SHOWN IN MN/m²)

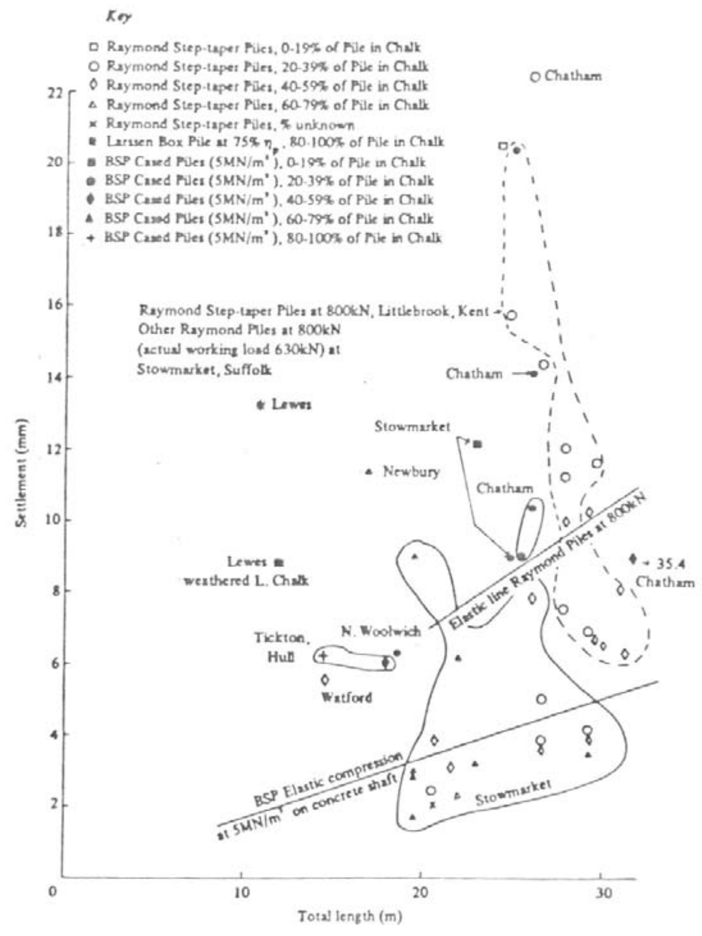


FIG. 13 - PERFORMANCE OF LARGE DISPLACEMENT STEEL PILES

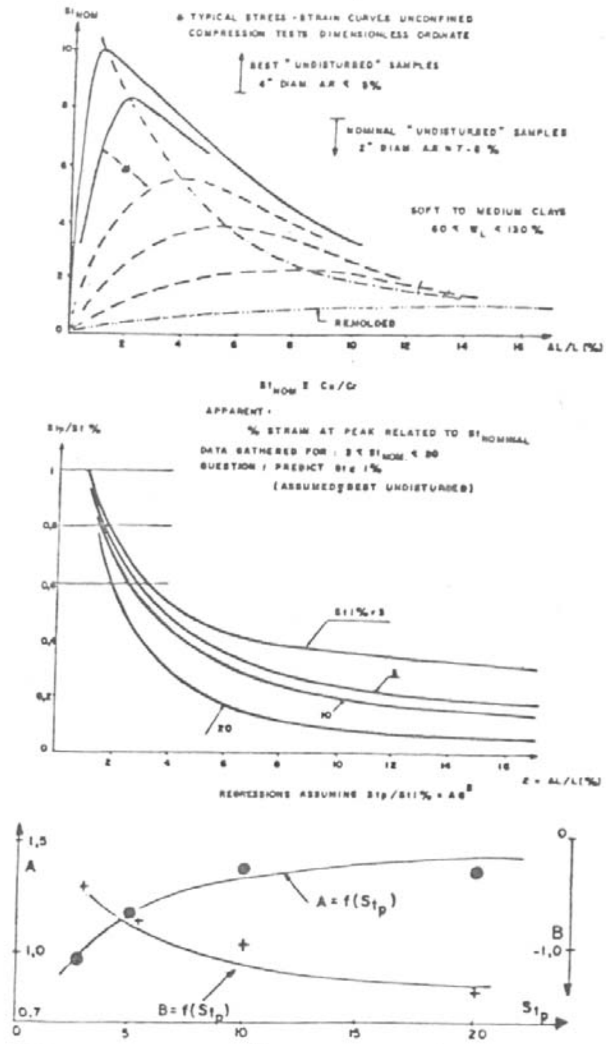


FIG.14 QUALITY OF "UNDISTURBED" SAMPLING REFLECTED IN PARTIAL St

HISTORIC FROM TUNNELING PRACTICE (apud Peck 1969; Terzaghi 1946)

SQUEEZING SOILS	VERY SOFT TO MEDIUM CLAYS	W.L. NOT OF CON- CERN (?)
FIRM GROUND	STIFF CLAYS. CEMENTED OR COHESIVE GRANULAR.	
RUNNING GROUND	PERFECTLY COHESIONLESS; DRY SAND; CLEAN LOOSE GRAVEL	
RAVELING GROUND	SLIGHTLY COHESIVE SANDS, FINE SANDS, SILTS (APPARENT COHESION); SAPROLITES.	
FLOWING GROUND	RAVELING OR RUNNING GROUND WITH $\boxed{\text{SEEPAGE PRESSURES}}$	

STAND - UP TIME ?

TRADITIONAL SOIL MECHANICS. STATE-OF-THE-ART (PECK 1969)

SENSITIVE CLAY (INCL.LEDA) NORMALLY LOADED	(cf. TABLE I, Ref.) $C = \frac{1}{2}$ qu (LOADING) $\Delta\sigma_1$
PLASTIC GLACIAL CLAY	(MODERATELY UNDISTURBED SPECIMENS) $\phi_{UU} = 0^o$
FISSURED PLASTIC CLAY (LONDON, BOOM)	SATURATED $\left.\begin{matrix} W.L. \\ U \\ K'_o \end{matrix}\right\}$?

<u>STABILITY IN MOST SOILS</u> ....

(P.228) "WITH THE EXCEPTION OF PLASTIC CLAY SOILS UNDER UNDRAINED CONDITIONS, THEORETICAL ATTEMPTS TO ESTIMATE THE FACTOR OF SAFETY ... OF A HEADING HAVE NOT YET BEEN SUCCESSFUL." .....

"DETAILS OF STRATIGRAPHY AND .... SECONDARY STRUCTURE OF THE SOIL DEPOSIT... SINCE THESE DETAILS ARE UNPREDICTABLE, <u>NO SATISFACTORY CORRELATION</u> BETWEEN F.S. AND MEASURED SOIL PROPERTIES CAN BE ANTICIPATED".

STAND - UP TIME ?

PRESENT MULTIPLE PROFILING ON GEOTECHNICAL DATA

SPT → estimated s

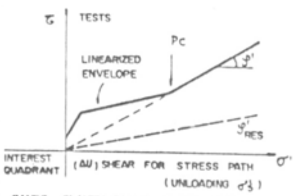
CPT, CPT_u → estimated s, E, C_v

UNDISTURBED → PERFECT → INTACT ELEMENT

<u>CLAYS</u>

S1 $\quad p_c$, C_c, C_v; OCR; K'_o (±)

W.L. + U SEEPAGE



<u>SANDS, CLAYEY SANDS, SILTS, ETC.</u>

S1

W.L. + U SEEPAGE

ESTIMATED $\left|\begin{matrix} U_c\text{: E: } \varphi', C'; \\ p_c\text{: } K'_o \end{matrix}\right.$

STAND - UP TIME ?

FIG.15 - IDEALIZED SUBSOIL PROFILE AND PARAMETERS ESTIMATED FOR FACE STABILITY

It has been contended repeatedly that once a theoretical reasoning establishes the backbone for a certain analysis-synthesis, the engineering method requires that we use that backbone for filling in the muscle and the trappings of experience. We cannot condone with Indices (either oversimplified, or complex-lumped-parameter) that do not fit into theorization, even if they may have been used as temporary struts. The fact that data (more specific or precise) are not available along the proposed line, does not excuse us from assuming the desired and necessary parameters: it only serves to expose the range of significance of our unknowns, and therefore, the technical and economic interest in seeking them. Meanwhile the engineer must, and can assume parameters as required, and can and must use approximations (often culled indirectly) for his working hypotheses.

In the three columns of Fig. 15, what stands out is our total neglect to-date of tests for design evaluation of "STAND-UP TIME".

Merely for the purpose of elucidating the above rationale as engineering technique, two crucial design questions of soft-ground shield tunneling in urban development may be listed.

a. Face stability

It is doubtless one of the most serious problems. In advancing a tunnel excavation we face a temporary condition of different degrees of proximity to provoking a failure at face and/or roof. Moreover it is particularly critical because of always advancing into the unknown and facing non-averageable localized conditions.

The "stability" involved has been associated almost exclusively with a "cohesion" value (historically and still generally deduced from unconfined compression tests, in the case of plastic saturated clays in which it is presumed that the UU or Q strength envelope is $s = c \simeq 0.5\ q_u$). Routinely one is led (Peck, 1969) to look for a Stability Number (Broms and Bennermark, 1967).

$$\frac{\gamma z - p_a}{s_u} > 5 \text{ or } 6$$

γz = total vertical pressure at depth z of center of tunnel p_a = air pressure above atmospheric s_u = undrained shear strength of clay.

The Broms and Bennermark (1967) paper, which follows closely the Bjerrum and Eide (1956) paper, clearly represents a significant contribution for its time and for the very specific idealized problem evisaged. It concerned saturated plastic clays ($s = c$; $\emptyset = 0$ undrained), normally consolidated (overburden total σ_v as the principal driving stress), and clearly demonstrated the association of the face stability with a bearing capacity formulation, cN_c. In subsequent discussions herein we shall limit ourselves to simple bidimensional conditions in order to elucidate comparative conditions at play. In the same way as is generally done in bearing capacity formulations, the circular face stability can be estimated from bidimensional formulations by use of adjustment factors and shape factors (often extracted from analogous situations).

The Broms and Bennermark tests were literally extrusion tests. There is the (conservative) assumption that failure caused by increasing σ_v would preserve the same maximum-deviator stress (function of q_u and cohesion) as failure caused by decrease of σ_h: the decrease of internal σ_h was simulated by an increase of σ_v external. This assumption is idealized, because in practice there is a tendency to compress and generate positive pore pressures in the first case, whereas in the second, any tendency to expansion at the face would inmediately create capillary tensions. There is a significant question regarding the method used to simulate confining pressure: "Confining pressure was used to investigate the effect of compressed air to prevent a cohesive material from flowing into an excavation or tunnel. Glycerin was used as a confining fluid".

The important influences of capillary tension and of differentiated interstitial pore fluids and liquid-liquid surface tensions had merited some attention in the early 1950's. Unfortunately, however, they are generally eliminated in idealized laboratory conditions, and/or often overlooked. Some representative data are summarized in Fig. 16 just as a reminder. The special importance of compressed air at a tunnel face cannot be dissociated from some capillary minisci, and the fact that soils generally are not fully-saturated. Depending on the magnitude of the air pressure, in fact there can be a favourable reversal of flow direction, and consequent favourable seepage pressures to complement the favourably propagating capillary tensions.

In the submerged saturated clayey sands of Sao Paulo, laboratory tests indicated that although under very small gradients (about 0.2) practically no change of moisture content W% was caused (about 0.2%), under much-higher gradients (up to 30) decreases ΔW up to 6% were achieved in less than 1 hour. The graphs of variation of unconfined compression strengths with W% are given in Fig. 17A,B As is well recognized, complete drying is unfavourable. But the benefits of somewhat higher air pressure (and local gradients at critical points) are so evident, that it need hardly be emphasized that there is direct and simple and beneficial cure for face drying of a sand: one need but spray the face with moisture, preferably muddy (dirty) water.

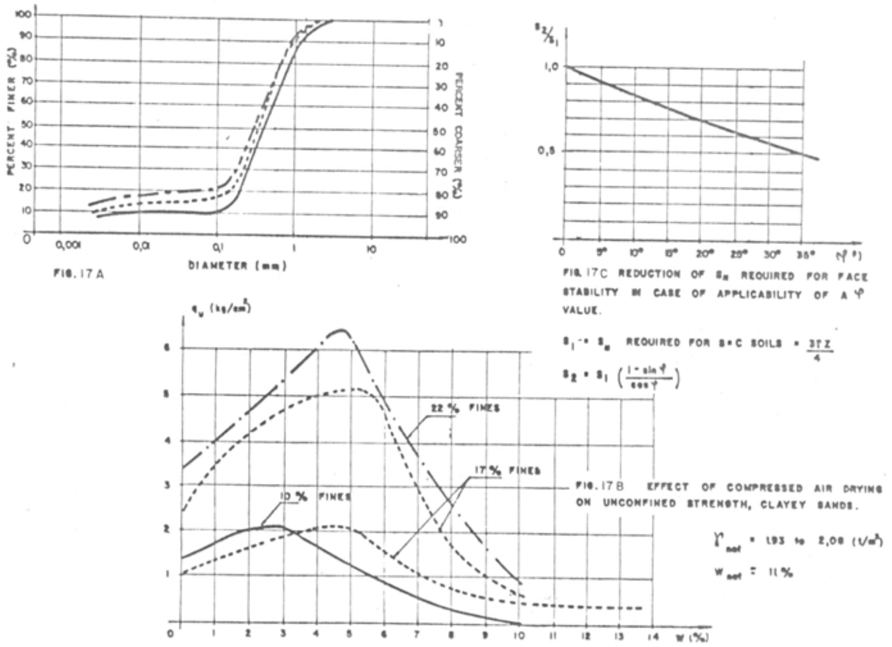


FIG. 17 DATA ON UNCONFINED COMPRESSION OF CLAYEY SANDS, SAO PAULO. (FIG.17A, 17B)
INFLUENCE OF φ ON FACE STABILTY (17C)

It has been contended repeatedly that once a theoretical reasoning establishes the back-bone for a certain analysis-synthesis, the engineering method requires that we use that backbone for filling in the muscle and the trappings of experience. We cannot condone with Indices (either oversimplified, or complex-lumped-parameter) that do not fit into theorization, even if they may have been used as temporary struts. The fact that data (more specific or precise) are not available along the proposed line, does not excuse us from assuming the desired and necessary parameters: it only serves to expose the range of significance of our unknowns, and therefore, the technical and economic interest in seeking them. Meanwhile the engineer must, and can assume parameters as required, and can and must use approximations (often culled indirectly) for his working hypotheses.

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The Broms and Bennermark (1967) paper, which follows closely the Bjerrum and Eide (1956) paper, clearly represents a significant contribution for its time and for the very specific idealized problem evisaged. It concerned saturated plastic clays ($s = c$; $\emptyset = 0$ undrained), normally consolidated (overburden total σ_v as the principal driving stress), and clearly demonstrated the association of the face stability with a bearing capacity formulation, cN_c. In subsequent discussions herein we shall limit ourselves to simple bidimensional conditions in order to elucidate comparative conditions at play. In the same way as is generally done in bearing capacity formulations, the circular face stability can be estimated from bidimensional formulations by use of adjustment factors and shape factors (often extracted from analogous situations).

The Broms and Bennermark tests were literally extrusion tests. There is the (conservative) assumption that failure caused by increasing σ_v would preserve the same maximum-deviator stress (function of q_u and cohesion) as failure caused by decrease of σ_h: the decrease of internal σ_h was simulated by an increase of σ_v external. This assumption is idealized, because in practice there is a tendency to compress and generate positive pore pressures in the first case, whereas in the second, any tendency to expansion at the face would inmediately create capillary tensions. There is a significant question regarding the method used to simulate confining pressure: "Confining pressure was used to investigate the effect of compressed air to prevent a cohesive material from flowing into an excavation or tunnel. Glycerin was used as a confining fluid".

The important influences of capillary tension and of differentiated interstitial pore fluids and liquid-liquid surface tensions had merited some attention in the early 1950's. Unfortunately, however, they are generally eliminated in idealized laboratory conditions, and/or often overlooked. Some representative data are summarized in Fig.16 just as a reminder. The special importance of compressed air at a tunnel face cannot be dissociated from some capillary minisci, and the fact that soils generally are not fully-saturated. Depending on the magnitude of the air pressure, in fact there can be a favourable reversal of flow direction, and consequent favourable seepage pressures to complement the favourably propagating capillary tensions.

In the submerged saturated clayey sands of Sao Paulo, laboratory tests indicated that although under very small gradients (about 0.2) practically no change of moisture content W% was caused (about 0.2%), under much-higher gradients (up to 30) decreases ΔW up to 6% were achieved in less than 1 hour. The graphs of variation of unconfined compression strengths with W% are given in Fig.17A,B As is well recognized, complete drying is unfavourable. But the benefits of somewhat higher air pressure (and local gradients at critical points) are so evident, that it need hardly be emphasized that there is direct and simple and beneficial cure for face drying of a sand: one need but spray the face with moisture, preferably muddy (dirty) water.

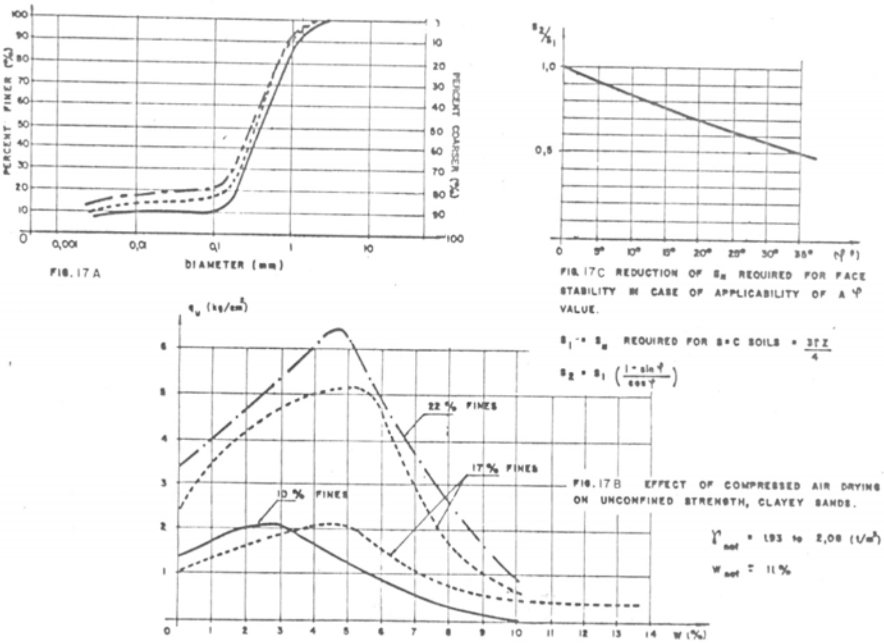
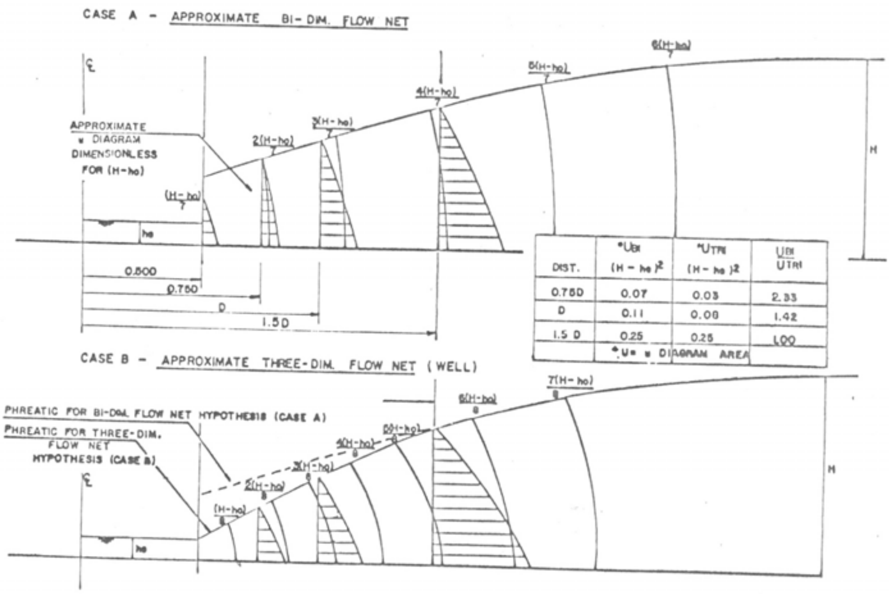
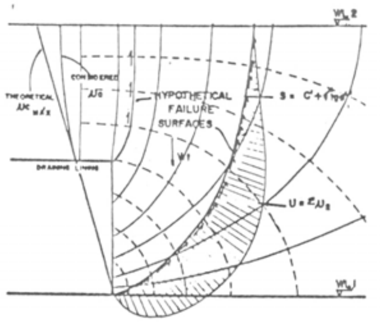


FIG. 17 DATA ON UNCONFINED COMPRESSION OF CLAYEY SANDS, SAO PAULO. (FIG.17A, 17B)
INFLUENCE OF φ ON FACE STABILTY (17C)



DIST.	$*U_{BI}$ / $(H-ho)^2$	$*U_{TRI}$ / $(H-ho)^2$	U_{BI} / U_{TRI}
0.75D	0.07	0.03	2.33
D	0.11	0.08	1.42
1.5 D	0.25	0.25	1.00
	*U = u DIAGRAM AREA		

FIG. 18 ASSUMING TRANSIENT UNAFFECTED PHREATIC AT 1.5D AHEAD OF FACE



1st CASE: | WL1
U = Uc
K0' = 0.5 and K0' = 2.0
s = C' + tg φ' and s = C
C VARIABLE

2nd CASE: | WL2
U = U flow net
K0' = 0.5 and K0' = 2.0
s = C' + tg φ'
s = C

W1 REMAINS CONSTANT: ONLY CHANGES U

NOMINAL s = C = $\frac{Rc}{2}$ (PECK, BROMS AT AL ASSUMED FOR $K_0' = 0.5$)

REAL, PROPOSED s = C' + σ' tg φ' (USING σ' = $\frac{\sigma'_V + \sigma'_H}{2}$)

	K_0' ASSUMED	U	F S	REFERENCE
PECK etc	0.5	——	0.57	100%
TOTAL STRESS	2.0	——	1.20	210%
EFFECTIVE	0.5	from flow	1.10	190%
STRESS	2.0	net U=42 t/m	1.77	310%
ANALYSES	0.5	Uc	1.60	280%
	2.0	U=220 t/m	2.88	500%

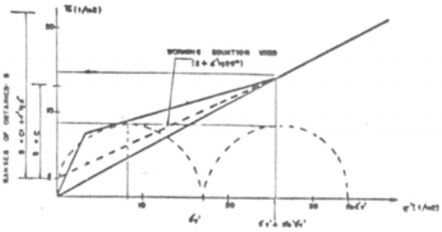
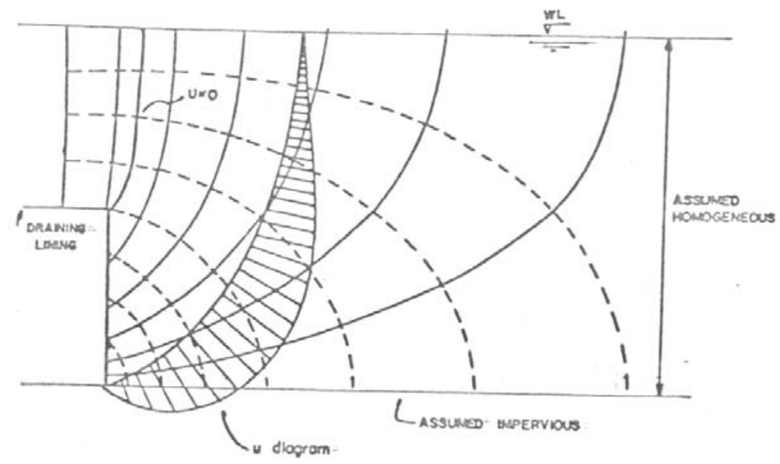


FIG. 19 SCHEMATIC INDICATION OF PROCEDURE FOR EFFECTIVE STRESS ANALYSES MERELY FOR COMPARISONS



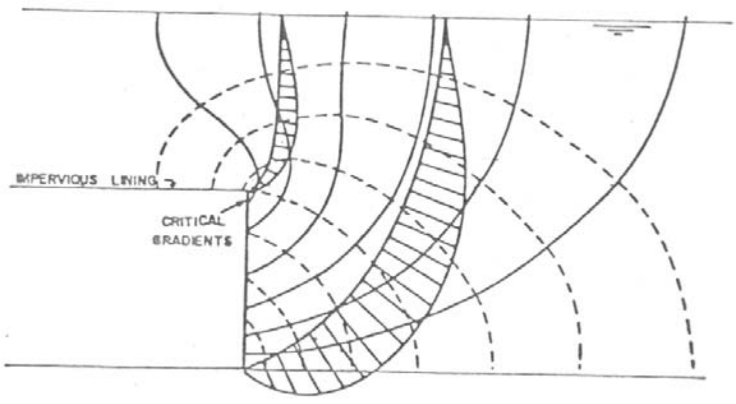


FIG. 20 SIMPLE FLOW NET CASES FOR COMPARISON

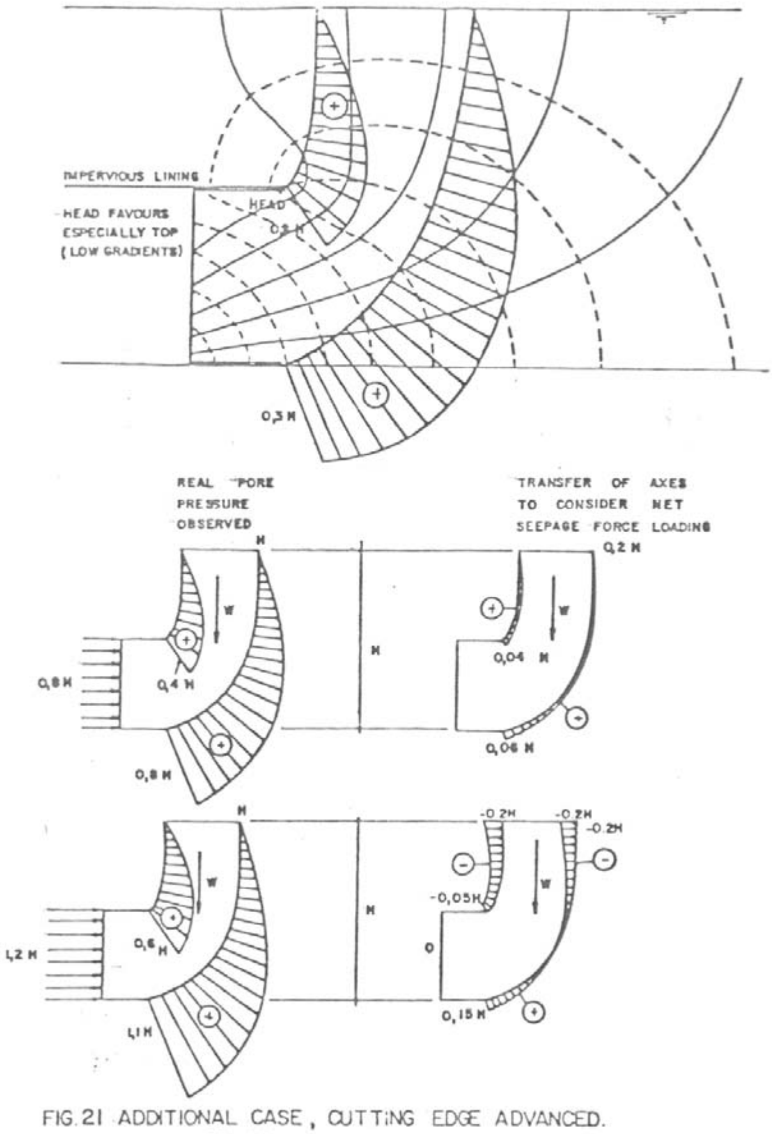


FIG. 21 ADDITIONAL CASE, CUTTING EDGE ADVANCED.
IDEALIZED CONSIDERATION OF COMPRESSED AIR.

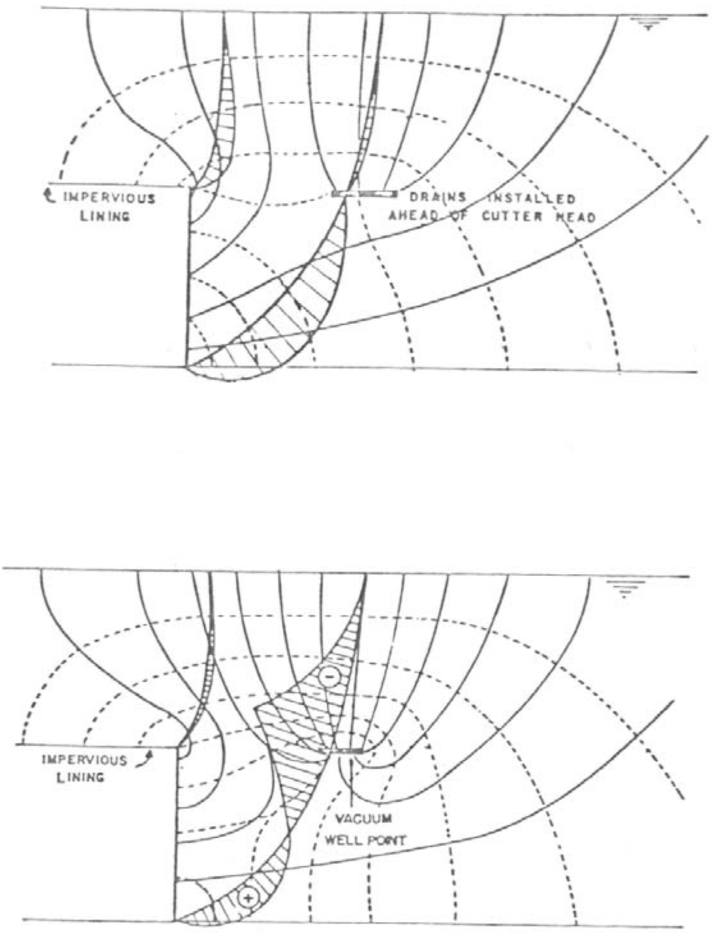
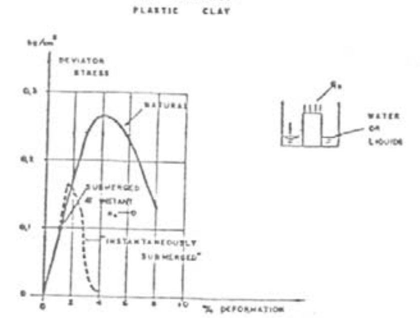
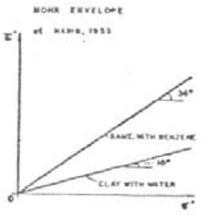


FIG. 22 ADDITIONAL CASES SPECIAL DRAINAGE FEATURES AHEAD OF FACE





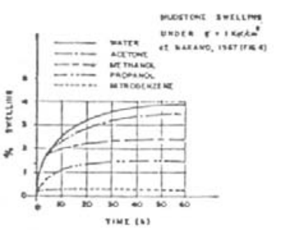


FIG. 16 INFLUENCE OF u_c AND DIFFERENT LIQUIDS IN UNCONFINED COMPRESSION

The first basic fact regarding failure under stress release is that, as a general principle, materials exhibit loading-unloading hysteresis (in greatly varying degrees), and, therefore under conditions of unloading there is always some "cohesion intercept" and $\phi = ds/d\sigma$, however small and/or temporary. When we deal with so transient a condition (tunnel face excavation) so close to FS = 1, 0, one cannot afford to neglect these minute components in comparing successful vs. unsuccessful experiences.

One adjustment factor that could be applied to the $s = c, \phi = 0$ Stability Number, in consideration of an applicable ϕ value, has been suggested by Rebull 1972. The comparative influence is indicated in the graph of Fig.17C. Other such analyses may be available and/or forthcoming. However, unless an analysis can really begin to take into account problems of pore pressures, seepage, K'_o as dominant parameters, it is not likely to facilitate appropriate comparisons.

Merely as an example of methods of working analyses available for assessing comparative solutions, we present a series of cases analysed on the basis of flownets and effective stress envelope. Firstly, it is emphasized that the flownets and analyses have been prepared for the two-dimensional condition (as a liberty, merely to exemplify). Fig. 18 shows the estimation of the adjustment factor that could be deduced in a simplified manner for the transfer of bidimensional to three-dimensional data as regards flownet pore pressures.

The next figure (Fig. 19) indicates schematically for hypothetical failure surfaces how the in situ undrained strength has been estimated, taking into account only flownet u values and K'_o, applied to overburden σ'_v and an effective stress envelope. It is recognized that in principle there can be a need for correcting the flownet u values because of tendencies to Δu as a function of shearing ΔV: judgment may be applied for such corrections, in the light of a feel for the material's behavior and the probable stress path. No matter what failure surfaces may be analyzed, it cannot escape notice that the Stability Number can vary most widely depending on u and K'_o.

In the following figures (Figs.20,21,22), we have sketched rough two-dimensional flownets for some of the conditions typically encountered in tunneling, and in methods used to control seepage pressures. The purpose is merely comparative. In the hypothesis of a slightly excessive compressed air pressure, for a short transient condition, it is assumed that there is essentially a reversal of the water flow in the saturated soil within a laterally confined variable section, therefore with the same pattern of flowlines and equipotentials.

Finally in Fig.23 we summarize the comparative "mass statics" that should give a feel of the influences of different drainage and/or - compressed-air treatments. Assuming that the resultant $\Sigma(u) = U$ values on the failure -- surfaces (rigid body statics based on total - stresses and boundary neutral forces of membrane hypothesis) are the key to the overall stability problem, the comparison is based merely on these values.

For the present comparisons (rigid body with boundary neutral pressures) the artifice is - used of reduction of the horizontal force to zero by "transfer of axis", because the real beneficial effect of the compressed air is to reduce (or occasionably even invert) the --- effective stresses due to seepage. The results indicate trends only, because we must - carefully distinguish between artifices employed for analysis of the statics of "rigid bodies", and the extent to which the "effective stress behavior" only sets in to the --- point that corresponding strains (compressions and expansions, void ratios) have materialized. In a perfectly saturated ideal clay the undrained instantaneous changes of pore - pressures do not generate any changes of in - situ strengths.

Many an important conclusion, intuitive in -- tunneling practice, may be drawn, not only regarding the overall "rigid body statics" but also regarding locally critical failure conditions. These are affected principally either by stress release of the higher horizontal -- stresses (with overall tendency to expansion and loss of strength concomitant with the --- principal stress reversal) but also due to positions of more critical seepage exit gradients, and corresponding tendencies to expansion, loss of strength, and failure. Such localized conditions may be approximately analyzed by Mohr circles. Depending on such localized conditions, the undrained stability solutions based on the bound theorems of plasticity may fail to reflect any semblance of --- realties faced in the field.

As a concluding comment concerning face stability it must be emphasized that the problem - matters not only as regards the transitory -- stability itself, but also as regards settlements. As is well known, deformabilities increase significantly as the FS decreases. --- Soil Engineering is not documented with plate load tests (compressive) on faces of test -- pits, although it is a test with much use for transverse loads on piles etc.., and is a --- test pregnant with practical possibilities. A fortiori, one finds absolutely no data on --- <u>unload-deformation</u> of plates supporting vertical faces (analogous to convergence observations across diameters of tunnels). If and -- when such data become available, they could - be plotted in a manner similar to that adopted in Fig. 24 , wherein we have analyzed the - foundation plate load tests of several São -- Paulo soils. The very rapid decrease of E as one Approaches "failure" is as expected. One suspects that under stress-controlled "soft-load" conditions the unloading behavior will possibly show an even sharper drop of E in -- the lower FS range.

b. Prediction of settlement troughs

Comprehensibly the estimation of the settlement trough constituted the second principal hurdle at the time when Peck (1969) offered - his great contribution towards mentally organizing the advances of the then strictly empirical art of tunneling, for the purposes of - making them amenable to a minimal geotechnical engineering treatment. So it was, therefore, that as has happened so often before, - the profession owes much gratitude to the --- fact that a man of stature was willing to --- step into the vacuum to offer: (a) as a first stilt, a PRESCRIPTION, that of a Gaussian --- settlement curve (earlier postulated by Litviniszyn, 1955), with the admonition "although the use of this curve has no theoretical justification, it provides at least a --- temporary expedient" (p.240); (b) the qualitative indications of the principal intervening factors; (c) the summary table of "all" the - data available, with the candid confession -- that "the information ...... is surprisingly meager", and with the appropriate call for -- "full-scale field observations".

It is herein contended, however, that the -- collection of data calls for a mental model, and we must urgently set aside in totum the unfortunate association with a Gaussian curve, because it is a dead-end road and carries no idea-fertility. We must foster some minimum theoretical analysing on the different - parameters associatable with the full-scale field observations, since progress in design procedures and predictions will only be --- achieved if we set about to dispell the un-necessarily pessimistic forecast "Because of the dependence of loss of ground on construction details, there seems little likelihood that theoretical investigations will prove - fruitfull except for some of the simplest of materials such as plastic clays" (p.245). Although PRESCRIPTIONS do constitute the valid base for design developments and decisions, they must be rapidly adjusted by statistical CORRELATIONS on observed behavior in order - to permit revision and progress. And we must make an effort to resist the widely spreading practice of statistical regressions at - random, since a statistical correlation is - meaningless and can be dangerous unless it - is based on theorization on the physical model, to establish the nature of the equation and its coefficients.

Surely it is accepted that in tunneling we - automatically face a greater proportion of - strictly localized conditions of heterogeneity and possible failure (loss of ground), as is herein emphasized under item 6.1 regarding SI of individual points or fractiles on a histogram. Such conditions are those that must either be bearable and borne as risks - unquantifiable, or must be resolved in design and construction by "a change of statistical universe" (i.e. a treatment that essentially excludes the problem). Our design engineering concern can only be with conditions that permit averaging, and quantifications based thereon. The fact is that settlements most often distribute well enough to validate statistics of averages.

Fig. 25 summarizes the Peck 1969 prescriptions regarding the settlement trough. The basic points are: (a) geometry, dimensions; (b) a Gaussian curve of settlements and no indication of displacements; (c) a graph plotting the available observed data (a point for each case history) with reference to index classifications, irrespective of association with geotechnical parameters.

The presumed Gaussian curve is really that of pseudo-elastic and/or elasto-plastic changes within the semi-infinite mass.

Such is the nature of the phenomenon at play when tunneling design and construction proceed under normal conditions, with minimized, erratic defective occurrences. There is absolutely nothing probabilistic or stochastic about it. Indeed, for local critical occurrences (cave-ins etc.) there are probabilities of occurance along the tunnel: but one hardly could predict, or presume, or even establish a posteriori the frequency distributions of such occurrences for the longitudinal advance of the tunnel (which, moreover, would most often represent a perceptibly varying geomechanical universe, and not random variations within a presumed constant universe).

It is, indeed, strange that a probability phenomenon and function should ever had suggested itself. Litviniszyn analyzed the subsidence that would be caused in a loess if there were a local underground collapse or cavity: representing the material (considered a discontinuous, rigid bodies, separated by cracks) as a mass of uniform spheres, and visualizing the cave-in as the downward movement of one sphere, he obviously concluded that the subsidence profile at surface could be represented as a Gaussian probability. The result is mathematically inevitable. Two phenomena that under idealized conditions lead to the same equation are not thereby similar phenomena.

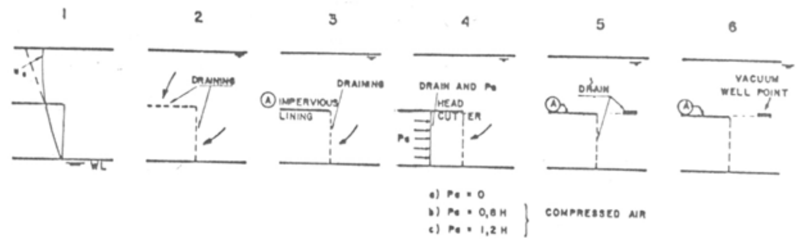
There is many a situation where, after making the necessary simplifying assumptions (usually averaging, and Gaussian) the mathematical equations of a given physical phenomenon become identical to those of many other totally distinct phenomena: for instance, the classical similarities between Darcy-Laplace seepage flownets and the electrical analogy models, or arrangements of iron-filings within appropriate magnetic fields. It would be absurd, however, to follow up with a dogmatization on the mathematical result (idealized) to insist on fitting experimental or observational data of the first phenomenon into the equation of the second: for instance, when capillarity intervenes in the flownet result, it surely is not against the electrical analogy models that one should force the data-fitting.

Peck well emphasizes that "every soft-ground tunnel is associated with a change in the state of stress in the ground and with corresponding strains and displacements", and therefore it is surprising that Litviniszyn's formulation should have detracted from a direct association with stress-strain changes in a pseudo-elastic medium (cf. Fig. 26), specially in view of Carrillo's early brilliant contribution , already mentioned, "Subsidence in the Long Beach-San Pedro, Cal. Area: the effect of a tension center" (1949). The principal problem, in my view, has been the early confusing use of the term "loss of ground", and the tunneling foreman's intuitive feel that settlements (i.e. big, most noticeable settlements) derive from loss of ground. Since in practice one's attention first concentrates on immediate cause-effect evidences, and especially on failure, the primeval confusion is understandable. However, it has nothing to do either with engineering quantifications, or with the "representative points" (without even a width of dispersion) plotted from data tabulated by Peck (and by most authors).

In fact, even for the "collapse of cavity" condition it should be recognized as much more conductive to fruitful experience collection and collating, if instead of adopting a geomechanically sterile stochastic postulation (dissociated from parameters physically comprehensible and derivable) authority had fostered resorting to plasticity formulations ("collapse of cavity" as an inverse of the widely recognized solutions of "expansion of a cavity in an infinite medium").

The most curious fact is that the fostering of the Gaussian curve design prescription predominates among the self-same Design Companies that are most eager to spread the use of Finite Element Analyses for the same problem whenever the shape of the cavity differs from the circular, or whenever in Rock Mechanics there is opportunity to insist on the problems of internal stresses. A single example (cf. Fig. 27) is sufficient to illustrate the obvious.

Peck's candid recognition (p. 231) "It is not yet possible.... to apportion the lost ground between the inevitable mevements associated with a particular method of construction, and the additional movements that may arise because of poor workmanship or faulty techniques" make it imperative to examine (statistically) the varying K'_o, FS, E (etc.) conditions along each tunnel (constant construction technique universe) in order to separate, as in hydrology, the "peak flows from the base flow".



U_{RIGHT} = Σ= (t/m)	- 161	42	43	a) 80 b) 1,5 c) - 7	19	- 30
U_{LEFT} = Σ= (t/m)	- 100	0	8	a) 19 b) 4 c) - 15	3	4

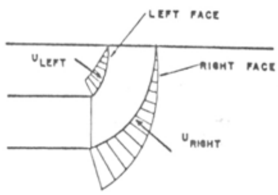


FIG. 23 SUMMARY COMPARISON OF BENEFITS TO MASS STABILITY
SCHEMATIC, MERELY BOUNDARY U VALUES..

FIG. 24 TYPICAL TREATMENT OF PLATE LOAD TEST DATA (LOADING)

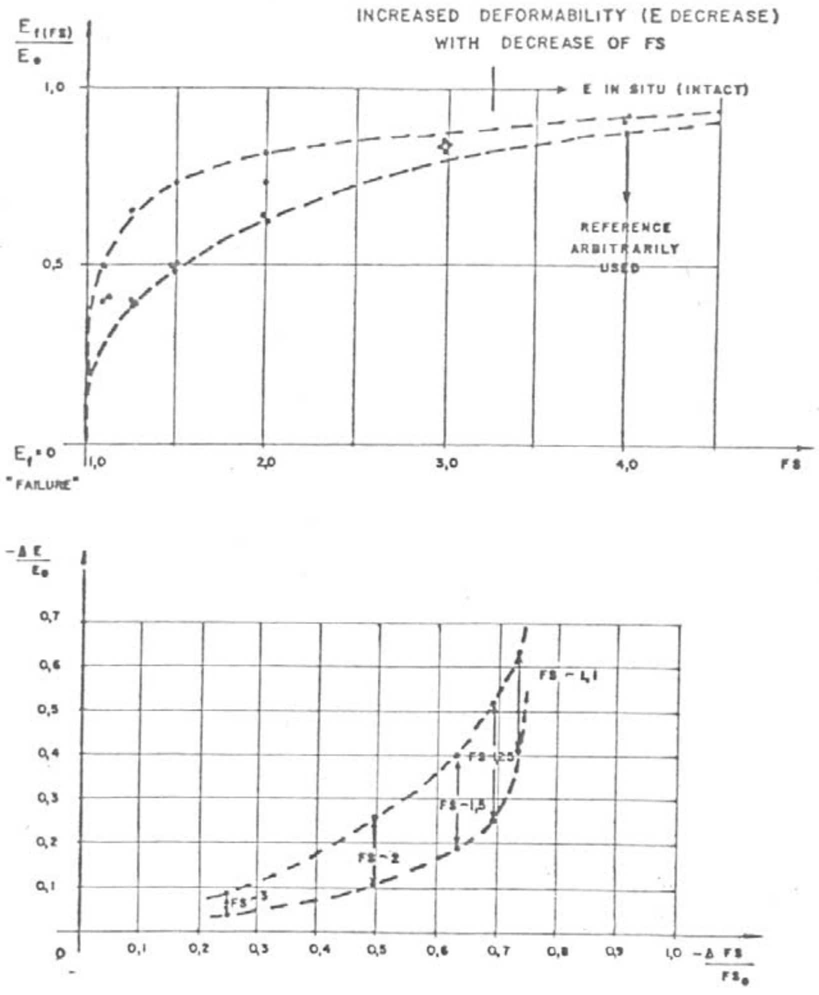
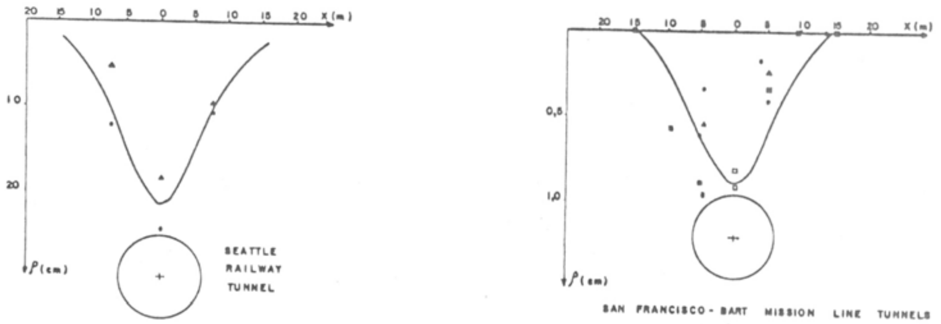


FIG. 25 DEFORMATION IN TUNNELS (PECK 69)



PROPERTIES OF THE NORMAL DISTRIBUTION USED TO REPRESENT DEFORMATIONS ABOVE TUNNELS (PECK 69)

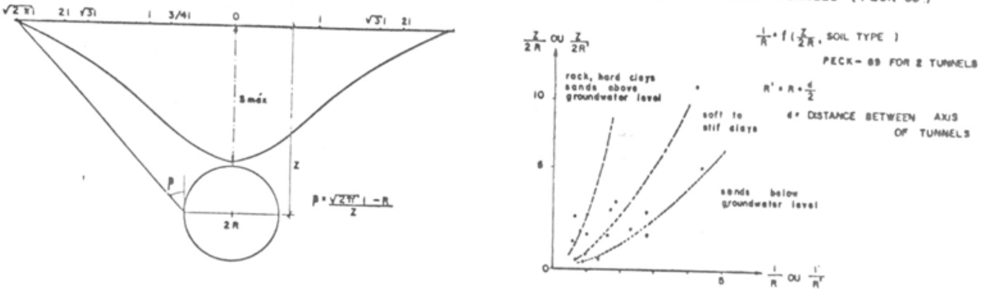


FIG.26 DEFORMATIONS DUE TO STRESS RELEASE VS. GAUSSIAN CURVE

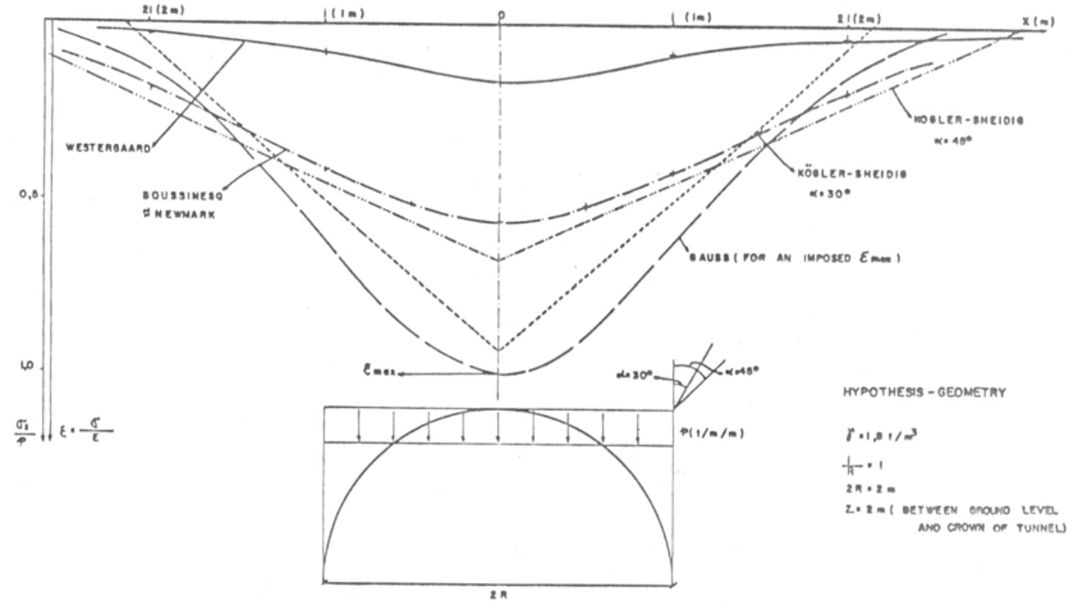
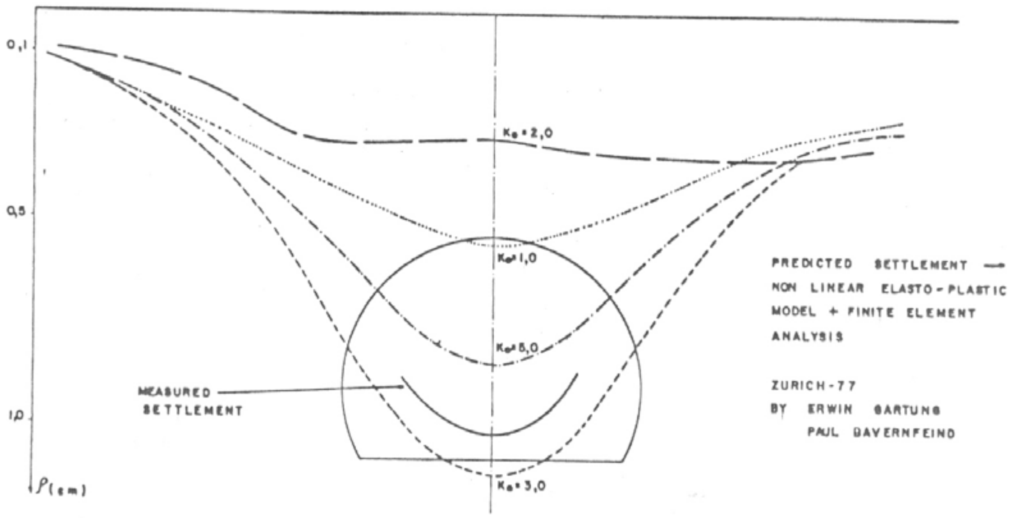
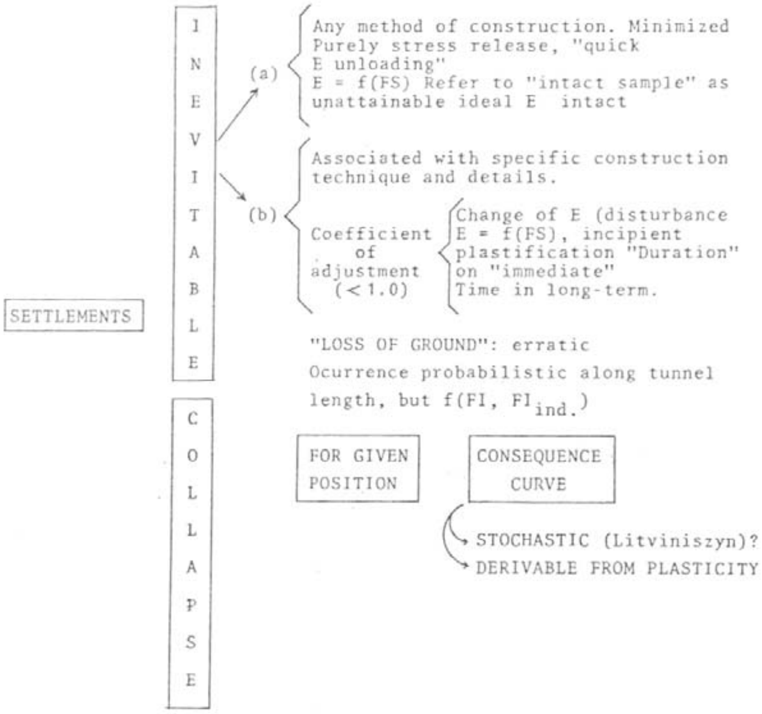


FIG. 27 COMPUTED SETTLEMENTS FOR VARIOUS VALUES OF K_o



SUBWAY TUNNEL AT NURENBERG - GERMANY - STATION NEXT ST. LORENZ CHURCH - REFERENCE SECTION



In this respect it could be a sadly moot question whether it is an advantage or disadvantage that the Litviniszyn collapse formulation should lead to exactly similar distribution of settlements as the elastic and elasto-plastic solutions. Widely different distributions could be sorted out. But, how could they be different, if the stochastic formulation represents nothing but a mathematical abstraction for conditions so idealized as to give the anticipated physical behavior? In short, the Gaussian prescription must be excluded in limine because it is sterile.

There is one additional point of greatest relevance to design. Peck (1969) would give us the shape of the curve, but no direct help in establishing the predicted maximum settlement of each section, directly above the crown. There was a first-order indication "Measurements have established within reasonable accuracy the equivalence of the volume of surface settlement and the volume of ground lost into the tunnel as a consequence of excavation". This indication is physically unrealistic, as there has to be some attenuation, always, and to differing degrees. Even if it referred specifically to "ground lost" as a failure condition, it is absolutely impossible that the volumes transmitted across the medium should, even "instantaneously", be equivalent. The attenuations across the medium have to depend very much on the FS at face, and on the $\Delta E / \Delta FS$ at face and across the medium, and of course, on stress-strain distributions.

Fig. 28 presents the indication that was published (Souto Silveira and Gaioto, 1969) based on an attempted correlation of Peck's data, without recourse to theorizable intuitions. Digestion of data from widely different tunnel case histories will inevitably lead to statistics at random, confusion, and spurious correlations. In the same Figure I have inserted schematically what could be realistic trends for the correlations: these curves can presently be extracted without difficulty from elasto-plastic finite element analyses.

Finally in the same Figure I have schematically indicated that even assuming unchanged geotechnical behavior parameters, there is a net difference between considering the face-plate support (or membrane boundary loading) and the realistic use of body stresses, effective stresses due to gravity composed with those due to seepage. Deformations are not equivalent. The routine computational artifice is perfect for rigid

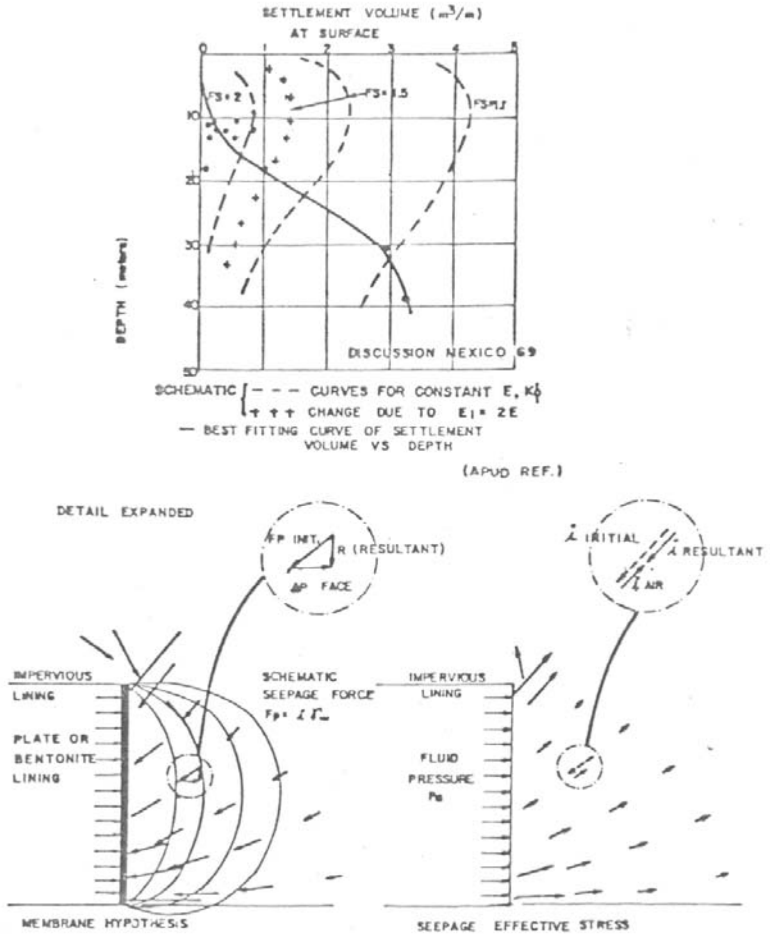
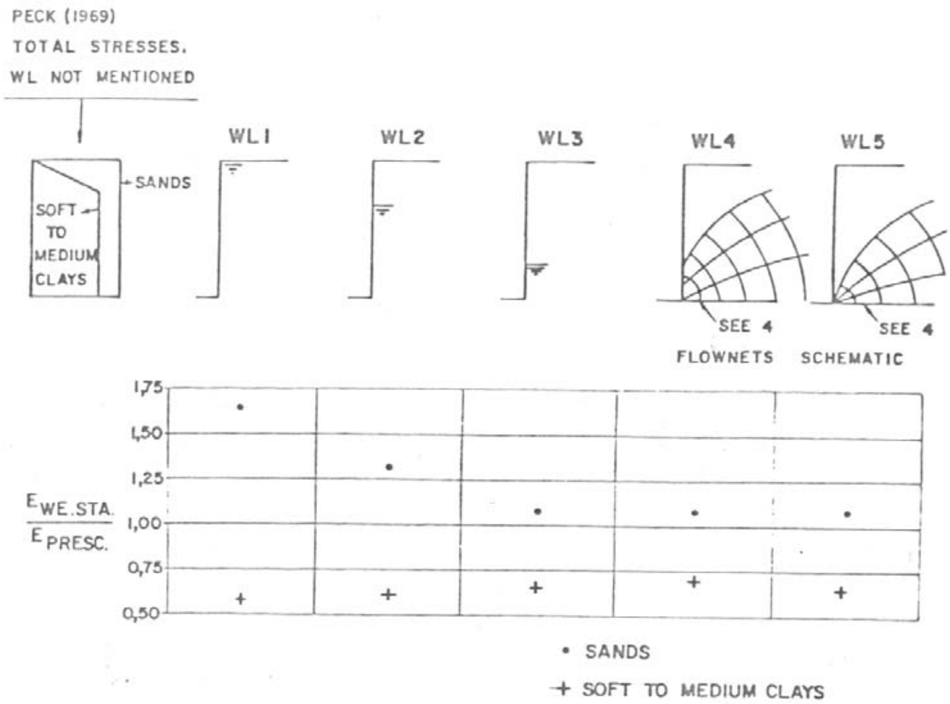


FIG. 28 REEXAMINING QUESTIONS ON TUNNEL SETTLEMENTS

BRACED CUTS : COMPARISON BETWEEN PRESCRIPTION AND WEDGE STATICS



NOTES

1. $E_{PRESCRIPTION}$ CF. PECK, MEXICO 1969
2. $E_{WEDGE\ STATICS} = E_{EARTH\ PRESSURE} \times 1{,}3 + E_{WATER\ PRESSURE}$ (ADOPTED)
3. SAND $\varnothing = 30°$ / CLAY $S_u \cong 50\ KN/m^2$ $H = 15m$
4. FLOWNET DEPENDS ON BOUNDARIES. ASSUMED IMPERVIOUS AS SHOWN

FIG. 29

body statics. As minute deformations and differential deformations have become important to buildings, this source of divergences of behaviors and opinions must be considered.

In summary, both the above problems illustrate the fact that the practising professional has been deprived of the opportunity of developing sensible histograms of non-failure behaviors along his tunnels because either he works with a grossly oversimplified PRESCRIPTION or he would have to go to the extreme of finite element analyses (most of them incompatibly sophisticated for the data and soil behavior models available). Collecting pseudo-statistical charted data from various tunnels around the world is akin to charting some index (e.g. height vs. weight) of all biped species of the world.

5.5 Earth-pressure on deep excavation supports.

Once again, for the design of braced excavations the practicing professional gratefully relies on the PRESCRIPTIONS by Terzaghi and Peck (1967) and Peck (State-of-the-art, 1969). During the past twelve years, with the exponential increase in projects requiring deep excavations, many important questions have arisen, such as, how to account for typical subsoil profiles with varying strata, how to adjust to different K'_0 and deformabilities, how to adjust the prescriptions to diaphragm walls (rigid-continous, therefore averaging, obviating the need for an envelope of worst local conditions), and so on. We shall set those aside. The truly disconcerting basic question posed by most practising professionals goes back to the roots of conventional soil mechanics, effective vs. total stresses, drained vs. undrained: the question posed is, how do the PRESCRIPTIONS take into account groundwater, seepage, and pore pressures?

From examination of the Peck 1969 report, the answer is, they do not: of the 23 excavated profiles presented in the figures, 17 do not have the indication of the W.L., while 6 do; in no case are the probable or adopted conditions of drainage and pore pressures explicited.

Obviously the intent of the PRESCRIPTION can and must be assessed before proliferating its application without regard to varying site conditions and developments of geotechnical knowledge. The two separate problems are; a. the total lateral force, necessarily divided into effective earth pressure and water pressure, b. the distribution of pressures. By back-analysing from observed strut loads (Peck) one obtains the lumped parameter for a, and can assess b. reasonably. It is quite understandable that in a soldier pile-and-lagging braced excavation we should have had to work with an envelope because any local failure could carry a catastrophic progressive castle-of-cards effect. It so happens that the PRESCRIPTION corresponds roughly to 1.3 times the adopted Active Pressure Force. Incidentally, a 1.3 "factor of safety" generally adjusts to satisfactorily-low deformations.

Thus, the minimal adjustment of the recommendations could be the application of a 1.3 multiplier to E-active. And for the E-active we can and should use our best current computations based on effective stress earth pressure, and pore pressures. (Fig.29)is intended to illustrate schematically the orders of magnitude of adjustments that can be at stake even if we restrict (oversimplified) the consideration of water effects to nothing but boundary neutral forces on the limit equilibrium active wedges.

In short, in both these and in many other instances, it should be emphasized, with our deepest respect and gratitude for the fruitful contributions that helped us to this point, that it lies in the glorious destiny of a fruit that it should mature, fall, and rot, so that from its seed may grow another tree for further fruitage.

6. NEEDS AND FUTURE OF GEOTECHNICAL ENGINEERING AND SOIL MECHANICS.

The intent has been to aim 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.

6.1 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. 30 , 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.31A, 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 defines 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.31B, 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."

6.2 Concentrated attention on meaningful 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)(Fig.32)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 of 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 the 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 --- 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.

6.3 Observations of 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 to 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.

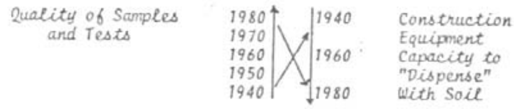
Laboratory research has led to very fruitful conclusions because it always respected the need to investigate two parameters at a time, all others maintained constant; and it early recognized the need to correct for "seating or installation errors" close to zero.

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.

6.4 Quantifications of quality of sampling -- for closing the experience cycly meaningfully.

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.

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.

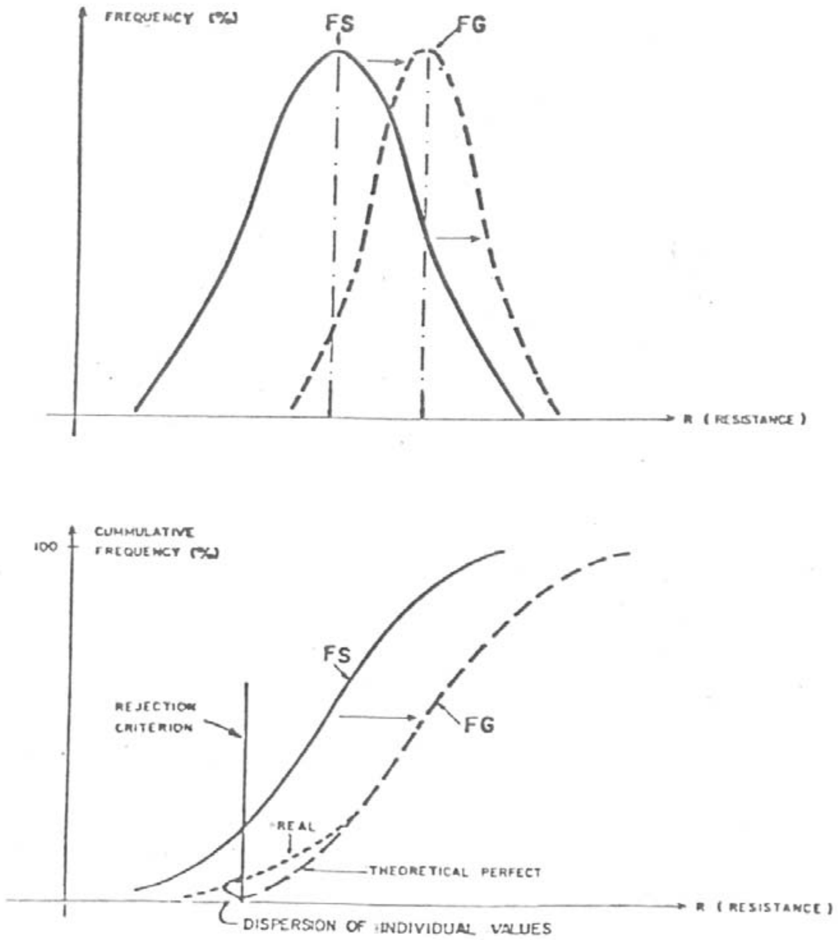


In (Fig. 14) I have reproduced the results of a simple analysis used a long time ago in an attempt to refer UU strengths to a presumed-common reference of "undisturbed quality". - Around 1953-56 I had opportunities to sample and test a significant volume of shelby samples of foundation clays, and obviously noticed the relationship between percent strain at failure and the degree of disturbance, as indicated by socalled 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.

6.5. In situ testing and multiple profiling

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.

FIG 30 PROPOSED DISTINCTION BETWEEN FACTOR OF SAFETY (FS) AND FACTOR OF GUARANTEE (FG)



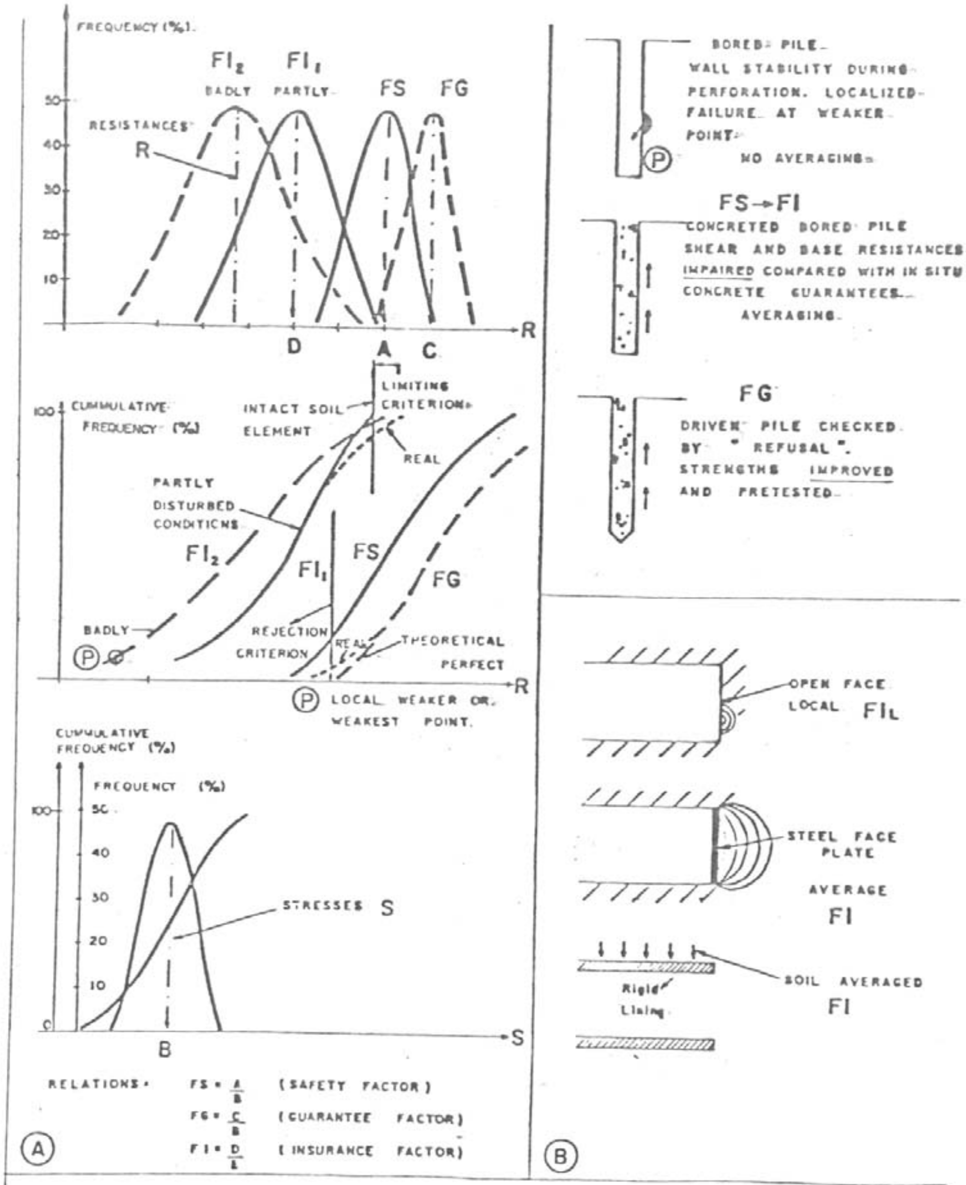
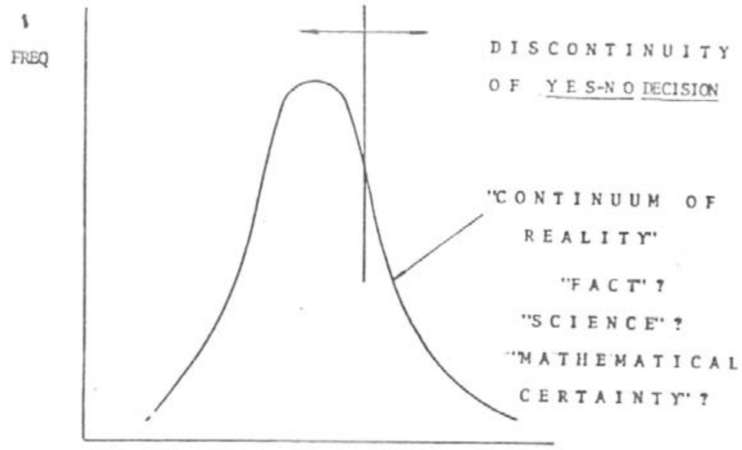


FIG 31 SUGGESTED DISTINCTIONS IN "FACTORS OF SAFETY"



"THEORIES" STARTED FREQUENTLY
WITH 3 OR 4 POINTS

INVENTION GENERALLY MUCH BETTER
THAN FOR SURVIVAL AT FS = 1.00

FIG. 32

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: Spurious logic:

samples $\longrightarrow$ disturbance

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

in situ testing does not sample

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 for never having been applied before and after a given loading, to check on their ability to reflect changes of conditions.

6.6. Extending theorization for soil behavior

Principal well-known factors of influence for the near future may be mentioned as Structure, Porosimetry, air-pores, time effects, comentations. Lack of inclusion of these effects is responsible for most of the unexplained scatter and discrepancies. I include discussion of in-situ stresses as affecting all of the fundamentally rational concept of stress-strain-time-testing and consequent design calculation, because I have long considered it a tool for understanding soil behavior and not for coping with design and construction variabilities and dispersions. We must recognize that the dispersions are not merely those of sampling and testing but originate already intrinsically in the rejection of a perfectly homogeneous natural condition in situ: not only do average $\gamma' z$ vertical stresses suffer considerable variations due to differentiated deformabilities and stress redistributions, but also the rather elusive horizontal stresses will be found highly variable (within the viable range).

Therefore, in soil mechanics, I propose that the principal new parameters for us to investigate more throwghly are those that may significantly affect our very acceptance (automatic) of such initial dogmas as the Terzaghi effective stress equation, the traditional grainsize analysis, etc. The soil-parameters mentioned affect a large proportion of the world geography and geology, and air pores are what matters for compaction and for many a soil treatment. Grain nucleations and crumbs often dominate behavior; macropores often influence behavior significantly.

In caricature we could say that in an early-period, soil mechanics was principally concerned with solids (individual 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 same advance of our investigation of the gaseous phase.

7. 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 pround 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?

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?

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