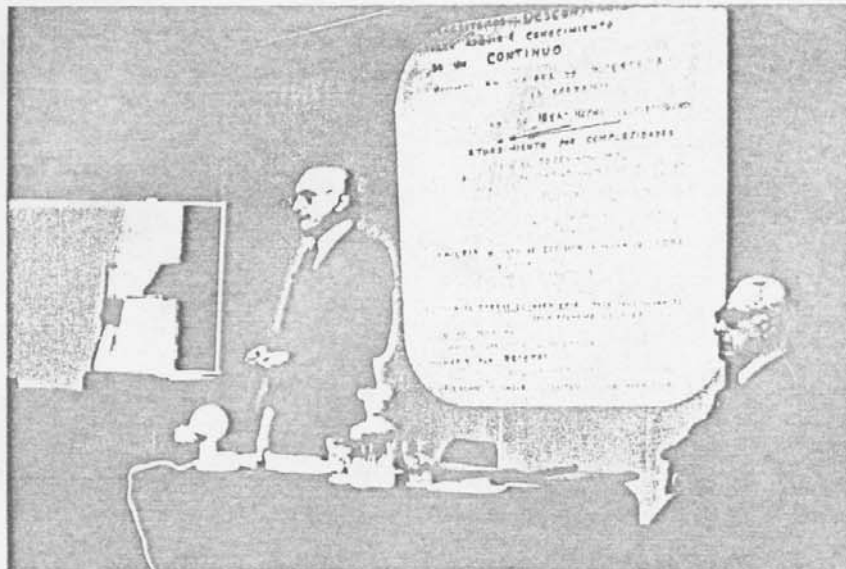Conferencia Inaugural

"ATTEMPTING AN HISTORICAL APPRAISAL OF SOIL

MECHANICS AND GEOTECHNICAL ENGINEERING"

"HACIA UNA APRECIACION HISTORICA DE LA MECANICA

DE SUELOS Y DE LA INGENIERIA GEOTECNICA"

Víctor F.B. de Mello
Presidente
Sociedad Internacional de Mecánica de Suelos
e Ingeniería de Fundaciones

1. INTRODUCTION

2. INVENTIVE OR INGENIOUS ENGINEERING, FOUNDATIONS AND EARTHWORK

   2.1 Historical Perspective
   2.2 Modern inventive engineering products and procedures
   2.3 Concept of inventive engineering in geotechnique, and presumed future.

3. PRESCRIPTIONS AND WORKING HYPOTHESES

4. THEORIZATION AND ANALYSIS-SYNTHESIS

   4.1 Personal interpretation of landmarks
   4.2 Dominant first-approximation correlations
       Examples: a. Virgin Compressibiltity. Remolded clays
                 b. At-rest lateral pressure coefficient K'_o, remolded and undisturbed clay
                 c. Undrained shear strength, cohesion
                 d. Proposed simplified-unified theory for plasticity indices
   4.3 Pseudo-statistical correlations, and engineering needs

5. APPRAISAL OF SOME "PRACTICAL ENGINEERING SOLUTIONS"

   5.1 Clay-core dams. Plasticity of core.
   5.2 Footing foundations. Sands
   5.3 Pile foundations
   5.4 Soft-ground tunneling
   5.5 Earth-pressure on deep excavation supports

6. NEEDS AND FUTURE OF GEOTECHNICAL ENGINEERING AND SOIL MECHANICS

   6.1 Revised definitions of nominal safety factors
   6.2 Concentrated attention on meaningful histograms of non-failure behaviors.
   6.3 Observations of incremental actions vs. consequences

Attempting an historical appraisal of soil mechanics
and geotechnical engineering

Víctor F.B. de Mello

1.   INTRODUCTION

It is very appropiate that both individually   and
collectivelly 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 un-
to milestones on a road, but preferably, as is the   case on
this special occasion, obviating the regularity of milesto -
nes, which automate themselves into a new drone of a conti -
nuum.

The special occasion now chosen is the First Chi -
lean Conference of Geotechnical Engineering. And it must be
noted as special not only because of the considerable amount
of work already done, and in execution at the moment, in the
fields of soil engineering, seismic behavior   and analysis,
mining engineering, and dam engineering, but also because des
pite the somewhat adverse world economic conditions, there has
been a courageous decision to demonstrate that the needs and
challenges of civil and geotechnical engineering continue to
be faced and met on behalf of Society and Man, under no ma-
tter what conditions. 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, for
mal events as discontinuities; and, above all, one must re -
mark that it is instrinsic to the very nature of the remarka
ble that it be subjective.

Inevitably subjective 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 cri-
tical appraisal of such a trajectory should be yet more sub-
jective. 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 communications we have been brought once again
to the dind of bewilderment that must have accompamied our fo-
refathers 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 ideas 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.

Thust 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 connec - ted with Nature. May each of you see in my present personal exercise nothing but the stimulation for your using a similar privilege.., 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 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 pro - portions 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 a. inventive or ingenious engineering, b. engineering by prescriptions, c. theorization and engineering by analysis-synthesis.

2, INVENTIVE OR INGENIOUS ENGINEERING, FOUNDATIONS AND EARTH-
WORK

Our discussions of the history of soil mechanics and soil en
gineering 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 validi-
ty 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 engi
neering 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 theo-
ries, procedures and rules, for others to apply unquestioning
ly. 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 re
cognize that whereas professionals prior to around 1940 were
sent out to find for themselves with relatively little subse-
quent subjugation to academic production, in the recent past
the rate of production of additional information and the in -
tensity of technological development and communication has great
ly increased, and perennialized throughout professional life ,
the subconscious dominance by academic activity. We are eter
nal students; but nowadays, much less so of 'life than of the
flood of writings of teachers. We have imperceptibly allo -
wed processes of information to occupy the biggest space of
professions, to the detriment of formation. Without any un -
due 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 a-
cademic structure whereby creative students are instigated to
question, callenge, disagree, and propose other solutions, pre
sumably more elegant. Yet we cannot deny the preeminence of
engineering creativity as a physical visualization of a solu-
tion that so elegantly and superabundantly sets aside or domi
nates 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 the 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 "co pying of success". Noticeable success to be imitated was al ways 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 o- pening a possibility that takes a considerable time to be u- sed up by increasing demands. It is thus good engineering, in design or construction, avoids being cornered, from its posi- tion of affluence of ingenious ideas, into better calculation or more conscientious engineering labour.

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 draina- ge) 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 anony - mous invention of foundation engineering that we take for gran ted, 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 mat- ter of optimizing the benefits of a driven pile and recovera- ble 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? Eas it by a geotechnical engineer, and was such ingenious engineering dependent on the teaching of conventional Soil Mechanics? The Mexicans 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 them 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 somewhat 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.

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 ob - ject 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 responsibility / feasibility, it is not in better analytical work that engi - neers 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 pu - blication 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, stonecolumn and lime-column stabilization, rootpiles, 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? Ne
cessity may be the mother of invention, and so there may be so
me inferences to be drawn from attributing the origins of ma-
ny such developments to Italy, France, Sweden, etc. Besides
cultural factors, could it be that the greatest fertility for
such production is associated with regions faced with the needs
of keeping abreast with bigness, and somewhat less favored with
economic abundance? I prefer to recall Dr. Land's affirmation,
when he described the invention of the Land camera (1948),that
the two components of an invention are, first, "to give free
bridle to your wishful dreams," and then, "to work hard to ma-
ke them come true".

          It is not demeaning to "engine engineering" efforts
of soil mechanics to give firts priority to intuitive ingeni-
ous engineering in most such developments. Some remarkable e
xamples to the contrary might only serve as the exceptionsthat
prove the rule, or as examples of creative breakthroughs ba -
sed 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 Pe
dro, Cal. Area: the effect of a tension center". 1949) ranks
as one outstanding example; the highly pofitable engineering
follow-up of judiciously employing pumping-in (recharging wells
etc.) for allaying subsidence (and, in the case of wells, con
tinuing to optimize oil production) can thus fall into the ca
tegory of fertile interaction between existing theoretical
tools and intuitive breakthroughs.

          May own early attempt at inventive work (1946'48)em
bodied in my doctorate thesis (and a joint patent of invention)
was conceived under theoretical reasonings that "solidifica -
tion" of clays could best be achieved by base-exchange with an
appropiate monomeric cation, and subsequent polymerization ,
thus achieving the strengthening of the linkages between clay
particles via adsorbed cation and polymerized chain. In a sen-
se, mine was the Acrylic Monomer N° 1, AM-1, a calcium acryla
te, and successive developments led to much work at M.I.T.and
the present solution-grouting product AM-9, used in specia -
lly 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 tho
se 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 monome -
ric solutions that might be catalysed into selective polyme-
rization 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 li-
quid that can be induced to solidify), but accompanied by two
factors of inefficiency and cost:firstly, the series of perfo
rations attempting to find the future preferential paths, and
secondly the pressure injection outwards from holes, quite di
fferent 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. Polymeriza
tion could be induced to generate a selective growth of "sil-
ting" sizes to improved matching with crack sizes. Apparen -
tly 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 in-
genious and engine engineering. In foundation engineering a
remarkable example is the development of Pilotes Control, ano
ther Mexican demonstration of how daring can be the solution
sired by ingenious engineering when necessity is the mother.

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 ever increasing in
tervenience: one is what I choose to denominate "the burden of
heavy and special equipment", the other is the "excessively e
xacting demands" from modern society on geotechnicians.

The trend during the past 35 years has been of such
exponential increases in weights and capacities of construc -
tion equipment that it could not fail to exert considerable in
fluence on several aspects of geotechnical engineering. I do
not wish to repeat the obvious: that geotechnical man has li-
terally moved mountains, and scarred the face of the earth .
My interest is in examining some of the psychology behind such
endeavors. Whereas our mentors, such as Terzaghi, Taylor, Ca
sagrande, Skempton, and Peck, nurtured a passionate love and
respect for the delicate frailty of soils, the modern tenden-
cy is to brutally disrespect soils as a nuisance that one can

do without. Some earthwork engineering and foundation solu
tions are superabundant to the point of achieving the desi -
red equal-to or better-than behavior irrespective of the soil
"When in doubt, grout: if still in doubt, grout throughout,"
exemplifies jestingly a frequent reality. At what cost, we
shall not ask; why is the world becoming unbearably expensi
ve for everybody, everywhere? Development of big capacity
for the tackling of the mammoth projects was unquestionable:
the problem lies in designing and building medium-size and
small projects as if they were mammoth jobs dwarfed.

Moreover, from some areas there have been remarka
ble systematic improvements of equipment capabilities, some
of them fortunately channeled directly into civil engineer-
ing construction. When we stop to think of the exponentia
lly exponential developments in electronics, and in most in-
dustrial developments, we are readily carried away into pri
de at what can be achieved by concerted collective efforts
at development: many centers may be mentioned, but the pri-
me example is conceded to be Japan. It is quite clear that
possibilities of creation in synthetic, industrialized fields
dealing with materials subject to manipulation of high bene-
fit/cost ratio, will trend toward exponentially growing pro-
portions in comparison with the modest manipulations of sub-
soil conditions.

Thus increasing proportions of problems might be
approached from the viewpoint of solutions despite subsoil
conditions. The emphasis has changed somewhat disconcertin
gly: one ceases to direct prior interest to knowing the soil,
or even to knowing what to do with it, and one shifts atten
tion to what to do to it, or even despite it. In a vicious
circle we presently ride. Highly developed industrial pro-
duction and its quality control offers fantastic possibili-
ties, but places more and more exacting demands on geotech-
nicians. We are required to guarantee foundations that will
not settle more than a couple of millimeters despite unu -
sual combinations of loads, temperatures, vibrations, etc,;
we are required to guarantee against risk of cracking under
hypothetical risks of seismic events. And so on. And with
out confessing our relative dissatisfaction with our availa
ble conventional solutions, insufficiently precise, guaran-
teed, and economic, we have found recourse in solutions that
essentially dispense with detailed concern for the soils's
personality and whims. The self-same industrial output gi-
ves us the means.

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

3. PRESCRIPTION AND WORKING HYPOTHESES

In my estimation, in a critical analysis of the deve
lopment of geotechnical engineering, the second place of im-
portance must be given to PRESCRIPTIONS, both for design and
construction and for such prior and subsequent activities as
investigations, testing, specifications, and so forth. No
other fundamental tool of our technological chain and ratio-
nale meets with so much incomprehension and misrepresentation,
PRESCRIPTIONS are most often not recognized as such, being ei-
ther promoted to the levels of dogmas, principles and theo -
ries, or paired with CORRELATIONS, or even derided as the prac
titioner's "fudge factors". Yet it is by PRESCRIPTIONS, ma -
nuals, codes, standards, etc., that the vast majority of our
efforts are conducted. And by a satisfactory prescription we
simultaneously take a step forward in our practice, and re-
tard immensely the stimuli for the dynamics of revision.

Doubtless every geotechnician recognizes that the
use of the C.B.R. criteria for pavement design is a sheer pres
cription. But how many would concede that almost every de -
sing and construction practice is similarly nought but a pres
cription or working hypothesis?

Well, let us begin by the general statement that
most of our works are designed either for some Factor of Sa-
fety FS against failure, or for some limiting allowable de -
formation: is there anything but PRESCRIPTION to back up the
adoption of FS $>$ 1.2 (say) or an allowable deformation at $<$
x mm or 1: $>$ γ distortion? If these final yes-no decision cri
teria are nought but prescriptions, everything leading up to
them cannot be much different or better.

Thereupon, as a second step, we could list speci -
fic 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 foundations.
Disposition of filter-drainage features within the
dam body.
Acceptable seepage losses.
Criteria for filters and transition materials.
External slopes, stability and deformability.
Compaction criteria and field vs. lab. procedures.
Acceptable plasticity of core material.
Deformation conducive to cracking, tolerable li -
mits.

Liquefaction criteria, seismic behavior and risks.

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 ancho - red facings.
Comparative diagrams on diaphragm wall.
Construction stability and deformations of diaph - ragm wall excavations with slurries.
Deformations of supported mass and foundations thereon.
Bottom heave, in general (c', $\emptyset'$) soils.
Choice of groundwater lowering, feasibilities, pre ferences, risks, consequent deformations.
Soil treatment (grouting etc,) and benefits therefrom.

Machine foundations:

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

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 grea - ter detail the first item listed. Analogous minimal discu - ssion 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 behavior 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 the 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.

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 cost 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 da
maging consequence in practice, because, of experience, effort
and curiosity, the greatest is curiosity. For some privile -
ged spirits, the modern world favors keeping perennial the fla
me of curiosity, of youth. Research is not an activity, it is
an attitude that can pervade any occupation. If we recogni -
ze youth as a period when we face a disproportionately high ra
tio of things unknown and new, to things already mastered, the
one fortunate fact of the exponential aggression of the tech-
nological world is that it can keep us all perennially child-
like; and none can deny that in the world of geotechnical en-
gineering 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 ac
tivity is of curiosity-stifled and effort-less resort to Pres-
criptions, the experience vector merely consecrates the unwan -
ted byproduct of an otherwise indispensable engineering wor -
king tool. In using a succesful prescription we may be using
so big an umbrella that there is gross and frequent overdesign.
Not only does Society thereby pay an immediately high price :
the higher price accumulates with time. There has to be pro -
tection of prestige. Failure conditions are difficult to quan
tify 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 ex
perience from failures, and from poorly undestood nominal Fac-
tors of Safety. If a histogram of behaviors under a given pres
cription 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 con
sider (and proposed, cf. Rankine Lecture, 1977) as fundamental,
i.e.: -

Design Principle N° 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 so -
lution 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 enginee -
ring fail to be specific to local conditions, as is the ensla-
ving trend, especially through well-meaning international 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 pro -
blem; a stepping-stone-one, to know at least one temporary accep
table solution.

4. THEORIZATION AND ANALYSIS-SYNTHESIS

Whereas creativity happens, and prescriptions achie ve engineering doing, hopefully engineering science accumula tes. 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 tra - jectory, what matters is the incremental advance with incremen tal effort along some direction, along our own individual. assessments 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 geology conducted as one of the na tural sciences of the time, and assuming the fertile mental model of deterministic quantitativism based on judicious tes- ting and accompanying mathematical analysis. The oedometer test and its use in settlement calculations basically repre - sented a model-prototype idealization. Soil mechanics theori zation was rational on the basis of single parameter associa- tions. Soil classification was determined by the dominant pha se, the solids. The interference of water ("neutral" pressu - res) had to be separated, subtracted. Most parameters and tests created and in use were consciously or subconsciously towards being dominant, dichotomic: cohesive, cohesionless; $(c, \emptyset)$ as $(c, 0°)$ or $(o \ t/m^2, \emptyset)$; pervious, impervious; compressible, in compressible; plastic, non-plastic; static, dynamic; active, pa ssive pressures; and so on. To some extent we can sense that the yet pervading pseudo-dichotomy of failure problems as dis tinct from settlement problems, and undisturbed vs. remolded (forgetting the inexorable adjectives, 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 natu- ral sciences, health and sanitation, etc., one might postula- te that the engineer (structural) brought into early Soil Mecha nics a greater proportion of the attitude of doing, the solu- tion syndrome, plus the cause-effect testing context of Strength of Materials, plus the priority preccupation 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 pro-
gress of our society, developed very noticeably despite res-
triction to observation and non-destructive testing, destruc
tive testing being essentially impossible in geologic settings
and taboo in the biological fields. Under the imposed conti
nued observation of thousands of units of the statistical uni
verse of minute multiple causes and effects simultaneously in
terfering, aided, no doubt, by the back-analyses of the mul-
titudinous case-histories of ultimate failure (death inexora
ble), the fields of biological technologies resorted to more
intense application of the statistical tools of multiple pa-
rameter regressions, multivariate analyses, factor analysis,
grouped observations in regression theory, etc.

The comparative rates of social and research invest
ment in the two approaches would merit assessment, and corres
pondingly the comparative benefit/cost ratios of the two tech
nologies serving society through civil and sanito-medical en-
gineering. The fact is that in situ geotechnique is much mo-
re 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 pseu
do-correlations, of statistics at random) may now pay the pri-
ce of frustrations in the face of heterogeneities. The phase
of respectful recognition of the sensitivities of natural con-
ditions, and of innate difficulties in each individual case as
distintic from all others, came into early soil mechanics as a
sequel of the first rapid advances, possibly as problems of con
sulting engineering over vicissitudes increased in relevan -
ce and proportion: we may term it the phase of the heteroge -
neous-problem-vision of soil engineering. It emphasized expe-
rience, which was excellent and inevitable; but it left a mute
feeling that the roads to gaining and asserting experience we-
re poorly mappable.

Having postulated the above two early trends, I sub
mit that the classification of soils on the basis of fully-di
sintegrated grainsize curves was obvious, considering the accep
ted dominance of solids (grains) and the interest in recent se
diments. The initial successes later retarded recognition of
the importance of shapes of grainsize curves and of grain sha-
pes 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 appropiate classifica
tion of soil behavior, has become one of the starting difficul
ties to adjustment of conventional soil mechanics to enginee -
ring.

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 re - molded materials. An elegant theorization on the liquid and plastic limit indices as worth revising into two simple index tests of undrained shear strengths (of the order of 0,17 kg/cm^2 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 Sensitivity 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. 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.

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 promo - ted, the principal landmark is interpreted to be the recogni- tion of K'o in situ stresses justifiably different from the assumed K'o $\simeq$ 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 ma turing of soil mechanics. Failure criteria (Mohr, effective principal stress ratio vs, deviator stress), predominantly strain- controlled testing, and effective stress (vs. total stress)ana lyses gained ground so convincingly, that possibly the pendu - lum 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 influen - ces in geotechnical thinking and practice.

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

In shallow foundation design the consciousness of deformations as the principal preoccupation had been camou - flaged under the reduced bearing capacity coefficients (Terzaghi, etc. of "local failure in compressible materials". Gradua - lly however the paractice fell by the wayside, and all atten tions concentrated on more realistic settlement computations, to be compared with PRESCRIPTIONS of proposed limiting allo- wable deformations. In deep foundations one special landmark might be the London Conference on Large Bored Piles .wherein the differentiated load-settlement coparticipations of adhe - sion 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 probably 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-plas- tic formulations of increased bearing capacity with depth.

Finite element analyses, and a good array of analy- tical solutions for elastic and elasto-plastic behaviors of soil masses and soil-structure interactions need not be men - tioned 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 resul - ting 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 ve ry significant: -

a. developments of in-situ testing (especially emphasized as undisturbed- sampling-plus-laboratory-testing came under grea ter questioning);
b. listing and reporting or peculiar soils, unsaturated, in - durated, fissured, expansive, collapsive (loesses etc.), sa - prolitic, lateritic, quick clays, etc., beckoning more gene - ralized theorization.

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

Finally, special mention must be made of field ob - servations and case histories. Therzaghi early began to em- phasize the importance of field observations, but it seems as if the case-histories were meant to constitute a warning of va garies exemplifying the importance of "experience", more than documentation for a histogram of natural quantifiable trends from which experience is acquired. In efforts towards PREDIC TION of behavior, another significant landmark, the frustra- tions 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 computation. Some of the frustrations have been assigned to questions on in- situ states of stress, destructuration of specimens under ty - pical sampling-testing, vitiation of strains and small strains even if specimen failure conditions remain relatively unaffec- ted, and so on. A wide open door has been opened to probabi - listic prognostications, and applications of decision theory. It must be noted that in many such pionnering applications the intents are much more commendable than either the methods, the results, or specially 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 the- refore has been aided but also somewhat straight-jacketed. But the sensorial possibilities are incalculable: for instan- ce, there have already been some successful trends towards

forewarning on damage thresholds by sophisticated recording
of microacoustic generation, and so on.

4.2   Dominant first-approximation correlations

There have been repetead 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 sa-
tisfying in concept nor sufficiently useful in practise for
quantifying estimations.

The main criticisms are that, having been extrac
ted from idealized laboratory experimentation, they were a.
single parameter correlations, generally without a minimum
recognition of even a second significant interfering parame-
ter; b. generally established by visual fitting of best dis-
persions; c. based on remolded specimen testing, with no hints
at natural effects of structure, time, cementations, seconda-
ry 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 sin-
gle 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 iden-
tifiable 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 expres
sed to reflect some interference of Ip as a minimum second
parameter, even though the Plasticity Chart classification of
clayey soils had emphasized such dual interference. There is
an intuition that at a given W the soils with higher Ip should
give a noticeably higher Cc: is that proven, and what correla-
tion $Cc = f(W_L,Ip)$ can be offered as corrective?

Moreover, it is quite liquely 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).

Let us note, therefore, the changes of attitude re
quired. In nature everything is different unless proved accep
tably similar, and all factors intervene, unless proved suffi
ciently 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 relati
vely insensitive to the fundamental behaviors and the conven
tional simplifications. Our great mentors of the early days
of soil engineering faced the humbling complexities of the un
quantified problems, and made an effort to achieve conventio
nal 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
who start from the bewilderment of reality and painfully reach
the ability to distill it to the essences of simplicity requi
red 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 with
out sufficient emphasis on hypotheses, and so the rational sim
plicity of rationalizations has seduced, and suppressed all
humility towards Nature.

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

A second example concerns the suggestion that in
"typical" normally-consolidated clays, the conventional K'o
= 1 - sin ϕ' be substituted by a linear regression K'o = 0.44
+ (0.42) Ip/100 for 20 < Ip < 80, and this essentially irres-
pective of being "disturbed" or "undisturbed". (Fig. 4).

Firstly we should like to substitute the dichoto-
my disturbed-undisturbed by values of partial Sensitivities
S_{tp} and, if possible, adjust for varying qualities of sam -
pling-testing by some form of extrapolation to what could
possibly be the intact soil element behaviour. The impor -
tance of intact soil element behavior at small strains is
presently meriting increasing attention because of the recog
nized 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 loadings (or passive pressure): in the latter the association of behaviors with erroneously low strengths and high deformabilities may pre - sently promote a cycle of unsuccesful designs.

At any rate, there have been repeated indications that we should not blindly accept $\emptyset$'und = $\emptyset$'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. 4).

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 uncer - tanty 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 ex - treme values of normally-consolidated K'o approximately corresponding to $\emptyset$' = 30° for Ip = 5 and $\emptyset$'= 5° for Ip = 350 (sodium-bentonite), as well as the asymptotic trend K'o → 1,0 as $\emptyset$' → 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 prag matic observation. At the top are the equations repeatedly quoted in textbooks. A direct comparisons 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). Incidenta- lly, it seems reasonable that K'o conditions be assumed to pre vail 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 <u>disconcerting conclusion</u> of "at rest" behavior up to much lower factors of safety.

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 envelo pe is assumed, including the stretch with cohesion, and if we arbitrarily maintain constant the FS ratio of at-rest "elas - tic" to failure stress envelopes, we could determine trigonome trically 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 com pare different clay soils, we would reapply similar reaso - nings to the varying $\emptyset$' values as function of Ip. Obviously other parameters and reasonings will interfere as more domi- nant. But, how we rest satisfied without testing out the me thod in our madness? (Fig. 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 un- confined 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) consoli dation pressure.

Inevitably came the advances of triaxial shear re- search, associating undrained shear strength directly with (pre) consolidation. Meanwhile a strictly empirical "corre lation" was proposed, and often repeated thereafter, for c_u vs. Pc, the single parameter correlation having been asso - ciated with Ip. Clays were automatically related to plasticity,

therefore the quantification of clayeyness should be reflec-
ted by the plasticity index (indicator of plasticity). Cu -
riously the equations are such (cf. Fig. 6B) that the higher
the Ip the higher should be the cohesion for a given consoli
dation pressure.

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

The first suspicions and questions would be with re
gard to the test values of c_u 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 po
ssible (Plasticity chart). The simplistic derivations shown
in Figs. 5 and 6 are meant to show the importance of investi-
gating regressions of su/Pc vs. the pair of plasticity parame
ters (W_L, Ip). The derivations assume that we might intuiti-
vely attribute trends for the probable interference of the se
cond parameter, not hitherto included in the currently quoted
correlations, Cc = f (W_L) and $\emptyset' =$ f (Ip).

In graph Fig. 6A we would conclude that around the
A-Line the undrained strength su at Wp would vary around 8
kg/cm^2 (apparently too high according to generalized feel of
experience), and that low values such as su (Wp) = 1.7 kg/cm^2
(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 c_u/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 c_u/cp and the simplified
idealized values of su/pc.

Why are clays above the A-line "fat" and "tough"
clays? The suspicion is that the reason why the presumed theo
retical trend results inverted may lie in the fact that cu is
more influenced by "internal porewater tensions" than we ima-
gine from our physical model. Besides the capillary tension
(negative pore pressure) there might be an interference of clay
mineralogical intercolloidal attractions and repulsions in hel -
ping retain the compression energy. Possibly a measure of such
trends could be insinuated by the histeresis loop between each
materials's Cc and Ce. As we presume, the area of such a hys-
teresis does not increase steadily in the direction of increasing Cc

(therefore W_L and/or Ip), but seems to exhibit a dish-shape, going through a minimum with moderately clayey-silty condi - tions.

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

d. Proposed simplified-unified theory for plastici ty indices.

Concepts pertaining to Critical State Soil Mecha - nics have been used to propose a basis of theorization for the significance of the liquid and plastic limit water con - tents of (remolded) clays (e.g. Wroth and Wood 1978). Indeed, since the Atterberg limit tests have generally been conside- red crude empirical index tests, it does capture the imagina tion to find the two absolutely independent values roughly associatable by a unified theory. One thus finds the propo- sal that "the index properties (be) logically redefined sim - ply and directly in terms of the undrained strength of the soil", and that "the rationale for redefining the plastic li mit 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 shea - ring strength, of as modern a theoretical model as the Cam- clay critical state theorization! Yet, for purposes of every day 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 lo- gical derivations depend heavily on assumed simplifications and average conditions (N.B. the A-line was initially a pro posed 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 "cons tant"? Definitely not (Youssef et al, 1965, Wroth and Wood 1978); a variation from 25 to 13 g/cm^2 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 mea sured at W_L and W_p really nothing but the conventional undrai ned 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-bento nite reasonably evidence an opposite effect of higher "impact" shear strength?

Many such questions may be raised before the geo-technical professionals could feel confident that a reasona · bly full range of conditions has been covered by the elegant young theory, so as to decide to pass the baton in the re - lay-race of competing theories.

But, the main point I could raise regards specifi- cally the intent. Do we not recognize 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 differentia ted taxa? Is not the demonstration that a single mathemati - cal simplified relationship could depict "all clays" a desire diametrically opposite to that of identification and classifica - tion of differences?

Quite definitely we should want to impro ve test techniques to decrease erratic errors; but not to su- ppress 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 ba - dly conceived, because it compresses 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 pro - blem.

4.3 Pseudo-statistical correlations, and engineering needs.

The place of CORRELATIONS is very important in en gineering as a sequel to the use of PRESCRIPTIONS for work- ing solutions. PRESCRIPTIONS provide broad UMBRELLA SOLU- TIONS, on the conservative side, so that we can exercise en gineering decision and action by guaranteeing that the solu tion is better than the "minimum necessary": thereby 1 have emphasized that in Civil and Geotechnical Engineering, expe rience is predominantly accumulated from the "silent majori ty of cases" that do not cater to any publication paper at all; thus we need not be too disheartened at the inability to predict what will or should happen, because it is generally sufficient to predict what will not happen. However, econo- my in Civil Engineering, and especially in Geotechnical Engi neering, is of crucial importance to Society and its cost of li ving: what matters most is first costs, and buried first costs, that act as the first insurance premium on everything there- on and thenceforth supported.

Thus arises the importance of CORRELATIONS: corre- lations should help us get closer to the limits of impunity,

by improving our ability to predict what will probably happen.

Obviously correlations have to be statistical. Soil mechanics and soil engineering have gradually and very slow - ly risen to such recognition. But are we deriving and emplo ying statistical correlations in a satisfying manner?

The most general answer is a resounding NO. Appli cations hitherto fail to satisfy either the men of experien- ce who are frequently able to estimate "prior probatilities" (Bayesian) and also "posterior probabilities" (the experien ced Observational Method) of significant parameters and re - sults within narrower bands of uncertainties than the publi- cations and "data" suggest; they also fail to satisfy the prac ticing geotechnical engineer who would be at a loss to have to decide on projects of responsability under such broad disper sions.

Recent publications teem with statistical regres - sion equations and graphs such as the ones selected at ran - dom 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-parameter 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 radical, they tend to follow moderately smooth trends of va- riation. (Incidentally, however, when geology does present an abrupt discontinuity, 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, be cause of the very small scale of most geotechnical tests, and because of the destructive ability of men and machines, tests tend to suffer and reflect variations more erratic than final ly observed in prototypes.

As an illustration of such experience one might re fer to data reproduced from outstanding publications, and qui te representative of dispersions of behaviors of footings on sands (Fig, 10) and/or of parameters of bored and driven pi- les in thoroughly investigated conditions (Figs. 11, 12), Dispersions appear disheartening. However, the silent majo- rity of successful foundations designed under much less meti culous studies would not confirm the probabilities of signi- ficant differential settlements.

b. An impressive number of publications furnish the regression equations for the correlation merely between the average values of X vs. Y. The least that could and should be done as a complement is to furnish the % confidence bands as tride the average. A PRESCRIPTION can only be interpreted to be an upper or lower bound recommendation, conservative: there fore, if we wish to substitute a CORRELATION for a PRESCRIP - TION, in fairness we must use an equation of an upper or lo- wer 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 popular houses for subsequent sale, it may be quite appropia te to work with confidence bands on averages; alas, however, for the engineer building a single house for a specific client it would be rather unfair to discuss anything but probabili- ties of a single event.

c. Because of many factors, including the above, it is comprehensible that in the hope of improving correlations, a number of specially dedicated workers have turned to collec ting 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 statisti- cal 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 meaning ful 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'o etc.) from site to site would add to the scatter of indivi - dual 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 para- meters of relative significance that are not explicited. Therein lies the most frequent source of error and frustra - tion 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 specific equa tion (etc.) without some attempt (s) to insert adjustment fac tors, hopefully reasonable. Unfortunately the more earnest the geotechnician, the more he stand likely to be instrumental

to the zealous importation of unadapted and unadaptable equa
tions.

Need one comment on the seduction of log-log plots
for linearizing regressions and for disguising the true widths
of dispersion?

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 diffe-
rential settlements, distortions. Although but scattered re
ferences signify that the only concern is with tensile crac
king (that can only occur near the top) since shear-plane dis
placements 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 con-
fusions to be expurgated, generated by mere irrational word
associations when a word is vaguely defined.

What is really desired is the "plastic behavior" un
der 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.

It is in the next step that the shocking confusion
arises because plasticity behavior is confused with plastici-
ty 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 beha-
vior at a given condition (temporary), say as compacted at a gi
ven water content, say the Proctor optimum compaction water
content. It so happens that soils of high Ip have to be com-
pacted 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 in a word association between plasticity index and plastic stress-strain behavior at the water contents as compacted (Fig. 10).

5.2 Footing foundations, sand

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 (from 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 unkowns (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 reproducted 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 behaviour, 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 truly remarkable simplifications of earlier times which should have been recognized but were clouded, and therupon 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 problems were "cohesion" under lateral stress release, seepage, and "stand-up time". Strangely the emphasis of soil mechanics theorization, related to soft satured 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 sub soil profiles the parameters of cognizance recognized in the two arbirarily quoted periods (c. 1946 and c. 1969) that represented reference milestones. In comparison, a presentday 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 1969 etc.) have been the emphasis on effective stress analyses and pore pressures (flownet u plus Δu due to shearing ΔV), appropiate stress-path testing, recognition of the importance of pore-air (S%), recognition of the range of va riation and importance of $K'o$, and, finally, at the crest and in the wake of the computational wave, the "elasticity" para meters (E, v), and so on.

It has been contended repeatedly that once a theo retical reasoning establishes the backbone for a certain analysis-sinthesis, the engineering method requires we use that backbone for filling in the muscle and the trappings of expe rience. We cannot condone with Indices (either oversimplified, or complex-lumped-parameter) that do not fit into theo rization, 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 ra
tionale as an engineering technique, two crucial design ques
tions 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 condi -
tion of different degrees of proximity to provoking a failu-
re at face and/or roof. Moreover it is particularly criti -
cal 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 $s = c \approx 0.5 q_u$). Routi -
nely one is led (Peck, 1969) to look for a Stability Number
(Broms and Bennermark, 1967).

$$\frac{\gamma z - p_a}{s_u} \not< 5 \text{ or } 6$$

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 fol-
lows closely the Bjerrum and Eide (1956) paper, clearly re-
presents a significant contribution for its time and for the

very specific idealized problem envisaged. It concerned satu
rated plastic clays (s = c, $\emptyset$ = 0 undrained), normally conso
lidated (overburden total σ_v as the principal driving stress),
and clearly demonstrated the association of the face stabili-
ty with a bearing capacity formulation, cN_c. In subsequent
discussions herein we shall limit ourselves to simple bidimen
sional conditions in order to elucidate comparative conditions
at play. In the same way as is generally done in bearing ca-
pacity formulations, the circular face stability can be esti-
mated from bidimensional formulations by use of adjustment fac
tors and shape factors (often extracted from analogous situa-
tions).

The Broms and Bennermark tests were literally extru
sion tests. There is the (conservative) assumption that fai-
lure caused by increasing σ_v would preserve the same maximum
deviator stress (function of q_u and cohesion), as failure cau
sed by decrease of σ_v external. This assumption is idealized,
because in practice there is a tendency to compress and gene-
rate positive pore pressures in the first case, whereas in the
second, any tendency to expansion at the face would immediate
ly create capillary tensions. There is a significant question
regarding the method used to simulate confining pressure:
"Confining presure was used to investigate the effect of com-
pressed air to prevent a cohesive materials 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 sur
face tensions had merited some attention in the early 1950's.
Unfortunately, however, they are generally eliminated in idea
lized laboratory conditions, and/or often overlooked.   Some
representative data are summarized in Fig. 16, just as a remin
der. The special importance of compressed air at a tunnel fa
ce 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 favoura
ble seepage pressures to complement the favourably propaga -
ting capillary tensions.

In the submerged saturated clayey sands of Sao Paulo,
laboratory tests indicated that although under very small gra
dients (about 0.2) practically no change of moisture content
W% was caused (about 0.2%), under much higher gradients (up
to 30) decreases of 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. 17 a,b. As is well recognized, com-
plete 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.

The first basic fact regarding failure under stress release is that, as a general principle, materials exhibit loading unloading hysteresis (in greatly varying degress), and, therefore under conditions of unloading there is always some "cohesion intercept" and $\emptyset = ds/d\sigma$, 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 experience.

One adjustment factor that could be applied to the $s = c$, $\emptyset = 0$ Stability Number, in consideration of an applicable $\emptyset$ value, has been suggested by Rebull 1972. The comparative influence is indicated in the graph of Fig. 16 (c). 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 appropiate 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 flow nets and analyses have been prepared for the two-dimensional conditions (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 fownet 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 compara
tive. 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 equipoten -
tials.

Finally in Fig. 23 we summarize the comparative "mass
statics" that should give a feel of the influences of differ-
ent drainage and/or compressed-air treatments. Assuming that
the resultant Σ (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 bound-
ary 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 occasionally even invert) the effective stresses due to
seepage. The results indicate trends only, because we must
carefully distinguish between artificies 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 co
rresponding strains (compressions and expansions, void ratios)
have materialized. In a perfectly saturated ideal clay the
undrained instantaneous changes of pore pressures do not gene
rate any changes of in situ strengths.

Many an important conclusion, intuitive in tunnel-
ing practice, may be drawn, not only regarding the overall
"rigid body statics" but also regarding locally critical fail
ure 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 ten -
dencies 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 realities 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 sig
nificantly 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 unload-deformation of plates supporting vertical faces (anal ogous 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 Sao 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 interviening factors; (c) the summary table of "all" the data available, with the candid·confession that "the infor mation ... is surprisingly meager", and with appropiate 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 to idea-fertility. We must foster some minimum theoretical analysing on the differ ent parameters associatable with the full-scale field observa- tions, since progress in design procedures and predictions will only be achieved if we set about to dispell the unnecessarily pessimistic forecast "Because of the dependence of loss of ground on construction details, there seems little likelihood that theo- retical investigation will prove fruitful except for some of the simplest of materials such as plastic clays" (p. 245). Although PRESCRIPTIONS do constitute the valid base for design develop- ments and decisions, they must be rapidly adjusted by statis - tical CORRELATIONS on observed behavior in order to permit re- vision 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 dan gerous unless it is based on theorization on the physical mo- del, to establish the nature of the equation and its coefficients.

Surely it is accepted that in tunneling we automati cally face a greater proportion of strictly localized condi - tions of heterogeneity and possible failure (loss of ground), as is herein emphasized under item 6,1 regarding FSI of indi- vidual points or fractiles on a histogram. Such conditions are those that must either be bearable and borne as risks unquanti fiable, or must be resolved in design and construction by " a change of statistical universe" (i,e. a treatment that essen - tially exludes the problem), Our design engineering concern can only be with conditions that permit averaging, and quanti- fications based thereon. The fact is that settlements most often distribute well enough to validate statistics of averages.

Fig, 25 summarizes the Peck 1969 prescriptions regard ing the settlement trough. The basic points are; (a) geome - try, dimensions; (b) a Gaussian curve of settlements and no in dication 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 geotech nical 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 condi- tions, 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 occurrence 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 re - present 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 loss, 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 speheres, and visualizing the cave-in as the downward movement of one sphere, he obviously concluded that the subsidence profile at surface could be re - presented 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 phe nomenon become identical to those of many other totally dis- tinct phenomena: for instance, the classical similarities between Darcy-Laplace seepage flownets and the electrical ana logy models, or arrangements of iron-filings within appropiate 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), especially 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 con - centrates 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 au- thors).

In fact, even for the "collapse of cavity" condi - tion it should be recognized as much more conducive to fruit- ful experience collection and collating, if instead of adopting a geomechanically sterile stochastic postulation (dissociated from parameters physically comprehensible and derivable) autho rity had fostered resorting to plasticity formulations ("col- lapse of cavity" as an inverse of the widely recognized solu- tions 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 suffi-
cient to illustrate the obvious.

Peck's candid recognition (p. 231) "It is not yet
possible ... to apportion the lost ground between the inevi-
table movements associated with a particular method of cons-
truction, 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,) con-
ditions along each tunnel (constant construction technique
universe) in order to separate, as in hydrology, the " peak
flows from the base flow".
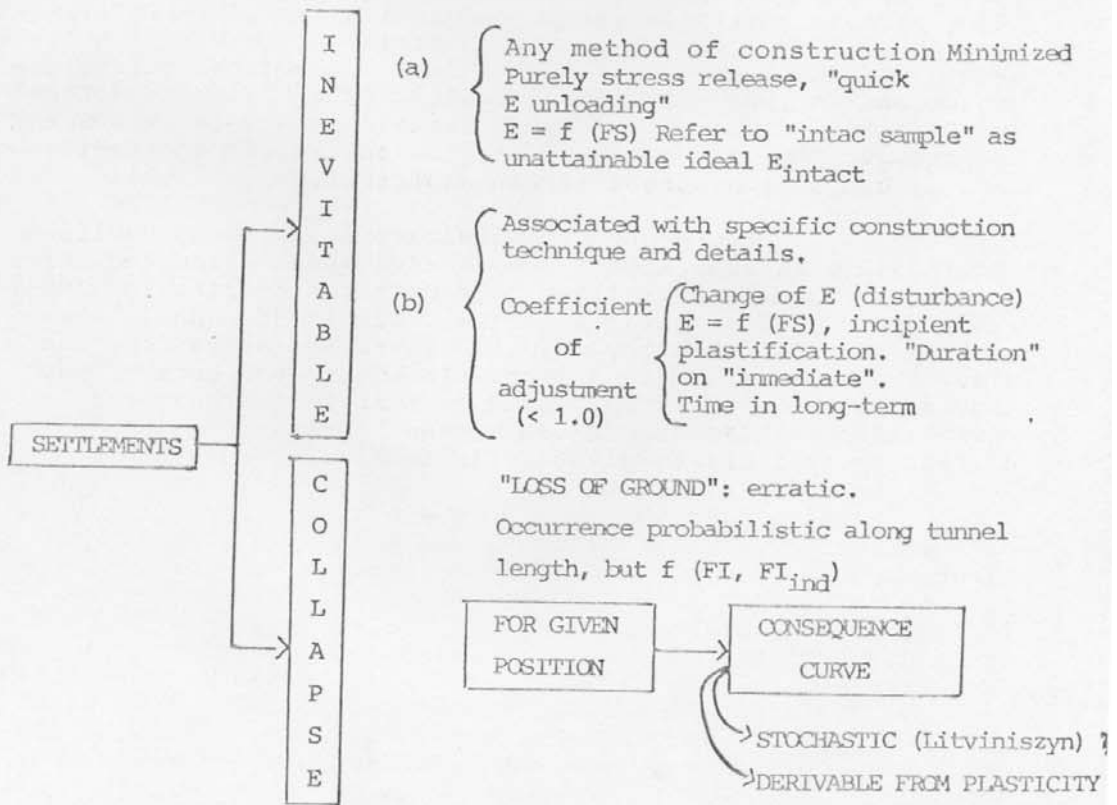
In this respect it could be a sadly moot question
wheter it is an advantage or disadvantage that the Litviniszyn
collapse formulation should lead to exactly similar distribu
tion of settlements as the elastic and elasto-plastic solutions.

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 liminc 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 esta - blished 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 attenua - tion, 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 corre lation of Peck's data, without recourse to theorizable intui tion. Digestion of data from widely different tunnel case histories will inevitably lead to statistics at random, confu sion, 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 in- dicated that even assuming unchanged geotechnical behavior pa- rameters, there is a net difference between considering the face-plate support (or membrane boundary loading) and the rea listic use of body stresses, effective stresses due to gravi- ty composed with those due to seepage. Deformations are not equivalent. The routine computational artifice is perfect for rigid 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-or-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'o and deformabilities, how to adjust the prescriptions to diaphragm walls (rigid-continuous, 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 de - formations.

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 restric (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 of 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 teast 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 bet
ween at least three nominal Factors, that of Safety (conventio
nal) 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 histo
gram), the ratio of resistance to predicted stress is no lon⁻
er 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 Guaran
tee FG in comparison with the routinely defined FS = (Resis ⁻
tance ± 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 pro
blems are often related to values of FI instead of FS.

For obvious innate psychological reasons our data
collection of allowable vs. unacceptable behavior will con-
tinue to require association with nominal Factors of "how dis-
tant the critical predicted condition will lie from the limit".

6.2 Concentrated attention on meaningful histograms of non-failure behaviors

We must ceearly 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 indi-
vidual value systems (inexorably varying). We have wasted too
much effort in the childlike quest of the "bang and fireworks"
of sudden failure: it is comprehensible, but "when I was a
child ,,.", and it is time that we grew up into adult attitu-
des. For instance, if we want to investigate embankments on
soft clays we should observe the varying behaviors 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 real
ly choose what to monitor be it deformations, or micro-acoustic⁻
emissions, etc,, so that it is significant, opens an easily
discernible wide-spectrum, and is preferably easy and cheap.

For instance, in discussing allowable (or unaccep-
table) 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 changes of dif
ferential 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 distor-
tions due to differential settlements of two adjacent columns
inevitably attenuate from floor to floor, a significant statis
tical 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 if zoology of all bipeds were
statistically analyzed as a single universe?

Two example may suffice. The list is long; in fact,
in almost all projects we have lumped together significantly
different conditions in single universes merely because of the
cloaks of similar names. Why is it so difficult to correct
such absurdity? Because of both Engineers and Clients. How
difficult it is to design and build a long dam with the same
slope varying longitudinally, say from 1:2 to 1:2.2, to 1:2.5,
to 1:2.8 at every hundred meters or so, just for the purpose
of collecting conscious data on varying non-failure deforma -
tion behavior, to prod a little and push a little our defini-
tions 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 ob-
servations 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 statemnt 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 sin-
gular, and "blushes". Close to zero of any parameter, dis-
persion and errors abound. What we have to do is to concen-
trate 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 ... every body is interested in the appearance of a crack, or can easily be invited to such pleasant cooperation. Right after the crack (associable to differential settlements) appears, we can instrument to observe its rate of growth; concomitantly we can instrument to monitor the settlements of the two adjacent columns. Moreover, we can monitor differential settlements at the levels of many floors above and below the said occurrence, and can be alert for similar cracking developing on other floors. Such are meaningful observations of $\partial C/\partial(\Delta\rho)$ where C = crack and $\Delta\rho$ = differential settlement, under conditions of as nearly the same physical universe as possible.

Laboratory research has led very fruitful conclu - sions 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 of 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 cycle 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 on 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.

Quality of Samples	1980		1940	Construction
and Tests	1970	?	1960	Equipment
	1960			Capacity to
	1950	?	1980	"Dispense"
	1940			With Soil
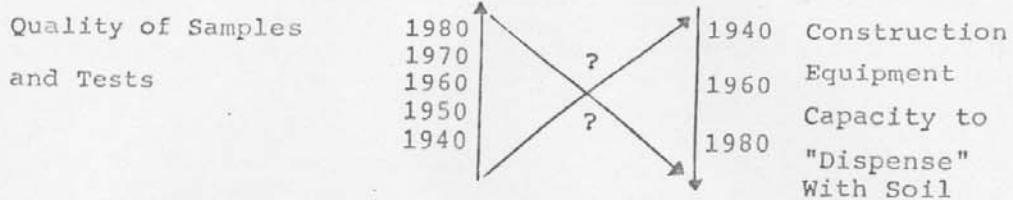
        In Fig. 14, I have reproduced the results of a simple
analysis used a long time ago in an attempt to refer UU strengths
to a presumed common reference of "undisturbed quality". Around
1953-56 I had opportunities to sample and test a significant
volume of shelby samples of foundation clays, and obviously
noticed the relationship between percent strain at failure and
the degree of disturbance, as indicated by so-called partial
sensitivities St_p. The test data were analysed statistically,
assuming regressions variable with nominal St of a presumed mi
nimally disturbed specimen. Thereupon the resulting coefficients
and regressions were used repeatedly to estimate a presumed "per
fectly undisturbed" specimen's behavior as corresponding to a
failure peak at 1% strain. These were candid working hypothe-
ses wich served a purpose, and may yet continue to serve, with-
out a presumption of "research truth". The surprising fact,
however, is that even in clays of moderate to high sensitivi -
ties, 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 asses
model-prototype conditions, and to obviate the disturbance as-
sociated with sampling and handling. The dynamic spoon penetra
tion testing (SPT), the static cone penetrometer (CPT) and its
developments (including local side friction, LF, for identifi-
cation, and especially the CPTU as a multiple profiler), the
recent Marchetti dilatometer, the vane shear test, the pressu-
meter (pressiometre) with multiple applications, the K'o pro-
filing (e.g., camkometer), the in situ permeability testing
by pumping-in and pump-out techniques, and finally load-defor-
mation tests, are a day-to-day array of expedients upon which
our designs are based. Oceanographic subsoil investigations
have employed much more multiple profiling, and could open much
greater promise if they recognized the errors, consistent and
erratic, of conventional soil mechanics tests.

All of these were developed under rational prognos-
tications, but, as was inevitable, under so simplified a theo
retical basis that only gradually have the illusions been ex-
posed. The great problem we face is to develop methods for
assessing quantifiably the qualities of the work. The ear-
ly association of disturbance with samples and therefore sam
pling, led to the search for in situ testing under the wish-
ful thinking of illogical associations:
Spurious logic:

    samples → disturbance

  . . non-sampling ≡→ non disturbance

  . . in situ testing does not sample

  . . in situ testing ≡ non-disturbance . .   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 in-
troduce compensations and/or mangifications of errors of ini-
tial 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 se-
veral times side by side at distances on the order of a couple
of meters, for assessment of dispersions: neither have there
been reports of cluster of such in situ tests compared side by
side. In comparison with laboratory tests, the principal pres
ent failing of in situ tests is of never having been applied
before and after a given loading, to check on their ability to
reflect changes of conditions,

6.6 Extending theorizations for soil behavior

    Principal well-known factors of influence for the
near future may be mentioned as Structure, Porosimetry, air-
pores, time effects, cementations. Lack of inclusion of these
effects is responsible for most of the unexplained scatter and
discrepancies. I exclude discussion of in-situ stresses as
affecting all of the fundamentally rational concept of stress-
strain-time-testing and consequent design calculations, 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 alrea
dy instrinsically in the rejection of a perfectly homogeneous
natural condition in situ: not only do average γ'z vertical

stresses suffer considerably variations due to differentia
ted deformabilities and stress redistributions, but also the
other elusive horizontal stresses will be found highly varia
ble (within the viable range).

Therefore, in soil mechanics, I propose that the
principal new parameters for us to investigate more thorough-
ly are those that may significantly affect our very accep -
tance (automatic) of such initial dogmas as the Terzaghi
effective stress equation, the traditional grainsize analy-
sis, etc. The soil parameters mentioned affect a large pro
portion of the world geography and geology, and air pores are
what matters for compaction and for many a soil streatment
Grain nucleations and crumbs often dominate behavior; macro-
pores often influence behavior significantly.

In caricature we could say that in an early period,
soil mechanics was principally concerned with solids (indi-
vidual 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 some advance of our investiga-
tion 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 bet-
ween 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 remark-
able 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 extend
ed to involving rapid flow of hundreds of thousands of cubic
meters of mud with village houses floating on it.

The fact is that despite our proud structures that
call attention to themselves, the vast majority of populations
live close to Nature's equilibrium of no-greater than nec-
essary. 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 sophistica- tion 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 conser- vative; 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 coopera- tion? 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 hundred of millions that of living in the more liveable world we already occupy?

BIBLIOGRAPHIC REFERENCES

1. BJERRUM,L, & EIDE, O, (1956), "Stability of Strutted Excavations in Clay", Geotechnique, 6 (1); 32-47, Mar.1956,

2. BJERRUM,L, & SIMONS,N.E. (1960), "Comparison of Shear Strength Characteristics of Normally Consolidated Clays", Research Conference on Shear Strength of Cohesive Soils, Boulder, Colorado, Jun. 1960, pp. 711-726.

3. BROMHAM,S,B, (1971), "The Measurement of Disturbance in Samples of Soft Clay", 4th Asian Conference ISSMFE, Proc. of Specialty Session 'Quality in Soil Sampling', Bangkok, Jul. 1971. vol.1, pp. 68-72.

4. BROMS,B.B. & BENNEMARK,H. (1967), "Stability of Clay at Vertical Openings", Journal of the Soil Mechanics and Foundations Division, ASCE, 93 (SM1); 71-94, Jan. 1967.

5. CARRILLO . N, (1969), "Hundimiento en el Area de Long Beach-San Pedro, Cal, A, El Efecto de un Centro de Tensión", Contribution of Texcoco Project to the VII International Conference on Soil Mechanics and Foundation Engineering, Mexico, 1969, pp. 15-45.

6. D'APPOLONIA, David J & LAMBE, T.W. (1970), "Method for Predicting Initial Settlement", Journal of the Soil Mechanics and Foundation Division, ASCE, 96, (SM2); 523-544, Mar. 1970.

7. DE MELLO, Victor F.B, (1973), Panel Discussion of Question 42: Impervious Elements and Slope Protection on Earth and Rockfill Dams, 11th International Congress on Large Dams, Madrid, Jun, 1973, vol. V, pp. 394-406.

8. DE MELLO, Victor F.B, & HAUSER, Ernst A. & LAMBE, Thomas W. (1953), "Stabilization of Soils", United States Patent Office, Patented Sept. 8, 1953. (U.S. Patent N° 2,651,619)

9. DE MELLO, Victor F.B, (1979), "Soil Classification and Site Investigation", 3rd International Conference on Applications of Statistics and Probability in Soil and Structural Engineering, Sydney, Jan/Feb. 1979, vol. 3, pp. 123-144.

10. LITWINISZYN, J. (1956), "Application of the Equation of Stochastic Processes to Mechanics of Loose Bodies", Arch. Mech, Stosow. N°8, pp. 393-411. (in Szechzy, 1973).

11. PECK,R.B. (1969), "Deep Excavations and Tunneling in Soft Ground", 7th International Conference on Soil Mechanics and Foundation Engineering- State of the Art Volume - Mexico, 1969, pp. 225-290.

12. REBULL, P.M. (1972), "Earth Responses in Soft Ground Tunnelling", Specialty Conference on Performance of Earth and Earth-Supported Structures, Lafayette, Indiana, Jun.1972, vol. I, Part 2, pp. 1.517-1.535,

13. SCHMERTMANN, J.H. (1955), "The Undisturbed Consolidation Behavior of Clay", Transactions of the ASCE, 120, 1955, pp. 1,201-1,233.

14. SHOFIELD, A. & WROTH, P. (1968), Critical State Soil Mechanics, New York, McGraw-Hill, 1968.

15. SILVEIRA,E.B.S. & GAIOTO, N. (1969), Discussion 7th International Conference on Soil Mechanics and Foundation Engineering, Mexico, 1969, vol. 3, Main Session 4, pp. 367-369,

16. TERZAGHI, K. & PECK,R.B. (1967), Soil Mechanics in Engineering Practice, New York, John, Wiley & Sons, 1967.

17. YOUSSEF, M.S. et al (1965), "Relationship between Shear Strength, Consolidation, Liquid Limit and Plastic Limit for Remoulded Clays", 6th International Conference on Soil Mechanics and Foundation Engineering, Montreal, 1965, vol.1, pp. 126-129,

18. WROTH,C.P. (1975), "In Situ Measurement of Initial Stresses and Deformation Characteristics", Proc. of the Conference on In Situ Measurement of Soil Properties, Raleigh,N.C.Jun.1975, pp. 181-230,

19. WROTH,C.P. & WOOD, D.M. (1978), "The Correlation of Index Properties with some Basic Engineering Properties of Soils", Canadian Geotechnical Journal, 15 (2): 137-145, May. 1978.

	A - CLAY TYPE	r	⊕	B - STRESS CONDITIONS	r	⊕	A - B - C	r	⊕
NORM. CONS n= 32 + Δ	0,0182(WL-18,05) CORRELATION 1	0,74	3⁴	C - PHYSICAL PROPERTIES (C_R)					
				f₂ 0,3299 + 0,01688 WL - 1,0817 (f'z)	0,80	1⁴	f₂ 0,1802 + 0,0026 WL - 0,1700 (f'z)	0,71	4⁴
				Pc - 0,232 + 0,01F22 WL - 0,3674 (Pc)	0,75	2⁴	Pc 0,04654 + 0,001704 WL + 0,0206 (Pc)	0,65	5⁴
				Eₐ Cʀ = 0,00275 (WL + 30,58)	0,65	5⁴			
Precon n= 13 3400rapc ○	0,007364(WL+8,74) CORRELATION 3	0,36	3⁴	f₂ 1,385 + 0,004054 WL - 1,115 (f'z)	0,51	1⁴	f₂ 0,364+ 0,0002587 WL - 0,1091 (f'z)	0,29	4⁴
				Pc 0,7533 + 0,007444 WL - 0,1855 (Pc)	0,42	2⁴	Pc 0,3001+0,0006 724 WL - 0,02224 (Pc)	0,23	5⁴
				Eₐ Cʀ = 0,0005767 (WL + 488,30)	0,19	6⁴			
ALL DATA n= 45	0,01426(WL-1,842) CORRELATION 2	0,63	2⁴	f₂ 0,4268 + 0,01272 WL - 0,6561 (f'z)	0,66	1⁴	f₂ 0,2044 + 0,001798 WL - 0,04214 (f'z)	0,54	3⁴
				Eₐ Cʀ = 0,00199 (WL + 74,20)	0,52	4⁴			
# + n= 20	0,02025(WL-14,8310,77 CORRELATION 4		2⁴	f₂ 0,2513 + 0,01809 WL - 0,7954 (f'd)	0,80	1⁴	f₂ 0,1933 - 0,002464 WL - 0,1208 (f'z)	0,71	3⁴
				Eₐ Cʀ = 0,002943 (WL + 38,22)	0,68	4⁴			

⊕ ORDER OF MERIT CORRELATION

+ NORM CONS RECENT SPECIALLY CAREFULL DATA FROM 32 TESTS OF CORREL. 1

FIG 1 - BRIEF INVESTIGATION OF C_C f_z CORRELATIONS IN COMPARISON WITH THEORETICAL EXPECTATIONS

SKEMPTON (1948) | • REMOLDED
$\varphi' = f(n)$ | ▲ UNDISTURBED UPPER
ASSUMED | $W_L \cong 1,05\ W_n$ | + UNDISTURBED LOWER
$I_p = 0,73\ (W_L-20)$

PERS. FILES METRO SP. ⊷ $\varphi' = f(W_L)$
BJERRUM·73 - ⊷ $\varphi' = f(I_p)$
LADD ET AL·77 - ⊞ $\varphi' = f(I_p)$ ▨

CORRELATION 1 — $\varphi' = 37,97\ e^{-0,008(I_p)}$
CORRELATION 2
$\varphi' = 26,02 - 4,537\ ln(I_p)$
CORRELATION 3
$\varphi' = 33,2\ I_p^{-0,013(I_p)}$
CORRELATION 4 — $\varphi' = 41,2 - 6,73\ ln(I_p)$
SIMONS (1963) - 3rd AFRICAN SMFE CONF.-VOL.I

CORRELATION 5
$\varphi' = 12,96 - 2,22\ ln(I_p)$
CORRELATION 6
$\varphi' = 13,39 - 2,28\ ln(I_p)$
CORRELATION 7
$\varphi' = 13,44 - 1,68\ ln(I_p)$

BJERRUM AND SIMONS -
- SHEAR STRENGTH
CONFERENCE - ASCE
(JUNE, 1960)

RANGE
OF φ'
ASSUMED

φ'

I p (%)

FIG. 2   BASIC DATA FOUND ON $\varphi' = f(I_p)$
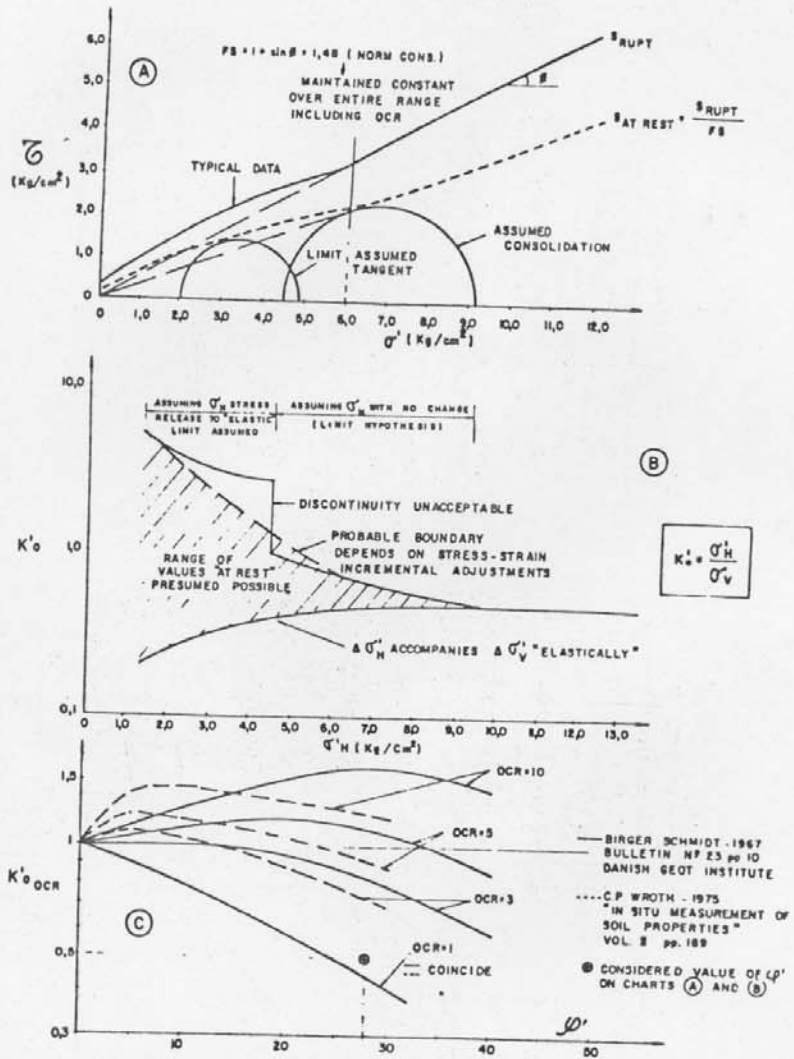
FIG. 3 SUGGESTED DERIVABLE K'_o "ELASTIC" VALUES, OCR CLAYS
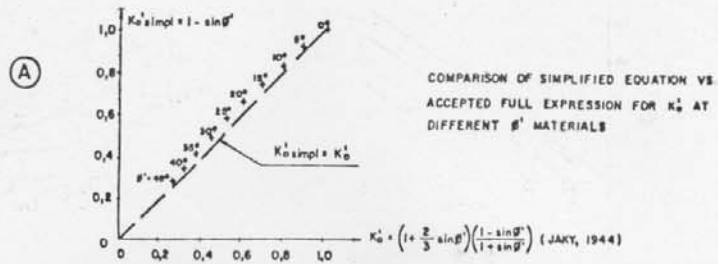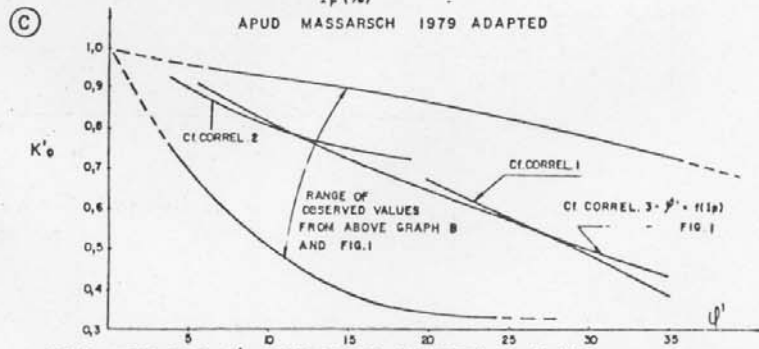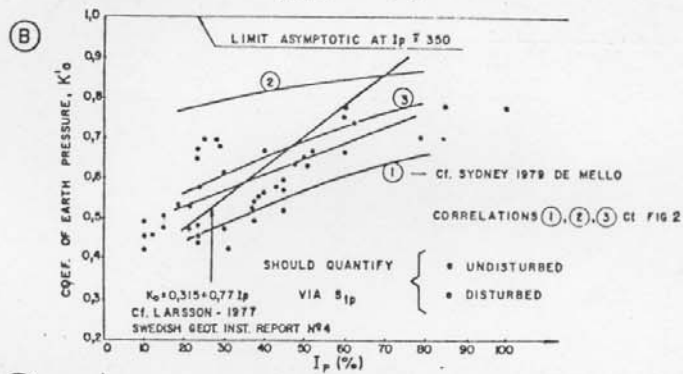
FIG. 4 NORM. CONS. K_o' POSSIBLE FOR DIFFERENT Ip CLAYS
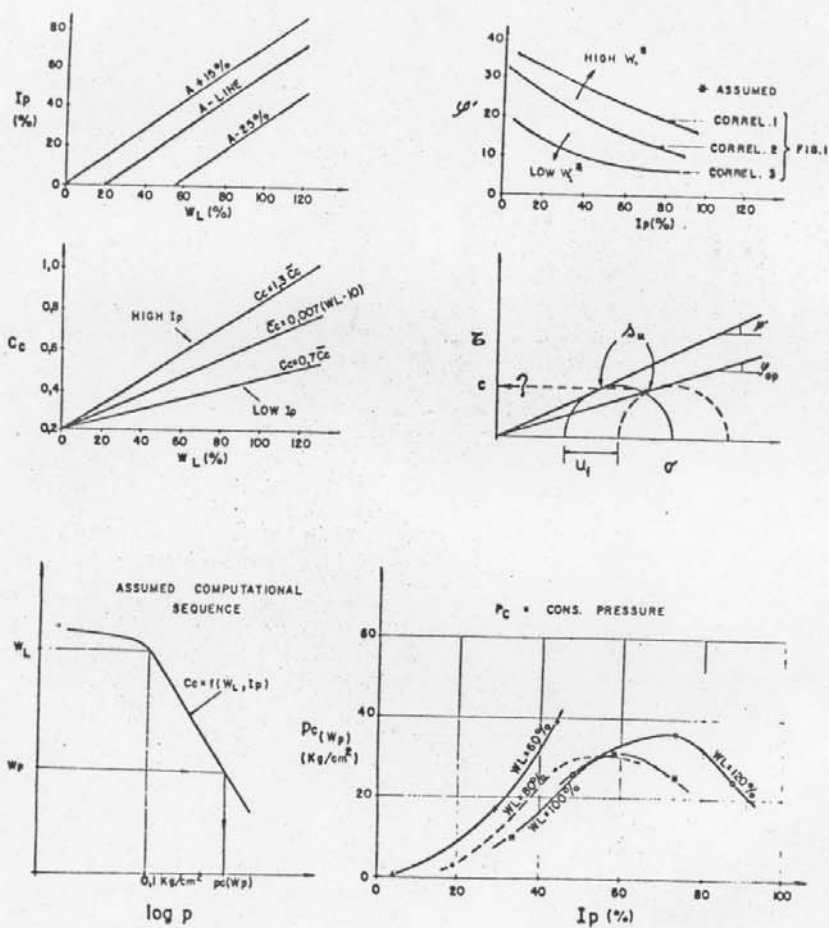
FIG. 5 ATTEMPTED JUDICIOUS INSERTION OF SIMULTANEOUS (W_L, pc)
FOR DERIVATION OF EQUIVALENT CONS. PRESSURE FOR W_P.
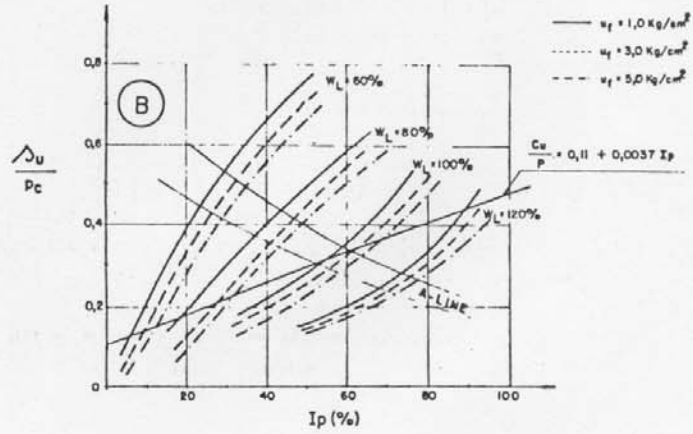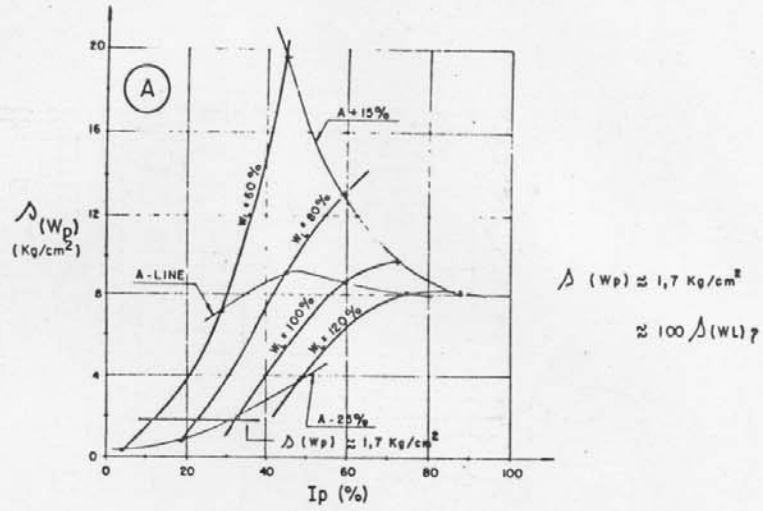
FIG. 6 ATTEMPTED DERIVATION OF $\Delta u_{(Wp)} = f(W_L, I_p, u_f)$ AND
RANGES DEMONSTRATABLY LIKELY
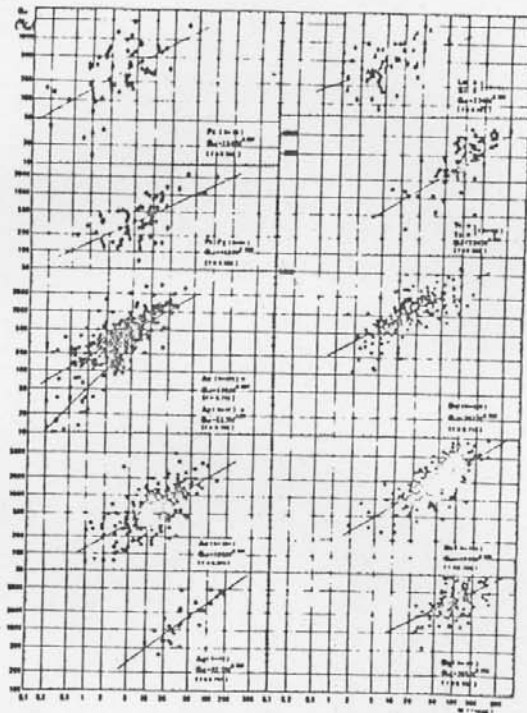
FIG. 7 -   STATISTICAL CORRELATIONS OF SHEAR MODULUS vs SPT

(DIFFERENT SOILS)
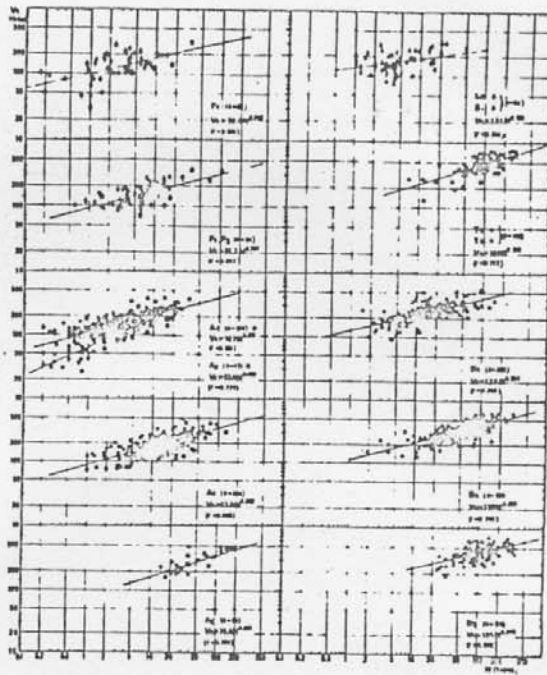
FIG. 8 - STATISTICAL CORRELATIONS OF S-WAVE VELOCITIES Vs SPT

(DIFFERENT SOILS)

N values-pressuremeter parameter relationships   (alluvial clayey soils)
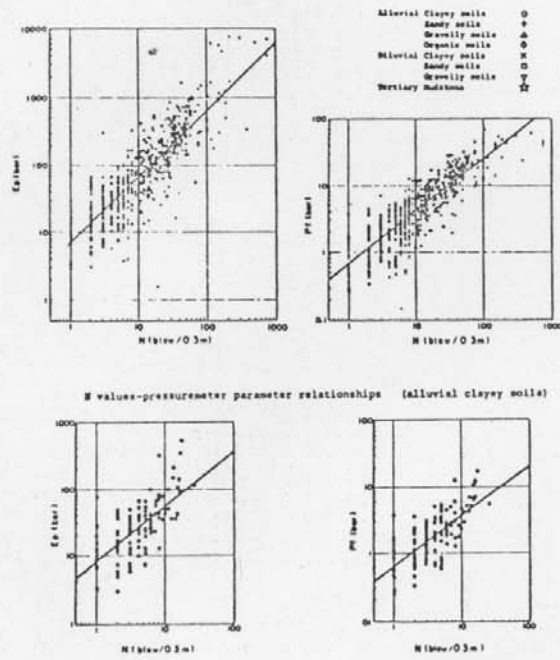
FIG. 9 -   CORRELATIONS   OF   SPT   Vs.   PRESSUREMETER   PARAMETERS

EXAMPLE OF IRRATIONAL WORD ASSOCIATIONS

PLASTIC BEHAVIOR
IN COMPACTED CLAY DAMS

↓

∴ HIGH STRESS-STRAIN
PLASTICITY

↓

∴ HIGH PLASTICITY INDEX ?

BRITTLE

PLASTIC

STRESS

0   STRAIN %   15
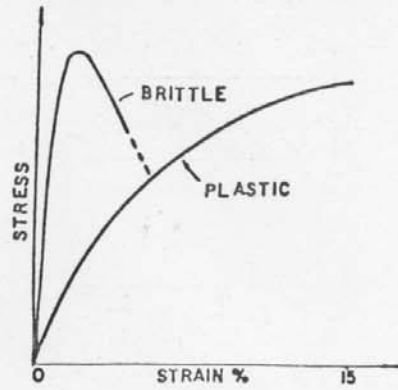
i.e. HIGH $W_L - W_P \equiv$ PROPERTY OF TYPE OF CLAY

$\equiv$ RANGE OF W FOR POTENTIAL PLASTICITY

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

DIFFERENCE $\Big\langle$ TO BE $[SER]^{*}$ ESSENCE

TO BE $[ESTAR]^{*}$ CONDITION, TEMPORARY

I_P (%)

35

30

$W_{OPT} \langle W_P$

20

$W_{OPT} \rangle W_P$

10

$W_{OPT} \equiv W_P$

0

-15   -10   -5   0   5

$W_{OPT} - W_P$ (%)
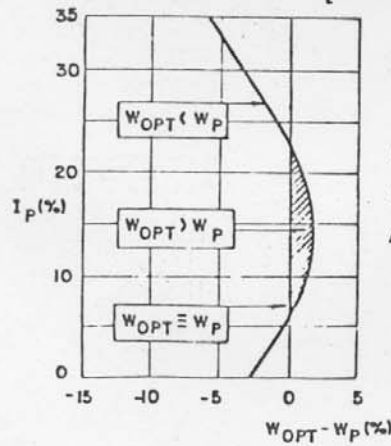
* HISPANO-PORTUGUESE DISTINCTION
OF VERBS NON-EXISTENT IN ENGLISH

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

FIG. 10 - PLASTICITY OF COMPACTED CORE Vs. CRACKING
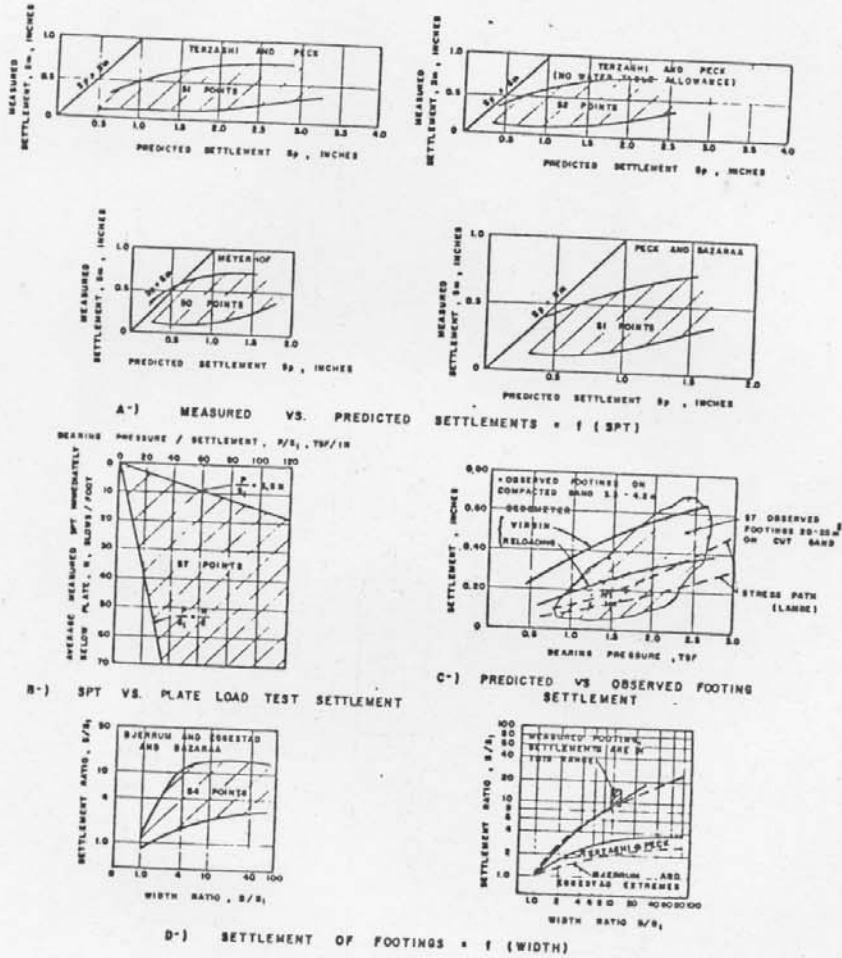
FIG. II   DISPERSIONS OF PREDICTIONS, FOOTINGS ON SANDS
(APUD D.D'APPOLONIA ET AL, JOURNAL ASCE SMFD VOL 94 SM3 1968, VOL 96 SM2 1970)
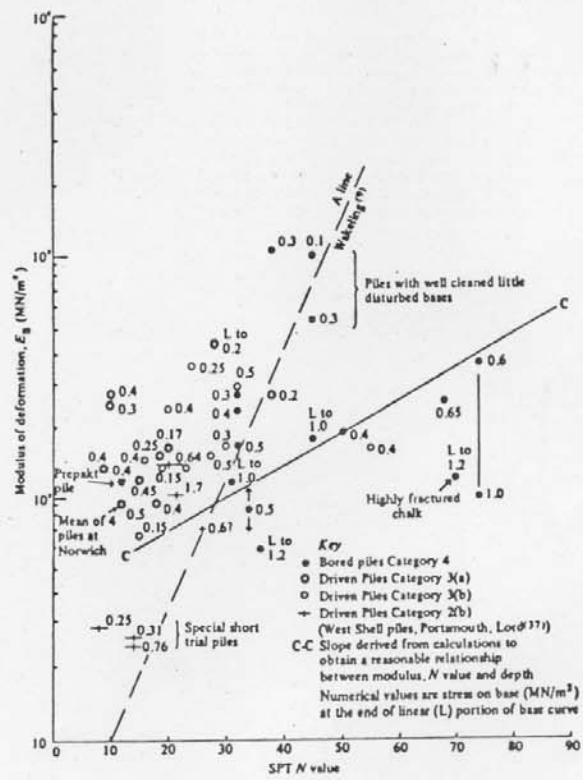
FIG. 12 - PLOT OF BASE MODULUS OF PILES AGAINST SPT N VALUE

AT THE BASE (FOR THE STRESSES SHOWN IN MN/m²)
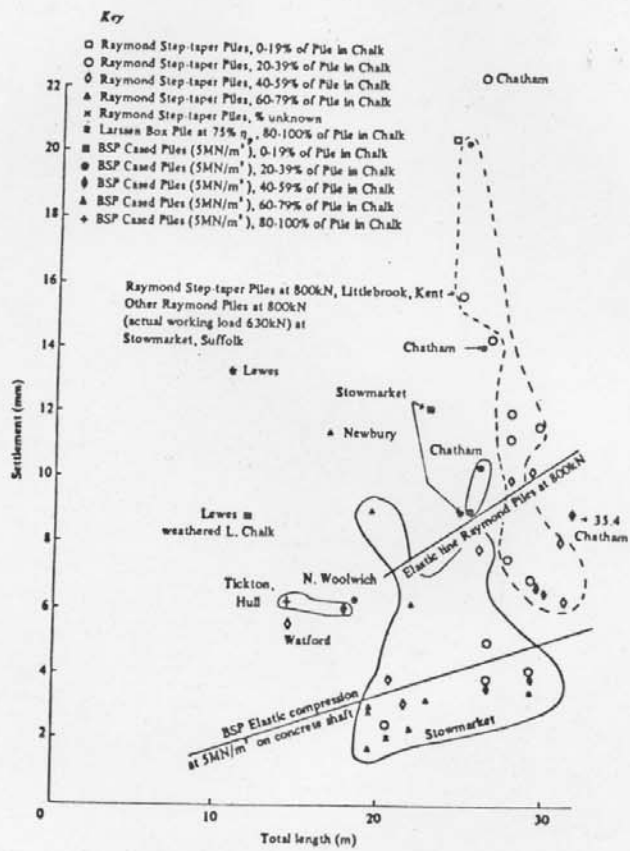
FIG. 13 - PERFORMANCE OF LARGE DISPLACEMENT STEEL PILES
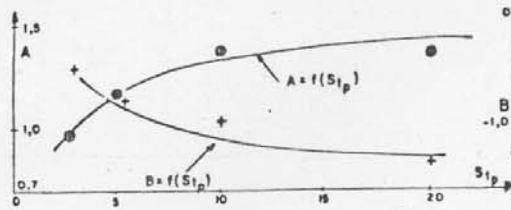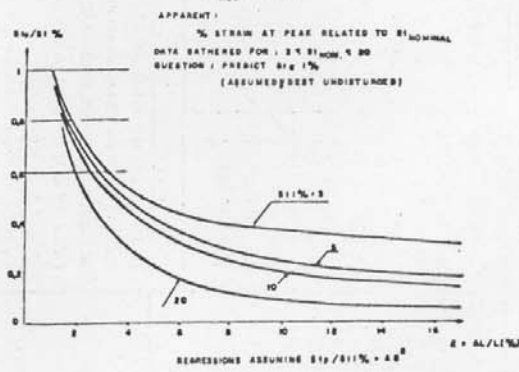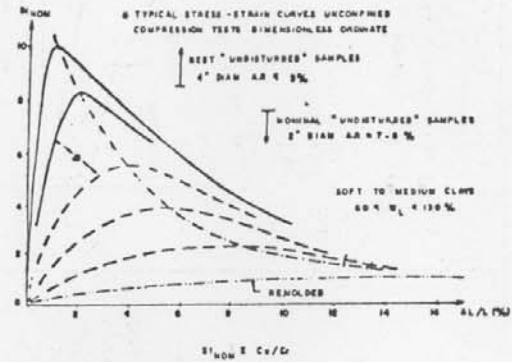
FIG. 14 QUALITY OF "UNDISTURBED" SAMPLING REFLECTED IN PARTIAL S_t

HISTORIC FROM TUNNELING PRACTICE (apud Peck 1969; Terzaghi 1946)			TRADITIONAL SOIL MECHANICS, STATE-OF-THE-ART (PECK 1969)		PRESENT MULTIPLE PROFILING ON GEOTECHNICAL DATA
SQUEEZING SOILS	VERY SOFT TO MEDIUM CLAYS	W.L. NOT OF CON- CERN (?)	SENSITIVE CLAY (INCL. LDA) NORMALLY LOADED	(cf. TABLE 1, Ref.) $C = \frac{1}{2}$ qu (LOADING) Δ%	SPT — estimated s
FIRM GROUND	STIFF CLAYS, CEMENTED OR COHESIVE GRANULAR.		PLASTIC GLACIAL CLAY	(MODERATELY UNDISTURBED SPECIMENS) $\phi_{u} = 0°$	CPT, CPT$_u$ — estimated s, E, C$_v$ UNDISTURBED — PERFECT—INTACT ELEMENT CLAYS
RUNNING GROUND	PERFECTLY COHESIONLESS; DRY SAND; CLEAN LOOSE GRAVEL		FISSURED PLASTIC CLAY (LONDON, BOOM)	SATURATED W.L. U K'$_o$	S% Pc, Cc, Cv OCR K'$_o$ (s) W.L. + U SEEPAGE

STABILITY IN MOST SOILS .... (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 .... SE-CONDARY STRUCTURE OF THE SOIL DEPOSIT..." "SINCE THESE DETAILS ARE UNPREDICTABLE, NO SATISFACTORY CORRELATION BETWEEN F.S. AND MEASURED SOIL PROPERTIES CAN BE ANTICIPATED".

RAVELING GROUND	SLIGHTLY COHESIVE SANDS, FINE SANDS, SILTS (APPARENT COHESION); SAPROLITES.
FLOWING GROUND	RAVELING OR RUNNING GROUND WITH SEEPAGE PRESSURES

SANDS, CLAYEY SANDS, SILTS, ETC.
S%
W.L. + U SEEPAGE
ESTIMATED U$_c$; E; ϕ', C'; Pc; K'$_o$

STAND - UP TIME ?	STAND - UP TIME ?	STAND - UP TIME ?

FIG. 15 - IDEALIZED SUBSOIL PROFILE AND PARAMETERS ESTIMATED FOR FACE STABILITY
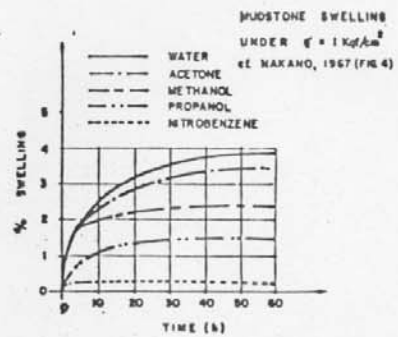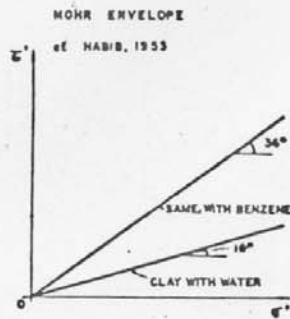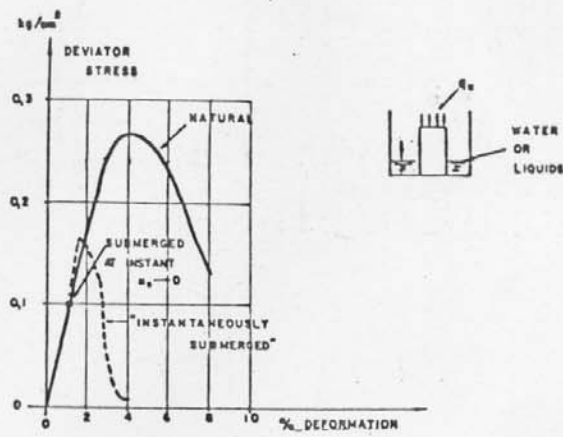
FIG. 16 INFLUENCE OF U_c AND DIFFERENT LIQUIDS IN UNCONFINED COMPRESSION
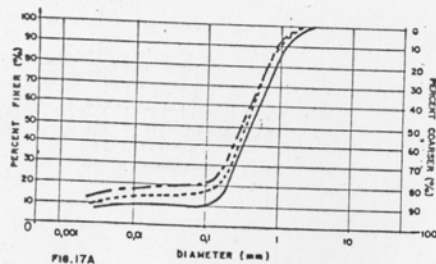
FIG.17A
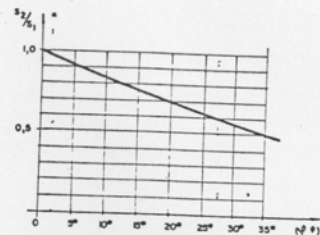
FIG.17C REDUCTION OF S_u REQUIRED FOR FACE
STABILITY IN CASE OF APPLICABILITY OF A φ
VALUE.

$S_1 = S_u$ REQUIRED FOR $S + C$ SOILS $= \dfrac{3?Z}{4}$

$S_2 = S_1 \left(\dfrac{1 - \sin \varphi}{\cos \varphi} \right)$
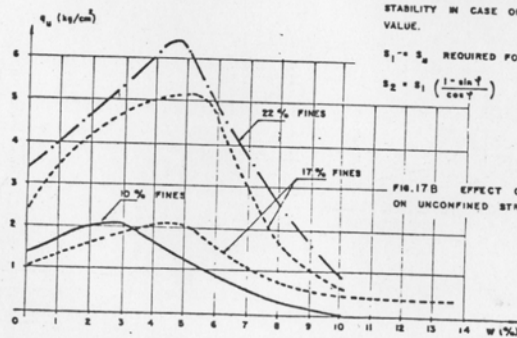
FIG.17B EFFECT OF COMPRESSED AIR DRYING
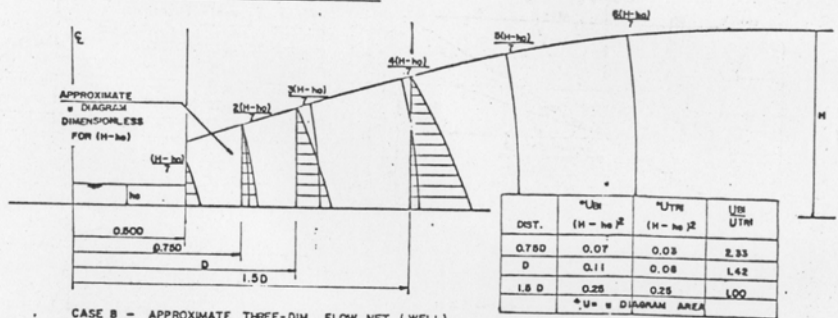ON UNCONFINED STRENGTH, CLAYEY SANDS.

$\gamma_{nat} = 1.93$ to 2.08 (t/m^3)

$W_{nat} = 11\%$

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

CASE A - APPROXIMATE BI-DIM. FLOW NET

CASE B - APPROXIMATE THREE-DIM. FLOW NET (WELL)
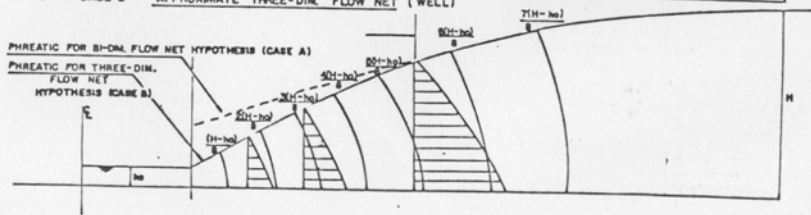
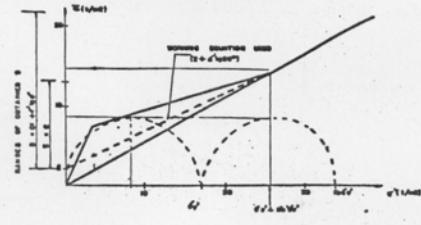FIG. 18 ASSUMING TRANSIENT UNAFFECTED PHREATIC AT 1.5D AHEAD OF FACE

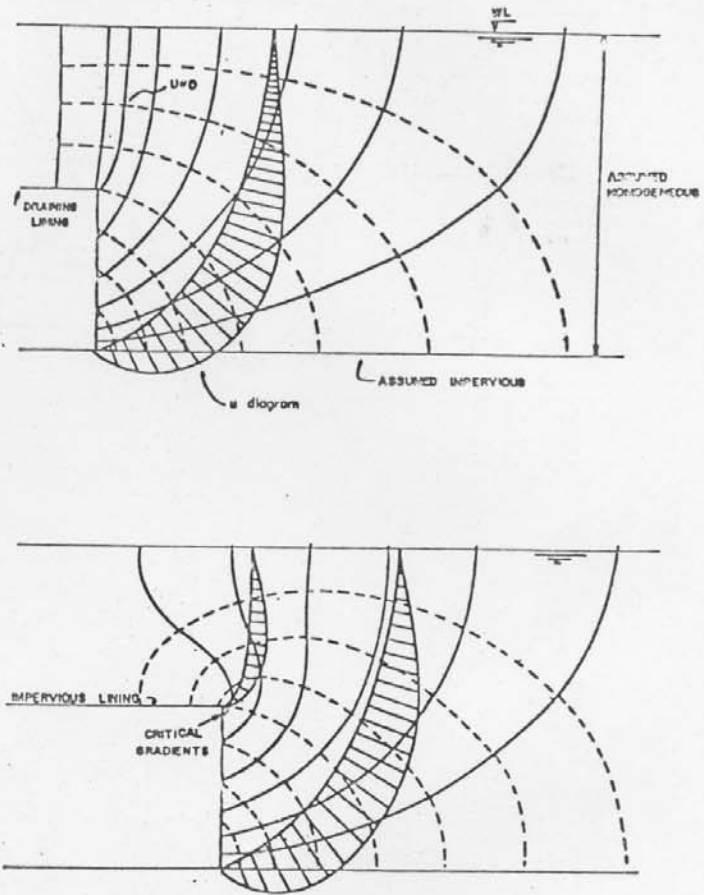FIG. 19 SCHEMATIC INDICATION OF PROCEDURE FOR EFFECTIVE STRESS ANALYSES MERELY FOR COMPARISONS

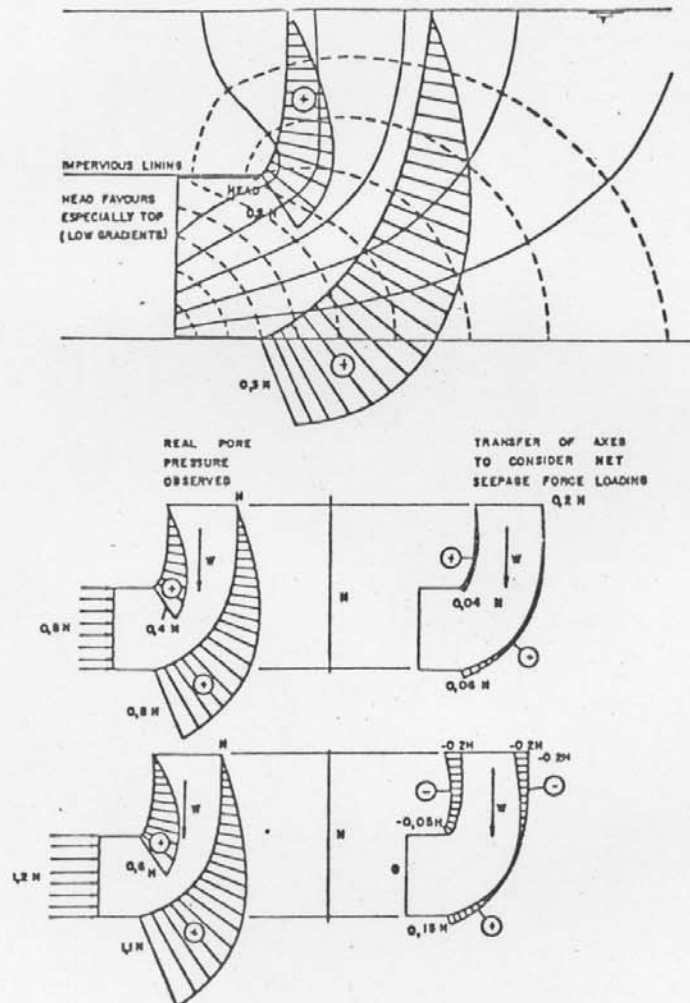FIG. 20 SIMPLE FLOW NET CASES FOR COMPARISON

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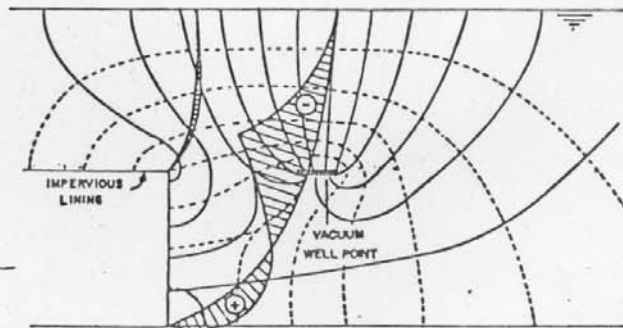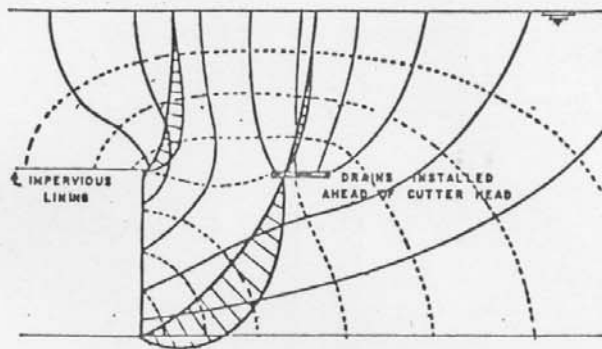IDEALIZED CONSIDERATION OF COMPRESSED AIR.

FIG. 22   ADDITIONAL CASES SPECIAL DRAINAGE FEATURES
AHEAD OF FACE
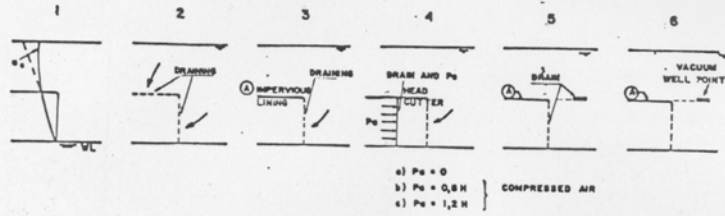
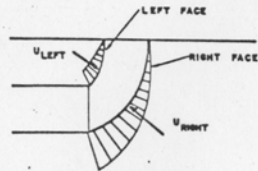|	1	2	3	4	5	6
$U_{RIGHT} \pm z_0$ (t/m)	- 161	42	43	a) 80 b) 1,6 c) - 7	19	- 30
$U_{LEFT} \pm z_0$ (t/m)	- 100	0	5	a) 79 b) 4 c) - 15	3	4

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

FIG. 24 TYPICAL TREATMENT OF PLATE
LOAD TEST DATA (LOADING)
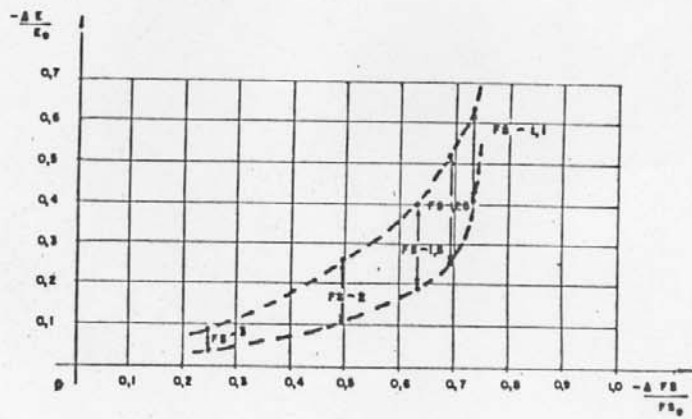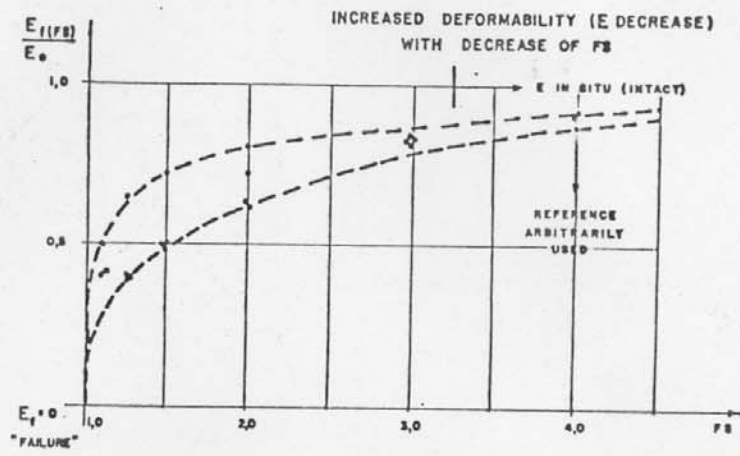
FIG. 25   DEFORMATION   IN   TUNNELS   (PECK 69)

SEATTLE
RAILWAY
TUNNEL

SAN FRANCISCO - BART   MISSION   LINE   TUNNELS
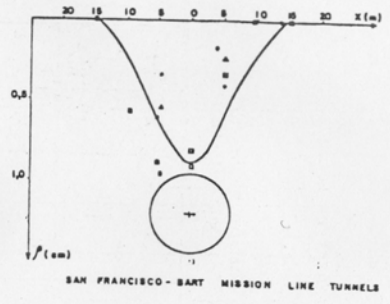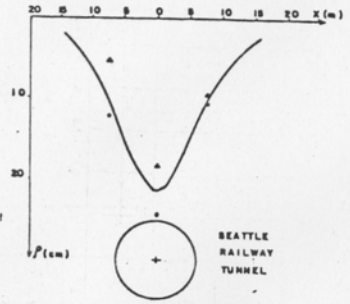
PROPERTIES   OF   THE   NORMAL   DISTRIBUTION   USED   TO   REPRESENT   DEFORMATIONS   ABOVE   TUNNELS   (PECK 69)

PECK- 69 FOR 2 TUNNELS

$R' = R + \dfrac{d}{2}$

d = DISTANCE BETWEEN AXIS
OF TUNNELS
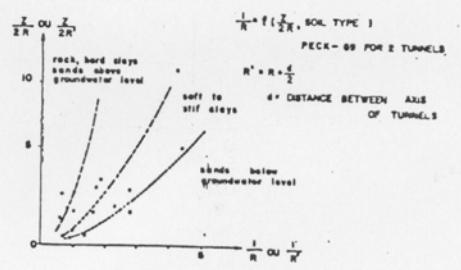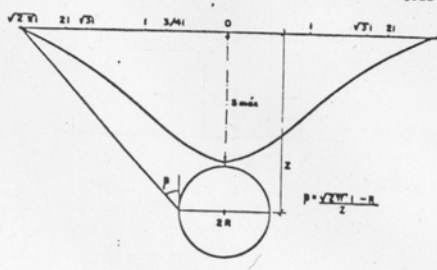
FIG. 26 DEFORMATIONS DUE TO STRESS RELEASE VS. GAUSSIAN CURVE
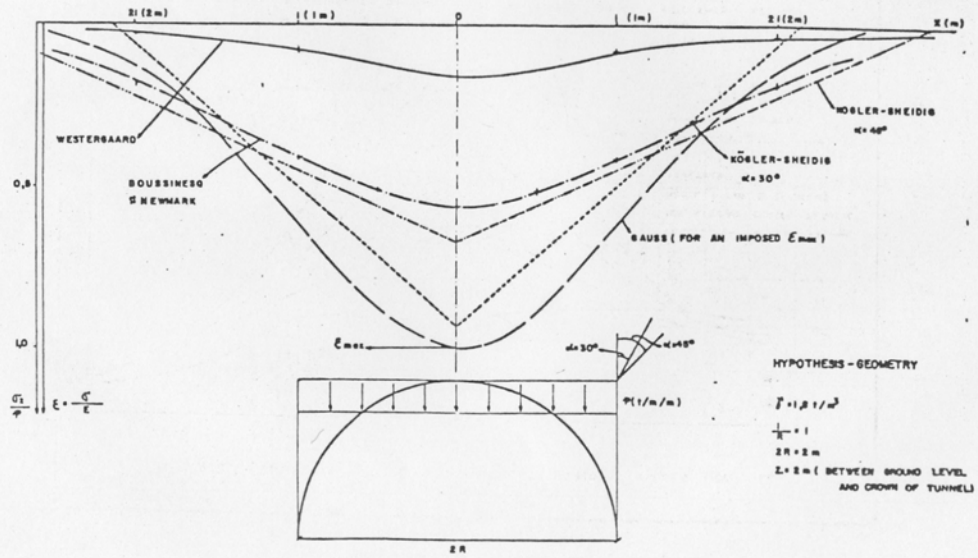
FIG. 27 COMPUTED SETTLEMENTS FOR VARIOUS VALUES OF K_o

PREDICTED SETTLEMENT →
NON LINEAR ELASTO-PLASTIC
MODEL + FINITE ELEMENT
ANALYSIS

ZURICH-77
BY ERWIN GARTUNG
PAUL BAVERNFEIND

MEASURED SETTLEMENT

SUBWAY TUNNEL AT NURENBERG - GERMANY - STATION NEXT ST. LORENZ CHURCH - REFERENCE SECTION
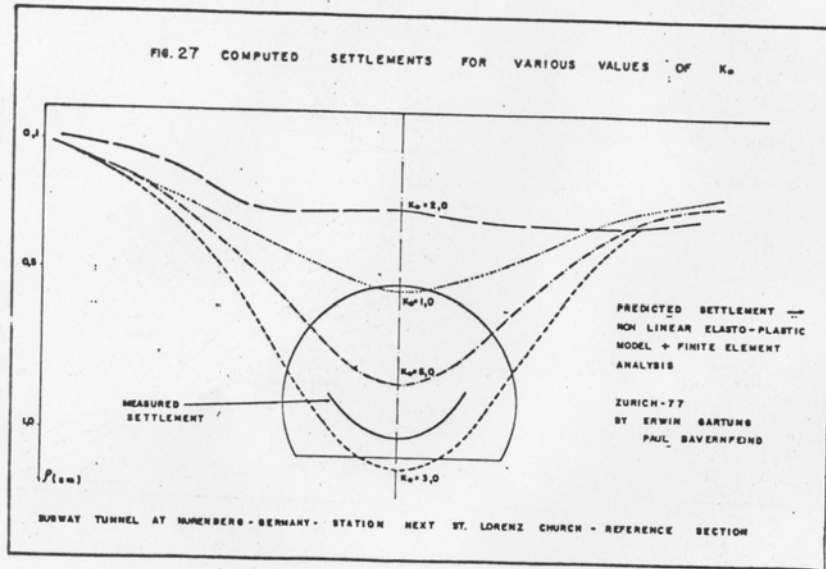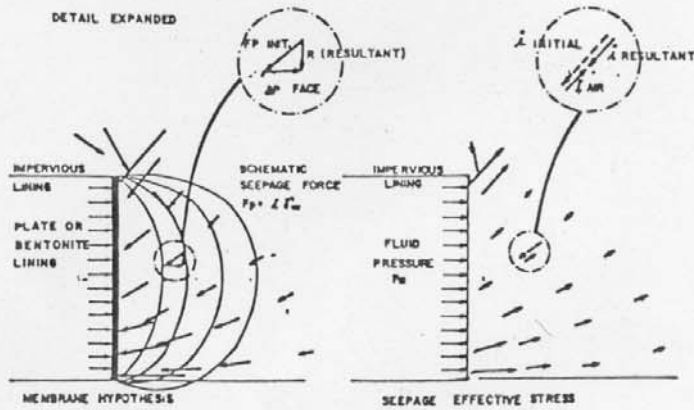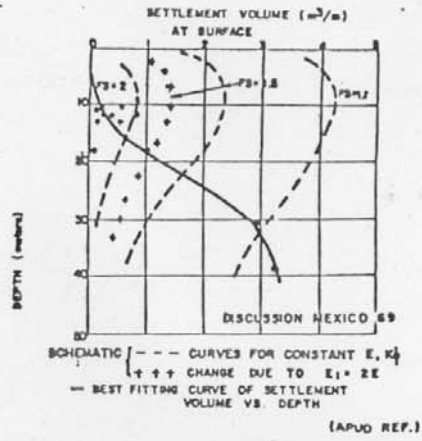
FIG.28   REEXAMINING   QUESTIONS   ON   TUNNEL   SETTLEMENTS

BRACED CUTS · COMPARISON BETWEEN PRESCRIPTION AND WEDGE STATICS
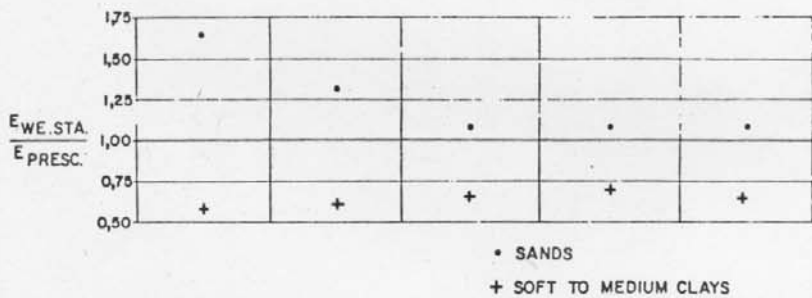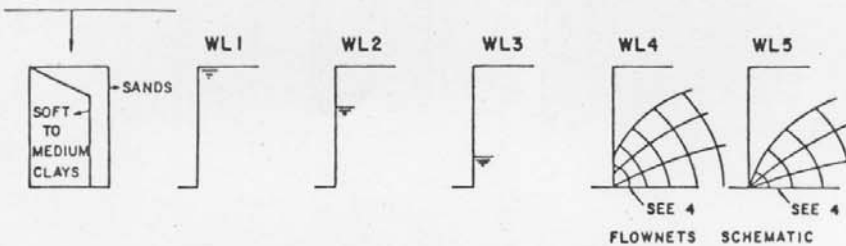
NOTES

1. $E_{PRESCRIPTION}$ CF. PECK, MEXICO 1969

2. $E_{WEDGE\ STATICS} = E_{EARTH\ PRESSURE} \times 1,3 + E_{WATER\ PRESSURE}$
                                       (ADOPTED)

3. SAND $\emptyset = 30°$ / CLAY $S_m = 50\ KN/m^2$ H = 15m

4. FLOWNET DEPENDS ON BOUNDARIES. ASSUMED IMPERVIOUS
                                       AS SHOWN

FIG. 29

FIG 30 PROPOSED DISTINCTION BETWEEN FACTOR OF SAFETY (FS)
AND FACTOR OF GUARANTEE (FG)
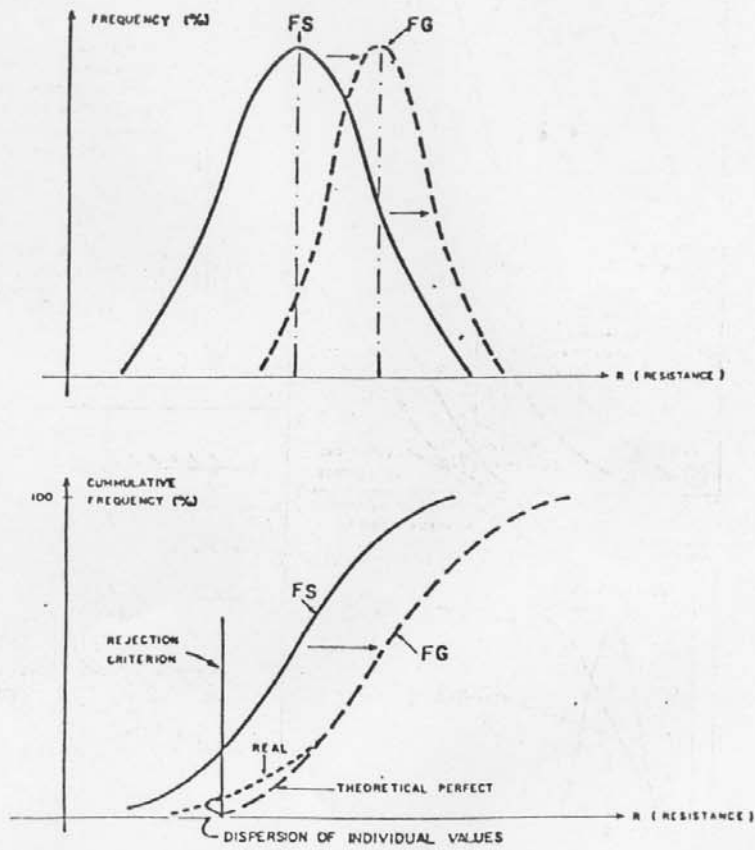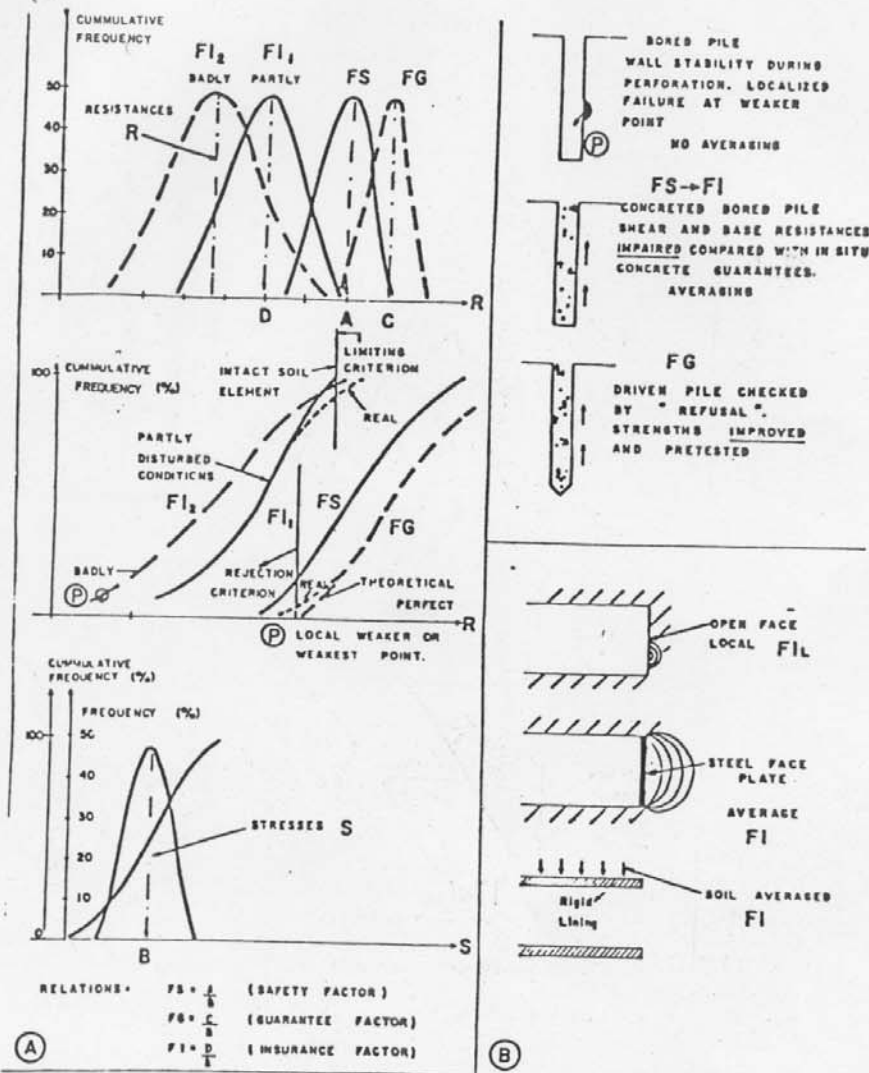
FIG 31   SUGGESTED   DISTINCTIONS   IN   "FACTORS   OF   SAFETY"

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 O F 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
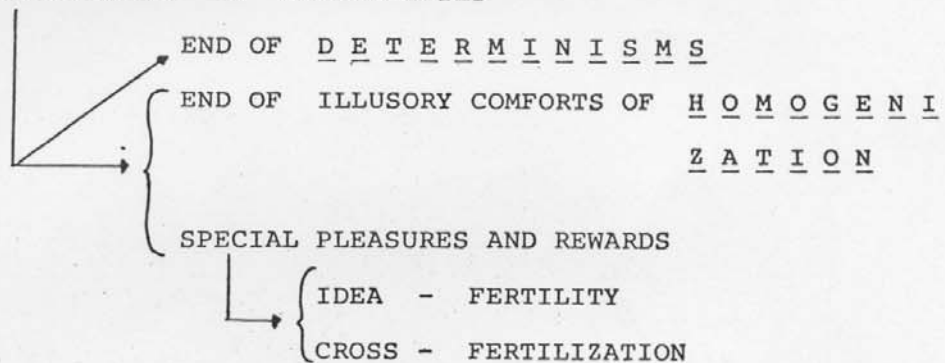
E N G I N E E R I N G = ACT OF DECISION DESPITE DOUBTS

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

FREQ

DISCONTINUITY
OF YES-NO DECISION

"CONTINUUM OF
REALITY"

"FACT"?
"SCIENCE"?
'MATHEMATICAL
CERTAINTY"? .
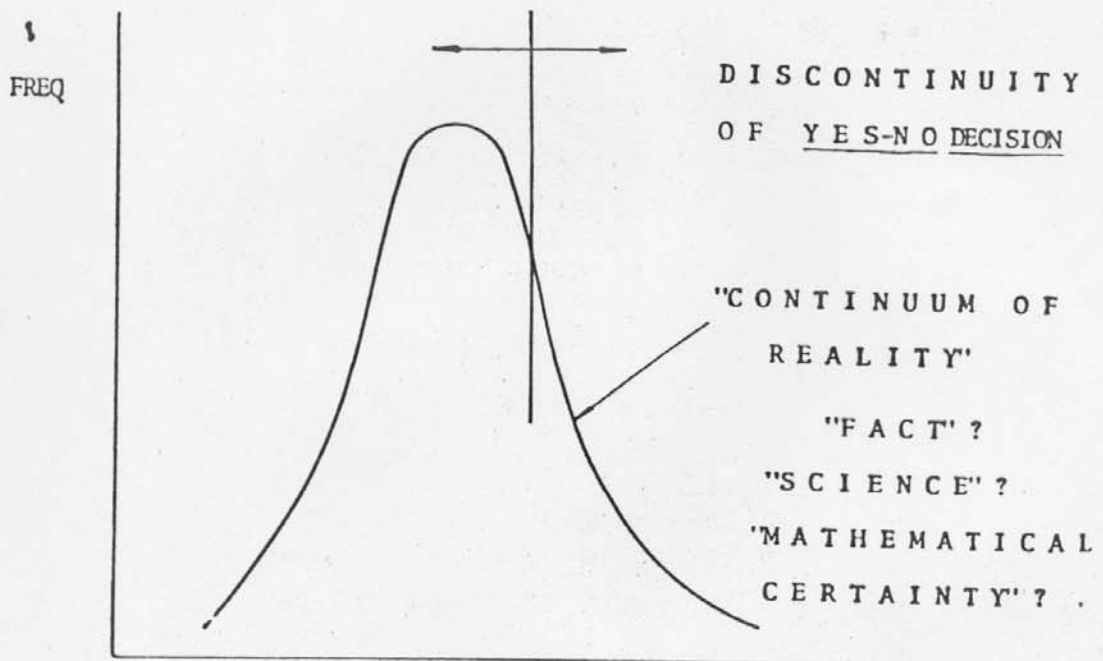
'THEORIES" STARTED FREQUENTLY
WITH 3 OR 4 POINTS

INVENTION GENERALLY MUCH BETTER
THAN FOR SURVIVAL AT FS = 1.00

2. WE ARE IN THE T E R Z A G H I E R A :

            THAT IS OUR FIRST REALITY

S O I L E N G I N E E R I N G

    → WHAT CAN BE TAUGHT AND LEARNT *

    → NOT WHAT CAN BE DONE

  * GREAT NRS. OF ENG'G. W O R K E R S TO BE PUT

                 TO T A S K S

D O M I N A N C E O F A C A D E M I C A C T I V I T Y

E X T E N D E D T H R O U G H P R O F E S S I O N A L L I F E

WE ARE ETERNAL STUDENTS

                   → MUCH LESS SO OF LIFE

     → NOWADAYS

                   → THAN OF FLOOD OF WRITINGS

                  → NOT CREATED IN FREQUENCY

C R E A T I V I T Y

                  → NOT GENERALLY TAUGHT

ENG'G "B R E A K T H R O U G H" = R E M A R K A B L Y BETTER

                 THAN JUST NECESSARY

  · (1) IMITATED, BECAUSE R E M A R K E D

    (2) LEAP AHEAD, OPENS POSSIBILITY THAT TAKES TIME TO BE

          USED UP, BY I N C R E A S I N G D E M A N D S

2.1 H I S T O R I C A L P E R S P E C T I V E

MANY ELEGANT INVENTION INHERITED,

        TAKEN FOR GRANTED

2.2 M O D E R N   I N V E N T I V E   E N G I N E E R I N G

P R O D U C T S   AND   P R O C E D U R E S

EX. INTERNATIONAL COMPETITION OF SOLUTIONS

F O R   T O W E R   O F   P I S A

E X A M P L E S   O F   D I F F E R E N T   P H Y S I C A L

S O L U T I O N S

E L E C T R O S M O S I S

V A C U U M   P R E L O A D I N G

B E N T O N I T E - S T A B I L I Z E D   D I A P H R A G M,
ETC.
S E L E C T I V E   C H E M I C A L   G R O U T I N G (e.g.
SERRE PONCON   DAM)
R E I N F O R C E D   E A R T H

G E O G E X T I L E S

F I B E R   V E R T I C A L   D R A I N S

S T O N E - C O L U M N.   S T A B I L I Z A T I O N

L I M E - C O L U M N   S T A B I L I Z A T I O N

R O O T   P I L E S

D E E P   C O M P A C T I O N   ETC.   ETC.

DR. LANDS'S COMPONENTS   F R E E   D R E A M S

T O   I N V E N T I O N

H A R D   D I R E C T I V E
W O R K

M E X I C A N   E X A M P L E S
F L O A T I N G   F O U N D A T I O N
CARRILLO'S   "T E N S I O N   C E N T E R" + PUMPING-IN ETC.
P I L O T E S   C O N T R O L   ETC.   ETC.

2.3 P S Y C H O L O G I C A L   T R E N D   O F   C O N S E Q U E N C E

FROM   I N V E N T I V E   E N G' G.   IN

G E O T E C H N I Q U E: -

B U R D E N   O F   H E A V Y   &   S P E C I A L   E Q U I P M E N T

EXCESSIVELY   EXACTING   DEMANDS   FROM   SOCIETY

P A S T: LOVE AND RESPECT OF FRAILTIES UN-KNOWN OF SOILS

T R E N D: BRUTALLY   DISRESPECT   SOILS   AS

NUISANCE, TO DISPENSE WITH.

SOLUTIONS   D E S P I T E   SUBSOIL   CONDITIONS

M A N,   IN   D E V E L O P I N G   C I V I L I Z A T I O N,

G O E S   A G A I N S T   N A T U R E

A T   W H A T $\left\{\begin{array}{l} S\ O\ C\ I\ A\ L \\ E\ C\ O\ L\ O\ G\ I\ C\ A\ L \end{array}\right\}$ C O S T   ?

3. P R E S C R I P T I O N S   A N D

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

4. C O R R E L A T I O N S   SUBSTITUTING

FOR   P R E S C R I P T I O N S

F A L L A C I E S

P R E S C R I P T I O N S   ≠   CORRELATIONS, EQUATIONS, LAWS

C O R R E L A T I O N S   ⟶   SPURIOUS STATISTICS,

FRUSTRATING DISPERSIONS

EXPERIENCE CYCLE IN CIVIL ENGINEERING

1. INDEX TESTS
e.g. CLASSIFICATION
REGIONAL EXPERIENCE,
S P T, etc....

D. REVISE ?

B. CORRELATIONS

RESIDUAL
UNSATURATED
SOILS

e.g. IN SAPROLITES
PRESENT INDEX
TEST MISLEADING

CORRELATIONS
B. 2

B.1 LINEAR
SINGLE-
PARAMETER?

4. OBSERVATION
of PROTOTYPE

INTUITIONS FROM
WELL - BEHAVED
JOBS NOT OBSERVED
SILENT MAJORITY OF
CASES

1.1 "FUNDAMENTAL"
PARAMETERS

STRENGHT
COMPRESSIBILITY
DEFORMABILITY
PERMEABILITY
etc..

A. PRESCRIPTIONS

FEW

WHEN

CASES

CORRELATIONS
B.3

3.1. COMPUTATIONS
ANALYSIS -
SYNTHESIS

2. SPECIAL TESTS
e.g. IN SITU,
COMPLEMENTARY,
MODEL - PROTOTYPE.

A.1 PRESCRIPTIONS

A.2 PRESCRIPTIONS

C: DECISION:
ACCEPT OR REJECT
AND REVISE

3. DESIGN
i.e. VISUALIZATION
OF PHYSICAL MODEL
FOR FUNCTION DESIRED

ENG'G ≡ PHYSICAL MODEL
COMPUTATIONS ETC.
ITERATIVE OPTIMIZATION OF THE MODEL

GOAL

KNOWLEDGE OF BACKGROUND COMPUTATIONS ETC.
BUT WISDOM TO USE MODEL THAT DISPENSES THEM, HOPEFULLY

A. C O N V E N T I O N A L S O I L M E C H A N I C S

1. T E R Z A G H I : FROM THEORY TO PRACTICE

1.1 A B A N D O N E D Q U A L I F I C A T I O N S A N D
    C O M P L E X I T I E S O F G E O L O G Y

1.2 D E T E R M I N I S T I C   ⟶JUDICIOUS TESTING

    Q U A N T I T A T I V I S M ⟵⟶MATHEMATICAL ANALYSIS

1.3 S I N G L E - P A R A M E T E R C A U S E - E F F E C T
         R A T I O N A L I Z A T I O N S

1.4 S O I L C L A S S I F I C A T I O N = (f(SOLIDS, ULTIMATE
                                                    PARTICLES)

1.5 I N T E R F E R E N C E O F W A T E R ("NEUTRAL PRESSURES")
    TO BE SEPARATED, i.e. S U B S T R A C T E D
    "E F F E C T I V E" S T R E S S E Q U A T I O N

1.6 D I C H O T O M Y : 100% - 0%, NO VISION OF DISPERSION
                              HISTOGRAM

                    COHESIVE, COHESIONLESS

                    FAILURE, SETTLEMENT

                    PLASTIC, NON-PLASTIC

                    DRAINED, UNDRAINED

                    "UNDISTURBED" (PARTIALLY). "REMOLDED"
                                                    (FULLY)

                    ETC.

    S O L U T I O N V I S I O N

            F A I L U R E .   .   DESTRUCTIVE TESTING & FS

2. P E R I O D O F D I S P E R S E D I S C I P L E S H I P &
    C O N S O L I D A T I O N, 1936 - '48 (ROTTERDAM) - W I D E

                                        S P E C T R U M

A. C O N V E N T I O N A L   S O I L   M E C H A N I C S

1.   T E R Z A G H I :   F R O M   T H E O R Y   T O   P R A C T I C E

1.1   A B A N D O N E D   Q U A L I F I C A T I O N S   A N D
      C O M P L E X I T I E S   O F   G E O L O G Y

1.2   D E T E R M I N I S T I C   JUDICIOUS TESTING
      Q U A N T I T A T I V I S M   MATHEMATICAL ANALYSIS

1.3   S I N G L E - P A R A M E T E R   C A U S E - E F F E C T
             R A T I O N A L I Z A T I O N S

1.4   S O I L   C L A S S I F I C A T I O N = (f(SOLIDS, ULTIMATE
                                                      PARTICLES)

1.5   I N T E R F E R E N C E   O F   W A T E R ("NEUTRAL PRESSURES")
      TO BE SEPARATED, i.e. S U B S T R A C T E D
      "E F F E C T I V E"   S T R E S S   E Q U A T I O N

1.6   D I C H O T O M Y : 100% - 0%, NO VISION OF DISPERSION
                                       HISTOGRAM

                    COHESIVE, COHESIONLESS

                    FAILURE, SETTLEMENT

                    PLASTIC, NON-PLASTIC

                    DRAINED, UNDRAINED

                    "UNDISTURBED" (PARTIALLY). "REMOLDED"
                                                    (FULLY)

                    ETC.

      S O L U T I O N   V I S I O N

             F A I L U R E .   .   DESTRUCTIVE TESTING & FS

2.   P E R I O D   O F   D I S P E R S E   D I S C I P L E S H I P &
      C O N S O L I D A T I O N, 1936 - '48 (ROTTERDAM) - W I D E

                                              S P E C T R U M

3. 1948 T A Y L O R - F U N D A M E N T A L S; P R O B L E M S

A N D   Q U E S T I O N I N G S

T E R Z A G H I - P E C K - S O I L   E N G I N E E R I N G

P R A C T I C E, P R E S C R I P -
T I O N S

4. P E R I O D 1945-'60

4.1 R E S P E C T F U L   R E C O G N I T I O N   O F

S E N S I V I T I E S   O F   N A T U R A L   C O N D I T I O N S

C O N S U L T I N G

4.2 H E T E R O G E N E O U S - P R O B L E M - V I S I O N

4.3 P L A S T I C I T Y   B E H A V I O R S,   C L A Y - F R A C T I O N

A C T I V I T Y

4.4 S T R U C T U R E & S E N S I T I V I T Y . . .

S A M P L I N G

4.5 I N I T I A L   I N - S I T U   T E S T I N G

4.6 T R I A X I A L   T E S T I N G - P R E S U M E D

S T R E S S - P A T H

4.7 B U D D I N G   C O N S C I O U S N E S S   O F

V A R Y I N G K'_o

5. P E R I O D 1950-'60

5.1 C O L L O I D   C H E M I C A L   E F F E C T S,

M I N E R A L O G Y,   T R A C E   E L E M T

S T A B I L I Z A T I O N, etc... RESEARCHER'S

IMPROVED OWN UNDERSTANDING

5.2 1957 L O N D O N:

W E T VS. D R Y   C O M P A C T I O N

$\emptyset$ VS. $\emptyset'$ 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
PREOCUPATON.

7.2 LONDON LARGE BORED PILE:
DEFORMATIONS.

7.3 FINITE ELEMENT ANALYSES.

7.4 CRITICAL STATE SOIL MECHANICS,
CULMINATION OF MENTAL MODEL
ONREMOLDED CLAY ETC.

7.5 FRUSTRATION WITH   STRESS-PATH IN
LABORATORY   PRACTICE,
TESTS   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.

QUALITY OF SAMPLING

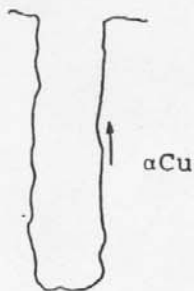QUANTIFICATION OF QUALITY?

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"

αCu

EX. FOR α VALUES FOR

PILES C_u IN 1948 $= R_{c/2}$

MODERN α_{VANE} etc.?

C U R I O S I T Y - E F F O R T - E X P E R I E N C E

Y O U T H - A D O L E S C E N C E - M A T U R I T Y

GREATEST OF THESE IS CURIOSITY

"T H E N E W S T U D E N T S K N O W N O T

T H E O L D L E S S O N S "

THE OLD STUDENTS HAVE BRED FOR THE

OLD PROBLEMS THE CONTEMPT

OF INTIMACY ?

R E S P E C T F O R T H E P A S T

A L L E G I A N C E T O T H E P R E S E N T

I N T E R E S T ⎫
              ⎬ IS IN THE F U T U R E
COMMITMENT ⎭

-+