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SOIL CLASSIFICATION AND SITE INVESTIGATION

KEYWORDS: identification, classification, index-tests, statistical, site investigation, profiling, soil engineering.

SUMMARY: The distinct specimen manipulations standardized for index tests affect and mislead prescriptions and correlations, suggesting necessary revisions. Importance of visual cognizance for experience is stressed, and so also the interest in indices of high resolution and graphical presentations of impact. The question is raised regarding weight of "theorization" in imposing prior preferences on correlative functions: sample discussions of interrelationships show present confusions. The future site investigation trend towards multiple parameter automatic profiling and multivariate analyses is postulated.

1. INTRODUCTION

At first thought one should really wonder at the pretentious ambition of formulating a state-of-the-art lecture on such a subject. It is simultaneously all-embracing and continually covered or implicit in every single paper published, as well as in every single problem of our professional activity. So it seems to lie at the extremes of the histogram of viable tasks, either as a very simple one (of merely summarizing the would-be "accepted routines"), or as extending towards the impossible one of reflecting the ever-present manifold questions, on many of which the data gathered and agathering are already infinitely numerous and exponentially growing. (Fig. 1)

The intent must be examined in the light of an International Conference on Applications of Statistics and Probability to Soil Engineering purposes, and therefore of an examination of conscience within an historical process of purposefulness: and the feasibility may draw assurance from the ontological evolutionary principles of nature.

We need not detain ourselves in examining the proportion of the early efforts (e.g. First International Conference of Soil Mechanics and Foundation Engineering, Harvard 1936) occupied with the problems of identification and classification of soils, yielding place to testing: but it is definitely relevant today to repeat Terzaghi's statement of the closing address: "To be successful in their work they need first of all a thorough grounding in physics, and second, an inquisitive attitude towards the ultimate purpose of their tests. Otherwise investigation degenerates into a habit, comparable to the pious act of an old peasant woman who was found absorbed in prayer while kneeling in front of a milestone on a mountain road. When a passing tourist asked her which saint this stone represented, she replied 'I dont know, but he is certainly good for something'."

Although natural phenomena occur with such myriads of variations that a stochastic rather than a deterministic system definition is inexorably necessary as

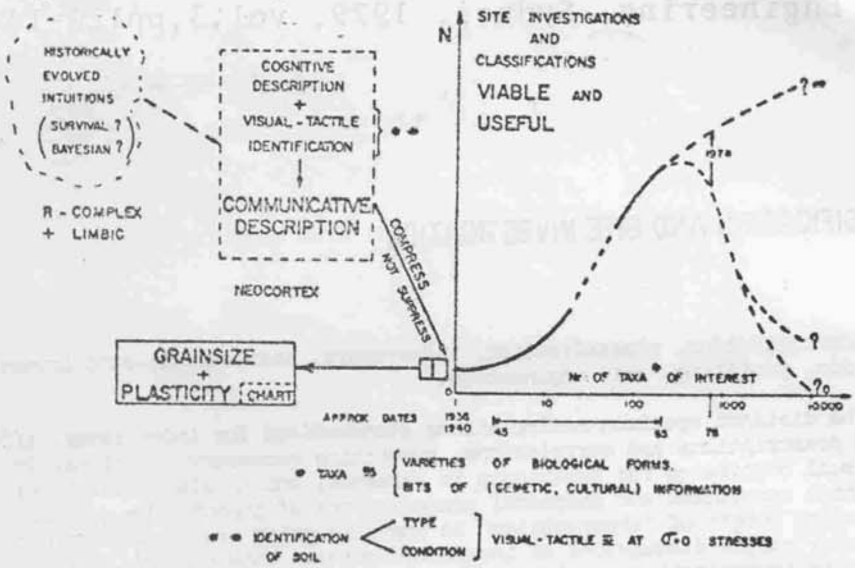


FIG.1 - SCHEMATIC PAST AND FUTURE OF INVEST. AND IDENT ASSOCIATED WITH CLASSIFICATION.

FIG.2 - DUALISTIC SEPARATION (YES - NO) BASED ON SALIENT APPARENT FEATURES

SANDS (PURE)	- CLAYS ("PURE") (CF BELOW)
COHESIONLESS	- COHESIVE (s = c, φ = 0°!)
PERVIOUS	- IMPERVIOUS
NON-PLASTIC	- PLASTIC
INCOMPRESSIBLE	- COMPRESSIBLE
"INSTANTANEOUS" DEFORMATIONS	- SLOW LONG-TERM DEFORMATIONS
(NO)	- SHRINKAGE / EXPANSION ON DRYING / WETTING
NON - SWELLING	- SWELLING
ERODIBLE	- NON-ERODIBLE (?)
NON-COLLAPSIBLE (?)	- COLLAPSIBLE
TRANSPORTED	- RESIDUAL
MICACEOUS	- (NO)
ORGANIC	- (NO)
NORMALLY CONSOLIDATED	- PRECONSOLIDATED
CLAYS: FAT	- LEAN
CLAYS: EXPANSIVE	- (NO) ?

ACTIVITY (SKEMPTON)	INACTIVE AC < 0.75		NORMAL 0.75 < AC < 1.25		ACTIVE AC < 1.25
SENSITIVITY (SKEMPTON)	INSENS. S < 1	LOW 1 < S < 2	MEDIUM 2 < S < 4	SENS. 4 < S < 8	HIGH S < 8
DISPERSIVITY (SHERARD)	DISPERSIVE ZONE A	C	(NO) B		

realistic, we cannot fail to observe the curious natural fact that at any early stage of evolution from ignorance, the intuitive system of guaranteeing usefulness of information, prediction and decision, for minimum gain of entropy, is associated with dualistic yes-no cognitive tasks on the most salient parameters.

It appears that in the same way as biologically, neurologically, and culturally, we repeat ontologically all of "our past", each new soil engineer, and/or any experienced engineer in the face of each new problem must in some way retrace the same basic steps. Presumably, important evolutionary change must needs be accomplished by superposing new systems on top of old ones, reminescent of Ernst Haeckel's doctrine called "recapitulation". Evolution by addition and the functional preservation of the preexisting structure seems to be imposed by good reasons, such as: either the earlier function is required as well as the new one, or there is no way of bypassing the old system that is consistent with the survival that we ourselves represent.

2. PSEUDO-HISTORICAL RECAPITULATION

From a cursory survey of the Proceedings of the First International Conference, Harvard 1936, and of the Purdue Conference 1940, a neat pattern emerges essentially as summarized by the very titles of papers, culminating in the paper by Terzaghi, specifically pertinent to this lecture. It is not surprising that at a visual-tactile degree of observation prevalent, a considerable lack of homogeneity in sedimentary deposits should be recognized, with "variations (that) depend on the geologic history of the deposit", and that the dictum should ensue for first-order working decisions of early Soil Mechanics (Terzaghi, 1940 p.151):

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

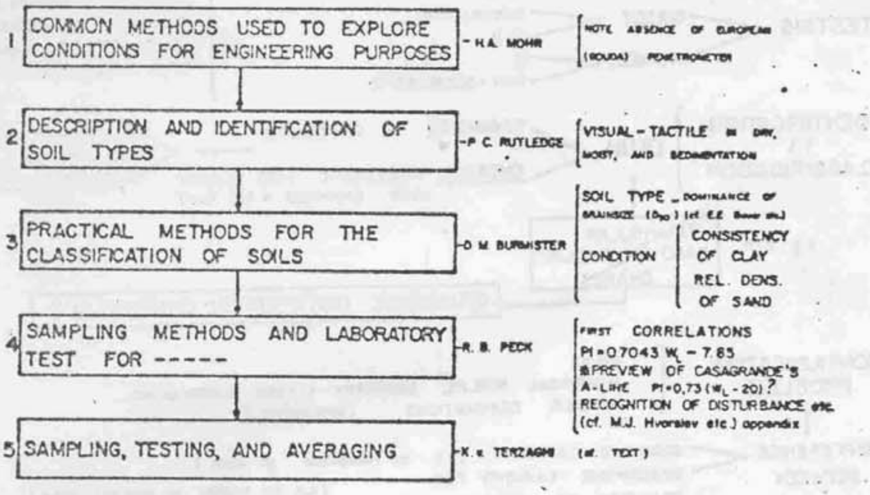


Fig. 3 TITLES OF PRINCIPAL PAPERS IN FIRST SOIL MECHANICS CONFERENCES

Steps 2 and 3 of Fig. 3 have remained as foster children. Regarding step 3, Casagrande (1947) in his classic paper began by denominating "soil classification, the most confused chapter" and well emphasized the "art of soil classification", the "need to know thoroughly not only one but all classification systems important in civil engineering" and the usefulness of the "tool with which the engineer can fashion, if necessary, a new classification to fit his needs in applying soil mechanics to a particular problem". The difficulty in adjusting those postulations to later statistical and probabilistic reasonings is patent. Moreover, it transpires that the facility at classifying by any system whatever (and not merely by the textural one singled out by Casagrande) arises from the felicity of ignorance, dating "back to the time when it was not realized that the physical properties (of apparently similar soils) can be widely different". Indeed, it is fortunate that "fools rush in where angels fear to tread", and whilst we proceed to deplore the growing confusion of the subject, let it not be said of each of us that he be "scorning the base degrees by which he did ascend"!

Mention must yet be made of the Symposium on the Identification and Classification of Soils (ASTM 1950) which essentially coincides with a milestone of dominant thinking and procedures of the subsequent 20 years of soil engineering routines, representing considerable assurance of cause-effect correlative ability and satisfactory discriminative identification of important geotechnical behavioral patterns and parameters. Burmister reaffirms that "classification of soils is probably the most confused aspect of soil mechanics" but in insisting that "the confusion need not be perpetuated" offers as the prop the intent that "soil investigations... provide the most accurate and complete information on soil character and soil behaviour...", and definitely implies that the knowledge has been "dominated" the sole culprit persisting as the "varied controlling conditions encountered in natural situations and in construction practices".

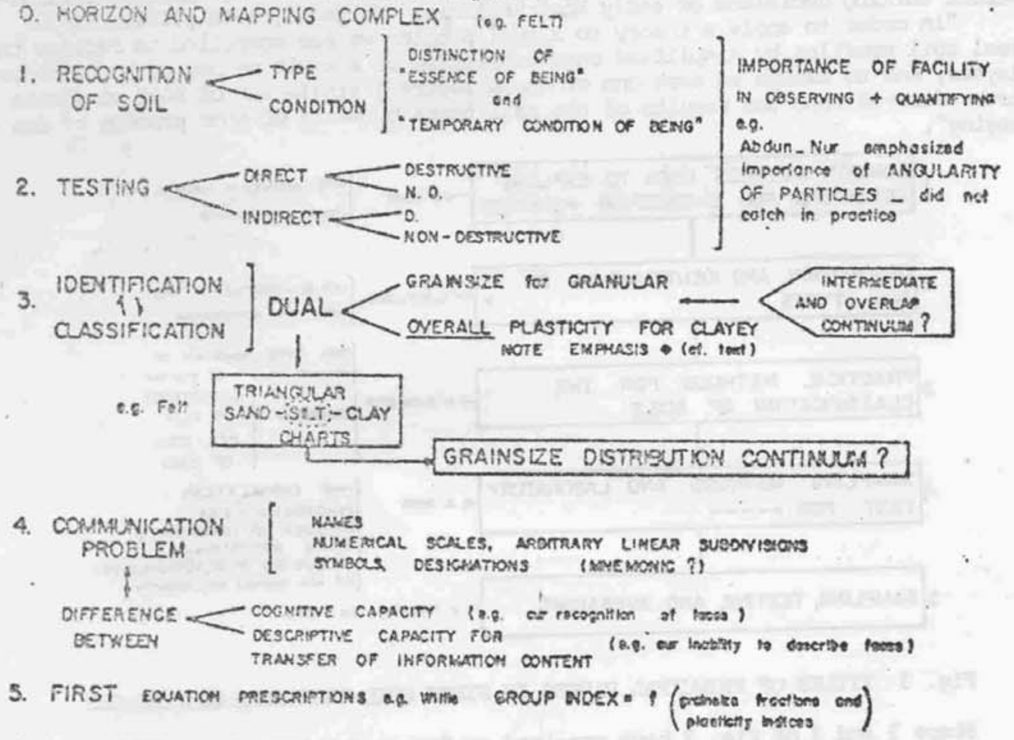


Fig. 4 KEY PAPERS AND CONCEPTS, ASTM SYMPOSIUM 1950

Fig. 4 has been prepared to attempt to summarize the key thoughts that were thereupon handed over as the inheritance of the second-generation soil mechanics. Important germs of upheaval simultaneously sown were:

(a) the indication, associated with grainsize distribution curves, that no matter what property was employed, inexorably a continuum (histogram) would be at play;

(b) the urge to substitute CORRELATIVE EQUATIONS for PRESCRIPTIVE EQUATIONS (boundaries etc.);

(c) the gradual honest recognition that it would be too much to hope that crudely and intuitively devised INDEX TESTS and coefficients could hit upon being the most significant, and fully satisfactory;

(d) the likelihood that arbitrary independent "invention" of such tests would automatically damage many inter-relationships that might be sought.

Fig. 5 has been prepared summarizing some of the basic requirements standardized for routine concomitant tests (as an example, ASTM 1972 Standards). One notes the widely differentiated grainsize components permitted into the separate tests. For instance, Burmister's (1950) emphasis on the OVERALL PLASTICITY "in contrast to to an arbitraty definition of plascitity on the basis of the material passing the ♀ 40 sieve" was set aside.

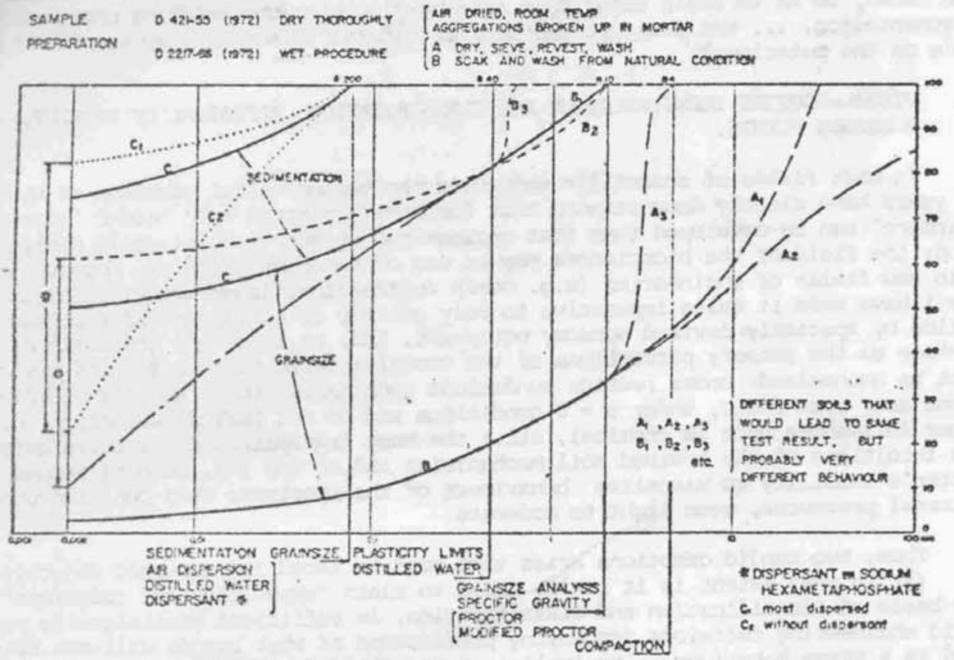


Fig. 5 DIFFERENTIATIONS IMPOSED ON SOIL IN ROUTINE TESTS; ASTM STANDARDS

Another point of grave concern is the (very frequent) use of drying and pulveri - zing of the material in preparation for index testing: no practice could have been more detrimental to Soil Mechanics, since in most clayey soils major irreversible changes occur upon drying, considerably reducing the plasticity indices of the soil in situ. Fig. 6 taken from Carrillo (1969) is purposely used to configurate the extremes to which such influences can interfere in natural soils, Mexico city clay having been used as a well-known clay of exceptionally high plasticity. Casagrande's (1947) single plotted value is also shown. Regarding the sedimentation testsit should be recalled that the erstwhile interest in dispersing the fines,and the requirement of trying out (visually, in test tubes) the distinct optimized defloccu - lant for each soil, aimed at indicating the minimum unit-particles capable of participa - ting in geotechnical behavior. The use of a single deflocculant surely thwarts the early intent: with what gain? On the other hand, if the colloidal activity of the clay fraction must be enhanced by deflocculant, what merit can there be in comparing the Plasticity In - dex (and Limits) in distilled water, with the percent clay fraction (colloidal Activity in- dex, AC) as deflocculated?

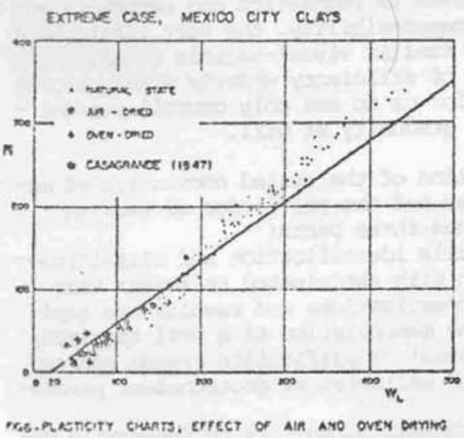


FIG.6-PLASTICITY CHARTS, EFFECT OF AIR AND OVEN DRYING ON THE ATTERBERG LIMITS

Is it not time for us to revise significantly many of our routine testing standards and definitions? Should we not recognize that it is with correlations (statistical) and varying degrees of behaviour of continua that we are dealing? If and when we honestly face up to it, will it not be preferable to coin new parameters, coefficients, and names, so as to begin using them side-by-side with the existing ones, without argumentation, ... and thus let the test of natural selection play its traditional role on the mutations?

3. VISUAL-TACTILE IDENTIFICATION AND CLASSIFICATION: APPRAISAL OF PRESENT, AND POSSIBLE FUTURE.

In most fields of scientific endeavour the technological advances of the past 20 years have clearly demonstrated that for many a purpose much better "sensory equipment" can be developed than that represented by our visual-tactile manipulation: the field of the biosciences may be one of the best examples. Moreover, certain new fields of engineering (e.g. ocean engineering, lunar soil investigations, etc.) have made it quite imperative to rely greatly or solely on indirect manipulation by specially devised sensory equipment. And, at least one intrinsic disadvantage to the sensory perceptions of the visual-tactile manipulation of soils must be emphasized: under routine conditions such manipulation and its observations are, ipso facto, under $\sigma = 0$ conditions and do not include detection of other influences such as chemical, etc.; the most transparent distinction between the intuitions of the trained soil mechanician and of the lay, centers around the latter's inability to visualize behaviours of the specimens when confined under external pressures, even light to moderate.

Thus, two candid questions arise that must be faced and answered objectively:

(1) To what extent is it really valid to claim "experience and judgement" on the basis of identification and classification, in sufficient precisions to permit valid engineering decisions (employing predictions of what bounds will not be exceeded in a given behaviour... an implicit probability formulation)?

(2) To what extent is it worthwhile "playing-back" to the visual-tactile sensory basis for first-degree approximation?

In the first question it is obvious that the practical validity of any such classification systems based on index observations and tests persists longer in such fields as transportation engineering (cf. Liu, 1970 or Ueshita and Nonogaki, 1971, etc.) dealing with minimum common denominator solutions over considerable variability of terrain, and with effects more closely associatable with surficial assessments.

But one should not fail to recognize that even in foundation and earthwork engineering of the greatest locally concentrated responsibility, the very first phase of assessment of subsoil problems must begin by similar visual-tactile observations and probable inferences. It is again a question of efficiency whereby within a principle analogous to "recapitulation" it is best for us to see only overall predominant problems first, and to focus in on details gradually at will.

It is the author's experience (and observation of the stated conviction of many highly experienced colleagues with whom he has had the priviledge of working jointly) that the answer to question (1) above has three parts:

(a) experience and judgement in visual-tactile identification and classification are so highly prized (in direct association with anticipated behavior) that frequently a stack of reports of geotechnical investigations and results are rapidly brushed aside, overt preference being for the manipulation of a soil specimen;

(b) seldom is any presently acknowledged formal classification system adhered to as a necessary link between identification and estimates of geotechnical parameters for design computations;

(c) finally, the attempt to quantify geotechnical parameters is frequently very poor indeed, even by great specialist consultants, and when well covered by routine index tests and visual-tactile manipulation, if somewhat peculiar regional soils are involved, situated beyond the specific context of experience of the specialist: in other words some of the present index observations can be misleading if there is no context of visual correlation with the results of fundamental tests and prototype behaviour.

As a consequence, the answer to question (2) follows automatically: it is highly profitable to "play back" from good tests to a good visual-tactile form of association and identification. On the other hand, apparently the usefulness of such a procedure cannot but remain extremely subjective and egoistic: unfortunately it is part of our neurological and cultural inheritance that our cognitive capacity is many many times better than the capacity to describe and communicate the cognizance acquired (Fig. 4 item 4).

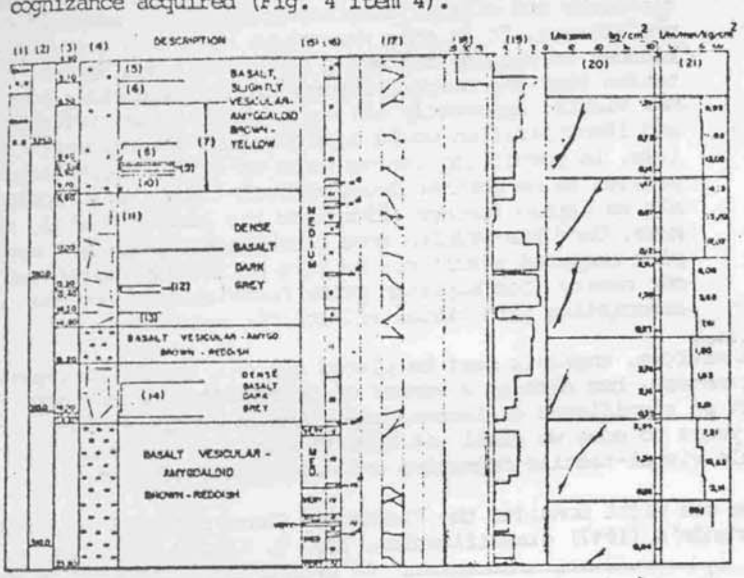


Fig. 7

TYPICAL PRESENT DAY CORE BORING LOG WITH INFORMATION PRESUMED IMPORTANT AND ABSORBABLE

(1) Diam., perf.; (2) elev. m; (3) depths, m; (4) lithology key; (5) fractured; (6) very frac.; (7) fractures with altered faces, silty-clayey yellow and white veneer; (8) (9) and (10) fractures with shear striations; (11) (12) and (13) fractures as in (7) and with shear striations; (14) fractures with white powdery infilling; (15) degree of weathering; (16) rock quality classification assessing both fracturing and weathering GRADES I best, to V worst; (17) cracks sketched; (18) % core recovery; (19) nr of joints per m; (20), (21) water loss pressure tests.

A striking example of the apparently contradictory situation may be taken from Rock Mechanics and the elaborate routines employed for the presentation of a core boring log, Fig.7, packed with information on parameters that have been reasoned academically to be of great importance. With the exception of the data extracted from the water loss tests, which are in situ tests, to most civil engineers and rock mechanics specialists, the colored photographs of Fig.8 really furnish much more of the meaningful and desired information content.

It appears of great interest to this conference to ponder on the evidence exposed by Hynes and Vanmarcke (1977) possibly confirming the impression, based on professional activities, that "experience may not act merely on the input parameters, but most frequently on the output", and "frequently experience intuitions are better and easier in connection with the complex lumped-parameter end result" (de Mello 1975) because we notice and remember the end results that affect us more significantly. In civil engineering we are not dealing with absolutely new fields such as neurology, biochemistry, genetics, and so forth, of unexposed experimentation, results, and possible associations of tests and behaviour: there is a considerable past of experience on the complex end-results, and therefore there should be much to gain from tying the future to the past.

The point in question however is whether or not the somewhat better intuitive prediction achieved by the audience in merely answering the questionnaire distributed (Hynes and Varmarcke 1977), would likewise have reached a modestly satisfactory histogram of estimates if the data put forward had been merely the classification and identification information on the subsoil. The negative answer appears obvious, suggesting that existing identifications and classifications hardly permit quantified predictions. It is only when there have been many experiences at closing the cycle of analysis-synthesis computation that the complex interpretation-prediction becomes viable. Apparently the early aims of classification and identification would be met within deterministic dualism, in permitting the decision as to (a) there being a problem to be further developed, or (b) it being acceptable to forego further efforts on the less worthwhile avenues. Once the problem area has been accepted, the complex computed result may be more strikingly driven into our memory (for Bayesian prior formulations) because of association with visual effects of importance.

In all respects therefore, emphasis must be placed not only on the appropriate choice of index parameters, but also on a manner of representation that may improve the visual impact of significant differentiation. And it is the author's contention that for some years to come we shall yet have to keep (or gain much from) tying back to the simple visual-tactile cognitive and descriptive efforts.

Just as an example one might consider the Plasticity Chart proposed as a fundamental part of Casagrande's (1947) classification, Fig. 9. Thirty years have gone by and the author has seldom found any specialist drawing any significant inferences based on the soil's plotted position in that chart. Might not the reason be to some extent the fact that by having compressed all possible soils within the angle contained roughly between 15° and 40°, the chart is not designed to provide any visual discriminating capacity between what would presumably be different soils of differentiatable behavior?

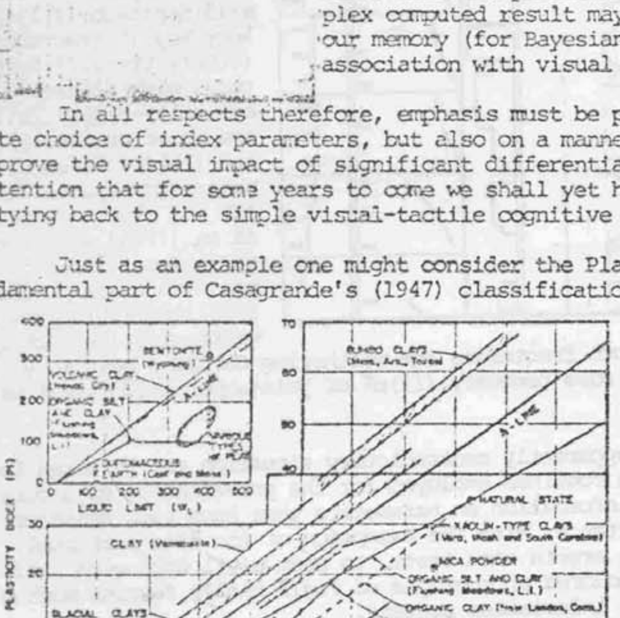


Fig. 9 ORIGINAL PLASTICITY CHART APUD CASAGRANDE (1947).

The original chart appears to have been somewhat dominated by the desire to evidence the essentially linear variations of PI versus W_L in each clayey stratum, as the percentage of inerts mixed with the clay suffers its inevitable variations. In comparison, Fig. 10 has been prepared to exemplify one possible representation that would enhance the distinction between soils representing "zones" as related to the A-line. An obvious expedient is to rotate the A-line to become the abscissa, and thereupon to plot (in expanded scale, chosen as found convenient) the ΔPI with reference to the A-line. If the positions

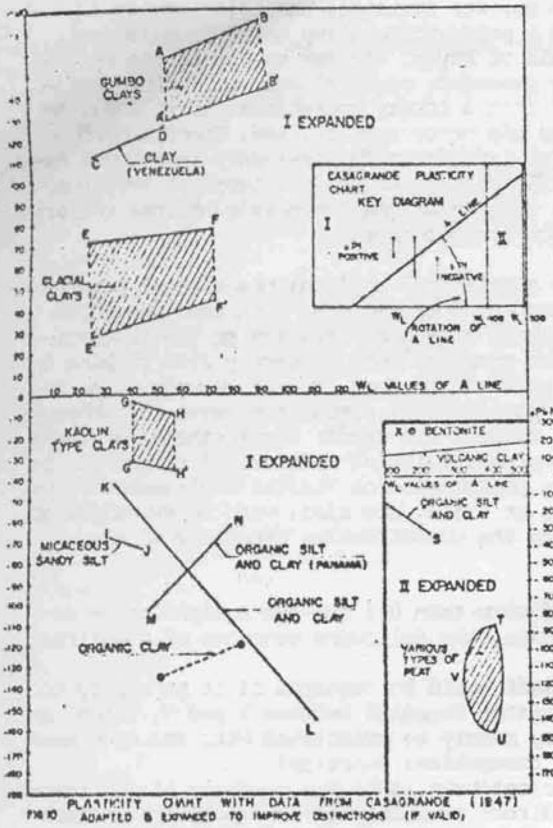


FIG 10 PLASTICITY CHART WITH DATA FROM CASAGRANDE (1947) ADAPTED & EXPANDED TO IMPROVE DISTINCTIONS (IF VALID)

of plasticity parameters of soils relative to the A-line (approximate average) really prove significant, such a chart should emphasize the distinctions, increase soil engineering's curiosity in investigating the influences, and, hopefully, develop the experience to be associated with the desired interpretative chart. Obviously it would be desirable to begin by revising the very test procedures of the liquid and plastic limits, to be somewhat more objective and reproducible, (cf. for instance Wroth and Wood 1978, Wood and Wroth 1978). Moreover, a further adaptation of such a chart may result even more interesting, if we incorporate the recognition that PI values include firstly a portion proportional to the w_L or w_P, and then a positive or negative complement: thus, some incorporation of a Plastic Ratio parameter (e.g. Saito and Miki, 1975) in a chart similar to our Fig. 10 may yield the most useful presentation.

4. NECESSARY CRITICAL ANALYSIS OF MANY A PRESCRIPTION OR CORRELATION.

The fundamental intent and purpose in site investigation and soil classification is furnishing prescriptions or correlations for analyses in first-degree approximation. Whereas early efforts in these directions were strongly empirical but tempered by intuitions on physical behavior of soils, and within a transition period the advancement was introduced of fitting statistical regressions to such intuitions, the recent and growing trend has been to apply statistical treatments as self-sufficient foolproof analytical tools to determine the presumed most appropriate regressions, even if they have no plausible association with the existing body of theorization, or, worse still, even if they go contrary to such theorization. The author however, has decried what he has termed statistics at random, under the reasoning that if existing theorization suggests a certain type of function for the relationship sought, the regression should firstly be sought with such a function as a prior imposition. How far is it valid to go along either of these alternate avenues?

The question poses to the author a rather challenging problem of conditional probabilities within our context of Bayesian advancement of knowledge (decreasing uncertainties); he thus prefers to defer it to statisticians. If we impose a given function obviously we would inhibit discovery of truly new facts, trends, theories. On the other hand, quite evidently statistics at random cannot be accepted. How does one employ the weight of conditional probabilities implied in "theory" in order to permit new data to impinge on old data in a Bayesian approach?

When analysing a specific function, such as, for instance, the relationship of $C_C = F(w_L)$ we well know how to establish a prior probability based on existing knowledge, and thereupon apply the formula of Bayes' theorem to obtain the weighted impact of the new data. Some similar procedure might already exist in quantifiable form for establishing the "prior" that a theory represents: if it does, however, the author is not aware of it, and has never seen it used. Theory represents basically a prior of several sets of laws (correlations) duly interwoven in its crosslinkages that should weigh heavily against any easy attempt at revision, since the impact of the change should be cross-examined not merely against the single function but against the many indirect consequences.

Regretably almost all the papers on statistical applications of soil parameters, and particularly of index parameters and correlations, have been developed as independent pieces of information, without a broader analysis or the theoretical implications, prior and posterior. The practice derives partly from rushing information to publication piecemeal, and partly from some divorce between those closely interested in statistics, and those particularly preoccupied with the theory of soil behaviour. At any rate, one must correct the trend: for instance, when one discusses statistical correlations on compressibility of undisturbed clays, it is not merely the Bayesian prior probability postulation on similar undisturbed clays, or even on all undisturbed clays, that is at stake, but also, with heavy weight of conviction, all the results of research on the consolidation behaviour of clay slurries and remoulded clays.

If with a correlation coefficient of more than 0,7 one has a right to be satisfied within an assumed random set of data, the following sequence of questions may be posed:

1- What minimum correlation coefficient would be required if it be merely to concede to a certain theoretically anticipated function between X and Y, since a greater or lesser part of the variance may merely be associated with the crudeness of the data and not with challenging the theoretical function?

2- How much higher would the coefficient have to be for credence if the function goes contrary to a prior degree of direct certainty of the specific function of X and Y (to a point that one may have claimed the said function to be theoretically backed)?

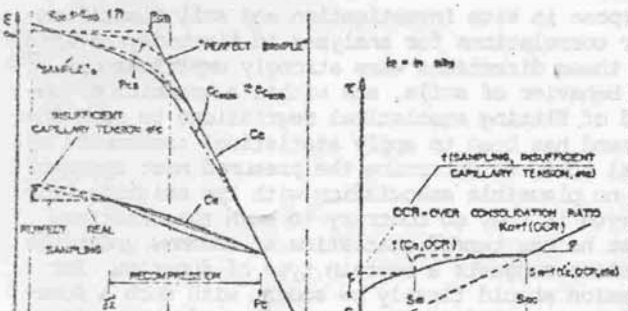


FIG. 11 - CLASSIC KEY FOR SEEKING INDICATIONS ON COMPRESSIBILITY AND RESISTANCE PARAMETERS

3- Finally, how much higher yet would the coefficient have to be if the function does not align with what the claimed theoretical knowledge, wherein such "knowledge" establishes a function not merely adequate for the direct relationship between X and Y, but also for a number of important indirect relationships X-M-Y , X-M-P-Y , X-A-B-C-Y, and so on? If some set of data, W, Y, do establish the required satisfactory coefficient for such a function contraty to theoretical formulation, is it not incumbent on the proponent to examine on the spot the impact of the finding on such other direct and indirect relationships?

The author does not mean these questions to be rhetorical, but earnestly requests of statisticians an authoritative report indicating procedures for such cross-examination of the applicability of presumed regressions, and for establishing varying acceptance levels of correlation coefficients.

In exemplifying the problem from the viewpoint of soil behaviour, the author submits an inquiring discussion on the two most commonly used index test correlations (cf. Peck 1974), oft quoted as $C_c \approx 0,009\ (w_L - 10)$ and similar, and $c/p_c \approx 0,11 + 0,0037$ PI.

Obviously the fundamental interest of the engineer would be for C_c values indicative of in situ behavior of the clay strata. It is nevertheless comprehensible, though regretable, that because of the much greater access to purely laboratory data, most papers on the subject have concentrated only on regressions between laboratory tests, without an iota of discussion of effects of sampling, disturbance, etc..

Two principal factors of sample disturbance are partial remoulding in high sensitivity clays, and the problem with porewater capillary tensions in the sample. Regarding capillary tensions the two problems are, one the one hand, insufficient capillary tension to maintain the sample at constant volume (either because of high consolidation pressures or because of coarser pore sizes and corresponding menisci), and, on the other hand, the release of dissolved gases as capillary tensions increase, (for bases of detailed treatment cf. for instance, Bishop and Hight, 1977). The basic inescapable knowledge therefore is that there is a definite bias in certain test data on "undisturbed samples", which makes it misleading to promote some regressions in lieu of others. Further, from a purely pragmatic sense if reasonable correlations may be established merely for $C_c \approx F(w_L)$, there is the advantage that the only test employed is not dependent on the sample being at in situ physical properties.

Conventional theorization regarding clay compressibility and shear strength is schematically summarized in Fig. 11 merely to establish the key to the symbols used below. Such fundamentals were backed by a very considerable body of experimentation notwithstanding its non-statistical treatment. Thus we are bound to accept that sedimentary clays consolidate from slurries of initial water contents analogous to their liquid limits and following approximately the e vs. log p straight lines of C_c values related to the respective w_L. Sampling disturbance tends to increase e_o' and generally decreases both the C_c (remoulding) and the preconsolidation pressure p_c indicated. Sampling disturbances tend to be least with clays of intermediate w_L values, increasing with the high w_L clays that tend to be more Sensitive, and also with clays of very low plasticity and w_L. The regression might thus improve if one substitutes the classic linear relationship by a flat curve concave down. Disturbance is also obviously affected by consolidation pressure, tending to increase with the latter.

At any rate it should be transparent that if the mental model of e vs. log p curves is used together with the corresponding essentially straight-line compression indices distinguishing sharply between compressions and recompressions, it should be:

a- impossible to pool together within the same correlations the pre-consolidated and normally consolidated clays;

b- similarly in preconsolidated conditions it is impossible to achieve correlations between C_c and initial specimen physical properties (e_o void ratio, and the like), and even if they did exist, they would be of no practical use since the settlements under first pressure increments would be along recompression;

c- in normally consolidated clays the very e_o is a function of C_c' and therefore correlations of C_c vs. e_o should be obvious, although subject to greater errors and dispersions;

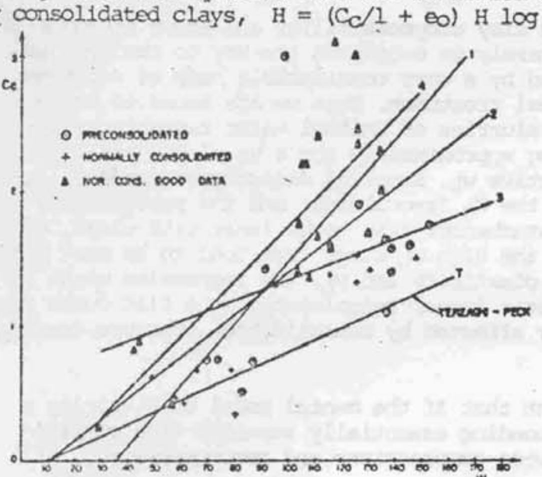
d- recognizedly C_c depends on the clay structure: therefore, under normally consolidated conditions which permit using interchangeably either e_o or w_L' in many a soil the better correlation may depend on which one of the two parameters is less damaged by obvious error. The w_L test consciously destroys clay structure totally, being run on the remoulded material, and therefore the correlation $C_c = F(w_L)$ assumes that structure exists proportionally to w_L: in many a soil (e.g. peaty, highly sensitive, etc.) the e_o may thus prove to be a parameter with less obvious error. Needless to emphasize the preference for e_o if the w_L test was done with drying and pulverizing (cf. Fig. 6). On the other hand, in some peaty soils the "undisturbed sample" may be very far from the $\Delta V = 0$ condition, which would yet not affect the laboratory correlations $C_c = F(e_o)$ but would be grossly in error for field estimates. Further, the water content determination may include a considerable error upwards due to burning off material at the standard 104°C drying temperature, which would impair correlations $C_c = F(e_o)$. In short, therefore, the papers favouring one or other of the regressions should very carefully and completely qualify the test results used.

e- in indiscriminate statistical treatments of a great number of test data, if the proportions of normally consolidated cases are high in comparison with unsuspected preconsolidated ones, the coefficients of correlation may still be good on general regressions, and yet individual cases will continue to be too widely erratic for use in engineering. One does not improve credence by increasing the number of checks (Pearson, Kendall, Spearman, Kolmogorov-Smirnov, etc.) for coefficients of correlation.

f- one should not confuse the above statements with the published indications of a possible general correlation between compressibility de/dp and e_o, wherein no attempt is made to distinguish between the normally consolidated and preconsolidated conditions, (e.g. Janbu 1963, etc.).

g- from the equation used for idealized calculations of settlements of normally consolidated clays, $H = (C_c/1 + e_o)\ H \log (pf/pi)$, it would appear that the most useful correlation in a given deposit would be referred to $(C_c/1 + e_o) = C_r$. Theoretically however it should vary significantly with depth and suffer more significantly from the bias due to disturbance, since C_c decreases and e_o frequently increases by swell.

The subject has been much debated, and will continue to be so (e.g. Krizek et al, 1977, Mohan and Bhandari, 1977, Croce et al, 1969, Kogura and Ohira, 1977, Dascal and Larocque, 1973 etc.). In the hope of confirming some theoretical preferences the author resorted to some select data furnished by the Instituto de Pesquisas Tecnologicas of São Paulo, and submitted them to linear regressions, Fig.12. Most questions persist, but the following theoretically expected trends appear to be vindicated: correlations result much poorer in preconsolidated cases than in normally consolidated ones; the inclusion of the preconsolidation pressure as a parameter is significant, but is better included as the average value extracted from the subsoil profile, than as individually determined p_c values from the specimens



	A - CLAY TYPE	r	*	B - STRESS CONDITIONS / C - PHYSICAL PROPERTIES (C_a)		r	*	A · B · C		r	*
NORM CONS n=32 + Δ	0,01620(wL - 15,05) CORRELATION 1	0,74	3*	f'2	0,3259 + 0,01605 wL - 1,0617 (f''2)	0,70	[illegible]	f2	0,1908 + 0,0021[illegible] wL - 0,1790 (f2)	0,71	4*
				pc	0,0237 + 0,01822 wL - 0,5674 (pc)	0,75	2*	pc	0,[illegible] + 0,00[illegible] wL - 0,0206 (pc)	0,83	5*
				eo	Cr = 0,00275 (wL + 30,58)	0,80	5*				
Precons n=13 O	0,00735(wL + 8,70) CORRELATION 3	0,36	3*	f2	1,380 + 0,004054 wL - 1,115 (f'2)	0,51	1*	f2	0,704 + 0,00258 wL - 0,108 (f2)	0,29	4*
				pc	0,7553 + 0,007444 wL - 0,1853 (pc)	0,42	2*	pc	0,300 + 0,0028 wL - 0,0224 (pc)	0,25	5*
				eo	Cr = 0,0005767 (wL + 488,30)	0,19	6*				
ALL DATA n=45	0,01426(wL - 13,42) CORRELATION 2	0,63	2*	f'2	0,4208 + 0,01272 wL - 0,6350 (f'2)	0,66	1*	f2	0,2044 + 0,00[illegible] wL - 0,0824 (f'2)	0,54	3*
				eo	Cr = 0,0099 (wL + 74,20)	0,57	4*				
+ ● n=20	0,02075(wL - 14,53) CORRELATION 4	0,77	2*	f2	0,2579 + 0,01809 wL - 0,7954 (f2)	0,80	1*	f2	0,1533 + 0,00[illegible] wL - 0,1203 (f2)	0,71	3*
				eo	Cr = 0,002943 (wL + 38,22)	0,80	4*				

* ORDER OF MERIT CORRELATION

+ ● NORM CONS RECENT SPECIALLY CAREFULL DATA FROM 32 TESTS OF CORRELATION 1

FIG 12 - BRIEF INVESTIGATION OF C_c C_a CORRELATIONS IN COMPARISON WITH THEORETICAL EXPECTATIONS themselves; the inclusion of the e_o parameter only retains acceptability (with no improvement, however) in normally consolidated cases, but leads to the expected absurd results in preconsolidated cases.

Is it not questionable to employ greater numbers of undiscriminated data in lieu of fewer, new, select data, expurgated of well-known sources of error? Should not all interlaced correlations connected with varying positions on the plasticity charts be investigated, in order to judge on probable inferences? For instance, using the candidly simple calculation of the consolidation pressures P_c required to densify different clays to their plastic limits (cf. Fig. 13) how would such indications confer with Wroth and Wood's (1978) S values of about 1,5 kg/cm^2 at w_p and with the classic but much debated c/p_c relationships F(PI)?

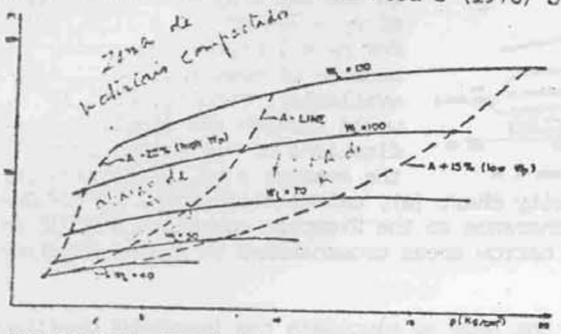


FIG 13 - CONSOLIDATION PRESSURE HYPOTHETICALLY NECESSARY TO DENSIFY CLAYS TO RESPECTIVE w_p CONDITIONS.

Regarding the c/p_c correlations the immediate reaction is that it seems to go diametrically opposite to theoretical reasoning as already shown by Bjerrum and Simons (1960) who also indicated a non-linear Skempton-Bjerrum correlation. If the higher the plasticity of the clay, the lower its friction angles (drained and consolidated-undrained), the trend should be of a decreasing c/p_c with higher PI. The subject is more complex, however, if we distinguish between in situ determinations of s (cf. Fig. 11) in normally or over-consolidated clays, or apply ourselves to considering the cohesion based on unconfined compression tests of would-be "undisturbed specimens", as early shown by Skempton and Henkel (1953): the latter depend greatly on the specimen's ability to sustain the required capillary tension. The published data (e.g. Aboshi 1969, Karlsson and Viberg 1967, etc.) usable to check the much used c/p_c empirical equation do not supply sufficient information for a discriminating analysis. It is therefore attempted in Figs. 14 to 17 to set up a minimum correlative background so as to exemplify the search for functions of trends against which to submit data to statistical regressions, and thus to exemplify the quest for the manner of prior imposition of a weight of "theory" in favour of preferred functions.

Firstly we are in great need of improved data on the variation of ϕ' and ϕ_{cu} versus PI and/or (PI, w_L, etc.): the present bands of dispersions are far too broad (Fig. 14). Summarized ϕ_{cu} values for various clays have not been found. The author has assumed that the trend is of ϕ_{cu} being proportional to ϕ', and therefore, since the very broad dispersion permits only a schematic analysis, the ϕ_{cu} values (well recognized to be applicable) have been avoided in order to manntain the possible trends more evident because of higher numerical values. In accounting for the loss of strength due to stress release and swell, the idealized calculations used the relationship $\Delta e = F(\log s)$ as linear parallel to C_c. The corresponding bands for a hypothetical compression to p_c = 4 kg/cm^2 are shown in Fig. 15 accompanied by three hypotheses of percent swell.

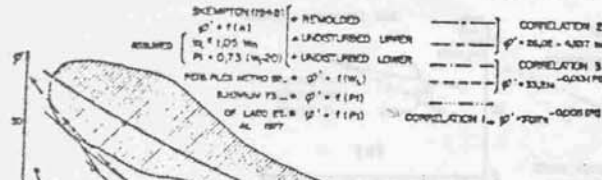


FIG 14 - BASIC DATA FOUND ON $\phi' = f(PI)$

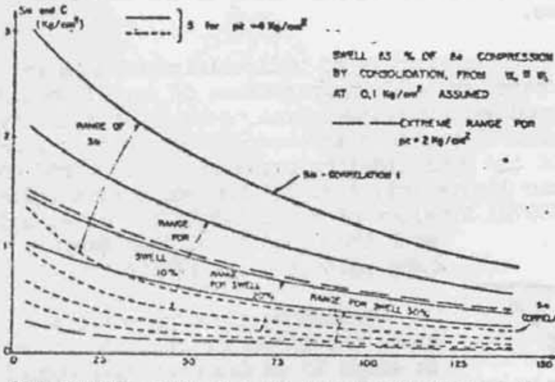


FIG 15 - SIMPLIFIED TRENDS OF S AND C VS. PI BASED ON FIGS 11, 14

Subsequently in Fig. 16(a) the presumed correlation of K_o vs. PI is used to indicate stress conditions, as well as the capillary tensions in a perfect specimen, as compared with those required for justifying a Skempton correlation: calculations were carried out for a hypothetical clay of w_L = 70% on the A-line, and for p_c = 1 kg/cm^2 (purposely low because of most empirical data available). Finally, Fig. 17 would furnish the simplified indications of variations around the average ϕ vs. PI regression, for different positions on the plasticity chart (a), calculations for 1 kg/cm^2. One would correspondingly conclude that adherence to the Skempton correlation would be comprehensible principally within the narrow areas crosshatched in graphs 17(d) and 17(a).

Once again, such exercises could not hope to elucidate the important questions on the subject (e.g. Karlsson and Viberg 1967, Matsuo and Asaoka 1977, Ladd et al 1977, etc.). One would inquire, however, why the separate interferences of w_L and PI should not have required concomitant investigation. The soil's ability to retain capillary tension should decrease with low w_L and PI values: but with soils of high w_L and PI the difficulty begins with the requirement of high p_c values in order to reach the s_{is}; thus the likely regressions should be concave downward. Moreover, it is frequently preferable to investigate variations, such as $\Delta u/\Delta p_c$ (cf. Lumb, 1977).

It may be hinted that if the plasticity chart had emphasized the importance of distances from the A-line (as in Fig. 10) the very first correlations would not have been proposed in connection with only one or the other of the two plasticity parameters. In a similar vein it is feared that many a problem may be generated for the future of soil engineering by such multiple regressions as have easily come into vogue rather unconditioned by theorization (e.g. Camargo and Salvoni 1978, Penna 1978, Teferra 1975, etc.) and unfethered to the interest in visual experience crystallization.

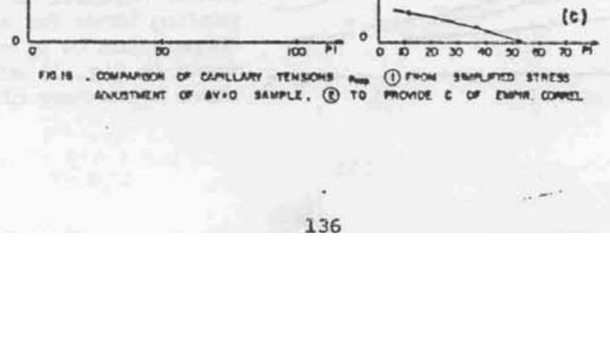


FIG 16 . COMPARISON OF CAPILLARY TENSIONS p_{cap} (1) FROM SIMPLIFIED STRESS ADJUSTMENT OF ΔV=0 SAMPLE, (2) TO PROVIDE C OF EMPIR. CORREL.

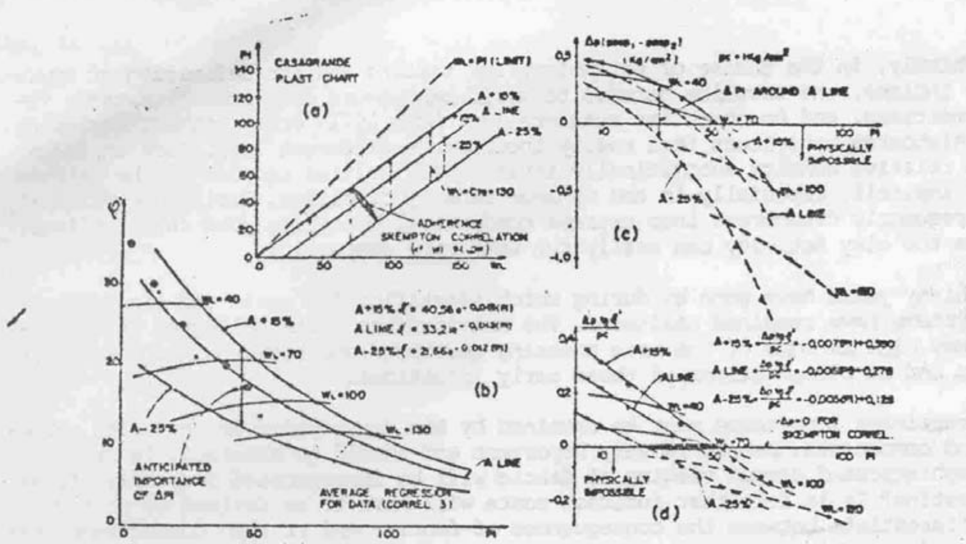


FIG 17 _ PLAYING WITH BACK-ANALYSES OF VARIATIONS WITH w_L AND AROUND A-LINE TO CHECK RANGE OF EXPERIM. DATA FOR WHICH CORRELATION JUSTIFIABLE.

5. BRIEF THOUGHTS ON REEXAMINING NEEDS REGARDING IDENTIFICATION AND CLASSIFICATION TESTS.

Firstly, for purposes of identification and classification, testing should not necessarily aim at direct determinations of some of the principal fundamental design parameters since frequently these are not sufficiently discriminating from soil to soil (for instance, the modest variation of drained ϕ' values from clays to sands), and in other instances may not create the necessary impact of cognitive perception or of descriptive capacity (for instance, the existing methods of describing angularity of grains). Often such direct determinations are much better substituted by indirect tests that measure the symptom rather than the cause, especially if such indirect tests have been devised to amplify the distinctions. A good example of the first type is the w_L parameter which makes it unnecessary in an engineering sense to determine the clay-mineralogy content: for instance, if montmorillonite is effectively present, to the extent to which it would influence fundamental engineering parameters it should begin by influencing the w_L and PI indices. The cone penetrometer has already been mentioned as an example of the desirable amplification of distinctions.

Secondly, there are many "lumped parameter" problems on which at present the existing prescriptions and correlations are seriously lacking and in pressing engineering demand. Among such parameters one might list rippability, groutability, drainability, erodibility, liquefaction potential, disintegration of grainsizes, susceptibility to piping erosion, seismic coefficients, and so on. There may be tendencies to attempt short cuts in the road to the solution, by devising new index tests, hopefully scientific, for each problem. The author would rather recommend a patient scientific examination of each case, to avoid as far as possible the multiplication of basic tests: one should preferably deploy revised versions of existing index tests by analysis-synthesis and suitable correlations, to compose the desired conclusions on the complex lumped parameter. Einstein is reported to have said that "the most incomprehensible thing about Nature is that it is so very comprehensible", which might be more of a truism than a truth, because that is so to ourselves, who presumably grew within the limits of her toleration of our incomprehensions. At any rate, we should not forget how few are the basic words from which languages developed, nor the persistent desire (and need) for a "unified theory".

Thirdly, in the choise of parameters for testing and for definition of appropriate indices, one shouldbe careful to avoid behaviours dominated by extreme value conditions, and to check for outliers (cf. Lumb 1971, etc.) without excluding outer histogram behaviours that really incorporate different laws. Such an index as the relative density automatically invites difficulties because of the extreme values implicit, especially in the minimum density condition. Tests on erodibility also frequently degenerate into extreme conditions. Meanwhile some ratio indices such as the clay Activity can easily run into wide dispersios.

Thirty years have gone by during which identification tests and classifica - tion systems have remained unaltered. The multitudinous data collected in the interim may hint at some of the more pressing questions and basic principles for revisions and complementations of those early intuitions.

Preeminent importance must be retained by the description of the solid structure and components. Fabric is very important and should be assessed: is it likely that sophisticated direct viewing of fabric will be incorporated into classification testing? It is felt that indirect tests will have to be devised to measure and differentiate between the consequences of fabric: and if such consequences can not be made striking, interest in the matter will not grow. The grainsize components must continue to be dominant in classification: but because of many soils there should be considerable interest in how different the grainsize distributions will result under two or three specially differentiated treatments (physical disintegration in varying degrees such as used by Annamalai et al 1975, colloidal dispersion, and even purposely strong chemical alteration). In coarse granular materials the grain shapes and resistance to crushing are known to be most important: the trouble is that no satisfactory index tests and presentations have been develcped. The standard grainsize distribution curve has not favoured visual perception of the very important problem of skip-grading, affecting piping: if the grainsize composition were presented as histograms on the semilog plot, the gap grading would cause immediate visual impact. Moreover, special treatments of grainsize and grain-shape distributions (e.g. Rousseau 1960, Matsuura 1963, Roman Alba 1973, etc.) may lie unheeded even when technically better, if but slightly more difficult to apply and not visually communicative.

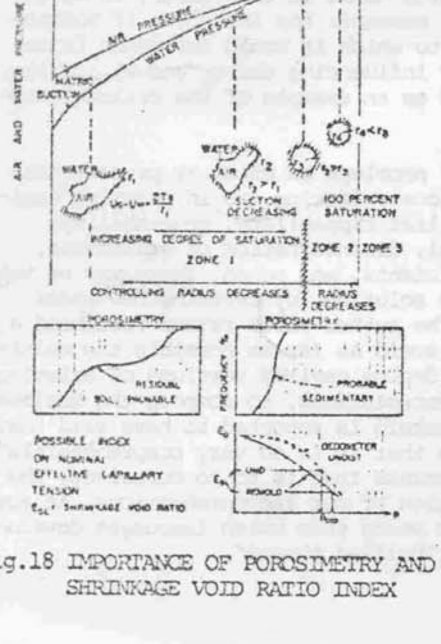


Fig.18 IMPORTANCE OF POROSIMETRY AND SHRINKAGE VOID RATIO INDEX

Secondly there has been growing recognition of the importance of porosimetry (e.g. Kezdi 1968, Sridharan and Venkatappa Rao, 1975, Auvinet, 1977, etc.). In the behaviour of residual soils and laterites the existence of a certain percentage of macropores appears to influence greatly, as schematically shown in Fig. 18. However, once again, it may be that indirect indices to such fabric and porosimetry may be more practical than direct determinations. For instance, shrinkage limit, or, more generally, a shrinkage void ratio (starting from undisturbed conditions), and especially its comparison with the respective index after remolding at constant water content may be a good indirect index of nominal effective capillary tensions: so also one might investigate the relevance of the sudden loss of strength in unconfined compression tests upon different rapid submersions of the specimens.

such classification indices would in no way supplant the tests for the properties directly at stake, such as the psychometers etc. (cf. Richards 1977, etc.).

Finally, the influence of colloid chemistry and dissolved chemicals in pore water, or even of different pore fluids, has long since been recognized (e.g. Genevois 1977, Lumb 1977, etc.). What is necessary is to devise index testing that would force such chemical influences to maximum discrimination conditions, reminescent of the Atterberg Limit tests forcing water contents to their limiting influences on clays: most functions are better assessed if saliently evidenced as extended to fairly extreme conditions.

6. TOWARDS AN OBJECTIVE SYSTEM OF SITE INVESTIGATION

It has been postulated that site investigations will probably progress towards indirect determinations of significant index parameters, and acceptance of vertical profiling as a continuum of varying degrees of occurrence of specific symptoms. In a sense the Dutch cone penetration test was a beginning in this direction. We shall not delve into discussions of the degree to which sediments include vertical or horizontal discontinuities in comparison with vertical or horizontal continua: geologic, hydrologic, and meteorological factors justify both the more frequent continuum condition and the occasional discontinuity condition. At any rate, a discontinuity reveals itself automatically when a medium is investigated as a continuum. The developments of specialized sensory equipment have begun but recently and show great promise (e.g. Schwab and Broms, 1977, Baguelin et al, 1977, Jones, 1977, Windle and Wroth, 1977, Sanglerat, 1976, Gielly, 1976, Wissa, 1975, Torstensson, 1975 Baguelin and Jézéquel, 1975, etc.).

The only thought that the author can offer in this connection is that there has been a comprehensible but regretably atrophying tendency for each inventor or developer of an equipment and technique to embrace and promote his development as singled out and self sufficient. Moreover, the unsatisfactory present situation lies in the fact that in attempting to confirm a specific new technique the usual simplest recourse is to compare with results of conventional tests that we have just shown to be fraught with difficulties and errors.

In fact, any soil that we have subordinated into a highly simplified category has already been emphasized to embody a multitude of interplaying information taxa, from which the visual-tactile multivariate cognitive ability of the professional (developing or possessing experience) draws a vast cognitive content. We should not allow ourselves to be fooled by the very limited descriptive content of our phraseology and/or index parameters. Thus, we shall only begin to substitute adequately, and even with significant gain, our visual-tactile perceptions if and when our profiling will be simultaneously on multiple differentiated parameters. Begemann's (1965) suggestion for classifying soils (by the friction jacket cone penetrometer on the basis of the interplay of two simultaneous equations for two parameters), embodies the first step in the direction that the author presumes to be required, inevitable, and fruitful (cf. Sanglerat 1976, etc.). Fig. 19 exemplifies schematically what may be the results of multiple sensory profiling and computorized statistical interpretation by multivariate correlative techniques (e.g. Alonso and Krizek, 1975 Muspratt 1972, etc.). Once again one emphasizes that one need not seek (in fact should preferably not) direct determinations of known index or fundamental parameters. Generally it is much better to develop new indices of high discriminating power and high capacity for resolution of the simultaneous equations and regressions. Classic geophysical testing and in situ tests to determine moduli of elasticity or shear strength are examples of solutions poorly fitted for classification and identification. The cone penetrometer based on point resistances at depth has already been mentioned as an example of a highly sensitive parameter. The author recognizes that such a recommendation goes contrary to what is most generally accepted as ap-

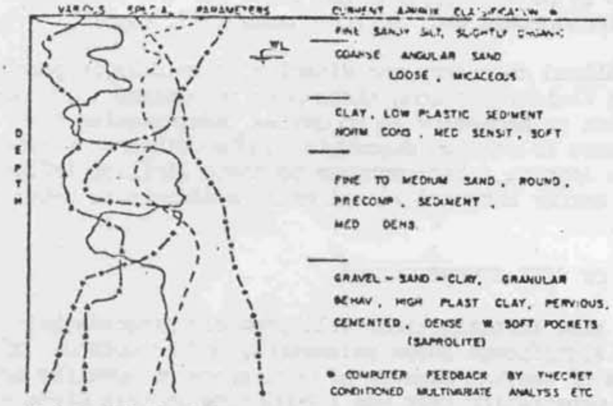


FIG. 19 - MULTIPLE HIGH DISCRIM PARAMETER INDIRECT PROFILING AND MULTIVARIATE ANALYSIS CLASSIFICATION FOR FUTURE?

propriate (e.g. Arman et al, 1975) which is the search for "test results compatible with present design methods and theories": as already mentioned, for purposes of identification and classification it seems to the author that emphasis should be on amplification of distinctions; the in situ tests for parameters applicable in design should constitute a second stage of investigations.

The question of breadth and intensity of a particular site investigation will doubtless be judged by Bayesian approaches or such proposed methods as the Minimum Akaike's Information Criterion (MAIC) procedure (cf. Matsuo and Asaoka, 1977 etc.).

Finally one should remember that although it is necessary to tie back any new tests with reference to experience with those being superseded, one must not fall into criticising the new test resultson the basis of such comparisons, which would imply validity of the old ones (cf. Baguelin et al, 1975, etc.).

7. CONCLUSIONS

Soil classifications have remained a rather confused chapter because of arbitrary test standards that cause such obvious alterations in the feel of the total soil that the experienced professional gradually sets aside test results and relies mostly on his visual-tactile inferences. Moreover, the identifications and descriptions have inexorably fallen into egocentric practices, fostered further by the same insufficiently striking descriptive indices and visual presentations. Test and parameters revisions can be suggested: possibly they have not merited much attention because of the egocentrism of each specialist in solving his problem. Indirect subsoil profiling with multiple highly discriminating sensory equipment appears to lie ahead with promisse, associated with multivariate analyses. Possibly a part of the yet modest success is due to the confusion between classification index testing and in situ tests for fundamental parameters: the latter seldom have the desired highly discriminating power for effectiveness towards identification and classification. It is comprehensible that older more experienced professionals may be pessimistic regarding any proposals that do not center on visual-tactile manipulation of specimens: but it is certain that sensory devices and interpretative correlations and indices will be developed to supplant the visual-tactile cultural background, since in most attempts hitherto it may be said that absence of evidence is not evidence of absence.

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