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TROPICAL SOILS WITHIN SCIENCE FOR SUSTAINABLE DEVELOPMENT

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ABSTRACT

The relatively neglected condition of tropical soil geotechnique is submitted through few examples of priority: and an earnest appeal is made for creative, and strenuously belaboured, autochtonous systematic cumulative developmental research. The absurdly broad generalization "tropical soils", belying distant angle of vision, is narrowed to Saprolites and Laterites, Unsaturated. Identification, Characterization, Classification fail grossly, through use of index tests developed for, and successful with, unitary-grain sediments: at the other extreme, revert to broad tabulations, incapable of orienting the indispensable Parametrizations. In situ stresses hypothesized, and measured, cannot avoid being far too erratic. In situ suctions, and their consequences on strengths invoke serious questioning. In short, one submits whether the idealized Terzaghian three-phase model, solids/ free water/free air, of conventional geotechnique, with its successes, doesn't hinder, by stifling any courageous independent generalized mental model.

INTRODUCTION

In the face of so very vast a subject one must necessarily dispatch promptly some oft-repeated platitudes, to reach the core of a message of some purpose.

It is interesting to note that the focus is not on Science **and Technology**, with premises, methods and vectors quite distinct. But the vector becomes transparent when insinuated by the words **development and sustainable.** Science presumably does not damage sustainability, whereas **applied science through technology and the prevailing view on development** can, and mostly does.

It is well understood that this brief presentation is concerned only with **Soil Engineering:** both with regard to soils as they occur in situ, and affect the environment by landforming processes of sliding, erosion and diverse depositions; and also with regard to their uses as they lie available (a) for foundations and underground construction, and (b) as the cheapest and most widespread construction material on site for borrow pits and embankments of all purposes. However, in a first attempt at effectiveness through conciseness, we shall totally exclude broaching the facet of construction materials (N. B. quite frequently rather favourable, through compositional heterogeneity and unsaturation).

The very denomination "Tropical Soils" belies too remote an angle of vision, permitting such a broad generalization as nobody would envision for, e.g. "Temperate Soils". Moreover, Tropical Regions obviously include vast areas of recent sediments, saturated, quite analogous to their counterparts in the much more investigated First World domains of conventional soil mechanics and forefront advances, widely divulged, learnt and followed.

Thus, attention is herein focussed on Tropical Residual Soils, Saprolites and Laterites of wetter hydrologies, since the trend is to associate tropical regions with much deeper horizons of **chemical weathering**. (N. B. The similarly vast areas of semi-arid to arid conditions which do not include such deep chemical weathering will be set aside for the present, although they constitute another important subgroup of Tropical Conditions with shallower weathering but frequent micro-cementations of saline crystallizations, evaporites, and collapsive/expansive peculiarities). What must be accepted as an uncontested siamese twin of so-called Tropical Soil attention, is the much more recent and complex behaviour of **unsaturated soil geotechnique.**

Besides sundry and persistent scattered papers on these soils, we may summarily list the following Conferences and International Group activities:

1 "Engineering and Construction in Tropical and Residual Soils". ASCE GT Eng'g. Div. Specialty Conf. Jan/82, Honolulu (1 Vol.).

2 1[st] Int. Conf. on Geomech. in Tropical Lateritic and Saprolitic Soils, Feb/85, Brasília (4 Vol.) sponsored by ISSMFE (International Society for Soil Mechanics and Foundation Engineering); also SPECIAL Vol. "Peculiarities of Geotechnical behavior of Tropical Lateritic and Saprolitic soils" ABMS, Tech. Com. of ISSMFE on Tropical Soils, Progress Report 1982-5.

3 2nd Int. Conf. on Geomech. in Tropical Soils, Dec/88, Singapore, (2 Vol.), ISSMFE sponsored.

4 Specially notable contributions from:

4.1 Geotechnical Control Office, Hong Kong (post 1978).

4.2 Prof.P.R.Vaughan, Civil Eng'g.Dept., Imperial College, London (with collaborators, post 1980).

4.3 Prof. D.G. Fredlund, Civil. Eng'g. Dept., Univ. of Saskatchewan, Canada (with collaborators), advanced work and book **Soil Mechanics for Unsaturated Soils.**

5 ISSMFE Technical Committees at work TC-6 **Unsaturated Soil;** TC-22 **Indurated Soils and Soft Rocks;** TC-25 **Tropical and Residual Soils.**

I submit that attention might be called to three rather frustrating points:

(1) There has been no 3rd International Conference, after Singapore 1988;

(2) The three Technical Committees have not submitted any summary written report on activities during this ISSMFE Presidential term, ending on Sept. 12, 1997;

(3) The South-Central American Region decided, in May 1994, to join the Sister Societies International Society for Rock Mechanics ISRM, and International Society for Engineering Geology, IAEG, in a Technical Committee entitled "**South American Committee on Weak Rocks**".

One cannot fail to sense much confusion, lack of zest and direction, and even energy repetitiously spent to no avail. Because of tightly limited space and time, and the yearning to appeal for direly-needed **research orientation and support**, this presentation focusses on a minimal number of facets flagrantly calling for **critical unfettered questioning and advance.**

2 Identification → characterization → classification → **parametrization.**

Avoiding any autocratic tinge whatsoever, I must emphasize the above sequence as obligatory by logic and goal. First you identify the global nature of the soil, then you characterize it (detail narrower characteristics, hopefully by **index tests**), then you classify (group into presumably analogous cases, hopefully to favour predicting behavioural properties): and, finally, you must take the final step (always dodged) of supplying the predicted parameters usable for engineering computations, and at each step one must be strongly conscious of (a) **averages and confidence bands of dispersions** (b) the fact that some behaviours, cumulative, are associated with averages, while others (e. g. sliding on a slickenside, or failing in tension) tend to be conditioned by meaningful extremes. At any rate, what cannot be countenanced is the shirking of parametrization (as responsible as possible): it is the only way to **develop experience,** and to expose sterility, however well intentioned.

Examples

No single paper over the past 50 years has failed to emphasize that the index tests of grainsize analysis and Atterberg Limits (and such a gross composite as Activity), developed for sediments of particles separated to the unitary condition with **individual developed lyospheres**, are absolutely and logically inapplicable to **highly structured soils**, dominated by fabric, micro-cementations, clusters and nucleations! Yet, rather than "daring" to generate different, invented, tests (as the above two were, in their time, logically invented), one faces the**absolutely illogical rearrangements of classification nomographs** (cf. Figs. 1, 2).

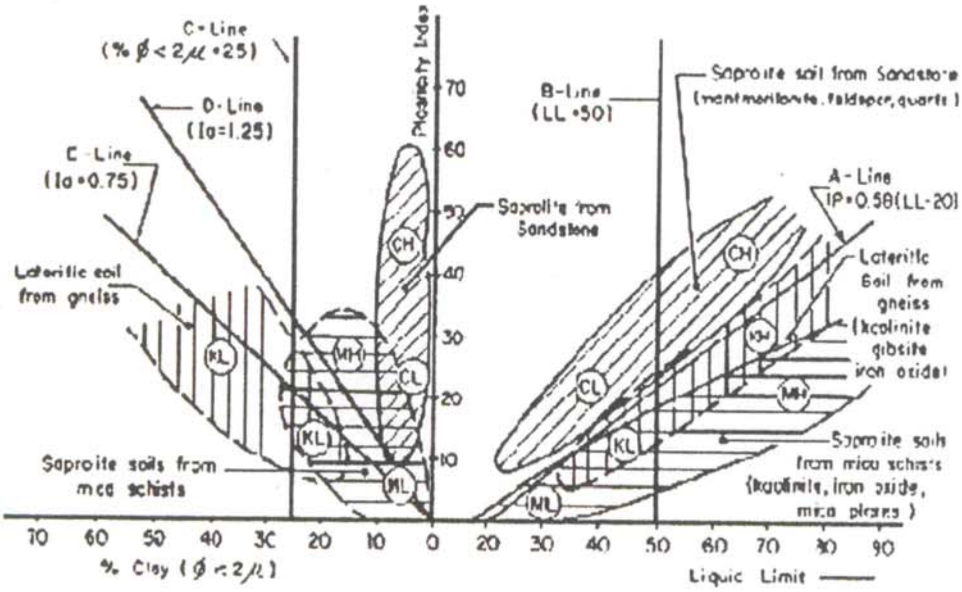

FIGURE 1 Associated plasticity and activity chart. Reproduced from Vargas, M., (1988)

At another extreme, retrogressive, we find such broad tabulations, Fig.3 as extracted from L. D. Wesley, 1988. With all respect for the inexorable indispensable background of **geology**, it hardly needs being emphasized that in order to be "of interest to **geotechnical engineers**" by being "expected to have **similar engineering properties**", such tables would have to narrow to much tighter (impossible) ranges. One recalls A. Casagrande's, (1960) reporting on Terzaghi's early intense effort through engineering geology "For two years Terzaghi went from dam site to dam site digesting geological reports and trying to correlate them with construction experience... In spite of utmost concentration on his self-appointed task, this first attempt turned out to be a **discouraging failure**". Indeed, along with all other fields, the tools of modern geological characterization have advanced exponentially: but (a) the needs and tolerances of Society have also become tighter; (b) academic research achievements have led to a sad loss of distinction between **scientific explanatory testing** and **engineering index testing.** Among countless examples let us cite: (i) such tests as scanning electron microscopy, x-ray diffraction spectrometry etc. for fabric, and its organization into the "levels" of **elementary, assemblage,** and **composite** (K. Collins, 1985) would never gain ground as **predictive index tests**, and so also would be rejected the traditional chemical and mineralogical tests (e. g. P. Rocha Filho et al., 1985); (ii) meanwhile, even so apparently obvious and practical a substitution as that of the saturated water content and limits of conventional clays by the **void ratio $\in_o$ concept** (e. g. Dib, 1985) has been thwarted by the **simple need to change the reconnaissance sampling** from the thick-walled SPT to driven hard-steel thin-walled shelbys, and collides against impracticability because of great heterogeneities and erraticities, **including those of real significance, to be assessed by porosimetry.**

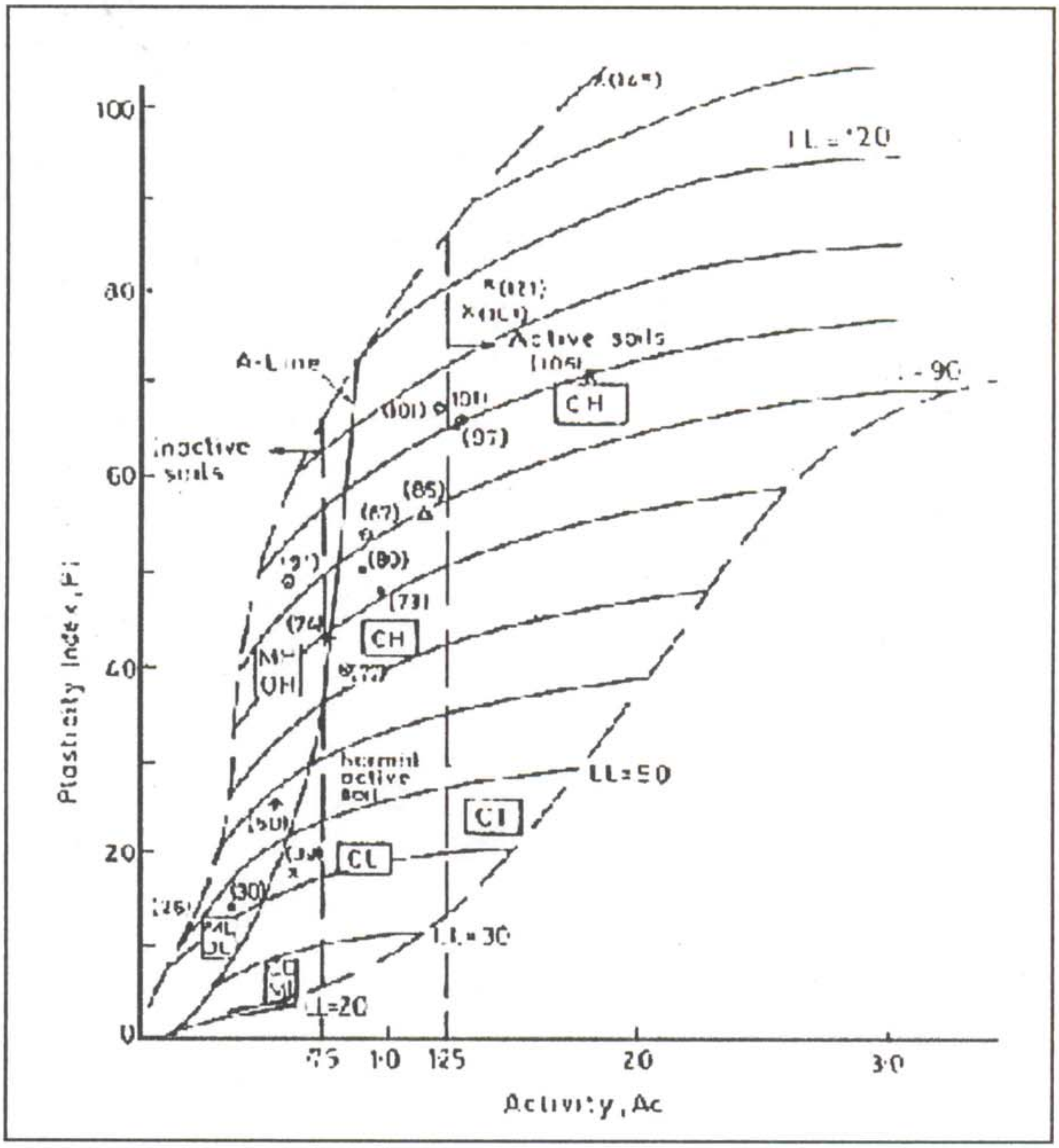

FIGURE 2 Proposed classification chart. Reproduced from Wesley, L. D., (1988)

How deeply and direly has well-intentioned teaching and **faith-full learning** stifled imagination and logical perception for a clear start , and fettered the courage to act with the necessary independence and risk?

3 In situ stresses under localized conditions, and consequences on important parametrizations

All geo-engineering computations start from overburden and seepage stresses as the natural causative factors: thus, as a consequence of a generally undisputed principle of homogeneous sedimentations (gradual increase of overlying weight), **assuming no shear stresses on vertical planes,** the uniform vertical stress γz (or $\gamma z \cos i$ in an infinite slope i) follows. However, even in sediments (and compacted fills) the principle of **stress redistributions and hang-up or silo-effect** between adjacent materials of different compressibilities (as proven since 30 years ago for earth-rock dams) should be **recognized as inevitable also in the subsoil,** prudently assessed (e. g. de Mello 1981), and guarded against. The situation is much more likely to interfere in saprolites and weathered rock horizons, ipso facto **differentially attacked.** By assuming uniform vertical stresses, the **logical natural heterogeneities** are disregarded, and such important **geotechnical conventional parameters** as the K'o (lateral stress ratio) and OCR (overconsolidation ratio) acquire gross additional erraticities. How can a professional geotechnician reach good decisions in the face of brutal erraticities?

Thereupon, in the light of the personal experiences with the widely different scales of erraticities between specimen-sizes and job-dimensions, and the systematic differences between **predicted behavior and performance** of projects, it was **postulated** (de Mello 1972), with independence but due diffidence, that sedimentary and saprolitic horizons be conceived in the light of statistics of "natural selection" starting from opposite directions. All horizons have inescapable statistical variations of soil elements. Thereupon in a sediment starting from zero strength progressively increased, no soil element would need to develop any more than the minimum capability of supporting the overburden stress: thus the engineering adoption of minimum values would be justified by concept, and not merely as dictated by prudence. Quite to the contrary, a saprolite starting from the very resistant condition of rock, would be gradually weakened at the weakest elements, progressively more prone to increased attack: greater attack would be permitted (even to the point of generating cavities, e. g. in karsts) to the extent that it was the stronger material elements that permitted it. Thus the load carrying capacities would be dominantly achieved by the stronger nucleations.

Any such candid hypothesis would entice curiosity and **systematic testing to prove/disprove**: normal and accepted destiny of any scientific hypothesis, free from preconceived notions. [N. B. Note that I am avoiding the **additional considerations on relict discontinuities**, of great importance, in a separate paper]. Regrettably there was no local sequel, in comparison with such brilliant theorizing efforts as those of Vaughan and Kwan (1984), for example. What are, meanwhile, the consequences on the important **parameters K'o and OCR**, defined and derived

Commonly used Names	Rigorous Pedological Names			Dominant clay minerals	Important characteristcs
	FAO	US Soil Taxonomy	French		
Lateritic soils Latosols Red clays	Ferrasols	Oxisols	Ferralitic soils	Halloysite Kaolinite Gibbsite Geothite	Very large group with wide variation in characteristics
Volcanic ash soils Andosols	Andosols	Andepts	Eutropic brown soils of tropical regions on volcanic ash	Allophane minor Halloysite	Characterised by very high water content and irreversible changes when dried
Black cotton soils Black clays Tropical black earths Grumusols	Vertisols	Vertisols	Vertisols	Smectite (montmorillonite)	Problem soils, high shrinkage and swell, low strength

TABLE 1 Distinctive tropical soil groups of interest to geotechnical engineers

MAJOR DIVISION	SUB-GROUPS	COMMENTS
GROUP A Soils without a strong mineralogical influence	(a) Strong macro-structure influence	Nature of macro-structure needs definition - stratification - fractures, fissures, void etc.
	(b) Strong micro-structure influence	Remoulding likely to strongly influence behaviour - sensitivity should be a useful indicator
	(c) Littlle or no structure influence	Probably a rather minor sub-group
GROUP B Soils strongly influenced by 'normal' clay minerals	(a) Smectite (montmorillionite) group	Problem soils, characterised by low strength, high compressibility, high shrink swell behaviour (similar characteristics to any montmorillonite soil)
	(b) other minerals ?	? ?
GROUP C Soils strongly influenced by clay minerals found only in residual soils	(a) Halloysite	Low activity soils, good engineering properties
	(b) Allophane	Low activity soils, with good engineering properties, characterised by very high water contents, and large irreversible changes on drying
	(c) Sesquioxide (lateritic)	Extremely variable group, ranging from silty clay to gravel

FIGURE 3 Reproduced from Wesley, L. D. (1988)

from conventional sedimentary geotechnique, and, if anything, **all the more important in residuals and saprolites** where stress-strain-time trajectories are unknown and judged determinant? Let us leave aside the test erraticities and biases and heterogeneities. The problem is aggravated by the fact that both parameters are **ratios** (increasing dispersion) **to a vertical overburden effective stress taken as uniform** $\gamma' z$. If a harder nucleus, revealing higher lateral stress, were compared with a higher (realistic) vertical stress absorbed by the more incompressible "partner", the K'o would naturally drop. And the higher laboratory-determined "nominal preconsolidation pressure σ'_p on harder nuclei" would similarly, ipso facto, lead to more realistic OCR values if compared with the **inexorable higher σ'_v prevailing in the harder nucleation.** [N. B. Setting aside the criticism against **OCR profiling** even in sediments, because for a constant overconsolidation Δ_p the ratio of $(\gamma' z + \Delta_p) / \gamma' z$ is algebraically variable, with **meaningless values at the top** where $\gamma' z$ starts close to zero, and OCR tends to infinity].

In short, as regards geotechnical testing and determination of parameters for honest and fruitful interpretation of engineering behavior, the impressive collective experience from conventional sedimentary geotechnique may have proved a hindrance? Incidentally, the split between scientific theory and practical engineering vision shows up pointedly with regard to the **K'o** value to be assigned to sound rock of high compressive strength: what is the role of real cohesion in comparison with preconsolidation-produced cohesion? Is the lateral stress in rock very low, zero, or would it follow the routines of elastic theory with its basic hypothesis of **constant volume at levels of differential equations?** It would appear that much of the dichotomy would be dispelled firstly by recognizing the totally different **levels of deformation of interest** (Note: Stress is a Philosophical Concept: Deformation is a Physical Reality).

At non-micro deformations, especially nearer plastification (N. B. **K'o** cannot be a constant, should be defined as $\Delta\sigma'_h/\Delta\sigma'_v$) the constant volume hypothesis collapses in rocks and weathered rocks. At the absolutely micro-to-zero lateral deformations, **of no interest to engineering,** the real stress should respect the idealized theoretical value. How then do soils transition from rock through weathered rock → saprolites → residuals, and, inversely, from sediments → overconsolidated strata → indurated soils, as regards **K'o** and **OCR**? One must quite dispell the subliminar impressions transmitted in **all publications** that these parameters are **basic - value properties** for a horizon : the varying **K'o** = $\Delta\sigma'_h / \Delta\sigma'_v$ if measured with scientific precision depends on proportions of incremental to original stress, proximity to yield, and dilatant-vs.- contractive behaviors. Have we reached reasonable knowledge of stress conditions of tropical residuals and saprolites in job-proportion dimensions compared with erratic soil elements?

4 Unsaturated horizons, suction, and consequences

The field is new and yet but incipiently documented, despite the long-standing recognition of the importance of wetting to compressibilities and strengths, and of suction-reduction to tropical natural slope destabilizations. At least seven different devices for measuring soil suction (Fredlund and Rahardjo, 1996, Ridley and Burland, 1993) are in experimental use, with presumed validities basically compared one vs. another. As regards **engineering behavior of slope destabilizations,** the suction values, subsoil suction profiles, and the very shear strength concepts and equations cry out for questioning. Note, meanwhile, that by inertia from the earliest conventional geotechnical profiling practice, saturated, not 0.1% of profiles presented in papers ever draw even the negative hydrostatic (capillary) pore pressures above water table!

The following data are taken from Fredlund and Rahardjo. First, the astounding magnitudes of suctions and suction differences. Note that in Fig. 4 we are presumably dealing with suctions of the order of 200 to 1000m of head of water, and with variations of about 200m at similar depths in analogous holes.

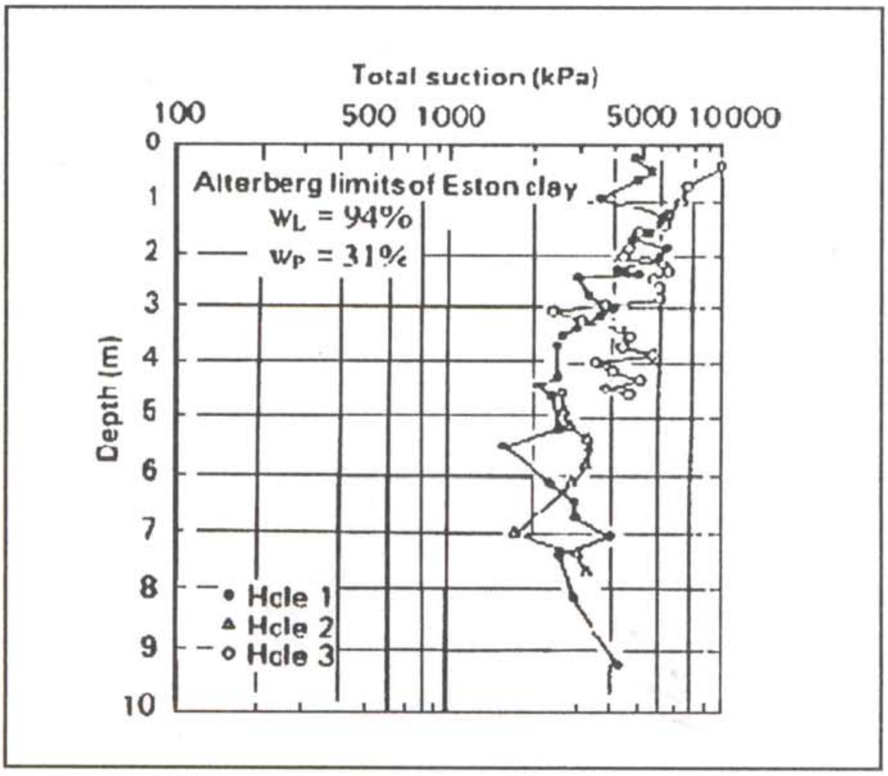

FIGURE 4 Total suction for Eston clay using the filter paper method.
Taken from Fig. 4.30 of Fredlund and Rahardjo

What can possibly be the **behavioral and engineering significance** of such astounding suctions, if realistic? What shrinkage-swelling variations should correspondingly prevail? In Fig. 5 we depict the data from a decomposed granite profile, with values already seemingly more acceptable despite similarly large erraticities. Finally Fig. 6 reproduces the quandary of a typical profile of test results. How does one interpret such condictions without **a)** scientifically checking through porosimetry etc, differences between nuclei and micropores (higher suctions) vs. short-term dominance of macro-pores? **b)** practically confirming with simple engineering tests such as controlled showering-infiltration runs on an instrumented area (e. g. Vaughan 1985) including additionally comparative infiltrations of non-wetting fluids?

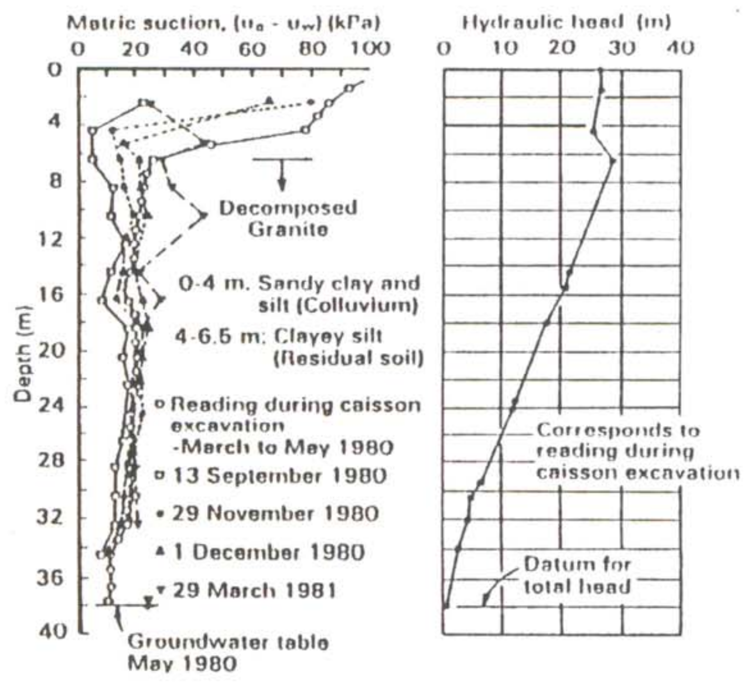

FIGURE 5 Matric suction profile along shaft A (from Sweeney, 1982). Taken from Fig. 4.48 of Fredlund and Rahardjo

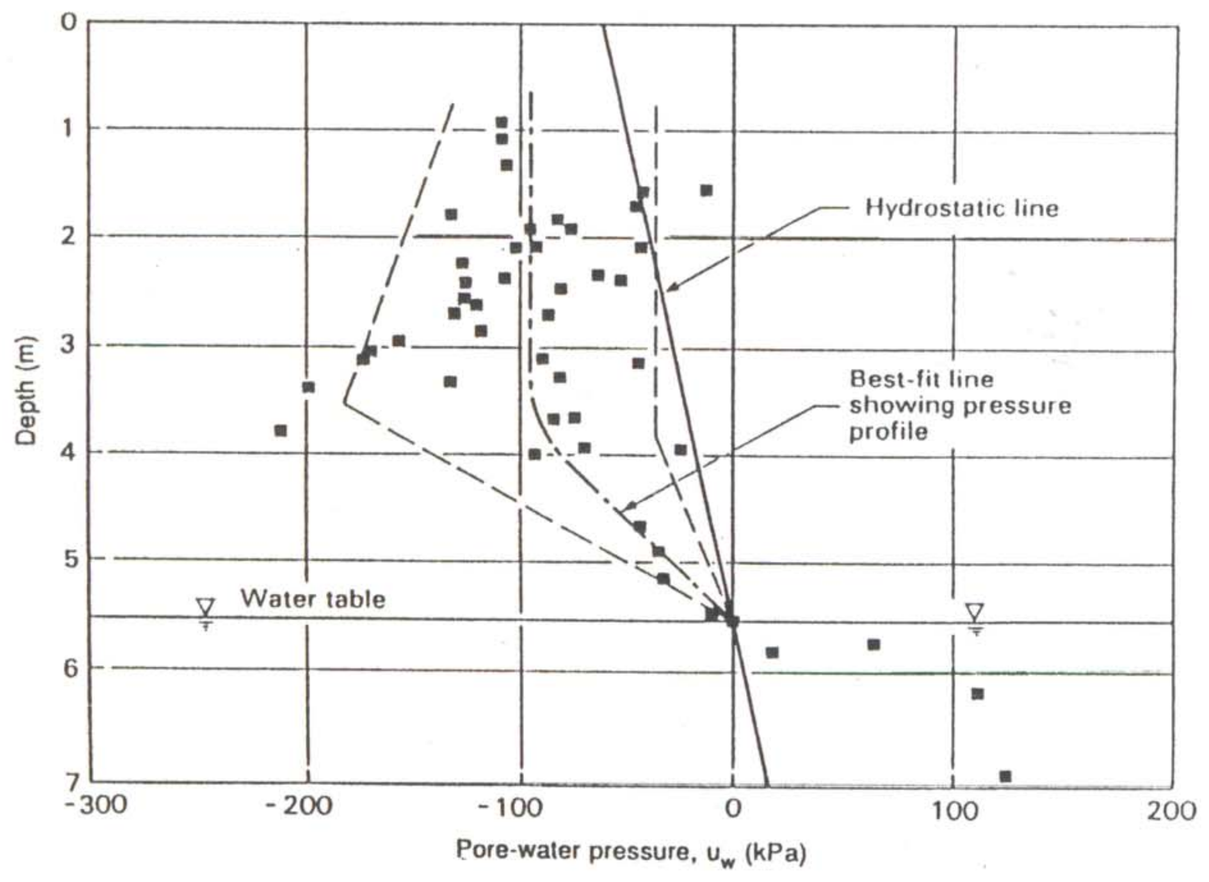

FIGURE 6 Negative pore-water pressures measured using the AGWA-II thermal conductivity sensors on undisturbed samples. Taken from Fig. 4.79 of Fredlund and Rahardjo

The point is that since suction is a very nevralgic transient state, one hesitates to imagine that high natural suctions should continue to be produced after tens of thousands of hysteretic natural cycles of infiltrations-evaporations.

Finally, upon examining the premises of the formulated shear strength equation $\tau_{ff} \approx c' + (\sigma_f - U_a)_f$ **tang** φ^b the following questionings arise:

(1) seeing that the theory as adjusted to unsaturated soils accompanies in essence some **trigonometric adjustments** to fit to experimental data, under what model of particulate behavior (with lyospheres) would one justify that the ds/dσ' generated by external isotropic stresses should reasonably be different from that produced by the internal spherical effective stressing generated by matric suction?

(2) In accordance with the scant experimental data plotted in Fig. 7 (reproduced from tables from Ho and Fredlund, 1982, and Fredlund and Rahardjo, 1996) by what intuition or reasoning would one unquestioningly accept a hypothesis that φ^b should show such poor and queer hypothetical statistical correlations, or lack of correlations?

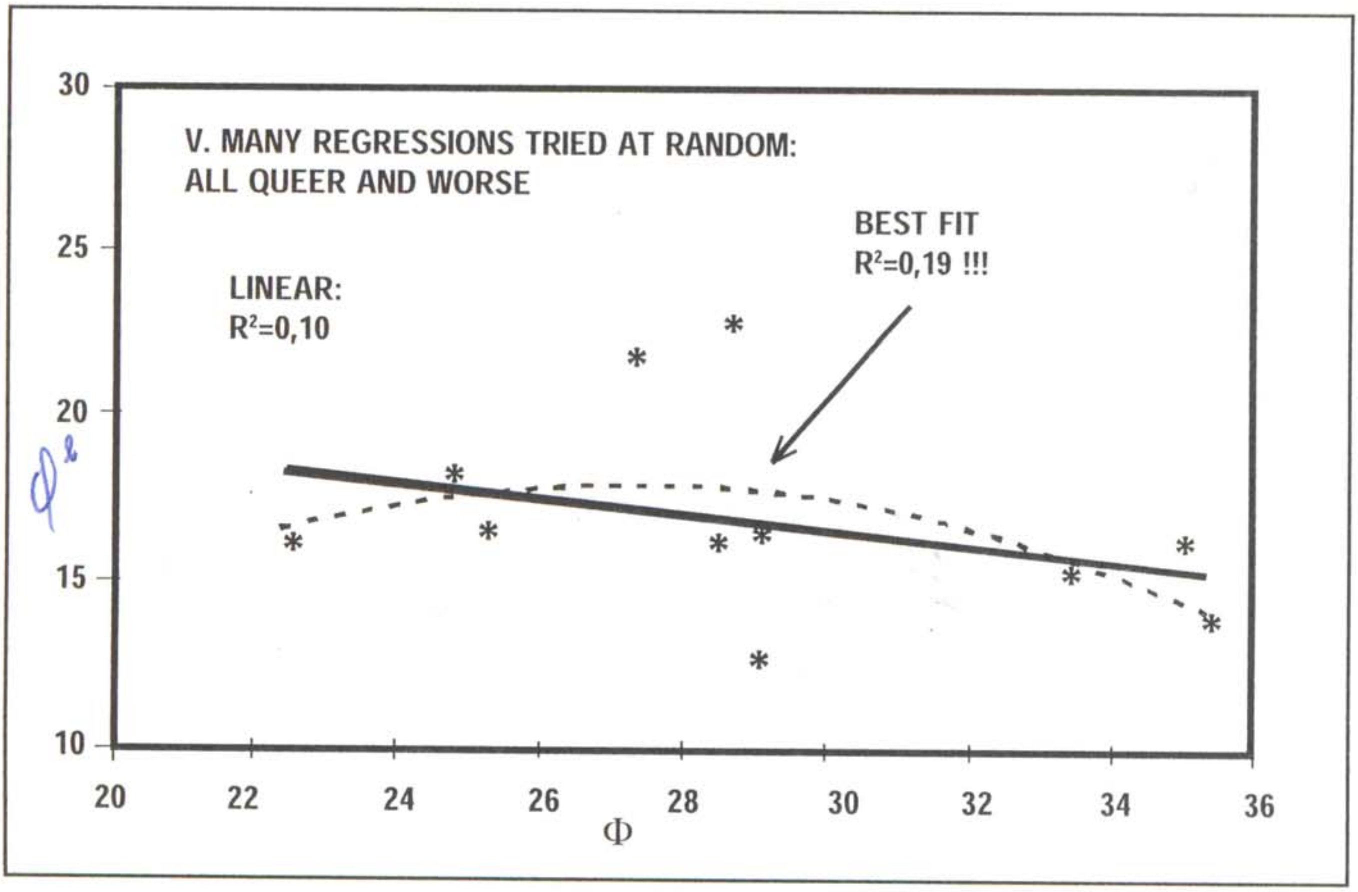

FIGURE 7 Scant data of comparative values of ds/dσ' (from Fredlund and Rahardjo)

Once again, another dominant topic for tropical residuals and saprolites that is being conducted in a submissive, hap-hazzard, left-handed manner of mere accumulation of scattered data through theorizations and testing procedures under use in conventional geotechnique, themselves convincingly confirmed under laboratory conditions but far from meeting the needs of prediction vs. performance in professional practice. Professional practice continues to rely on **conservative prescriptions,** and to be met with occasional flagrant failures.

5 *Frustrated conclusion and construtive proposal*

Space limitations are a tragic additional shackle. I shudder at the fear of appearing to be negative: but it is inevitable that we only take cognizance of the continuum when recognizing the discontinuity, and of the extent of knowledge when its limits and limitations are exposed. Etymologically the word and concept of **laboratory** came from **labor** and **orare** (praying, for illumination and supernatural help, not oratory). We must first imbue ourselves with the important difference between **a posteriori** erudite scientific theorizations of explanatory interest, and the free/courageous **development of a priori index tests,** of progressive precisions/costs, for **a priori uses for a purpose achievable within acceptable precision.** Sophisticated testing can never be considered for predictive index testing: it can be useful in a-posteriori scientific justificative testing to complement index testing. For instance, for "fabric" conditions the predictive index testing could be envisaged from the angle of comparative shrinkage tests (undisturbed vs. remolded, affected by suctions from corresponding porosimetries as resisted by effective stresses) or by differences of grainsize curves with water disintegration-activation vs. as influenced by different electrolytes and non-wetting. Reasonably supported imaginative compounded with intense work.

It is indispensable to stop the trend of piecemeal adjustments of the complex generalized soil mechanics within the successful waistcoat of Terzaghi's milestone theoretical-practical achievement for "conventional soils".

The examples summarized should merely serve to be of impact, possibly suggesting that a generalized theoretical cause-effect model for **deformations** (under all causes, not merely stress) should include **more than the three conventional solids-water-air phases;** possibly micro-clusters besides grains, cementations, two water phases (free water and lyospheric water) and so on. An intense debating workshop is highly recommended, with "authorities" present but kept silent in the background. And thereupon, a program of systematic **research/development** must be established, for a **renewed school of thought,** and hard work. Give free bridle to dreams, and then work to make them reality, without the pressure of immediacy.

In the words of Antonio Machado "Nuestras horas son minutos, cuando esperamos saber, y siglos cuando sabemos, lo que se puede aprender", or "Our hours are minutes, when we presume to know, and centuries when we know, how much can be learnt".

REFERENCES

BJERRUM, L., CASAGRANDE, A., PECK, R. B., 'SKEMPTON, A. W. (1960) "From theory to practice in Soil Mechanics", *Wiley. (p.5)*. COLLINS, K., (1985), "Towards characterization of tropical soil microstructure", *1st Int.Conf. TROPICALS '85, Brasília, Vol.1, pg.85.*

DE MELLO, V. F. B., (1972), "Thoughts on Soil Engineering applicable to residual soils", *3rd SEASIAN CSMFE, Hong-Kong, p.5-34.*

DE MELLO, V. F. B., (1981) "Facing old and new challenges in Soil Enginering", *M. I. T. Symposium, Past, Present and Future of Geotechnical Eng'g. p. 160-204.*

DIB, P. S.,(1985) "Compressibility characteristics of Tropical Soils making up the foundation of the Tucuruí dam in Amazonas (Brazil) *loc.cit., Vol.2, p.131-141.*

FREDLUND, D. G., and RAHARDJO, (1996) "Soil Mechanics for Unsaturated Soils" *Wiley.* HO, D. Y. F. and FREDLUND, D. G., (1982), "Increase in strength due to suction for two Hong Kong soils" *Proc. ASCE GT Specialty Conf. 1982, Honolulu, p. 263-295.*

RIDLEY, A. M. and BURLAND, J. B., (1993) "A new instrument for the measurement of soil moisture suction", *Geotechnique 43, 2, p. 321-324.* ROCHA FILHO, P. et al (1985) "Qualitative influence of the weathering degree upon the mechanical properties of a young gneiss residual soil", *1st Int.Conf. TROPICALS '85, Brasília, Vol.1, p.281-294.*

VARGAS, M., (1988) "Characterization, identification and classification of Tropical Soils" *2nd.Int.Conf. Geomechanics in Tropical Soils, Singapore, Vol.1, p. 71-75.*

VAUGHAN, P. R. and KWAN, C. W., (1984) "Weathering, structure and in situ stress in residual soils", *Geotechnique 34, 1, p. 43-59.* VAUGHAN, P. R., (1985) General Report "Mechanical and Hydraulic Properties of tropical lateritic and saprolitic soils, particularly as related to their structural and mineral components", *1st Int.Conf. TROPICALS '85, Brasília, Vol.3, p. 231-263.*

WESLEY, L. D., (1988) "Engineering classification of residual soils" *loc.cit. Vol. 1 p. 77-88.*