Instabilizações de Nossos Taludes Residuais-Saprolíticos: Conceitos Esquemáticos reapreciados

Destabilizations in our tropical Weathered Slopes: Concepts Schematically Revisited

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ABSTRACT: Many have been the publications, with some definite advances. It is felt, however, as we seem to approach diminishing returns, that questionable postulations persist, principally because this important field has been handled left-handed, with too little research specific to it. The practices of conventional sedimentary soil mechanics have also hindered independent approaches. An exercise of questioning is submitted, with due provisos, for promoting a needed dense workshop.

RESUMO: Já há muitas publicações, e alguns avanços definitivos. Porém, enquanto nos parece aproximar-se a exaustão de retornos, persistem postulações questionáveis, em parte porque este campo importante tem sido atendido pelo flanco, sem pesquisa específica para ele. As práticas da mecânica dos solos sedimentar também dificultaram aberturas independentes. Submete-se um exercício de questionamento, com curiosidade difidente, a favor de um workshop denso.

1. INTRODUCTION

I shall set down the classic paper-chapter by Terzaghi (1950) as the start of our trek. Doubtless subjective, and now indefensible. It arose out of the major slide of the Serra do Mar, because of excavation for extension of the Cubatão Powerhouse of the Canadian-Brazilian Light and Power Co. . I worked at the tail end of this problem (1949-51) together with Portland P. Fox, Geologist. The slide was really abetted by three factors: a deep toe excavation, a rising water table cut by the excavation, and heavy rains; it was stabilised by obvious toe confinement, by applying an impervious bituminous cover, and a brilliant geologically-oriented drainage gallery depressing the water table deeply (Bjerrum et al 1960).

Terzaghi's creative intuition and undisputed merits allowed him liberties in favour of the profession, but note the interest on different emphasis of priority destabilising factors.

Firstly, quoting from a Consulting report (p.413 loc.cit.) we note no emphasis on infiltrations, cleft-water pressures, or suctions: "Provisions should also be made to determine the effect of a bituminous coating on the position of the water table beneath the treated area. The surface treatment costs money and THE EFFECT MAY BE MUCH LESS IMPORTANT THAN WE THINK". Secondly, we note in the case characterized as "During a tropical cloudburst, involving a precipitation of 9 inches in 24 hours, a slide OCCURRED on a slope rising at an average angle of 30° [1]" that by subsequent heaps of observations, diagrams such as Fig. 1 herein (from the idealized Fig.15, p.242, original) were idealized to present relations between piezometric head and slide movements. Fox and I, who then handled the monitoring data, were surprised at the selective handling of buckshot diagrams, and the linearization, which today must be critically viewed on multiple counts. But, was it not a useful STARTING REMINDER? And must not

[1] It is necessary for me to note that according to the oral testimony of Senior Engineers internally involved, the sliding was started in the dry season July-Oct. 1946, and is far from representing a typical case, having been irrefutably triggered by speedy excavation below the groundwater table at the toe. The "tropical cloudbursts" and risen water table acted on the mass already sliding and (presumably) cracking at surface.

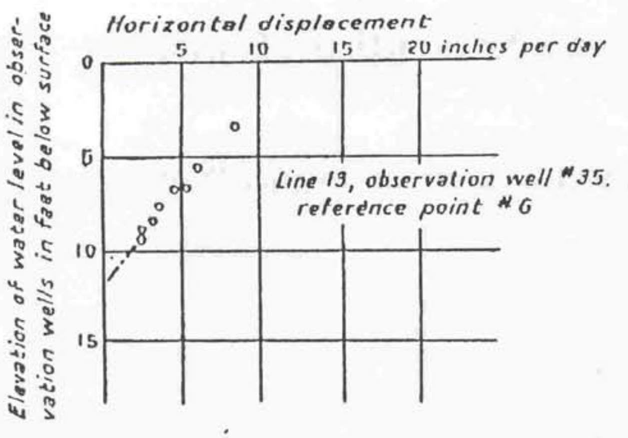

Figure 1 - Extracted from a buckshot diagram.

our technologies always go through such steps? Thank you, Karl Terzaghi, for so good and so oversimplified a starting idealization: is it your fault that we have stuck to one, then two, then three, well-intended hypotheses rather than SEEKING TO SEE AND DIAGNOSE REALITY always much more complex?

It is sterile to struggle for solutions in the dark, without trial diagnosis and data: once areas of ignorance are honestly defined, the formulation of solutions, and their successes/ defeats, only invite concerted efforts.

A pathology that at scattered moments of given outward signs strikes like an epidemic killing great numbers, is generally interpreted at the start as due to a single bacterial/virus infection: but, like the common "flu" it is provocable by a number of different complex composite factors; it is our psychology that illudes us into seeking a SINGLE FACTOR... by the Philosopher's Stone Complex of mediaeval times. Any thesis and action needs a background THOUGHT INCENTIVE. Recognizing likewise my inevitable distortion to a personal bias, I start by confessing that my reasonings have been influenced by: a) vague concepts of Nature's equilibrium at minimized potential energy, b) principles of NATURAL SELECTION moving weakest combinations, c) the multitudinous varieties of conditions within which events occur under statistical laws, either statistics of averages (Gaussian etc) when involving larger integrated effects, or statistics of extreme values when centered on localized point-scale episodes, near limiting (tail-end fractile) conditions (Lumb, 1970).

For me the possible causes of localized destabilizations are many, most of them small under average conditions, but capable of growing to untenable proportions under extreme combinations. Under exacting repeated recurrences natural slopes geomorphologically exhibit a nominal FS $\approx$ 1.0+, gradually lowering with time because of strength loss by weathering (de Mello, 1972)[2] . But episodes of much lower recurrences intervene. Since destabilizing factors are MANY, with various probabilities of intervening, generating COMPOSITE probabilities INCLUDING EXTREME VALUE CONDITIONS, our quests and solutions have to start from a big tree with branches to be systematically pruned under ENGINEERING PRIORITIES AND CONVENIENCE. Since in engineering practice of DECISION AND ACTION we are always required laudably to accept things as known, the importance falls on emphasizing at each step WHAT IS NOT KNOWN, so that the subsequent step of progress be always our goal.

My offer boils down to some thought experiments for natural, or near natural, slope conditions. Terzaghi's Serra Slide solution represents a typical forceful CHANGE OF STATISTICAL UNIVERSE, (cf. Rankine Lecture, 1977) used compoundly: a) since rainfall (infiltration) is the major varying contributor, an impervious surface cover dominates by excluding that effect; b) at the other end, a truly efficient lowering of the groundwater excludes the uplift pore-pressure mass destabilizing effect. Unfortunately we have to deal with natural conditions where such radical exclusions can rarely be applied.

2. GRADUAL LOSS OF STRENGTH BY WEATHERING.

2.1 *Chemical.*

The thought experiment makes it irrefutable. Some recent episodes along the Dutra and Anchieta Highways, opened 50 years ago, have given proof evident, to some surprise, that hitherto "immovable" slopes suddenly slid due to rains, in APPARENTLY RIGID-BRITTLE behaviour.

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[2] Incidentally, since to my knowledge not a single research reference can be found to quantifying the minute rates of change of parameters under (accelerated) weathering, it may be of interest to transcribe an indication set down by Terzaghi in one of his consulting reports, 1949: possible settlement of the anchor blocks due to weathering 0.007 to 0.75mm per year, average 0.03mm/yr.

Note, however, both for field comparisons of natural vs. cut slopes, and for planning research, that one would anticipate a linear semilog exhaustion-attenuation behavior, whereby natural effects would be negligible unless seepage waters change (e.g.acid rains?).

Geology has been vociferous, a posteriori, in slide events, but under provisos: (1) seldom descends to specifics in dimensions of typical slides (e.g. 30m wide x 70m long x 10m deep) with corresponding parameters; (2) never, to my knowledge, heeds the need of comparing the SLIDING area with the four contiguous NON-SLIDING areas (top, two sides, and below) (de Mello repeatedly e.g.1984). Geology is acceptably fixed for time intervals of our interest, but not so the micro-geochemistry of cumulative change, difficult to investigate and thus furtively set aside, UNATTRACTIVE TO RESEARCHERS. The cases are few, but sufficient to induce one presumed APPARENT OBSERVATIONAL FACT, to which to adjust our reasonings and research programs. The strength loss seemingly occurs with too little deformation for detection by monitoring of displacements: apparently a rigid-brittle loss of cementitious cohesion, and, in steeper slopes evinced by tension cracking of relict planes. Moreover, it is obviously impracticable to use monitoring of slopes, associated with sampling-testing (with wider dispersions) of the pre-failure (unanticipatable) and failed material. The observations of creeping slopes (via inclining and curving trees, a GOOD MINIMUM DENOMINATOR INDEX) result from many factors besides micro-loss of strength.

CONCLUSION. The research to establish time-affected risk of cumulative FS reduction needs idealized laboratory research. Civil Engineering cannot forego respecting in every point the obligation to forecasting to operational-life periods. The geochemistry of seeping waters is needed for research of Soil Science level, because it is such seepage that caused and causes weathering, and affects osmotic suctions.

2.2 *Overburden stress redistributions.*

Localized in situ stresses were postulated (de Mello, 1972) as likely to be distinct from those of routine overburden calculations and parametrizations in conventional sedimentary soil mechanics. All geo-engineering computations start from overburden and seepage stresses as the natural causative factors: thus, under an undisputed principle of homogeneous sedimentations (gradual increase of weight over areas several times the depth of interest) ASSUMING NO SHEAR STRESSES ON VERTICAL PLANES, the uniform vertical stress γz (or $\gamma z \cos \alpha$ in an infinite slope α) follows. However, even in sediments (and compacted fills) the principle of STRESS REDISTRIBUTIONS BY 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 more likely to affect in saprolites and weathered rock horizons, ipso facto DIFFERENTIALLY ATTACKED. The expected trend is for the chemically softened subvertical bands adjacent to hard corestones or clods (e.g. in widely-banded gneisses), to be DOUBLY WEAKENED, not only by the CHEMICAL ATTACK, but also by a RELEASE OF EFFECTIVE STRESS.

Thus, by the personal experiences with widely different scales of erraticities between specimen-sizes and job-dimensions of foundations, with resulting differences, prediction vs. 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.

Presumably for compressibility this would FAVOUR IMPROVED BEHAVIOR(cumulative), but for shear it would be UNFAVOURABLE, since the (critical) surface would follow weaker zones (kinematics permitting).

Any such candid hypothesis would entice curiosity and SYSTEMATIC FIELD TESTING TO PROVE/DISPROVE: normal and accepted destiny of any scientific hypothesis, free from preconceived notions. [N.B. Note that I forego ADDITIONAL CONSIDERATIONS ON RELICT DISCONTINUITIES, of great importance. Presumably the present best instrument for field determinations would be the Marchetti Dilatometer DMT, vertically for lateral stresses, and pushed horizontally in pits by reacting against the opposite face, with the spade horizontal checking vertical stresses.]

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 PROFILING PARAMETERS K'o AND OCR, derived from routine sedimentary geotechnique, and ALL THE MORE IMPORTANT IN RESIDUALS AND SAPROLITES where stress-strain-time trajectories are unknown and judged determinant? Set aside test erraticities, biases, heterogeneities. The oddity worsens because both parameters are RATIOS (increasing dispersion) TO AN OVERBURDEN EFFECTIVE STRESS TAKEN AS UNIFORM $\gamma' z$. By assuming uniform vertical stresses, the LOGICAL NATURAL HETEROGENEITIES are disregarded, producing greater spurious erraticities. How can a professional reach decisions in the face of such erraticities? If a harder clod, revealing higher lateral stress, were compared with a higher (realistic cf. nominal preconsolidation pressures σ'_p determined in oedometers) vertical stress absorbed by the more incompressible "partner", the K'o would naturally drop. And the higher σ'_p laboratory determined on harder nuclei would, ipso facto, compute lower OCRs if compared with the INEXORABLE HIGHER σ'_v PREVAILING IN THE HARDER NUCLEATION. [N.B. Forget the criticism against OCR PROFILING even in common strata, because for a constant over-consolidation Δp the ratio of $(\gamma' z + \Delta p) / \gamma' z$ is algebraically variable, and loses MEANING NEAR TOP, with OCR tending to infinity with $\gamma' z$ near zero].

In short, regarding testing for geotechnical parameters for honest valid engineering behavior assessment, the impressive collective experience from CONVENTIONAL SEDIMENTARY GEOTECHNIQUE MAY HAVE PROVED A HINDRANCE? Twenty-five years ago (de Mello, 1972) it was suggested that the faces of test pits should be statistically investigated by some quantitative index (e.g. pocket penetrometer) for orienting block sampling and laboratory testing. Presently add the suggestion of much statistical DMT vertical and horizontal testing: rapid and cheap. To any avail? Hopefully the institutionalized soil mechanics accepts the interest in real "dirty testing" in lieu of computational modelling on established hypotheses: out of curiosity, against fumbling in the dark.

3. RAINFALL, AND CONSEQUENTIAL EFFECTS.

Comprehensibly most sliding episodes are associated with heavy rainfall. Guidicini and Nieble (1984) published charts relating rains to localized landslides, and have been much more frequently misused, than respected on their inexorable proviso: HEAVY RAINS ARE THE NECESSARY, BUT NOT THE SUFFICIENT NOR SINGLY DETERMINING CONDITION (cf. pg. 11 of their text). The real problem of concept behind their pragmatic analyses is that they refer to storms covering areas of about 20x20km, and isohyetals from daily precipitation gages, thus invoking AVERAGED CONDITIONS, greatly so in area, and moderately so in time.

We must start with intensity precisions afforded by pluviographs, from which to compose the data considered relevant (for different mental modelling) and/or discard irrelevant components. Obviously from the two basic components (infiltration and runoff, forgetting for short storm durations the evapo-transpirations etc...), it is ONLY THE INFILTROGRAPHY COMPONENT that can affect subsoil destabilization (excluding surface erosion by high runoff velocities). Infiltration rates vary with time during a storm (cf. Blight, 1997) and constitute a PRIORITY COMPONENT to be discussed: computer simulations can be used to integrate effects.

Regarding area of influence represented by each pluviograph, the desired condition of extreme value statistics is surely met, because each record is for A POINT INSTALLATION (possibly applicable to the many spot areas of slides that appear as yellow-brown "freckles" in the green forest). It is of considerable side interest that in general successive rainfall episodes DO NOT TEND TO ACTIVATE THE

SAME SLIDES, nor the SURROUNDING NON-SLIDEN AREAS.

The emphasis on pluviographs is inexorable. Brand (1985) states tersely that "slope failures in Hong Kong are a function almost solely of short-term rainfall intensity, antecedent rainfall being of little account" , "thought to be... for understanding of landslide mechanisms, not just in Hong Kong but elsewhere", conclusion corroborated based on "sophisticated system of 42 automatic raingauges... every five minutes... rainfall distribution and intensity over the past few years, .. AVAILABLE EVIDENCE NOW INDICATES THAT ANTECEDENT RAINFALL IS NOT OF MAJOR SIGNIFICANCE" (cf. also Premchitt, 1991). Begging leave to disagree because of the difference between PLUVIOGRAPHY AND INFILTROGRAPHY, and some inevitable antecedent effects on suctions and infiltrography, there is agreement on the need for short term (24hs) graphisized monitoring.

Is it so overwhelming a task ? Maybe not.

It is my privilege to share the conclusions of an internal report, prepared for me and Clients in 1990, by the eminent hydrologist-statistician A. Santos Jr., using the data of 48 years (1937-85) of 3 pluviographs along the Cubatão slope: at the base (PH), at elev. 350m, and at the summit (Pedras Dam, approx. elev. 750m). He used the yearly 24-hr. maxima (independent) at each, analysing for 1, 2, 3, 4, 6, 9, 12, 18 and 24 hrs. Incidentally much more data, equally relevant could come from using all heavy independent events, not restricting to a single max-yearly one. The results were: (a) the equation, for all three elevations:

$$i = 7{,}319\ T^{0{,}1258}\ t^{-0{,}5187} \qquad (1)$$

where: i = intensity, mm/min
T = recurrence, yrs (cf. Fig. 2)
t = duration, mins

(b) the interesting result that the yearly maximum 24-hr. at the three stations (same storm meteorology) COMPLY WITH ESSENTIALLY THE SAME EQUATION.

Since one would suspect such a meteorological consistency it follows that other pluviographic data along the Serra should be analysed to confirm, or to establish the trend and range of divergence and dispersions.

The practical result will be two-fold: one, to accept minimizing the pluviographic stations within the analogous geographic-meteorological storm-area; the other, to minimize the number of conditions to be used for ARTIFICIAL INFILTROGRAPHY FIELD TESTS, as schematically suggested in Fig.3 .

Regarding short-duration rainfall intensities one point on which one lacks data, worldwide, is the spatial distribution of intensity-time WITHIN AREAS OF DIMENSIONS COMPATIBLE WITH THE TYPICAL AREAS OF SLIDES (and of surrounding influence). For water resource designs gauging stations have

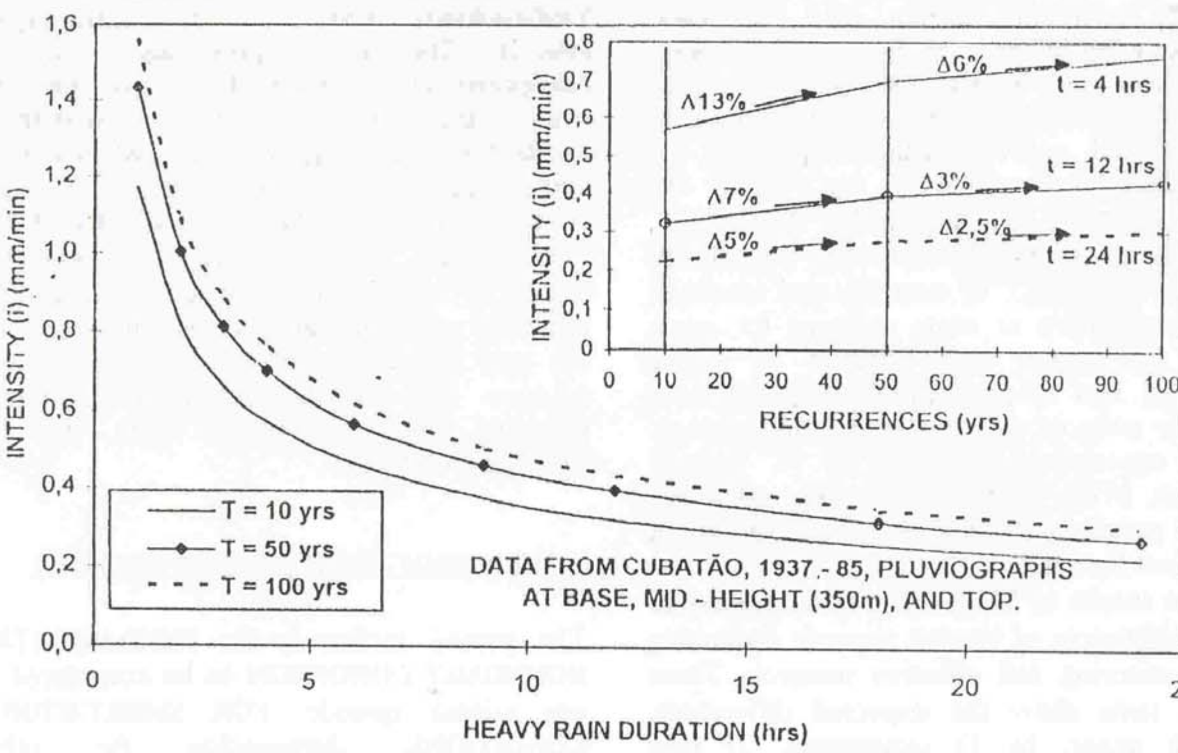


Figure 2 - Heavy rainfall intensities, extreme - values, short durations.

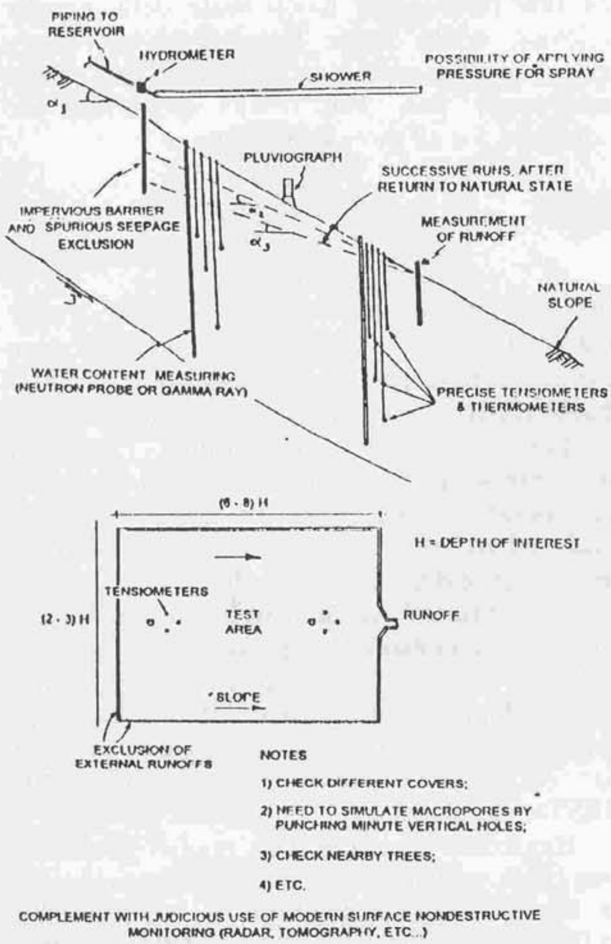


Figure 3 - Schematic indications for infiltrography tests with different pluviographies and surface covers.

naturally been quite distant apart, and each provides point information, but there is nothing intermediate, and we know that clouds pour at visibly different rates within dozens of meters. Why should so significant a problem as landsliding be restricted to PROFITING OF DATA DESIGNED FOR OTHER PURPOSES, deprived of aiming at monitoring specifically for itself? For logic of curiosity and coverage one should have at some stations, for some years (to collect some dozens of heavy episodes) sets of pluviographs installed in a reticular array of, say, 3 x 4 at 50m spacings. From expectations fostered by A. Santos' analyses, a few such stations well monitored should provide enough data to clear the doubt and move forward.

The results of Santos Jr.'s analyses lead to other inferences of interest towards optimising the monitoring and effective research. These heavy rains show the expected differences, though minor, by 1) recurrences, 2) time periods (cf. fig. 2); but, as stated, their daily maxima (and hourly components) are similar at the three points. To be checked, easily, against other analogous data. Does it not seem unreasonable, however? Perhaps not! If we reason that the gauge is an EXTREME VALUE CONDITION because of geometry (point) it seems acceptable that its rare-event data already embrace the maxima pertaining to time-and-location (geometry) distribution within the storms. The CONVENIENT CONCLUSION would therefrom ensue that for heavy rain intensity monitoring we DO NOT NEED TO SEEK MANY AND SPECIAL DISTANT POSITIONS OF DIFFICULT ACCESS. Such a hypothesis calls for prompt, interested and open, cross-examining via other analogous pluviographic data: in order to program most efficient monitoring of meteoro-hydrological CAUSATIVE INCIDENT REALITIES to be assigned probabilistically to small area heavy downpours, for infiltrography testing.

Also one sees the maximum data for different recurrences (e.g. 1 : 10, 1 : 50, and 1 : 100 years) changing very little. Again, does this seem unreasonable? Again, apparently not; by the same extreme-value distributions already dominantly PROVIDED BY THE POINT AND INSTANT MONITORING.

In short, I submit that this important field OF FAST-GROWING MENACE has been handled as the UNINVITED GUEST within civil engineering, with regard to the obvious CAUSATIVE FACTOR (to be followed by the TRIGGERING FACTOR of infiltrography effects). The above proposals must be challenged and elaborated for well-planned work: the FUNDAMENTAL PRINCIPLE REMAINS. Technologies advance when started with understanding of the governing DIFFERENTIALS AND DIFFERENTIAL EQUATIONS, and then INTEGRATED AT WILL in critical combinations, using the powerful mathematical and computer tools of the past 30 years. There is no prospect of advance by roughly examining complex, scattered, lumped-parameter data, in the REVERSE ORDER.

4. INFILTROGRAPHY FIELD TESTING.

The ground surface is the FUNDAMENTAL BOUNDARY CONDITION to be considered in any rainfall episode: FOR SHORT-STORM CONDITIONS, disregarding the other components of the water balance, the excess of rain intensity over infiltration intensity establishes a thin boundary layer of downslope runoff flow of piezometric head essentially zero irrespective of the excess rain intensity and resulting depth of flow. It is in the transient period until rainfall intensity exceeds infiltration that the upper soil horizon is being significantly altered by what has been termed the advance of the "wetting front" (Lumb, 1962).

In a "homogeneous" pervious soil surface-horizon the first excessively simplified "pseudo-saturation satiated" Darcy flow formulation (1962) was later (Lumb, 1975) modestly revised to include varying degrees of saturation, as per an equation quoted by Brand (1985, Gen.Rept.) as reasonably applicable to Hong Kong conditions. Fig.4 shows the nominal results of both formulations, representing a typical evolution in a "school of concerted technological effort" (as was, for instance, over roughly 20 years, the research on residual φ' influencing slope destabilizations in London fissured clays). It is clear, however, that the interferences of SUCTION and of NON-HOMOGENEITIES ("pipes", cleft-water pressures, permeability gradients, air bubble compressions, etc...) await being incorporated.

It seems appropriate to separate the two steps, a) infiltrography establishing UPPER BOUNDARY CONDITIONS FOR THE FLOWNET, b) subsequent elaborations of flownets and variations thereof. They interact continually and varying, but it is expedient, as one reassuringly finds also in Blight (1997). He gives data on the significant changes (intuitively obvious) on infiltrographies for sloped vs. flat surfaces (his Fig. 9) and on suction profiles (his Fig. 32) for different covers: he also cautions against the standard double-ring infiltrometer test (relatively small dimensions, boundary effects). On the one hand it is important to document with field test data, of relatively high benefit/cost prospects. On the other hand one is reminded that the possibility of acting on the surface condition is an obvious gate to modern shallow non-destructive investigation, and subsequent engineering design-construction measures (cf. some data in Blight, 1997).

An infiltrography field test plot can have an area of about (2-3) H in width and (6-8) H downslope length, for representing comparative conditions of a destabilizable horizon thickness of H m. With synthetic rains (pluviated somewhat beyond the monitored area) the facility can be used for sequential investigation of numbers of added conditions (e.g. vertical"pipes" from rotted rootholes or termites, cracks with cleftwater pressure, etc..) in a test sequences. Let us emphasize advantages of CHECKING TRUE CHANGE OF CONDITION EFFECTS.

Pending refutal by more learned study and/or fide digna field test data, I repeat the emphatic need to analyse destabilizations by ΔFS analyses from a presumed initial FS_1 to an inflicted FS_2 (de Mello, 1972): and continue(repeatedly mentioned) to question the

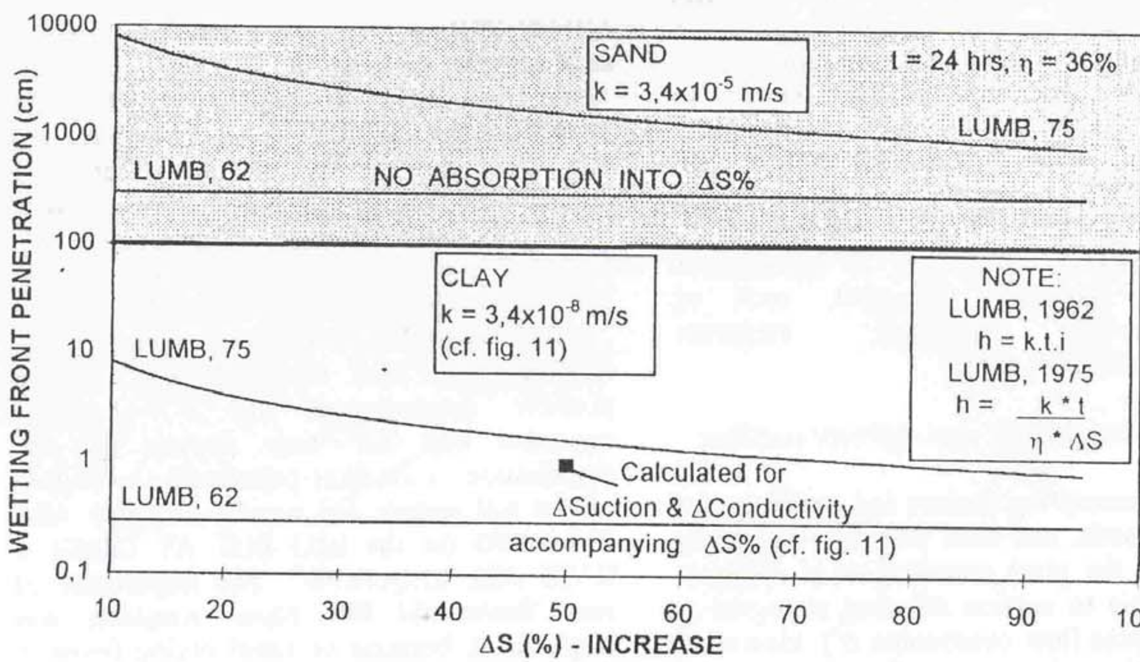


Figure 4 - Advance of wetting front, Lumb (1962) (1975).

(presumed) priority importance given to matric suctions (starting, it seems, from Morgenstern and Matos, 1975). This inference comes from lay observation that very many drizzle infiltrations over several days occur per year, without any failure, and occur also as antecedent "wetting" (to which suction elimination is intuitively felt to be very sensitive) before the TRIGGERING BY THE HIGH INTENSITY RAIN (external generator of internal effective factor).

Fig.3 submits to project elaboration a preliminary sketch of a possible sample infiltrography test. For many pertinent points, though as most common, applied to horizontal surfaces, sample reference is made to Trautwein & Boutwell 1994, and the "Sprinkler method" summarized by Stephens 1994.

5. SOIL SUCTIONS, RAPID CHANGES THEREOF, AND EFFECTS ON FLOWS AND SOIL EFFECTIVE STRESSES.

The present-day basic reference on suctions and flow in unsaturated soils is the book by Fredlund and Rahardjo (1993). Far from my scope to delve into so specialized and broad a topic except in order to pose some candid intuitions and questions. Without seeming to be too academic, I recall that in this reference (and most) the "Flows due to chemical, thermal and electrical gradients are not discussed" (loc.cit.p.107) and the first two may have more importance than imagined, for osmotic phenomena and our problem. Fig. 5 shows from their data some comparative ranges of osmotic, matric, "total" and thermal suctions.

It seems that the principal points to emphasize are the starting need to require 1) PRECISION AND RAPID RESPONSE OF MEASUREMENTS because of the probable influence of SMALL EFFECTS, CUMULATIVE, and the RAPID TRANSIENTS, 2) due account of effects generally discarded, such as chemical/thermal gradients, PROVEN DISCARDABLE OR NOT.

5.1 *Measuring devices, and different suctions.*

Regarding measuring devices and methods, for matric, osmotic, and total suctions, one firstly emphasizes the great contribution of apparent cohesion due to suction affecting strengths at shallow depths (low overburden σ'). However, professional interest focuses on the

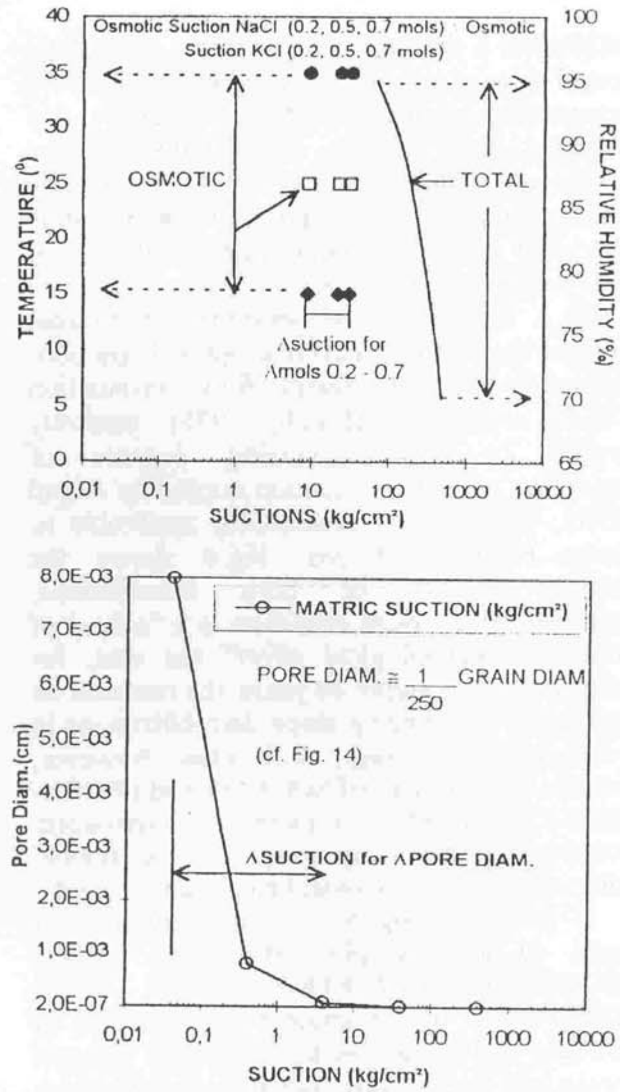


Figure 5 - Some comparative ranges of different suctions quoted.

ENLIGHTENING PRACTICE OF TECHNICAL DISCUSSION between eminent sectors of the same Department (at Imperial College). In 1986 Chandler and Gutierrez demonstrated the convenient applicability of the "filter-paper method" for matric suction (capillary pressure, height of water column) warning that "the degree of contact between the filter-paper and the soil sample is important. The present ...WITH DIRECT CONTACT ... care that the filter-paper IS NOT COMPRESSED" (giving possible overestimates of 2-3 kg/cm^2); conceded that the "time periods for the equalization of the filter-paper with the suction of the soil sample (is) usually 7 DAYS ARE ALLOWED (in the lab.) BUT AT LEAST 5 DAYS ARE REQUIRED". The importance of very fast/careful filter-paper weighing was emphasized, because of rapid drying (even in the lab.): DRYING SHOULD ALWAYS GIVE

OVERESTIMATED SUCTIONS, e.g. from 1→ 10 kg/cm² in London clay from 47→ 30%w.

Needing suctions measured beyond 1 bar, and RAPIDLY, Ridley and Burland (1993) developed a transducer device of minimized water volume, achieving response time below 4 mins, with excellent precisions in the range of 1 to 15 kg/cm², and respect of the required (cf. Stannard, 1992) "continuous hydraulic connection between the porous material and the soil" (no effect of contact compression), "and minimal disturbance of the natural infiltration pattern...". The ensuing discussion (Marinho and Chandler, 1994) apparently did not notice the emphasis on SPEED AND PRECISION, and, claiming that "when the filter-paper (or other) is not in contact with the soil water (i.e.soil surface ≡ soil water surface?) it measures the total suction, the air gap acting as a semi-permeable membrane", postulated that the new instrument would be measuring effects GROSSLY INFLUENCED BY OSMOTIC SUCTIONS".One result inferred from this fertile discussion is in Fig.6. Interestingly the single result on suctions measured, as influenced by 0.1 molar NaCl pore fluid seems to show the osmotic as NOT THE SMALLER COMPONENT vs. matric, while (obviously) electrolyte concentrations have definite influence.

(1) The question of RESPONSE SPEED is irrefutable (N.B. important for short intense rains). Responses of the instrument are needed very much faster than those of the: a) changes within the EFFECTIVE PORES AND GRAINS OF THE SOIL, b) responses of transfers from soil clods to the measuring surface .

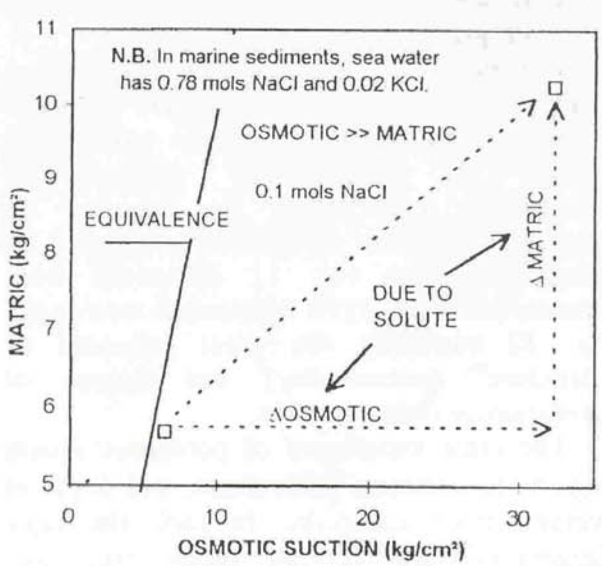


Figure 6 - One inference from the Ridley - Marinho discussion.

(2) Two other points invite more impartial research and DENSE WORKSHOPS. (A) First the disparaging data erraticities, whether or not by imprecisions: however low the φ' of the soil, an error/change of suction of 5 kg/cm² leads to strength variabilities in a range of 1 - 5 kg/cm² (!!). (B) Secondly a need to understand better the separate and/or interactive roles of matric and osmotic suctions, as pertaining to clods or grains, and macro-vs.-micropores, and regarding flows and effective stresses. Facing the general quote "the matric suction is the largest component of soil suction in partially saturated soils" (with distilled water), the data do not seem confirmatory.

Scrutiny of the data from the cited book indicates points to be cleared. Most sensors report times of equalization of 500 to 2000 hrs (Figs. 4.75 - 4.78, and 4.80, 4.81). Seasonal variations in Regina clay, Saskatchewan, at depths of 1 - 4.4m, show matric suctions varying from 2 to 6.5 kg/cm², with equalizations in 2000 hrs., attenuating peaks (cf. Fig. 4.83): thus, in sharp peaks of intense infiltrations after very hot, dry spells, the response speed is a sine qua non requirement. Fig. 4.74 shows a temperature effect about 2 kg/cm² differential for temperatures between 21° and 32° C (quite likely to occur). Figs.7 and 8 herein exemplify the dispersions in profiles of matric suction, surely DISPARAGING TO A DESTABILIZATION PROBLEM faced. Fig.9 reproduced shows TOTAL SUCTIONS in the range of 50 - 100

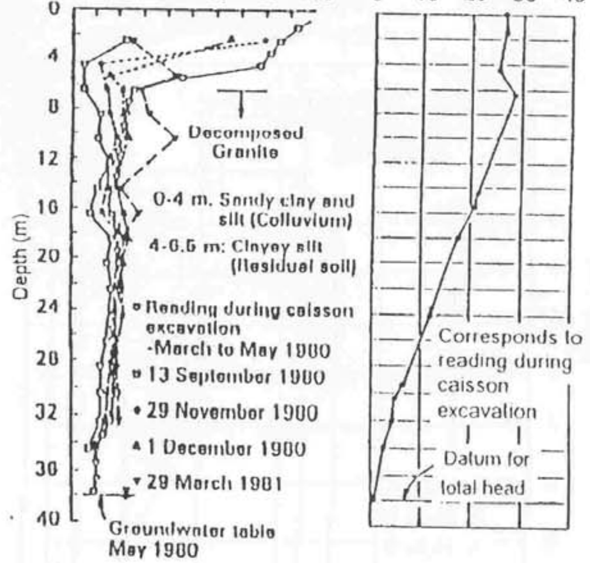


Figure 7 - Matric suction profile along shaft a (from Sweeney, 1982). Taken from fig. 4.48 of Fredlund and Rahardjo.

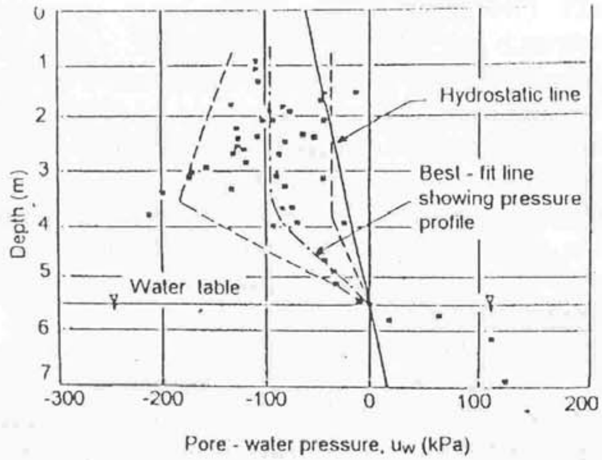


Figure 8 - Negative pore - water pressures measuring using the Agwa-II thermal conductivity sensors on undisturbed samples.

kg/cm^2 in the top 5 m of the profile, and erraticities of about 10 kg/cm^2 . Further, Fig.10, pertaining to two recognized methods of very slow responses (psychometer and filter-paper) exposed presenting a dispersion from 4 to 11.9 kg/cm^2 at the average 1 : 1 equivalence on 10 kg/cm^2 .

Need one repeat emphatic calls for prepared Soil Science Workshops on the topic?

5.2 *Infiltration flow into unsaturated horizon.*

Firstly since the upper horizon soil experienced countless infiltration-evaporation cycles, hysteretic effects are acceptably negligible (de Mello, 1972).

Water permeability is simply taken as

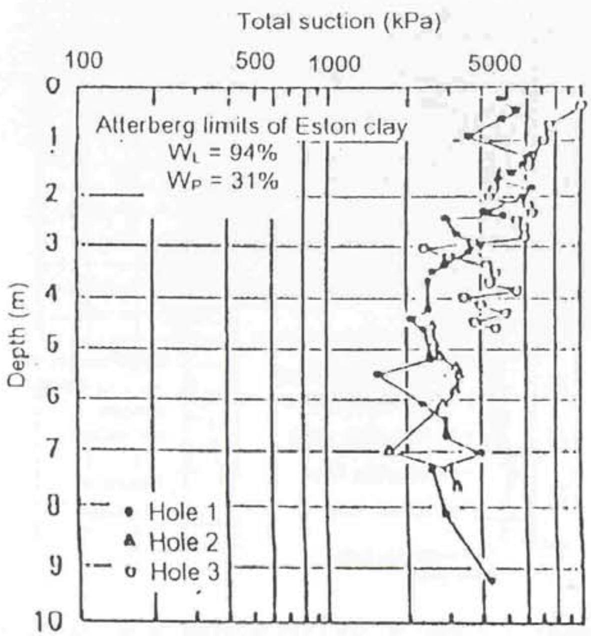


Figure 9 - Total suction, Eston clay, by filter-paper. (Loc. cit. fig. 4.30).

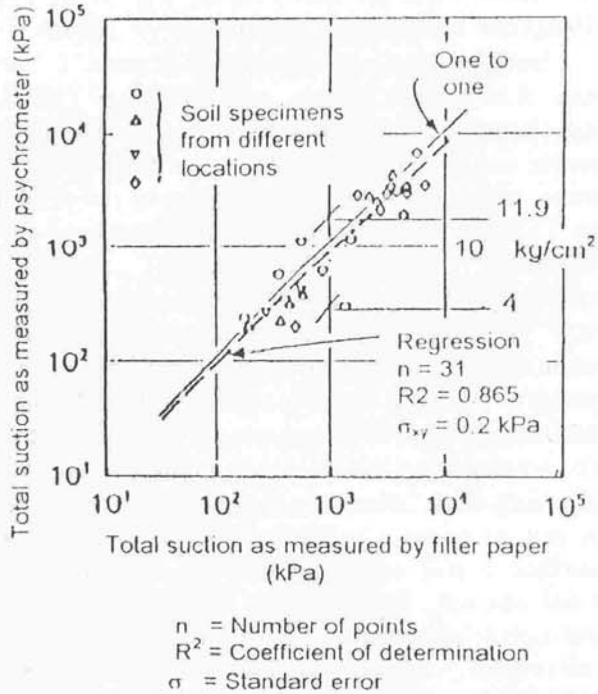


Figure 10 - Dispersions compared in consecrated slow methods.(Loc. cit. fig. 4.28).

flowing only through continuous water pores. The coefficient of (water) permeability is affected by combined changes in void ratio (disregardable), degree of saturation and Δsuctions changing the air pore sizes. Changes in air solubility by suction and temperature changes have not (to my knowledge) been included, though possibly involved in raindrops. The principal factor, besides Δpermeability, is the major change in flow gradients: compared with gravitational gradients (limited to 1) the suctions, and Δsuctions along the profile, are dominant gradients.

The net result depends on comparative changes of permeability vs. suction gradient. Depending on behaviors of the water-pores (macro vs. micropores?) decrease of permeability may offset the increase of gradient. A case is exemplified on Fig. 4 by using data from Fig. 11 composed from Thomas and Rees, 1990. Meanwhile data as per Fig. 12 exemplify the great influence of "structure" (porosimetry) and degree of saturation on matric suction.

The great importance of porosimetry with regard to suctions, infiltrations, and depth of wetting front transpires. In fact, the rapid descent of water through "pipes" may cause bypassing and occluding many air pores excluded from the flow.

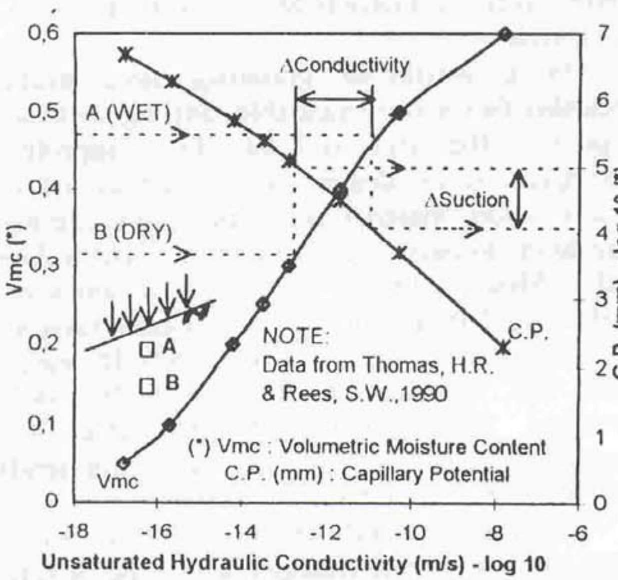


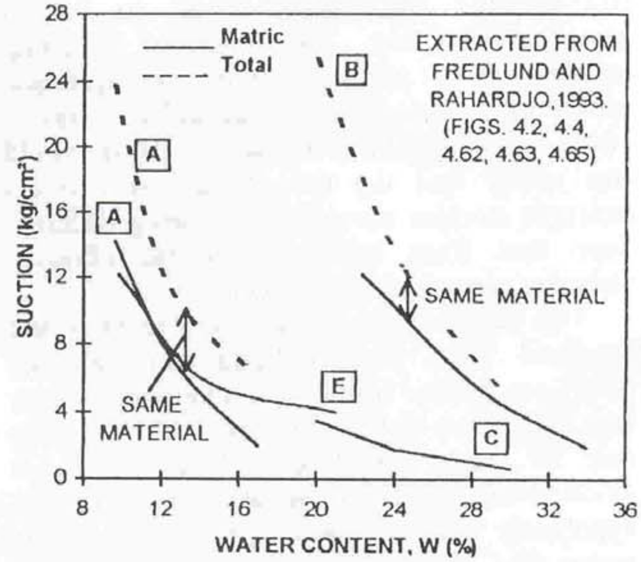
Figure 11 - Example of Δsuction + Δpermeability effect on flow, and wetting front.

5.3 *Non-singular flownet formulations.*

Many are the natural hypotheses on special flownet conditions, such as by raising the flownet elevation, and/or having it redistribute flowlines and exaggerated seepage stresses at strictions etc. Cleft-water conditions both in series of "pipes" and in cracks near surface, are mentioned. In weathered rock horizons, under rapid transients, one is reminded that a principal pervious stratum is the underlying cracked horizon, and such cracks are not continuous; therefore cleft-water transient conditions may be set-up with modest inflow volumes, raising pressures considerably. Frequent mention is made of a need of an impervious base for the flownet to rise: this must be reconsidered since for an "infiltration mound transient" an underlying phreatic is, in principle, an impervious membrane.

Vaughan's (1985, 1989) hypothesis and formulation of the importance of non-linearity in seepage problems merits attention. However in many cases some further adjustments seem required: (a) partly as per multiply repeated near-failing episodes, suggesting no hysteresis, and stable pores, (b) partly because of the time for achieving the changes of void ratios under effective stress changes, and the very rapid types of slides experienced.

To my questioning a point to be equated is the participation of osmotic and thermal gradients in the unsaturated horizon flow patterns. What equilibria change, adjusting to changing osmotic suctions of nearby trees?



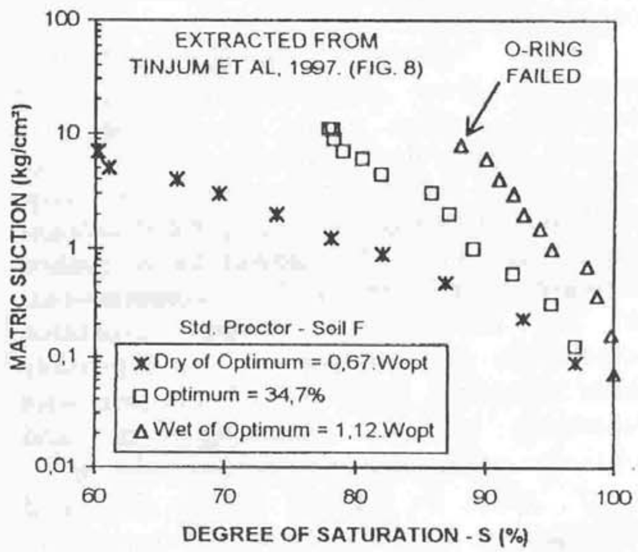


Figure 12 - Sample published evidence of significant Δsuction for ΔW% and ΔS%.

A daring quest follows on conditioning effective stresses due to suctions and seepage.

6. DIFFERENTIALLY WEATHERED GRANITO-GNEISS: FIVE-PHASE SOILS ?

Fredlund and Rahardjo (1993) propose the shear strength equation for unsaturated materials, for convenience, as:

$$\tau_{ff} \approx c' + (\sigma_n - u_a)\tan\varphi' + (u_a - u_w)\tan\varphi^b \quad (2)$$

where: c' = effective cohesion
$\sigma_n - u_a$ = effective normal stress
$u_a - u_w$ = matric suction

This simplified theory in essence drew from trigonometric adjustments to fit to (scant) experimental data, to facilitate reverting unsaturated to saturated soils. The question arose: under what model of particulate behavior (with lyospheres in colloidal particles) would one justify that the ds/dσ' due to external isotropic stresses should reason being different from that from internal spherical effective stressing caused by matric suction?

The scant data readily available (Ho and Fredlund 1982, and Fredlund and Rahardjo 1993) are plotted in Fig. 13. The wide scatter may hint at test dispersions: but could it also be due to other interferences? Under what intuition or reasoning would one accept a hypothesis that φ^b should be systematically lower than φ'; and, also, should give such queer pseudo-correlations, or lack of correlations?

Since "nominally homogeneous" soil specimens were used, the erraticities of weathered profiles cannot be invoked. One thought regards the RESPONSE TIMES: a) of recording vs. transmitting suctions effectively; b) of suction changes to consequent σ'changes (with micro-ΔVs), b1) slower under suction decrease, σ' increase, involving compressions, or b2) comparatively smaller expansion adjustments under decreasing σ' (possibly faster responses). Could it be that in tests with decreasing suctions, decreasing σ', and relatively quick test observations, the φ^b, ds/dσ', might be higher? During the rapid infiltrography and decreasing suctions in the unsaturated horizon, and increasing underlying pore pressures, could it be that the σ' decreases above and below may not be

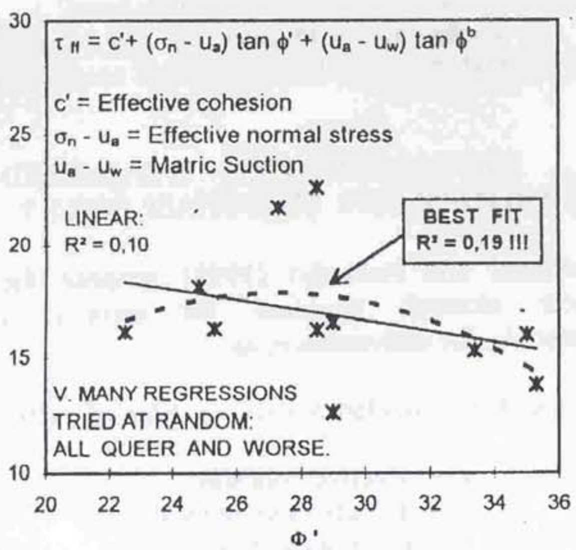


Figure 13 - Scant data of comparative values of ds/ds' (from Fredlund and Rahardjo).

effectively transmitted? Moot question, open to research.

In a weathered granito-gneissic profile another factor is postulatable. Setting aside soil "pipes", the fact is that these saprolites comprise some denser clods, surrounded by more porous material: the former are confirmed by high nominal preconsolidation σ_p' values (de Mello, 1972). Fig. 12 summarizes established trends of much increased suctions either based on w% decreases or S% decreases. The fact, therefore, is that the clods should possess much higher suctions than the surrounding porous material. The latter tends to the condition of mature residuals.

What if each clod were conceived as a big-size porous particle (imagine a volume of tuff, but non-cementitious) held together by the high suctions (matric? or total?) such as are correlated (in each material) with its physical indices? Within these clods we cannot escape having higher suctions than in the nearby porous material (possibly smaller difference than as interpreted from Fig. 12, because a part of the grain-to-grain adherence is by remnant cementing of the parent rock). In like manner there are suctions in the porous volumes, which do not occur in the "pipes". The differentiated stress-strain-time and suction behaviors of the contiguous volumes are in continual interactions. In such interactions all gradients (gravity, thermal, osmotic, and, in cases, electrical) must take part, and there are time-lags of rheology thwarting the idealization of instantaneous cause-effects. Is it clear why only matric (capillary) suctions should be of interest? Could it be that the others are "spherical, pluridirectional" (as per to the "neutral" effect of pore-pressures on grains, in the Terzaghi effective stress equation) while the capillary apply a net vertical stress?

In gist, it seems that for a generalized theoretical cause-effect model for rheologies (under all causes, not merely stress) we should, in these saprolites, include more than the three conventional solids-fluid-air phases. The clods or micro-clusters should be a fourth phase. And because of electrolyte effects the lyospheric solute should be a fifth phase besides the "free solute".

7. POROSIMETRY AND EFFECTIVE GRAINSIZE DISTRIBUTIONS. DISCARDING INDEX TESTING FROM CONVENTIONAL SOIL MECHANICS.

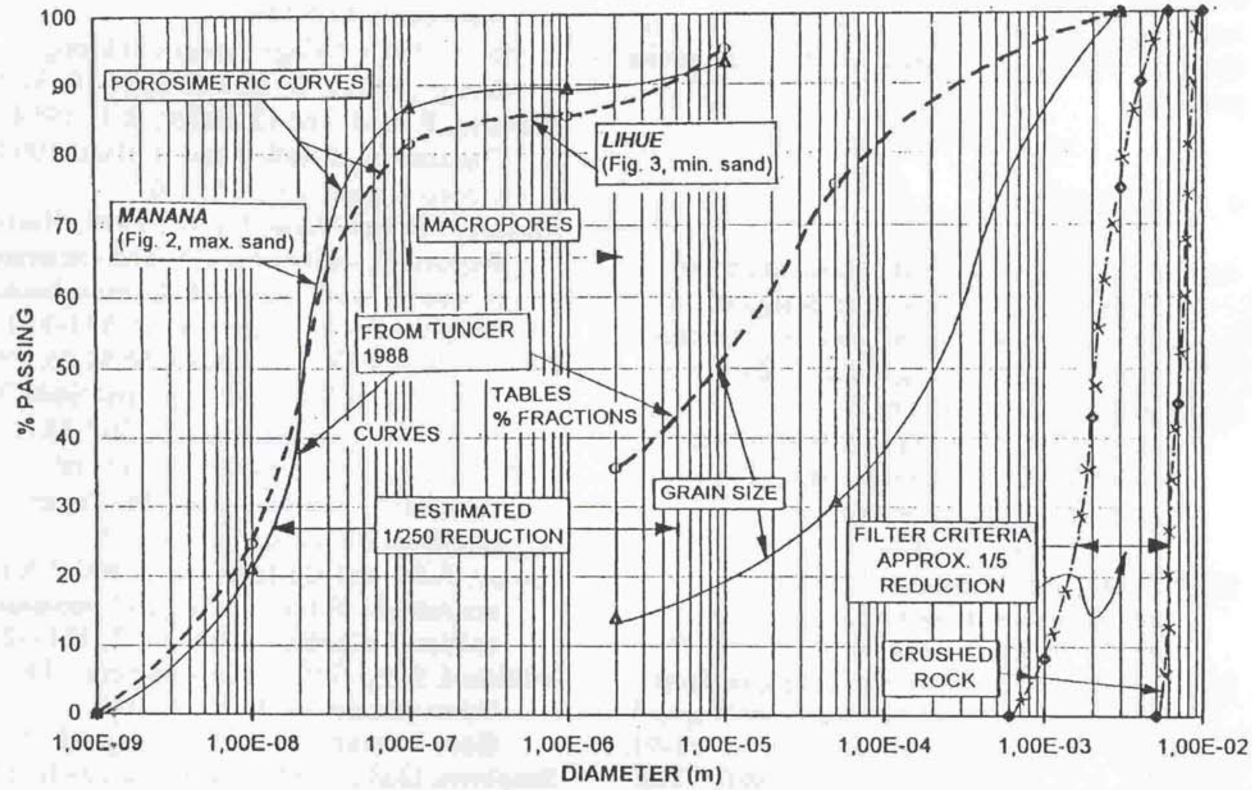


Fig. 14 - Almost inexistent data tying grainsize vs. porosimetric curves.

One foregoes repeating the fact emphasized along four decades, that the traditional grainsize and Atterberg Limit index tests are "destructive", herein absolutely meaningless. However, sizes of nuclei plus grains may be of interest, if techniques are developed with non-wetting fluids for partial disintegrations/ sedimentations: why should water, "pure water" be in a singular position? Inertia?

Similar, though smaller, criticism may be levelled on routine testing by which laboratory research would implement professional data from field testing. For instance, no sampling/testing developments are known, for the micro-deformation range as necessary. Quite a vast subject for pertinent discussion, elsewhere. Herein I limit myself to deploring that although suctions are so closely related to porosimetry, one hardly finds publications ostensibly supplying the porosimetries as associated with the grainsizes.

From the many studies performed on sands and gravels because of filter criteria, we are accustomed to expect poresizes of the order of 1/5 of the corresponding grainsizes. Fig. 14 reproduces porosimetry data from Tuncer, 1988, associated with nominal grainsizes plotted from the tabled percents sand, silt, and clay fractions. The presumed proportion surprisingly drops to 1/250 on average. Both for macro-pores establishing flow "pipes" and for micropores controlling suctions (inversely proportional to the square of the radii) the porosimetries of residual soils should be very much more important than grainsizes. If truly significant grainsize testing becomes more difficult and expensive, possibly that test (adjusted) may be dropped, in lieu of pursuing the cross-correlating from "solids" to pores.

8. CONCLUSIONS.

At the Toronto Landslide Conference, 1984, a Director of the World Bank discussing natural disasters across the world (for the International Disaster Prevention Decade) showed that the global damage potential due to heavy rainfall landslides is much higher than that of earthquakes; this derives from the numbers of episodes, the areas affected by the causative factors, the areas of effects of consequence, the risks, and the cost, both of attempts at preventive reinforcements, and of damages. The menace, risks and costs have increased progressively, and will continue increasing. It behoves us to convene a Workshop (such as the Shear Research Conference at Boulder, Colorado, 1960) to organize a "COMPLETE MENTAL TREE" so that ulterior research efforts be specifically sought, each endeavour and result to supply the knowledge into specific recognized gaps, with sequential priorities.

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