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THOUGHTS ON SOIL ENGINEERING APPLICABLE TO RESIDUAL SOILS

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The subject chosen for this presentation concerns an area of soil engineering that has been receiving increasing attention over the past decade, as ever-belated civil engineering efforts are concentrated towards the immense task of construction that is required in the so-called 'developing countries'. Indeed, residual soils of some type or other, and developing groups or subgroups of some type or other, may be associated with any and every area of the world, but it seems appropriate to restrict consideration to but some principal residual soils common in humid temperate and tropical climates: within this context lies much of the personal experience of the writer and of his colleagues in Brazil, and it is presumed that within the same restrictive but recognizably very vast subject may lie the interests of many of the participants in this Third Southeast Asian Conference on Soil Engineering.

Recognition of the importance of the subject in current civil engineering thinking prompted the inclusion of the topic 'Slope Stability in residual soils' as one of the State-of-the-Art papers of the Fourth Panamerican Conference in Puerto Rico, 1971, and the writer is most fortunate in that the presentation can profit from that excellent report as a jump-off platform, and that the authors, Deere and Patton (6)* also focussed their attention on tropical climate residual soils based on chemical weathering. Needless to say, the authors achieved a very significant contribution which brought together the rather voluminous production from hitherto quite distinct professional areas and consequent conceptual approaches. But it may have struck some of us who are called to take decisions of responsibility on projects involving residual soils, that the trends of thinking have grown so steadily and quietly that on the one hand the effective growth becomes imperceptible, and, on the other, ipso facto, every iota of a step forward is too heavily conditioned by earlier pronouncements, which really do not tend to the Civil Engineer's real needs.

The time is fully ripe for an independently oriented look at the problem.

The writer proposes no certainties and no proven solutions, but merely wishes to offer some thoughts to catalyze thinking, action and new solutions.

* ( ) references as numbered at the end of the text.

To this purpose but two residual profiles will be considered, and within them merely the cases of two types of engineering problems faced: the weathering profile of the *granito-gneiss* complex encountered around Sao Paulo, Rio de Janeiro, etc, and that of the immense area of *basalt* flows of the Brazilian hinterland; and the problems of *cut slope stability* and of *foundations*. The weathered basalt profile is appropriately included as a complement to Deere and Patton's state-of-the-art paper, not merely because 'basalts weather much as do intrusive igneous rocks' (loc. cit.) but because the authors avoided 'attempting to describe the nature of the weathering profile in basalt on the basis of incomplete data'.

1. *First and foremost a question of terminology*

The writer strongly favours the pragmatic point of view of forthcoming usefulness instead of that pertaining to historical tradition, frequently fraught with presently recognized misconceptions. It is useful to communicate that a given subsoil profile is a *weathering profile* (as distinct from a sedimentary one, for instance). The writer fails to see why, however, one should thereupon embark on the practice of describing what such a profile should be like, as though to suggest that one may dispense with subsoil investigations, borings, etc. Any foundation engineer will strongly recommend guarding against the thought. Such thinking derives from geologists and road engineers concerned only with fill and cut slopes: incidentally, many a railway or highway slope in sedimentary soils is similarly cut without any investigation and without the presumption of anticipatingly describing what the profile is 'typically like'—the practice is merely associated with a question of risk on low-cost engineering.

1.1 *Concept implicit in the term 'weathering profile'*

What does one wish to convey by the term 'weathering profile'? Obviously, the basic implication is that an *absolute continuum exists, of gradual transitions of average geotechnical parameters with depth.* The would-be distinct horizons really merge into one another, and their depths and thicknesses vary erratically in the order of meters vertically within horizontal distances of the same order, so that for many foundation engineering purposes *the real inexistence of horizons is the truly disconcerting* *fact*. 'Natura non facit saltus', especially in slow geologic-duration infinitesimal-increment cumulative processes of weathering. The horizons are apparent, indispensable: but the degrees of precision with which they are establishable, or indicate distinguishable soil behaviour, are too small for usefulness in final design or construction decisions. It is fundamental and indispensable that one recognize this reality quite honestly, lest one slip into contradictions. For instance, Deere and Patton state 'Descriptions of each zone provide the only reliable means of distinguishing one zone from another' and 'One of the more difficult problems in classifying residual soils is to determine into which division one should place the highly weathered saprolitic material', and yet 'There can be no question that each zone in the weathering profile has significantly different engineering properties'. The writer postulates that the separations within the continuum are arbitrary and vague, and the average geotechnical parameters are really not, and cannot be, significantly different; the problem seems to lie instead, in the need to consider the dominant engineering behaviour of the major horizons under conceptually different approaches. (Cf. Fig. 1).

1.2 *Pragmatic concept implicit in the term 'residual soil'*

What does one wish to convey pragmatically by the term *residual soil* (to be appended as a complement to an appropriate soil classification of the sample)? Two basic thoughts are imperative: on the one hand, the thought of *extreme heterogeneities* within the soil mass (witness the corestones surrounded by soft silty clay); on the other, the thought that the *relic structure discontinuities* of the parent rock are likely to condition and dominate the behaviour associated as a first-degree approximation with the geotechnical parameters of the soil continuum.

Obviously, however, since both the heterogeneities and the relic structure pertain to a given parent rock (type and structure) it need hardly be re-emphasized (16) that one should abolish the meaningless practice of discussing residual soils (or saprolites, if you wish) dissociated from the respective parent rock. For simplicity one may state, for instance: archaean granito-gneiss residual soil, or granitic colluvium saprolite, or amygdaloidal basalt saprolite, and so on; but always solely as a descriptive complement to the classification as soil. Thus one really summarizes along the boring profile, for instance: coarse to fine angular friable sands, slightly clayey, with micaceous seams, grey-white and yellow mottled and banded colour (gneiss saprolite, jointing predominantly N 45 E dipping 60 S).

1.3 *Horizons of engineering significance*

One is loth to advance yet another suggestion on subdivisions of the weathering profile, but it has seemed to the writer that existing suggestions have depended too much on appearance, visual impressions of geotechnical parameters, and intuitions from broad-term geologic descriptions, as well as the academic concept of respect for 'geologic truthfulness' rather than the approach of usefulness, so that in the face of a particular engineering problem little meaningful orientation is provided. Why must a subdivision into horizons be aimed at advancing generalized impressions, fatally as often false as not, with regard to the principal behaviour parameters (strength, compressibility, permeability), if such behaviour must needs will be indicated by appropriate index or special tests? In a sedimentary subsoil profile does one limit oneself to estimating the 'relative strengths' or 'relative permeabilities' of successive sandy or clayey strata merely upon such description, or does one rather base, upon such initial indications and the visualization of the problem faced, the planning of the testing, interpretation, and analytical approach to solving the specific problem? What is the practical use of a subdivision into a horizon on which, for instance, a defining characteristic is the percent corestones taken practically as extending over so wide a range as between 10 and 90%? (6). Indeed the overall concept of horizons for indicating probable general trends may be quite useful if the sequel and corollary is not that of stifling determination of engineering parameters because of assuming them qualitatively.

The writer recommends the concept of basically restricting the weathering profile to *three major horizons* as follows (Fig. 1): the upper *mature soil*

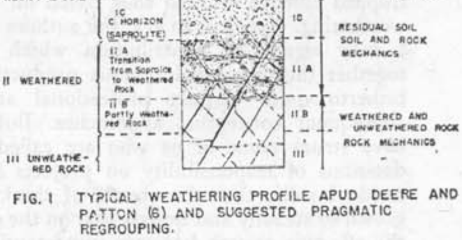


FIG. 1 TYPICAL WEATHERING PROFILE APUD DEERE AND PATTON (6) AND SUGGESTED PRAGMATIC REGROUPING.

horizon to be treated exclusively as *soil*, within conventional Soil Mechanics methods; the intermediate *residual soil* (saprolite) horizon in which predominant behaviour is still as soil but consideration must be given to extreme heterogeneities and relic structure discontinuities; finally, the lower *weathered rock* horizon (and its gradations) where behaviour is predominantly of a weak rock with weaker discontinuities, as treated routinely by Rock Mechanics.

Within such a pragmatic proposal the term residual soil will not be used for the upper maturely weathered horizons in which the participating solid elements have been unequivocally reduced to soil particle sizes and behaviour. What advantage is there in classifying the widespread 'porous red silty clay' as residual or not, if such clays have been repeatedly mistaken for residual when they were transported (4), and when their engineering behaviour can be perfectly well established by routine soil mechanics, permits very satisfactory predictions therefrom, and leaves the geologic origin as quite irrelevant? If one advances into considering residuals of sediments, what does one gain, and how does one avoid calling all soils residual of something but shortly past?

If we accept the concept of significant heterogeneity and relic structure-discontinuity conditioning as defining the 'residual (saprolite) horizon', obviously we relinquish the temporary comfort of such traditional hypotheses as the linearization of stratum thicknesses or density horizons in between borings, as well as of such arbitrarily oversimplified stilts as the suggestion of upper and lower limits of 10 and 90% corestone. It may indeed be that such limits may quite frequently prove satisfactory, but since they do not embody a concept, the writer would not favour adopting them as such. Not only is there a full range of variation of corestone in percent depending on how detected and how expressed (the percent of length of core in a boring should be of the order of the cube root of the percent volume of corestone excavated in an exploratory pit, and the percent weight is rather more difficult to evaluate without density determinations), but also there is a full range of variation of induration reminiscent of corestone that remains in the residual soil, as nuclei of much higher density and Standard Penetration Test SPT values, without any percent corestone recovery at all.

Deciding when a heterogeneity and/or discontinuity becomes significant or not requires experience, which may appear abhorrent to the yet inexperienced: but the only way of acquiring experience is by exercising at it and facing both the errors and the successes against a basic reasoning that thereby suffers iterative revision, and not by dodging the need to decide on the real problem.

Obviously the scale of the heterogeneities and discontinuities is most important in determining their significance: and there are always two scales of significance, one the relatively small scale put to evidence in the standard field and laboratory tests of the investigation stage, and the other the usually much larger scale which will be at play in any of the typical soil engineering problems (one to a few meters in the case of foundations, and tens to hundreds of meters in the case of earth works).

It seems appropriate that a decision on the upper boundary between a mature soil and the residual soil be established on the basis of a wider than usual dispersion of results in the routine small-scale investigation tests used currently in the upper soil horizon: the writer is not yet prepared to suggest numerical bounds for such a tentative procedure, but feels that upon applying more and more statistical interpretation of test data, it will be possible and practical to establish suggested limits, within each parent rock saprolite, for distinguishing between conventional soil mechanics computational treatments applicable to 'mature soils', and those called-for in 'residual soils' as pragmatically defined by the writer. It seems conceptually inevitable that the distinction be first established on the basis of the so-called *routine investigation tests* (current in mature soils) since a systematic classification must always start and proceed from a single root thought: moreover it is a useful principle that *dispersions be investigated on the smallest scale practicable* (test scale involving specimen-dimensions, few to tens of centimeters) and that the corresponding dispersions in medium or large scales involved in engineering problems be thereupon established by appropriate integrations.

As regards the lower boundary for the pragmatic saprolite horizon, it appears appropriate again that one establish it at the level where routine Standard Penetration Test borings stop in 'impenetrable material'. Such obstructions of more than a few inches in diameter really establish a convenient practical limitation, because thereupon or thereafter special techniques and dimensions of sampling and testing become necessary, more akin to field testing routinely pertinent to Rock Mechanics in Civil Engineering.

2. *Secondly a question of aim: prediction*

It has been very ably and forcefully pointed out by T. W. Lambe (9) that the aim of all engineering studies centers around prediction, and the sole test by which the practical merits of theories and computational procedures may be justified or rejected is the degree of precision with which the predicted engineering behaviour is confirmed by performance observation.

Only thus can one deal with *economic feasibility* and *estimable risks*, which are two basic columns supporting the entire structure of our technological society.

In this respect it must be conceded in all candour that the problems of soil engineering in residual horizons have proven baffling and frustrating, with but the slimmest probability of success at prediction except through mental application of big correction factors derived from intuition and experience in analogous cases. Indeed, the writer does not know of any publications that report on comparisons between predicted and observed behaviour in granito-gneiss or basalt saprolites.

Local experience on the acceptability of project performance seems to indicate that on the *average* the *behaviour has always been significantly better than assumed;* thus one would reason that the use of the working hypotheses and computations of our conventional soil mechanics would lead to pessimistic estimates and therefore over-conservative design wherever average or cumulative behaviour is at play. Nevertheless, despite such apparent assurance of achieving successful bases for design, one must distinguish between design parameters aptly selected to guarantee the satisfactory completion of a project, and those that may prove applicable to the realistic analysis of project performance.

It is not merely the obvious question of the Factor of Safety, whereby a successful project merely establishes that one is 'on the safe side' (with no indication of how far), whereas in prediction one must reproduce the true behaviour, and not one guaranteed to be 'safe'. It includes also the reminder that one frequently may be safe through an unsuspected set of compensating errors.

In short, therefore, a well supported working principle recommends that wherever possible *in residual soils one should force cumulative action,* obviating the predominance of localized erratic behaviour.

On the other hand, it is also systematically observed that *strictly local conditions have proven much worse* than as anticipated through conventional soil mechanics working principles, leading to frequent disconcerting failures. One must, in this connection, be quite candid in recognizing that as regards such failures (e.g. slides in natural or cut slopes, etc.) the technical literature that sprouts from the case histories belongs strictly to the category of easy a posteriori justifications: the practising professional is ever left in the position of receiving excellent indications of how to shut the stable door after the horse has run out.

Such experience on failures, covering extreme cases of smaller probabilities of occurrence have led to the oft recommended *'working principle of accepting the risk and preparing to face it* (22). The attempts at quantifying such risks have naturally reminded one of statistics and probabilities, (13, 26) and one faces presently the lure of approaches purely statistical.

The writer feels that before any purely *statistical interpretations* and formulations are attempted, it will be *indispensable* to establish appropriate *models of physical behaviour,* for comparisons of predictions with performance.

Finally it is necessary to discuss the concept of the so-called *'design by precedent'* which, in the face of impossibilities at engineering prediction, is earnestly recommended to work 'best when applied where climate and geological conditions are similar to those where the design has previously been successful' (6). There prevails a chimerical *delusion that in the use of precedent one avoids formulation of a law of prediction:* quite to the contrary, what one really does, implicitly, is to formulate the *simplest and least likely of such laws*—that of identity, which usually boils down to *geometrical similitude.* Even within an absolutely identical geologic and climatic setting, where meandering secondary roads earlier called for average cuts and fills of 5 to 10 meters, the ever-increasing pressures of society, progress and population, rapidly impose new highways with cuts and fills of 25 to 35 m, often descending significantly below groundwater table and with rapid excavation: moreover, considering the great importance of rainfall, it need hardly be emphasized that statistically the worst rainfall is ever in the offing. Would design by precedent imply using the same cut slopes? Why not, if nothing is noted to alert that such and such factors interfere, and do so in such and such a manner?

The trouble with design by precedent is that every period and condition limits itself to considering but *the most evident, minimum necessary, intervening factors,* and never is there prediction about which will be the *next controlling factor* as situations progress. So, design by precedent can only progress at the expense of failures—other people's failures we hope—and, since respect for precedent far outweighs the recognition of the need to take all precedents with a pinch of salt, the net expenditure in failures, before any precedent is challenged for change, is really very high. The writer decries the implications of the otherwise useful recommendation. The very preparation of a 'check list' to permit assessing whether or not 'climate and geologic conditions are similar' presupposes theorization far ahead of the precedent routine: unless one uses such an ample check-list, everything always seems similar *until after the failure* when with disarming hindsight a consultant comes to explain why the failure occurred, that is, why one was wrong in assuming things similar that really weren't so. Truly, nothing is ever similar, unless reasonably admitted otherwise.

However, it is inescapably true that in a material that cannot be separated into homogeneous volumes or horizons, and whose heterogeneities and discontinuities frustrate most attempts at defining effective working parameters, *one must let Nature speak from past performance.* The problem is that of *establishing the appropriate vocabulary for interpreting Nature's language.* If that is what one means by 'design by precedent', nothing could be truer.

One must, therefore, establish the probable bases of theoretical reasoning for such interpretation: and obviously, in closing the cycle the same theoretical postulations will be used in design and prediction for subsequent cases. In the face of heterogeneities, the use of statistical correlations based on great numbers of cases, suggests itself as obvious.

However, since statistics in the face of complex problems affected by numerous parameters should never be applied at mathematical random, but should always be patterned within the laws of presumed behaviour of the physical model postulated, the first step lies in postulating the plausible laws. It is relevant to reflect thereupon that any prediction based on a study of previous cases embodies really a *study of comparisons or* *changes*. Moreover, every engineering work is always concerned with the introduction of a change of condition to the masses and materials under consideration.

It is very important to observe that in any problem to be analyzed even if the interpretative laws be relatively far from correct, the error is always very much greater in analyzing the basic condition itself in the light of such laws than in analyzing in closed-cycle reasoning the change of condition from Prototype 1 to Prototype 2. For instance, although it may be quite difficult to determine the existing Factor of Safety FS on a slope, it may be relatively easy and precise to determine how much the FS changes (Δ FS) due to a change of condition, such as, for instance, flattening the slope or lowering the water table W.L. And, in the settlement analysis of a foundation it is more precise to extrapolate from settlement performance of certain loaded areas in order to predict performance of other areas and pressures of loading, than to attempt computations synthesizing directly from the best available theories.

3. *Thirdly, a question of reasoning and approach*

In twenty years of professional consulting practice involving side-by-side the problems of residual soils and those of sediments in such different conditions as those most unfavourable represented by the impressive settlements of high-rise buildings on the very compressible slightly organic marine clays of Santos and other coastline cities, and some quite favourably represented by preconsolidated tertiary sediments in Sao Paulo, the problems of prediction and performance have gradually but consistently impressed on the writer the notion that what is fundamentally at question in residual soils is the application, by direct unadapted transplant, of some implicit reasoning and approach derived for conventional Soil Mechanics on the basis of sediments. The disparity is felt principally in the first steps of an engineering problem, when in the light of index tests one is bound to assess the principal problem, and to select representative conditions for sampling and testing; and, it is felt emphatically again when the circle closes, requiring the interpretation of the results: of course, both disparities are tied to the same fundamental concept, since the *significance of index tests can only be established insofar as the final interpretations and performance vindicate them.*

The first part of the problem is connected with what has been termed as 'structure' and 'fabric' of the residual soil and the fact that most probably in such a fabric the soil constituents do not participate in their 'unit condition' but as clusters. The existing index tests (grainsize distribution, and Atterberg Limits) are based on thorough manipulation and plastification of the soil clusters, in all probability reducing the clusters to the condition of unit grains surrounded by their individual lyospheres, etc. A material that has been eroded, transported and sedimented can well be reasoned to have gone through such a stage of individualization.

The same cannot, however, be concluded of the residual soil, the behaviour of which may still be quite dominated by the fabric created by the soil clusters. *The conventional index tests are therefore pertinent only as an index to limiting potentialities upon full plastification.* This part of the overall problem may be said to concern principally the degree to which the *plastified index tests fail to represent the behaviour of undisturbed soil clusters,* and it will merit brief further discussion in connection with recommendations on investigation and testing.

However, the principal part of the problem may concern the manner in which one might reasonably assume the coparticipation, side by side, of elements of soil that are totally different as regards consistency and deformability.

Generally such consistency and compressibility data come from borings, pits and samples so distant that one has been left with no recourse but automatically using the reasoning (sedimentary) that distinct 'thicknesses' ('layers') of different materials were at play. Recently however, in connection with three major damsites and several kilometers of deep railroad and highway cuts, the writer is having an unusual opportunity of studying the weathering profile of basalts, having thus come to a new concept.

It is difficult to convey fully the three-dimensional picture of fabric culled from meticulous examination of test pits and cuts. As usual, one records the great variations in degree of decomposition both horizontally and vertically even within a space of centimeters, so that every test specimen differs from the neighbouring ones. Of paramount importance, however, is the fact that between significantly harder lumps there are *pockets (or would they be the matrix?)* of material very soft and wet, very much nearer the liquid than the plastic limit, although laboratory results show natural water contents predominantly in the vicinity of the plastic limit. And it is in such material that 20 to 25 m cuts at 35° to 45° have been made, quite rapidly and even going down a few meters below water level, in general without failures. Fig. 2

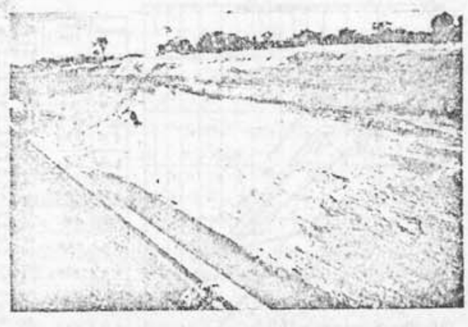


Fig. 2 Railroad cutting 25m deep in basalt saprolite.

shows a stretch of the railroad cut where local slides developed: it must be emphasized that there was no sliding during or slightly after excavation, but only several weeks later, a couple of days after a significant rainfall. Fig. 3 furnishes typical SPT

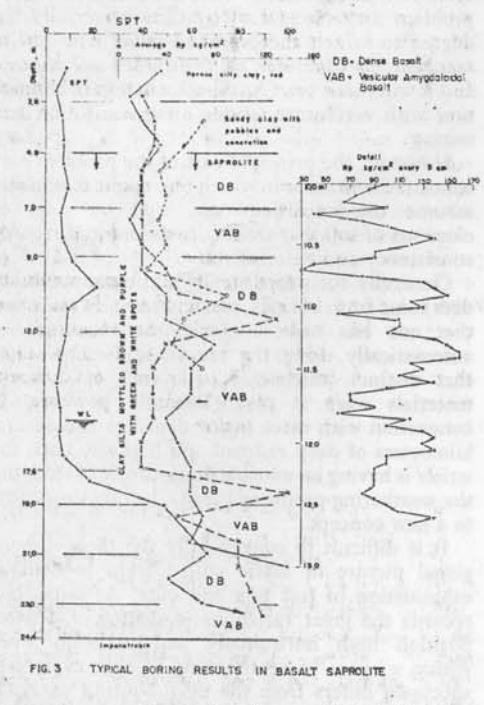
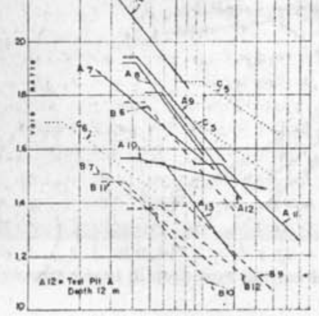


FIG. 3 TYPICAL BORING RESULTS IN BASALT SAPROLITE

results, as well as Dutch-cone Point Resistance R_p kg/cm^2. It is in such boring profiles that routinely built foundations for high-rise buildings have treated SPT values essentially without any adjustment in comparison with the rule-of-thumb procedures in use on sands and insensitive clays (18) and, no disconcerting behaviour has been brought to special attention.

The principal hint of a conceptual problem



Sample	Basalt Type	w_l	w_p
A 7	VAB	58 85	44 46
A 8	VAB	72	48
A 9	DB	89	60
A 10	DB	61	55
A 11	DB	78	52
A 12	VAB	79 105	46 54
A 13	VAB	81 127	46 45
A 14	VAB	77 100	40 41
B 6	DB	99 63	56 42
B 7	VAB	62 105	45 57
B 9	VAB	80 107	45 53
B 10	VAB	74 55	44 36
B 11	VAB	50 65	35 38
B 12	VAB	68	37
C 5	VAB	79	52
C 6	VAB	63	33

FIG. 4 SIMPLIFIED COMPRESSIBILITY CURVES, BASALT SAPROLITES

arose in connection with settlement computations. Fig. 4 summarizes in a simplified manner the e-log p curves, represented merely as the virgin compression straight line starting from the point of the 'virtual preconsolidation pressure'.

The great heterogeneity of initial void ratios and compressibility is quite apparent, and it can be seen that there is no correlation with lithology (subdivision of the basalt flows into Dense Basalt, Vesicular-Amygdaloidal Basalt, etc.) or with depth and overburden pressures. Meanwhile, repeated oedometer tests with unloading and reloading, such as the one shown on Fig. 5, prove that the material does record well a maximum past pressure, as do plastic clays.

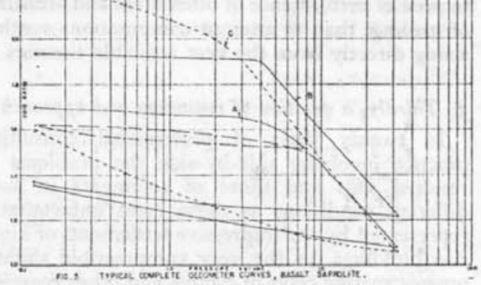


FIG. 5 TYPICAL COMPLETE OEDOMETER CURVES, BASALT SAPROLITE.

How to visualize the existence and concomitant behaviour of such different materials side by side, intricately woven in a fabric that leaves the impression of larger-scale homogeneity despite the intense small-scale heterogeneity? If one wishes to use statistics, what mean, lower confidence limit, and upper confidence limit, compressibility parameters could be taken to furnish reasonable design estimates compatible with performance?

Since the settlement performance on the projects above mentioned is mostly still in the offing, it is important to summarize the 'intuitions' derived by the writer from a vast number of other smaller projects which unfortunately rush along providing little or no basis for quantitative re-evaluations of the design. In short, in comparison with an inescapable trend of attributing pessimistic parameters (lower confidence limits of test value, etc.), the final field behaviour of the residual soil has generally been very much better. In foundation work, although one has never taken relic structure weaknesses into account, there has never been any problem of foundation shear failure, not even with heavily loaded caissons or with 20 to 25 m high embankments placed on 1:1 slopes. In settlement computations, the observed field behaviour is generally much better than as would be computed by using the poorer test results, through the traditional prudence required of the engineer.

The facts are therefore taken to hint at the hypothesis that it is the less compressible soil elements that play the dominant role in carrying the overburden stress.

In alluvial soils the various soil elements (dx) (dy) (dz) side by side are very similar and very weak, and their resistance develops on the basis of the really uniformly distributed overburden pressure: no soil element needs become any stiffer than as minimally required to receive the overburden stress γ z, starting from initially fluid conditions. Therefore it is rational to accept a priori the absolutely uniform initial stress throughout. However, in residual soil the analogy is of a very stiff (high E) material being corroded, and the higher initial stress locked-in a soil element would only gradually suffer degradation into the homogeneous conditions of the maturely weathered horizon. Obviously if there are volumes (or columns) of very soft material close to the Liquid Limit, it is only so because the adjacent high-E columns carry the stress, allowing the weaker material to soften at will, because it is structurally inactive and unnecessary (as in extreme cases we have the examples of cavities in soluble rocks). Therefore it is postulated that during the chemical weathering process of the basalts under study, some of the initial differences of internal stresses are retained in the residual soil while adjacent soil elements adjust their deformations to be equivalent.

It is true that within the rock there are conditions when stresses release without the possibility of full adjustment of deformations, and thereby indeed arises one of the important physical weathering agents: but it is accepted as reasonable that by the time the material has degraded into soil the *principle of equivalent deformations* may be postulated.

The basic premise is that we can initially *determine the percentages of occurrence of different materials*, and that the *deformability parameters of each are known*. Moreover it is postulated that *there is no reason why in situ stresses in the different materials be equal to the average overburden stresses.* On the contrary, in close analogy to the demonstrations of Rock Mechanics, it is quite possible and likely that *initial overburden stresses in the different materials be different*, possibly retaining some of the internal stresses of the parent rocks. However, it is postulated that the initial stresses in the separate materials must be in *such an equilibrium that under small stress increments the deformations will be equivalent*. It cannot escape notice that all subsoil profiles are continually subjected to small stress increments, for instance, by seasonal fluctuations of the groundwater table, and it seems reasonable to assume that under such increments the several materials of significantly different deformabilities are not separated by internal fissuring, etc.

Finally, under all conditions the equations of static equilibrium must be observed.

For a necessary temporary simplification of the above general concepts, the principle will be exemplified in connection with available oedometer tests and unidimensional (vertical) deformation, as well as merely the static equilibrium in the vertical direction. Moreover it will be assumed that the percentage of occurrence of materials 1, 2, 3 . . . n, is a percentage in horizontal areas a_1, a_2, a_3 . . . a_n. Finally it will be assumed that the fabric is such that the different materials may establish continuous contiguous 'columns' although highly sinuous three-dimensionally. The probability of collapse of the stiffer columns is expected to be much smaller for the case in which the column is formed by 'corrosion' starting from higher imposed stresses than for the case in which the column formation (e.g. loess, etc.) is at very low imposed stress.

Simple analogies with pretensioned concrete columns suggest themselves.

Assume initial vertical pressures p_1, p_2 . . . p_n under overburden. By vertical equilibrium $\Sigma a_n p_n = \gamma Z$: in computing by trial and iterations obviously one should assume higher initial pressures carried by the less deformable materials.

It is postulated as physically most reasonable that a *small increment of stress will be distributed among the different materials in direct proportion to the respective initial pressures:*

$$p_m/p_n = (p_m + \Delta p_m)/(p_n + \Delta p_n)$$

The total stress increment continues to maintain vertical equilibrium, so that $\Delta p = \Sigma a_n \Delta p_n$.

Thus, as a first step one can compute the probable initial stresses in the distinct materials. And, by exactly the same reasoning, as the second step sought, one may compute the final settlement due to a given applied pressure increment as resulting from the accumulations of settlements due to small increments of pressure all of which have satisfied the above equations.

The procedure has not yet been run through to the full, for prediction on a full-scale test, and comparison with performance, because in the light of the hypothesis it has become necessary to develop and assess procedures for determining in the test pits, the percentages of occurrence of the different materials. Of course, the computed settlement will be intermediate between the values that would result by considering the softest, most compressible 'normally consolidated' curve, and the stiffest, least compressible 'highly preconsolidated' material. And since any computational procedure incorporating the results of both the stiffer and the softer materials will automatically result in an intermediate value of predicted settlements, it is conceded that it will not be easy to prove that the above hypotheses are truly valid.

At any rate, the fundamental hypothesis that the harder nuclei may be at significantly higher stresses than average overburden pressure, is offered with some degree of expectation of consequence. For instance, in extracting weighted average shear strength parameters for stability computations it may be of great significance to distribute initial stresses and deviator stresses in proportions compatible with the rigidities of the materials. Moreover, there are several variations on the basic theme that may be tried and found more attuned: for instance, there are obviously conditions of three-dimensional fabric within which harder nuclei are entirely surrounded by a matrix of different deformability, and thereupon the stress partition for adjustment of volumetric deformabilities would follow another routine.

Finally it must be emphasized that in practice the adjustments will have to be statistical. And, in such a step it may be most useful to employ the equivalence of deformations not only under the small increase of pressure, but also under a small stress release, in accordance with the unloading curve.

A simple example has been worked out as shown in Fig. 6 for the purpose of exploring the manner in which such principles would be employed in computing a weighted average consolidation curve. The Figure presents the e vs. log p curves of two

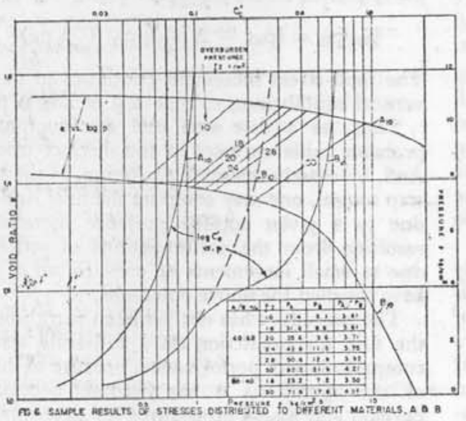


FIG 6. SAMPLE RESULTS OF STRESSES DISTRIBUTED TO DIFFERENT MATERIALS, A & B

materials (A 10 and B 10 of Fig. 4) of widely different stiffnesses that occur at a depth of 10 meters, where the overburden stress is 18 t/m². If we assume that the initial pressure is uniformly 18 t/m² and that the compressibility settlement is dictated by the soft material, B10, we would compute for an increase of pressure $\Delta p = 10$ t/m² that the settlement should be $p = 0{,}0083$ H. Meanwhile, if the only effective material is the stiffer one, the corresponding settlement would be $p = 0{,}0023$ H, which is roughly 28% of the former.

Assuming that there is 40% of the stiffer and 60% of the softer material, we would first compute the initial stresses compatible with the principle of equivalent compressions. A settlement due to compression has been expressed as

$$\frac{C'_{c1}\ H_1}{1 + e_1} \log \frac{p_1 + \hat{n}p_1}{p_1},$$

and if one assumes that both 'columns' have the same effective height H, it would be necessary to satisfy the equivalence

$$\frac{C_1}{1 + e_1} \log \frac{p_1 + \hat{n}p_1}{p_1} = \frac{C_2}{1 + e_2} \log \frac{p_2 + \hat{n}p_2}{p_2}$$

and since the two logs are automatically equivalent, it would degenerate into finding such values of p_1 and p_2 as satisfy vertical equilibrium as well as $C_1/(1 + e_1) = C_2/(1 + e_2)$. The values of C for small increments of pressure have been taken as tangents to the e vs. log p curves at various pressures and are plotted in Fig. 6. By running the problem through a computer program it is found that the initial stresses are 31,8 t/m² for the stiffer material and 8,8 t/m² for the softer one.

In such a case the settlement under the same overall pressure increment of 10 t/m² would be $p = 0{,}0043$ H, and the final stresses on the materials would be 50,6 and 12,9 t/m² respectively, while the two materials followed compression trajectories along their individual e vs. log p curves.

Under the present highly simplified assumptions obviously the computation of 'initial stress' under, say, 28 t/m², yields the same result as the computation of the stress and settlement trajectory from, say 10 t/m² with an increment of 18 t/m², or from 18 t/m² with an increment of 10 t/m².

For the sake of checking on the sensitivity of the computation to basic data, some computations have been repeated under the assumption of inverting the percent occurrences of areas A and B to 60 and 40% respectively. The resulting deformation-equilibrium stresses are tabulated for comparison in Fig. 6.

A more extended computer program can take into account any frequency curve of occurrence of different materials, each with a well-defined compressibility.

The interesting question that is posed is whether or not the virtual preconsolidation pressures end up revealing the initial stresses effective on the various materials, thereby defining indirectly but automatically the percentages of their effective occurrence or participation, so that settlements would be computed through attributing to each material small stress increments proportional to their virtual preconsolidation pressures (as presumed initial stresses).

Very many questions are laid open.

4. *Fourthly, the problem of method of investigation*

In view of the above discussions a few comments must be included to summarize the writer's principal thoughts with respect to the problems of field and laboratory investigation in residual soils.

Conceptually the present situation is indeed most unsatisfactory.

Field and laboratory investigations should be designed to furnish data for use in some context of theorization: most often the engineer loses sight of such a simple and fundamental truth, merely because chronologically it appears that investigations come first, and then the theorization is adapted to make the best possible use of the test data.

So, investigations are standardized as though they could be of absolute purpose and value, and not merely relative, as *investigations for something* or towards use in some pre-established line of reasoning.

Even if a pre-established line of reasoning could be accepted as meritable within a moderately matured conventional soil mechanics, the discussions above should serve to emphasize that in residual soils it appears detrimental to presuppose rigidly the same line of reasoning. Even the use of statistics in such a situation proves to be no more than an attempt to patch up an untenable situation.

Three fundamental parts of the problem must be tackled. (1) *First, the problem of coverage and representativeness.* The use of *Standard Penetration Test* exploratory borings *appears quite satisfactory for indications on average conditions* (without relic structure conditionings) because it does not exaggerate highly localized heterogeneities as does the static cone penetrometer Rp; moreover, without too much ado it can be forced to penetrate down to the moderately weathered rock. As a routine, when the drive-sampling reaches an obstruction it is necessary to confirm whether it is a corestone or not, and that should be done either by employing rotary coring to drill through it, investigating its vertical extent, or by starting on additional exploratory percussion borings at slight shifts from the original one, to bypass the corestone and thereby estimate its extent in plan. The geophysical method of investigating boulders and corestones as suggested by Lundstrom and Stenberg (14) is presently under experimental use in some projects in Brazil.

An important problem is the *assessment of heterogeneity*. Therefore it is suggested that within the routine exploratory borings SPT drive-sampling be performed almost continually, that is, at every 0,5 m.

Moreover, as regards horizontal variation, it is necessary to dispel a false notion on the presumed utility of split-spacing: it is current that after a set of borings have been put in at, say, 50 m center-to-center, engineers somewhat preoccupied with possible intermediate heterogeneities will immediately suggest split-spacing to 25 m centers, and so on. Truly almost nothing is gained by such efforts of absolutely analogous scale of investigation of variability, conditioned by linearized thinking. Indeed, after the variability to the scale of tens of meters has been roughly established, it is necessary to investigate the variability to the scale of 1–2 m, and finally, within the representative samples, the variability to the scale of tens of centimeters (test specimen scale). Thus, it becomes appropriate to drive two or three borings within distances of 1–2 m of each other, rather than to waste money on split-spacing.

The acute lack of index tests of real significance makes it very important to permit visual-tactile observation of the residual material.

The dry-sample from the SPT spoon can be satisfactory for a first-order classification: it is important to remember however that classification by *visual-tactile routines should attempt to assess and describe the specimen as nearly as possible as it is,* with its crumbly granular behaviour, etc, even when upon manipulation it would acquire totally different features of high plasticity.

For improved characterization by visual-tactile observation, above W.L. very much use of test pitting has been a rule, and below W.L. the recommendation is to use Denison or Pitcher undisturbed sampling.

(2) *Secondly, the problem of determining the fundamental geotechnical parameters.* Hitherto most of the experience on compressibility, shear strength, and permeability of residual soils has been almost automatically associated with block samples extracted from test pits, and with small specimens (mostly 1.4 in. dia. for triaxial, etc) cut therefrom. To begin with, it seems obvious that until a fair amount of in-situ testing is achieved, to confirm or reject the validity of test pit block sampling, every conclusion from laboratory testing should be quarantined under suspicion. Rock Mechanics has emphasized the importance of in-situ testing in comparison with any sampling and laboratory testing; and the interference of Rock Mechanics thinking in residual soils is re-emphasized. The *irreversibility of deleterious effects of stress releases* and even *minute deformations capable of destroying delicate remnant cementitious bonds,* should be the major preoccupation. Presently, from the Soils Mechanics tradition the only in situ tests in frequent use are the static cone Rp, and the plate load test. The pressiometer test (Ménard, for instance) should have great interest, but the writer has not heard of any application in residual soils yet. The trend of thinking has been that in the face of the extreme heterogeneity one should multiply the number of tests (therefore subconsciously restricted to the cheaper ones), and thereupon employ statistics. The writer must therefore emphasize the need for *fewer purposeful and well-designed tests, even if more expensive,* until the phenomena at play can be ascertained.

Since sampling and laboratory testing must be employed to some extent, the optimization of the quality of samples and specimens must be sought.

The writer had demonstrated (17) in connection with the Ashford Common Shaft block samples and concomitant investigations on highly preconsolidated London Clay, that there are cases (confirmed more recently by Morgenstern and Thomson, 21, also with reference to clay till) in which exposure and stress release causes much more damage to samples and specimens than the presumed remoulding in smaller-size tube sampling.

It seems evident that such will be the case with residual soils, in consonance with performance observed in Rock Mechanics, and in accentuated difference from the case of sensitive clays, which residual soils definitely are not.*

Thus a Denison or Pitcher sample of suitable diameter may prove better than block samples. Naturally the question of diameter of sample and specimen cannot be set aside, merely from a point of view of minimizing minute-scale heterogeneities and specimen surface discontinuities. It may be of interest to note that in the writer's experience the accentuated heterogeneities are frequently reflected much more in oedometer tests than in routine small-diameter triaxial tests; it may connote the fact that the stress releases rapidly damage shear strength parameters along a weak plane to their minimal values irrespective of denser initial condition of the specimen, while the average performance of the specimen still makes itself felt in a cumulative compressibility phenomenon.

In compressibility testing the use of big diameter triaxial consolidation seems naturally recommendable, all the more so because of the interest in adjusting lateral stresses at will. It is important to note that irrespective of classifications by routine grainsize and plasticity index tests, most residual soils indicate a very rapid compression and consolidation, almost instantaneous in standard oedometer tests.

Testing for permeabilities in residual soils presents a real problem because tests on block samples may be much affected by the relic planes of discontinuities under stress release, while in situ tests predominantly rely (except when using tracers and direct measurements of flow velocities, etc) on formulae derived for mathematically idealized masses and boundary conditions. In this specific case, however, the writer's experience leads him to lend much more credence to laboratory tests on specimens under appropriate confining stresses. A hard problem arises because open cracks are not sampled presently. It must be noted that permeabilities do tend to be more and more accentuated in cracks or solution and erosion canaliculi as initial degradation develops, and therefore whereas a crack or canaliculus may have little significance on compressibility and shear strength problems, if having *ipso facto* transferred stresses to the more competent zones, their influence on problems of seepage and cleft water pressures may be very great.

The writer knows of no reference to Ko values measured on residual soils.

Judging from the hypotheses formulated above, it would appear that Ko values determined on laboratory specimens would have little relevance, and in fact that the internal stresses presumed to obtain in situ in nuclei of differentiated stiffnesses would require determination by adaptations of field techniques in use in Rock Mechanics.

Finally, in connection with application of all the results of compressibility and shear strength testing, special observations on areas and volumes of harder and softer materials distributed within the fabric are expected to be required in view of the reasoning proposed on non-uniform initial and incremental stress distributions. Work along such a line has just begun. As a first try a soft pocket penetrometer in being used against the faces of test pits.

(3) *Thirdly and finally one must discuss the use of index tests and observations.* As has already been justified and somewhat qualified above, it is inevitable that as a first step one retain in routine use for all soils the index tests of grainsize distribution and liquid and plastic limits. The practice has been criticised for saprolites (e.g. Little, 10, and many others) because of the fact that the results of such tests depend very much on the amount of reworking and 'plastification' effort, making them erratic and not reproducible.

At least inasfar as concerns the grainsize distribution there may appear to be some justification in assessing the potentiality of a residual soil disintegrating into its unit constituents. Presumably, for instance, with regard to the prospects of erodibility, a grainsize distribution curve run 'in the wet' should be a satisfactory index.

To the criticism that the presumed unit grains themselves are poorly defined, subject to variable degrees of further breakage by mechanical effort or of 'plastification' and deflocculation depending on how manipulated, the advocates of the test will remonstrate that there is considerable additional interest in checking, on each soil, what variations of result are introduced by various degrees of altered preparation and test procedure.

As regards the routine Atterberg Limit tests the initial criticism is that the residual soils do not have a 'plastic' and impervious behaviour *in situ* to begin with, and there is little or no likelihood of such plastification in connection with engineering projects that retain them in situ (excluding most obviously such projects as use the residual soil as borrow material, in which case the investigation is of a borrow material and not of a residual soil). Moreover, the lack of reproducibility of test results is again a problem. For instance, Fig. 7 shows comparative results on presumably similar samples of basalt saprolite tested in four separate laboratories, and also shows the distinctions between Atterberg Limit tests on specimens manipulated from natural water content, from an air-dried condition, and from the oven-dried condition. However, again, upon such different results some specialists have quickly rejoined that one should therefore run the tests in different fashions and the extent to which results would be different would be claimed to be another index of interest.

* A Brittleness Index similar to Bishop's (3) but with reference to τ ultimate at 20% strain. $I'_B = \frac{\tau f - \tau\ ult}{\tau f}$ has been found to range around 0 to 0.3.

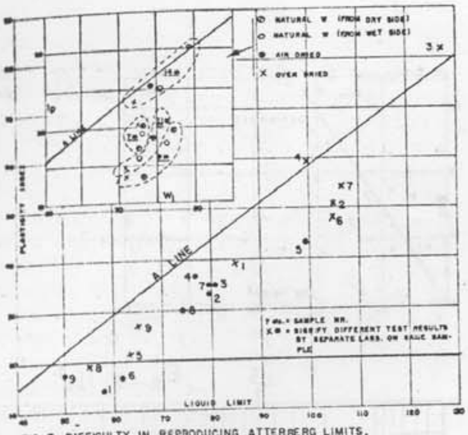


FIG. 7 DIFFICULTY IN REPRODUCING ATTERBERG LIMITS, BASALT SAPROLITE

Indeed such entertainment may be of interest, and may turn out to be an index, but the *basic question is an index to what*. One must guard against allowing an index test to creep into self-justified overimportance: an index test should be an index to other desired less accessible test values, and not an index unto its own singularly interesting variations.

Borrowing from the Peter Principle which demonstrates that in bigger and older institutions employees get promoted to their individual levels of incompetence, the writer would affirm that the Liquid and Plastic Limits have, in residual soils, been institutionalized to their levels of incompetence. For instance, the empirical correlation of C_c vs W_1 is subject to gross dispersions, as can be seen in Fig. 8; the writer has found no possibility at all of correlating cohesion with preconsolidation pressure, as has been suggested for sedimentary clays by such empirical relations as $c/p \approx 0{\cdot}115 + 0{\cdot}00343\,I_P$; finally, absolutely no inkling could be obtained, from W_L values, of the speed at which

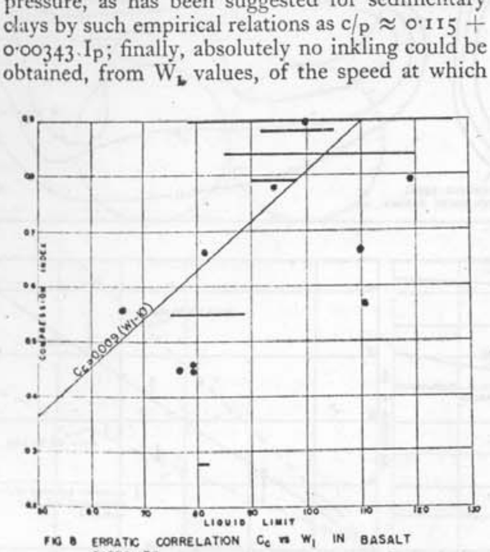


FIG 8 ERRATIC CORRELATION C_c vs W_1 IN BASALT SAPROLITE.

settlements occur both in the oedometer tests and in field performance, in the saprolites under discussion. Since compressibility depends not only on plasticity of the soil constituent but also very significantly on fabric and collapse of structure, the writer has found considerable interest in appraising the ratio of (Swelling Index Cs)/(Compression Index C_c) from oedometer tests, which has been found to vary between 40% and 3%, the lower proportions being applicable to nonhydrophilic collapsible structures. Simultaneously the simple linear shrinkage test on undisturbed specimens, and further the comparison of the same test on the remoulded specimen, has served as an indication of 'structure'.

Obviously, absolutely *new index tests must be developed,* that should serve as a *first index to the fundamental geotechnical parameters* of compressibility, shear strength, and permeability. It cannot escape notice that such index tests will have to be based on undisturbed or semi-undisturbed specimens, or based on penetrometer devices (or similar) that assess the soil element in situ. The aim is clear, and the sooner one stops wasting further effort in unpromising directions the better.

5. *Sample problems and suggested solutions*

5.1 *Foundations*

From the very many cases of foundations satisfactorily designed and built, and from the lessons culled from cases that suffered from unsatisfactory performance, the writer has attempted to extract three recent cases that can be considered to embody a suitable demonstration of the principles enunciated.

(a) Preloading of tank foundations

The writer was consulted on the problem of design of foundations for a group of 47 m diameter water tanks, computed to exert a pressure of 8·5 t/m² on the subsoil profile exemplified in Fig. 9 by two of the many reconnaissance borings that had been put down in a grid. The appearance of soft 'clayey' subsoil had earlier suggested to the Engineers a pile foundation, and because of the light loading, precast concrete piles had been selected: however, upon attempting to drive the piles of estimated lengths of 10 to 14 m, it was discovered that adjacent piles would either penetrate the full length, or be obstructed after penetrating but a few meters, or even would break under the driving, upon meeting corestone obstacles. Driving of precast piles had therefore been abandoned as hopeless, as is most frequently the experience in granite, basalt and diabase saprolites.

It seemed evident to the writer that a grade support should be tried. The problem was to estimate settlements and differential settlements, to which the tanks would be very sensitive. Two approaches were tried simultaneously. On the one

SPT 3

E_{0-5}

100

200

D = 10

10-20 = 500

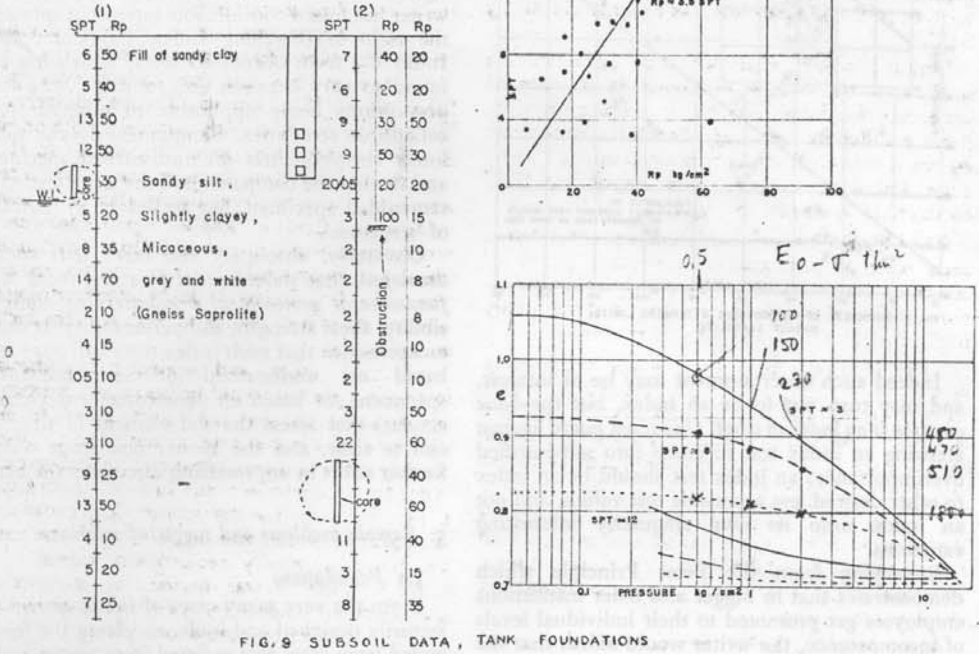


FIG. 9 SUBSOIL DATA, TANK FOUNDATIONS

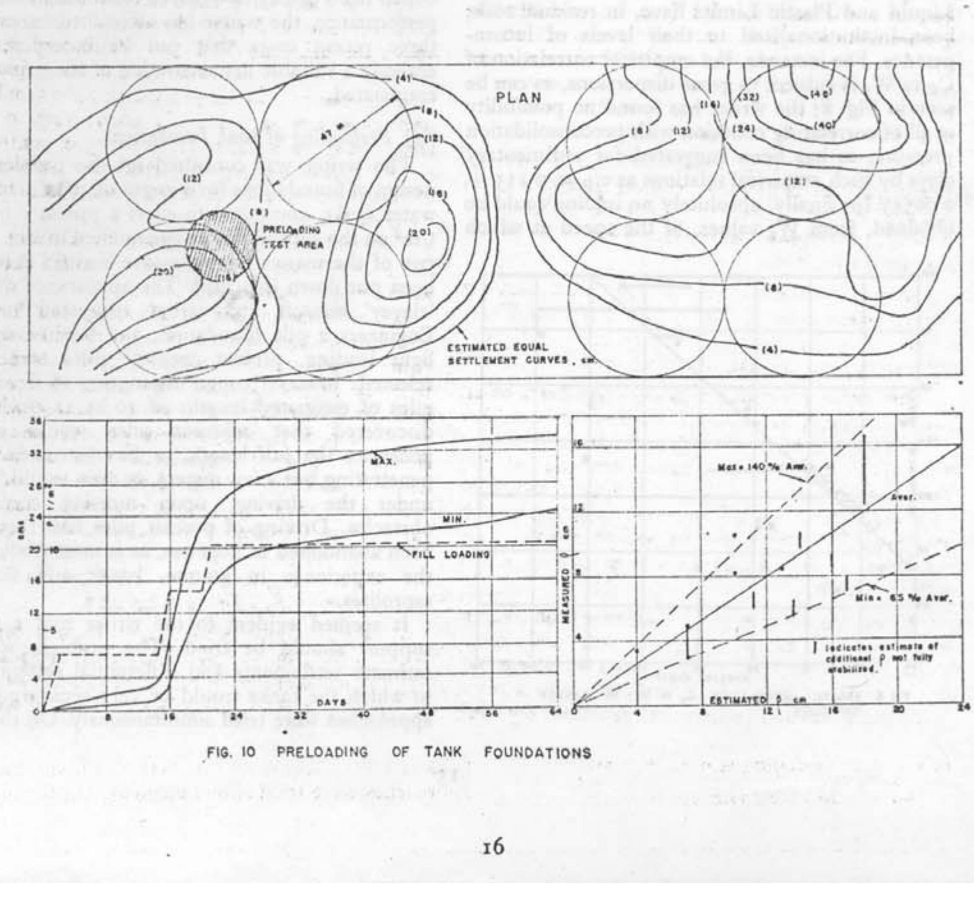


FIG. 10 PRELOADING OF TANK FOUNDATIONS

hand, three Dutch-cone penetrometer tests were put in at 0,5 m from existing SPT borings: despite extreme scatter of occasional sets of data, by judicious selection of relevant sets of values a general trend of a highly simplified (cf. 18) correlation could be established as roughly $R_p \approx 3{\cdot}5$ SPT.

Simultaneously a shallow test pit was dug down to W. L. at the location of a boring that appeared promising for furnishing block samples of significantly different densities. The three oedometer curves determined were roughly estimated to be associated with material of corresponding SPT blowcounts as shown in Fig. 9.

The important conclusion from the consolidation tests was, as anticipated, the great speed of consolidation (no attempt at classical determinations of C_v values could be made) and the totally negligible rebound on release of pressure.

Thereupon, the SPT data of the great many exploratory borings were transformed into would-be R_p profiles at each location, and based on Buisman's method of settlement computation for sands, a set of presumed equal-settlement curves was drawn up (Fig. 10).

Representative computations of settlements by the oedometer curves, and assumed interpolated curves for other SPT values, lead to settlements 18,33 and 41% higher than by the Buisman method at the same locations.

A preloading solution was suggested, and was very conveniently applied because considerable earthmoving was going on, and the overload fill could be placed in areas of 50 × 20 m (the smaller dimension specified not to be smaller) and could be reexcavated for placement in a contiguous area in a matter of a few weeks.

Because of a need to introduce a correction factor on the settlement computations, and to achieve a specific recommendation on time of loading, settlement observations were made on the first preloaded area.

Simple so-called Swedish box (2) settlement devices placed on grade before overload filling began, supplied very satisfactory records for the interpretations desired, as shown in Fig. 10.

The initial settlement estimates had been based on a Buisman coefficient of incompressibility $C \approx 2\, R_p/\bar{\sigma}\, \gamma\, z$, purposely taken slightly higher than recommended for sands because of the writer's experience that measured settlements in saprolites tend to be somewhat smaller than estimated. To attempt to compensate for long-term settlements the preloading was carried to about 20% overload.

It can be seen that the average settlements measured were of the order of 2/3 the anticipated ones, but with longer duration of loading the settlements under the $10{\cdot}4$ t/m² preload may reach what was estimated as final settlements for $8{\cdot}5$ t/m².

(b) *Compaction piling for shallow support of high pressures*

The case concerned an intake tower structure, schematically shown in Fig. 11, on the subsoil profile as shown, computed to exert pressures of the order of 20 to 25 t/m².

Important considerations in the design decisions on the foundation to be used include:

1. Against the use of conventional piled foundations: The rapidly dipping elevation of firmer saprolite essentially parallel to the original ground surface, making it impossible to drive structural piling along the rear face of the concrete structure; the proportionally significant horizontal loading and deformations anticipated because of water pressure and the rockfill slope accommodations; the proportionally very significant incremental vertical loading due to the rockfill surrounding the concrete structure, leading to negative skin friction on any piling.

2. On the other hand, against a hypothesis of footing support: it would be necessary to excavate and substitute the very low density saprolite under the front of the concrete structure to depths of roughly 10 and 15 m respectively, with ground-water level essentially at the surface; experience is most unfavourable in attempting deep excavations with ground-water lowering, even with the use of vacuum, in such silty micaceous saprolites.

As a result it was decided to support the structures on grade after a treatment of the low density saprolite by compaction piling.

On the basis of preliminary estimates of the improvement of density desired of the saprolite above the arbitrary SPT $\leq$ 13 boundary, it was concluded from earlier experience with compaction piling in sandy materials, that such piles should be used representing nominally about 10% of the area, at a spacing not wider than $1{\cdot}5$ m centre-to-centre, and occupying in overall area dimensions of about $1{\cdot}1$ times the length and width of the concrete base.

Compaction piling was done by a Franki rig, using coarse sand and fine gravel to fill the hole as the casing was tugged out.

Performance has been satisfactory, as settlements and horizontal movements were roughly accompanied during construction but not recorded. It must be pointed out that the stress-deformation behaviour of the initially loose saprolite with its rigid compaction piles obviously cannot be simplified to the mere consideration of an average compaction effect; but field performance of cases estimated by such a simple routine has been satisfactory, pending improved methods of analysis.

(c) *Footing, caisson or Franki pile for an exceptionally high building load*

The third case involves the highest building column load the writer is aware of, supporting a reinforced concrete building equivalent to about 30 storeys of floors 46 × 37 m, centred within a

C.a.c. = 3d

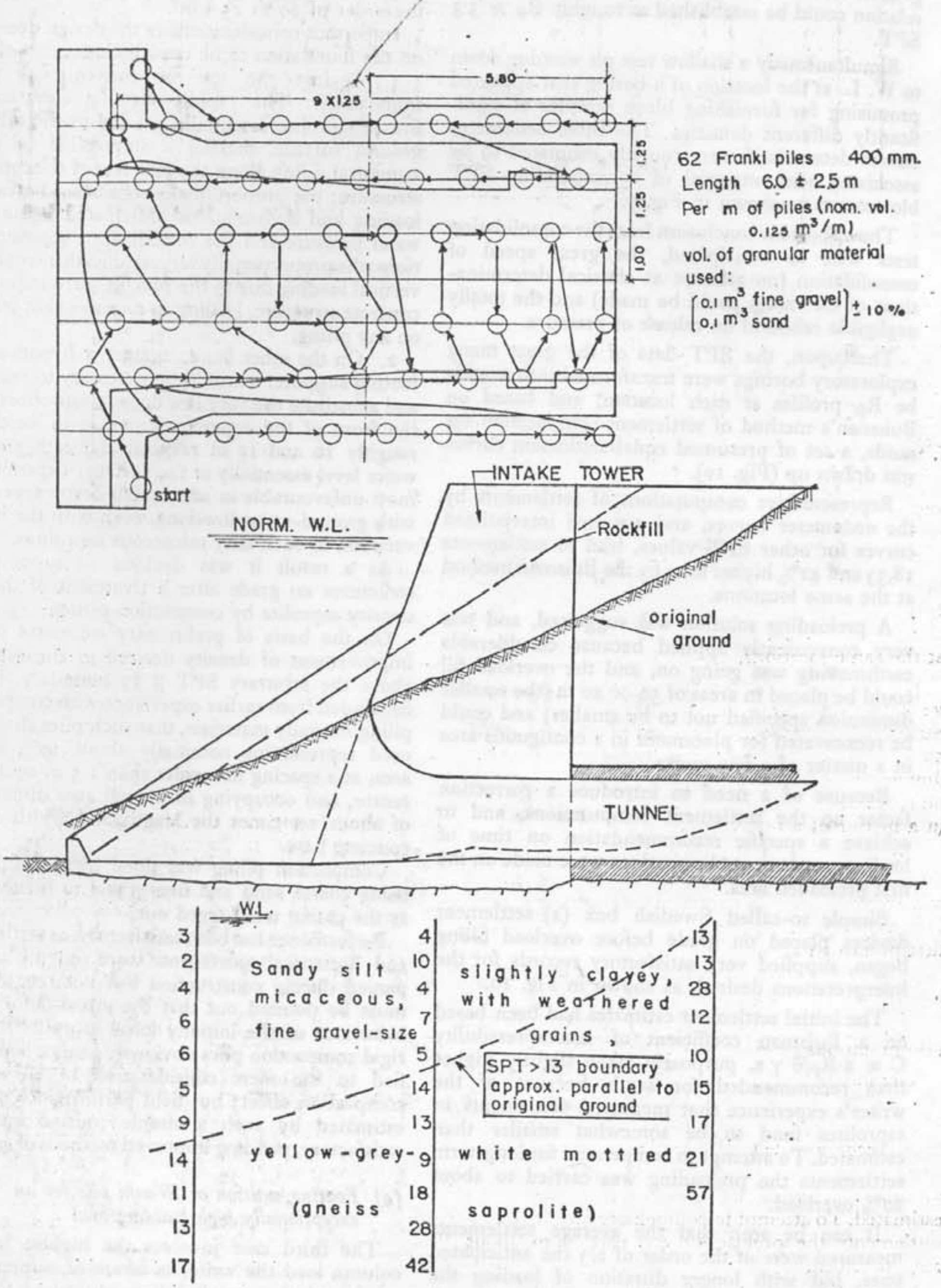
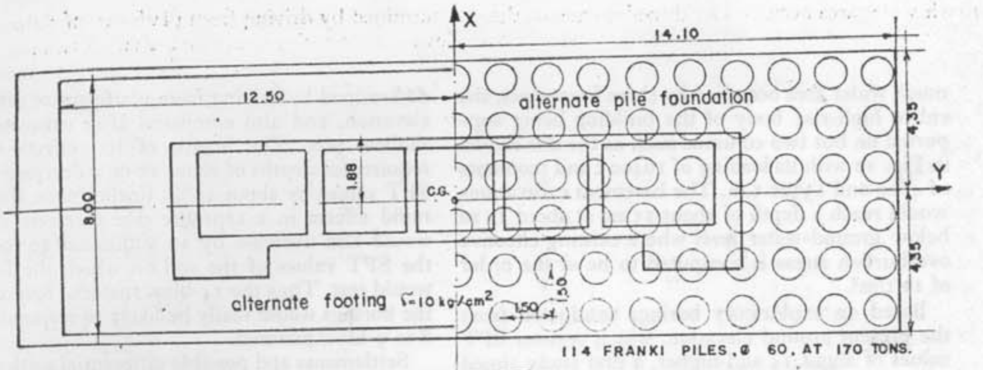
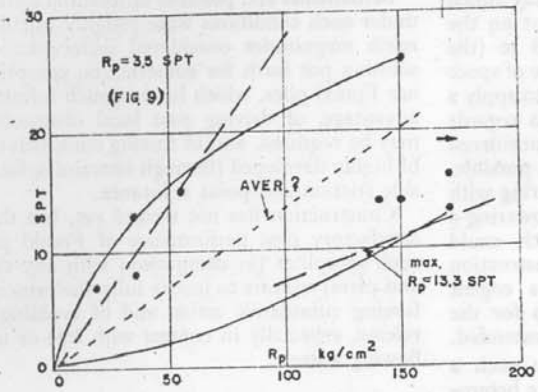


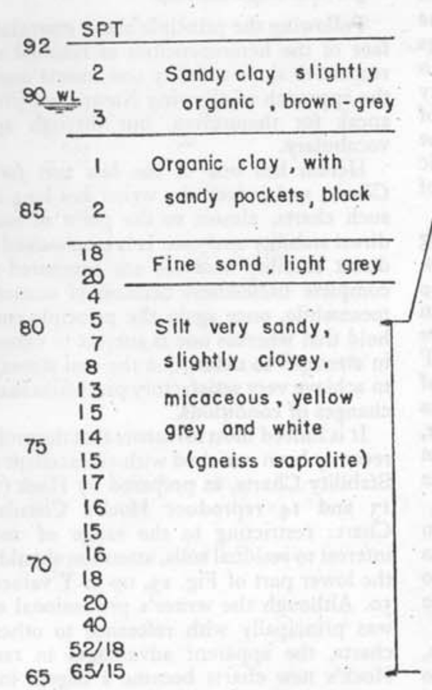
FIG. 11 USE OF COMPACTION PILING





Two borings 0.5 m apart

DEPTH	SE II	SE II A
6	5	4
7	7	5
8	7	4
9	8	3
10	6	1
11	7	7
12	7	10
13	9	10
14	7	10



SIX BORINGS ≈ 20 m APART

Elev.	S 1	S 2	S 3	S 4	S 5	S 6	Aver.
80	16	5	16	22	17	19	16
79	18	7	14	23	14	14	15
78	15	8	15	20	16	14	15
77	15	13	15	12	28	12	16
76	18	15	18	13	28	12	17
75	19	14	15	12	27	12	16
74	18	15	17	13	24	17	17
73	22	17	11	14	21	17	17
72	26	16	14	14	26	16	19
71	30	15	13	57	34	15	27
70	65	16	16	55	35	28	36
69	45	18	55/20	57	39	45	48
68	42	20	24	41	41	45/15	45
67	50/15	40	20	37	36	50/15	
66	50/15	52/18	21	37	44		
			19	56	30/5		
			50/15				
			50/5				

FIG. 12 VERY HIGH LOADING. FRANKI PILE FOUNDATION

much wider area occupied by three basements, the entire high-rise body of the building being supported on but two columns such as the one shown in Fig. 12 with its loading of 16400 t and moments of 4600 and 13300 t.m. The basement excavations would reach a depth of about 15 m, at about 12 m below ground-water level where existing effective overburden stress is computed to be of the order of 12 t/m^2.

Based on exploratory borings conducted from the present ground elevation, which register SPT values of about 14 and higher, a first study aimed at the possibility of using direct support on the saprolite at around elevation 77 to 78 m (the basement floor being at elev. 80). Because of space limitations in plan it would be necessary to apply a bearing pressure of about 10 kg/cm^2. As regards method of execution the alternatives considered were a footing foundation, as shallow as possible, requiring very careful ground-water lowering with vacuum pumping, or the possibility of lowering a very big compressed air caisson which could profitably go deeper: in both cases construction problems were judged to constitute a cogent deterrent. However, the subsoil studies for the hypothesis of direct support had to be extended.

The very difficult problems faced in such a design begin with the investigations. The heterogeneous densities of the saprolite, which have been repeatedly emphasized, are well revealed by the tabulation of Fig. 12 of the SPT values of borings at average distances of about 20 m. Moreover in the case of three borings the scale of heterogencity was further investigated in brief, within a range of 0·5 m centres: a sample result, as tabulated in the same Figure would indicate even more erratic results, such as appear untenable in a design of such responsibility.

Since differential settlements capable of causing the tower to lean would be of principal concern, an attempt was made to obtain representative R_p data from cone penetrometer soundings, for use in Buisman-type computations. Three soundings were put in at about 0,5 to 1,0 m from existing SPT borings. Unfortunately, due to limitation of penetration capacity of the static cone very few sets of data for correlation could be obtained: moreover, the extremely erratic values recorded (especially in denser saprolite) hardly permit any correlation, as can be seen from Figure 12.

Thus, whereas it is extremely important in saprolites to depend on in situ tests, it is obvious that the Dutch-cone penetrometer cannot tend to the needs by far, whenever denser saprolites are involved.

Finally, in resorting to the use of SPT values, corrections would have to be introduced for the important effects of depth (length of rods) and of stress releases upon excavation (19). Three series of tests were conducted using SPT borings very close to one another; to assess the probable magnitudes of the two effects the SPT values were determined by driving from platforms of different elevation, and also compared after excavation of shallow pits. The length of rod effects would require, for depths of about 15 m, a decrease of the SPT values by about 30%: furthermore, the very rapid effects in a saprolite due to stress release would also decrease by an additional 20 to 10% the SPT values of the soil on which the footing would rest. Thus the 14-blow material recorded in the borings would really be likely to respond as an 8 to 9-blow material.

Settlements and possible differential settlements under such conditions were roughly calculated to reach magnitudes considered unacceptable. The solution put forth for construction comprises the use Franki piles, which have in such terrain every advantage, of driving past local obstructions as may be required, and of forcing cumulative action of highly developed (through lateral displacement) side friction and point resistance.

Construction has not started yet, but the very satisfactory past performance of Franki piles in such saprolites (in comparison with any caissons and piers) appears to justify fully the principles of forcing cumulative action and of avoiding stress release, especially in contact with free or upward flowing water.

5.2 *Cut slope stability*

Following the principle above enunciated, in the face of the heterogeneities of residual soils, with respect to slope stability one resorts once again to the approach of allowing Nature and precedent to speak for themselves, but through appropriate vocabulary.

Herein lies one of the *best uses for Stability Charts,* and indeed the writer has long since used such charts, almost to the point of exclusion of direct stability analyses. It is emphasized again that direct stability analyses are hampered to almost complete uselessness because of scatter of data; meanwhile, once again the principle continues to hold that whereas one is subject to excessive error in attempts to determine the real status, it is easy to achieve very satisfactory precisions in computing changes of conditions.

It is indeed most fortunate that the profession has recently been enriched with an excellent new set of Stability Charts, as prepared by Hoek (7,8). Figs. 13 and 14 reproduce Hoek's Circular Failure Chart: restricting to the range of more likely interest to residual soils, attention should centre on the lower part of Fig. 13, up to Y values of about 50. Although the writer's professional experience was principally with reference to other stability charts, the apparent advantages in resorting to Hoek's new charts become a cogent inducement towards exclusive use of them in this presentation. It must be emphasized that Hoek presents simultaneously another chart for the case of plane failure along a preferential plane of sliding, which is not only the more frequent case in rock but also

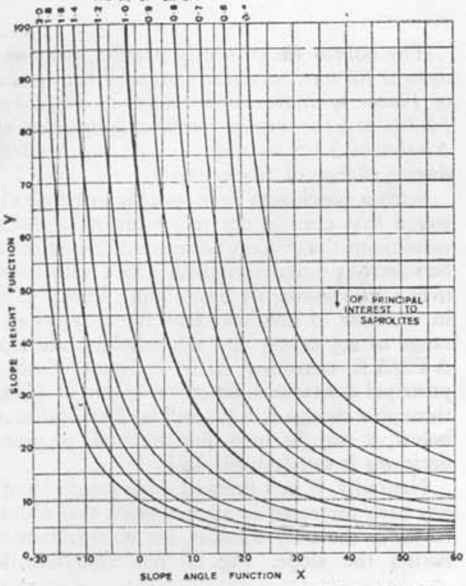


FIG. 13 SLOPE DESIGN CHART - CIRCULAR FAILURE (APUD HOEK 7, 8)

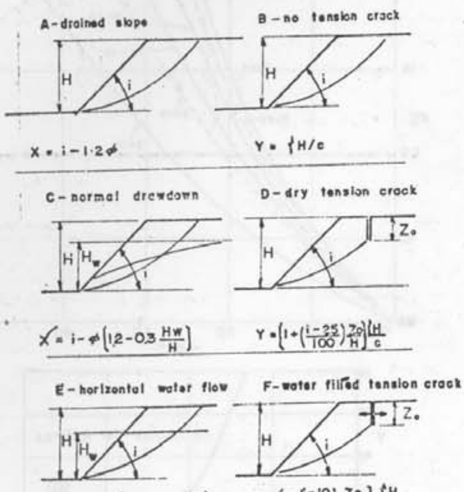


FIG. 14 TABLE TO ACCOMPANY SLOPE DESIGN CHART OF FIG. 13 - CIRCULAR FAILURE (APUD HOEK 7, 8)

may seem more appropriate for saprolite. The writer will avoid extending into the plane failure case, mainly because the principles once expounded are similarly applicable under other conditions, and partly because it is more difficult to generalize conclusions and charts which will depend on the inclination of the weakness plane discontinuities, and partly yet because the pseudocircular failure surface has in the writer's experience been very much more frequent in deeper slides than would appear at first thought.

Hoek's charts have been drawn up for slopes cut from horizontal topography, which in our case is found quite applicable to the gently sloping terrain derived from weathering and erosion in the basalts. Moreover, they are derived for a 'homogeneous' material of constant density and Mohr-Coulomb strength envelope: adjustments to more likely conditions will be discussed later. Let us consider, in the light of typical changes of conditions that are brought about affecting engineering works, what would be the charts most appropriate for interpreting past performance in the attempt to design by precedent.

Problem 1. Horizontal topography. *Drained slope, or cut above ground-water level and not subject to infiltrations.*

1.1 *Increase of depth of cut. Flattening of slope in compensation*

Since the Slope Height Function is $Y = H (\gamma / c)$, a change of height, ΔH, reflects in direct proportion as a change ΔY, and if we use a constant slope, we are interested in the change of Factor of Safety ΔFS with Y, at constant Slope Angle Function X.

Fig. 15 has been prepared for the purpose. Obviously the steeper the slope (higher X) and the smaller the corresponding Y value for a similar original FS (because of greater dependence on high c values) the more rapidly does the factor of safety change with a change of height ΔH.

The graph is not, however, very useful in practice, unless one possesses some reference failures that may help to pinpoint the value of $FS \approx 1$ as associated with a given height of a known cut slope. Since the estimate of φ values is not subject to much variation or error, and therefore X values may be expected to be pinpointed within $\pm 5°$ roughly, it will thereupon be possible to estimate Y and the weighted average cohesion within errors of roughly $\pm 25\%$ for most conditions.

If a given slope was so satisfactory that from its performance one cannot well estimate FS, it is natural that one should wish to increase the height H at constant slope i (and constant X). It can be seen (Fig. 15) that what matters principally in

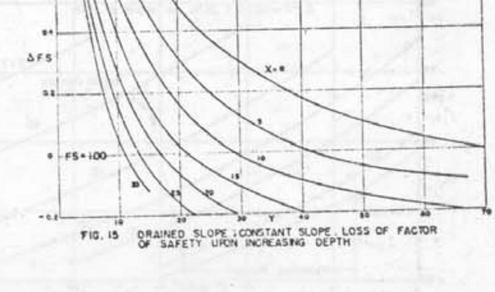


FIG. 15 DRAINED SLOPE: CONSTANT SLOPE. LOSS OF FACTOR OF SAFETY UPON INCREASING DEPTH

permitting a reasonably consistent interpretation of rates of change of FS with H is the ratio of slope heights H/Ho and thus Y/Yo. Thereupon, Fig. 16 has been prepared to indicate how closely one can judge the effects of such changes H/Ho, irrespective of a magnified presumed incapacity at estimating φ (and therefore X).

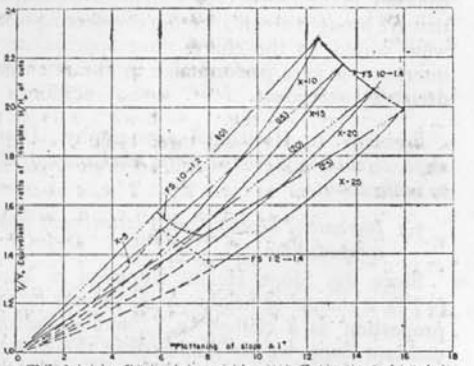


FIG. 16 Drained slope; Flattening of slope required for maintaining FS, at increasing ratio of depth of cut.

The chart has been prepared for a range of factors of safety $1{,}0 < FS < 1{,}4$ which is most applicable. And the possible error in estimating φ has been presumed within a range of roughly $\pm 6°$ leading to admissible Slope Angle Functions within the range $10 < X < 25$. The chart serves to indicate how much the slope will have to be flattened in order to recover the drop of FS due to increased depth H. It may be seen that the conclusion is *acceptably close to linear,* and may be expressed *as requiring a flattening of about 10 to 16°* (for $X \approx 10$ and $X \approx 25$ respectively) for a doubling of the height (or height function Y/Yo). In other words, a *20% increase of height requires for compensation,* in order to preserve the same factor of safety, *a flattening of the order of 3°.*

Fig. 17 has been prepared for another insight into the problem. The graphs represent, within the typical ranges of FS close to unity, the changes Δ FS with respect to changes of Slope Factor Δ X, for constant Y values. One important conclusion transpires at sight.

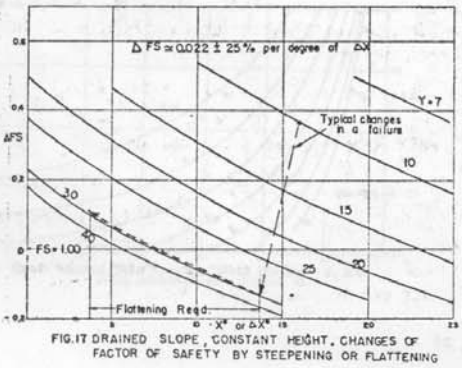


FIG. 17 DRAINED SLOPE, CONSTANT HEIGHT, CHANGES OF FACTOR OF SAFETY BY STEEPENING OR FLATTENING

The curves are so nearly parallel that one can assume for most practical purposes that the *change of Factor of Safety with change of Slope Factor (Δ FS/Δ X) is roughly constant, independent of Y.* A value of $\Delta FS \approx 0{,}022 \pm 25\%$ is suggested per degree change of Slope Factor.

Such a conclusion bears out the principle above stated that even under major errors of assumed conditions, the effects of changes of conditions may be relatively well determined. The Y values ranged over a ratio of about 1:6 signifying a possible error in estimate of cohesion (and thereby Y) in the range of 2,5 times; and nevertheless, the change Δ FS/Δ X remained fairly constant. Since the principal deleterious action on slopes is definable through a change Δ X, as will be further discussed below, it may be seen that the basis of practical decisions is satisfactorily laid.

Naturally, if in a drained slope the depth of cut has been increased to such a point that failure is reached, the only solution for stabilization is to flatten the slope. Fig. 18 has therefore been

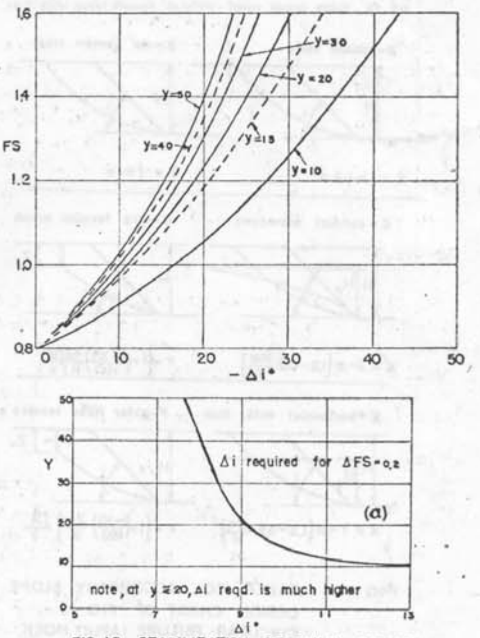


FIG. 18 STABILIZATION OF FAILED DRAINED SLOPE BY FLATTENING. FLATTENING REQUIRED FOR ΔFS=0.2

prepared to indicate at various values of Y what changes Δ FS can be achieved by flattening by angles Δ i. The curves are of greater interest close to FS = 1, and therefore the graph of Fig. 18a has been prepared to assess the flattening Δ i that will be necessary for an improvement of the factor of safety in the order of 0,2. It is concluded that for a range of variation as wide as $15 < Y < 50$ the flattening has to be of the order $10 > \Delta i > 7°.5$.

In a typical failure in residual soils it is accepted that the principal effect on the shear strength parameters consists in a sharp loss of 'cohesion' (11) which reflects in a proportional drop of Y. Meanwhile there is likely to be some change of φ value from the 'peak' to the 'ultimate' (20% strain in triaxial), and further to the 'pseudo-residual' or even 'residual': which lead to increases of X. Moreover, the very movements tend to decrease H (and Y), although generally very little, and tend to cause a natural flattening, or decrease of X, to a small degree. To accompany such probable changes, and thereby to determine what additional flattening ΔX to introduce to stabilize the failed slope, Fig. 17 may be used most conveniently, as is shown by the trajectory therein schematically superposed.

1.2 *Stable drained slope unstabilized by rise of W.L. (remote infiltration)*

Except for the frequent need to adjust on the strength parameters because of the effects of soaking principally on the apparent cohesion the problem can be treated as similar to the cases that follow (Problem 2), once the height of the water table is presumed known. The changes of FS with increased ratios of Hw/H can be extracted directly from Fig. 19 as will be shown forthwith.

The three questions that are of prime practical interest are believed to be: a) How much does the Factor of Safety drop as a function of the rise of water level Δ Hw? The case is of fixed height of cut H and of change of factor of safety as a result of changes in X only, $\Delta FS_{\Delta x}$. Since for the case of horizontal flowlines $X = i \# - \varphi\,[1{\cdot}2 - 0{\cdot}5\, H_w/H]$ it can be shown quite simply that

$$\Delta FS_{\Delta x} = \left(\frac{\delta FS}{\delta x}\right)(0{\cdot}5\,\varphi)\left(\frac{\Delta H_w}{H}\right) \ldots (1)$$

b) The second question is how much would one gain in FS of the slope by flattening it, once the rise of water level has caused a lowering of the initial stability. The gain can be simply obtained from $FS = \left(\frac{\delta FS}{\delta x}\right)\Delta i$, since all other factors remaining fixed, $\Delta X = \Delta i$.

c) Finally, one of the treatments to be assessed quantitatively is lowering the W.L. back by subhorizontal drains from the toe; the required Δ Hw for a desired Δ FS is computed directly from equation (1) above.

In these expressions the value of $\delta FS/\delta x$ has already been assessed as roughly $0{\cdot}022 \pm 25\%$ per degree of angle.

As will be further expatiated under Problem 2 below, for the case in which a drawdown hydrodynamic condition is assumed instead of the horizontal flowlines, expression (1) will have the (0,5 φ) factor changed to (0,3 φ) in accordance with Hoek (8).

1.3 *Unfavourable case of infiltration flownet within the failure wedge*

One of the critical cases to be considered is that in which heavy infiltrations just back of the slope are to set up a flownet towards the toe of the slope as shown in Fig. 20. The critical conditions would correspond to an impervious boundary starting from the level of the toe of the slope; two limiting theoretico-practical conditions are exemplified in the figure, (a) with the impervious bottom horizontal, and (b) with the impervious boundary (or lower flowline of interest) essentially accompanying the critical failure circle).

A comparison of the boundary neutral pressures of the two alternatives shows the difference between them to be of second-order.

Thereupon, the horizontal impervious bottom hypothesis was employed to assess conditions as obtain with various slopes, with such infiltration, as compared with Hoek's three basic cases, of a drained slope, slope with normal drawdown, and slope with horizontal water flow. The comparisons on the three flownet conditions were of necessity based on a groundwater elevation coincident with the original ground ($H_w = H$).

Pending the development of complete stability charts, it must be emphasized that the results are bound to be of great interest. For instance, the drop of factor of safety Δ FS due to the infiltration flownet in comparison with the drained slope is essentially constant, irrespective of the cut slope (35° to 65°). For slope angles up to about 45° the resultant boundary neutral forces on the failure plane are about midway between those on the horizontal flowlines and the normal drawdown case: therefore one might suggest temporarily estimating such stability problems from Hoek's chart using $X = i - 0{,}8\,\varphi$ since $H_w/H = 1$.

Interestingly, however, for steeper angles (e.g. 55°, 65°) the infiltration flownet yields about the same result as the very unfavourable horizontal flowline condition, $X = i - 0{,}7\,\varphi$, whereas the drawdown flownet becomes significantly more favourable.

The paramount importance of avoiding infiltrations close to the cut cannot be overemphasized.

Problem 2. Horizontal topography. *Cut slope going below the water level.*

Presumably the condition most current comprises a deepening of the excavation at a constant slope i, and it is of interest to determine the Δ FS as one goes to a depth $H = H_o + H_w$, wherein H_o is the depth to the water level and H_w the additional depth below the water table.

Above W.L. we get merely $\Delta FS_{\Delta Y}$, the change of FS as result of change in Y, because the slope Factor X has no factor depending on H.

Thus $\Delta FS = \Delta FS_{\Delta Y} = (\gamma/c)(\delta FS/\delta Y)\,\Delta H$

Below W.L. For going below W.L. it is necessary to subdivide into two cases, analogous to a very rapid excavation, and to an excavation slow enough to achieve drainage drawdown.

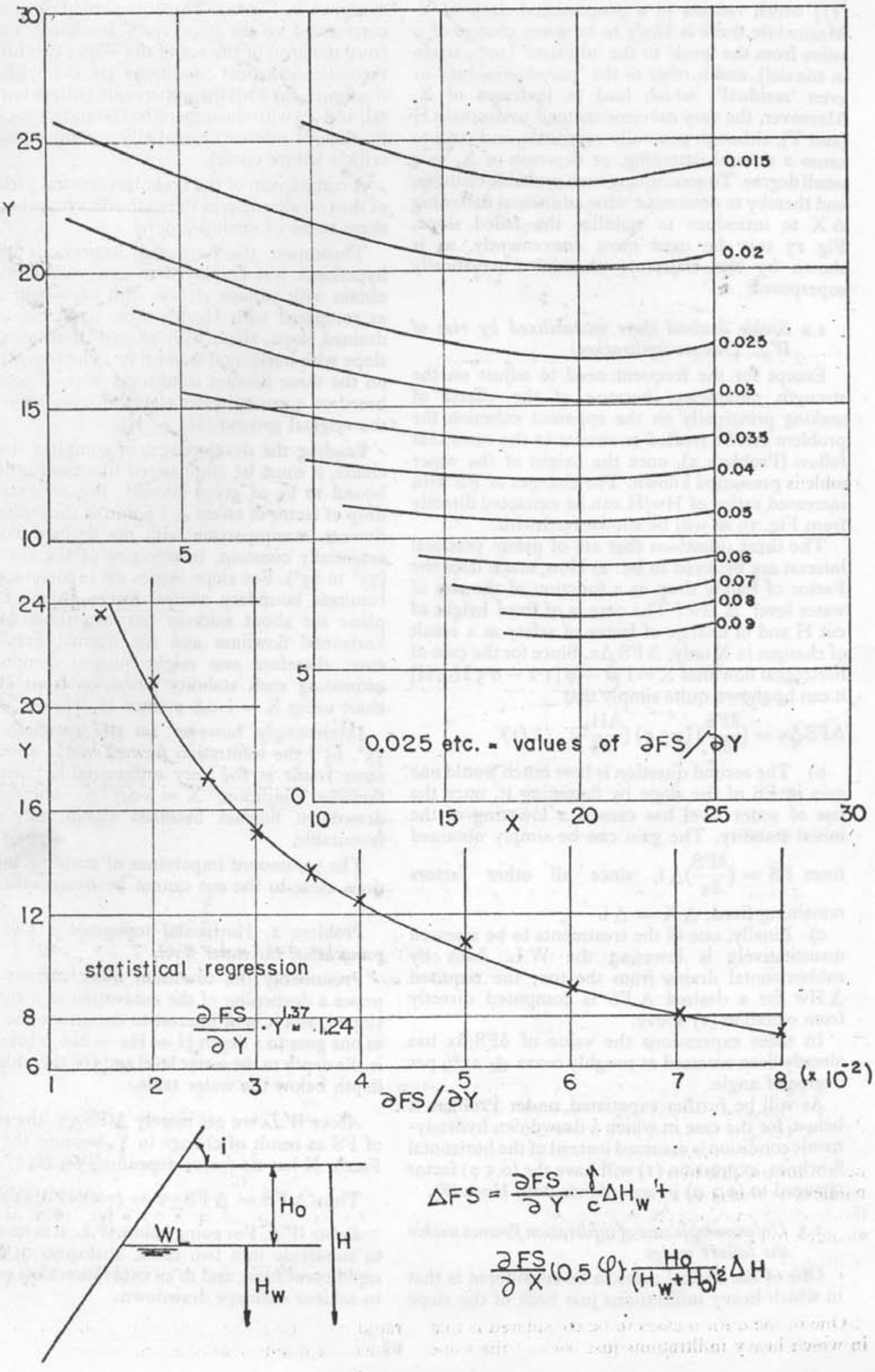


FIG. 19 DEEPENING CUT AT CONSTANT i BELOW WL. VALUES OF $(\partial FS/\partial Y)$ NEEDED FOR COMPUTING OVERALL ΔFS

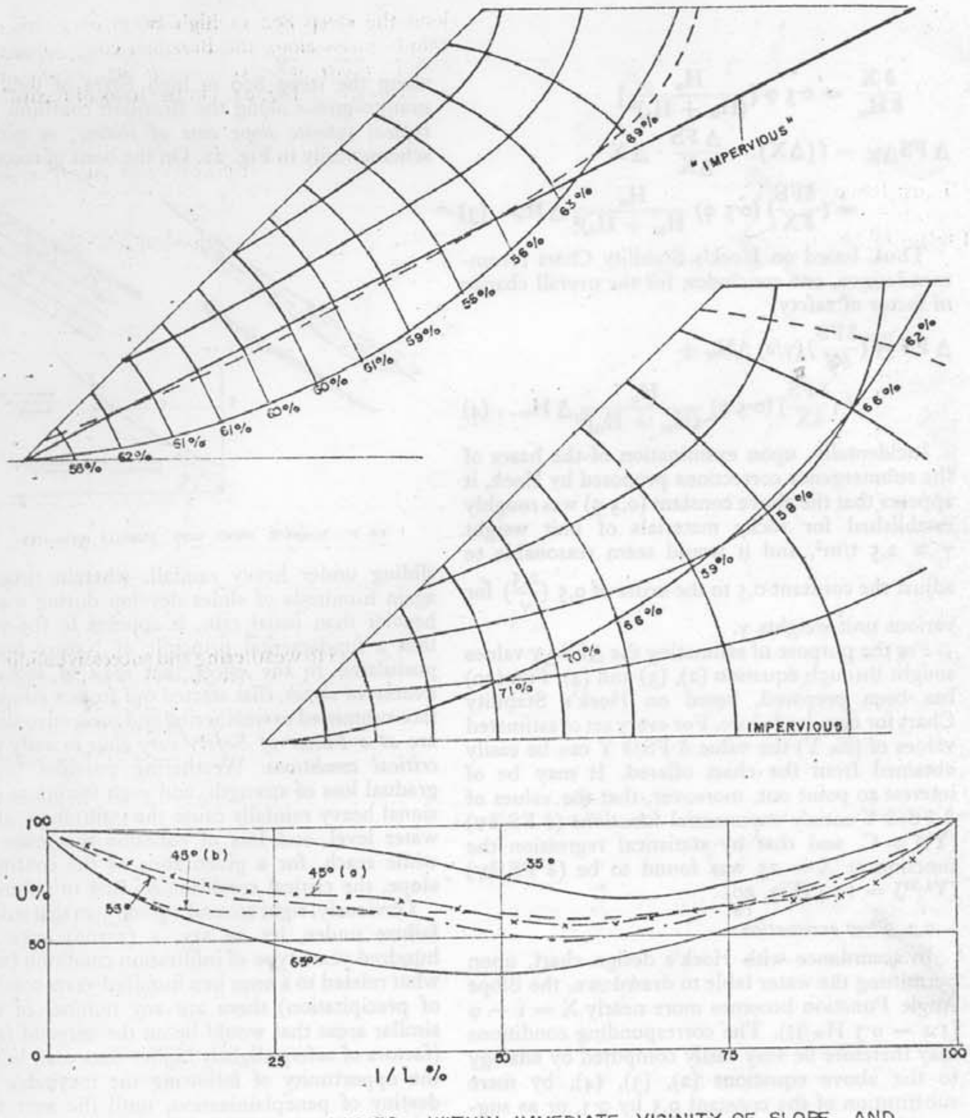


FIG. 20 INFILTRATION FLOWNETS WITHIN IMMEDIATE VICINITY OF SLOPE, AND BOUNDARY NEUTRAL FORCES COMPARED WITH HORIZONTAL FLOWLINE CASE.

2.1 *Rapid excavation*

Essentially equivalent to an extreme case in which highly preferential horizontal flow maintains a nearly horizontal set of flowlines.

$$FS = f(x,y)$$

$$\Delta FS = \frac{\delta FS}{\delta x}\, dx + \frac{\delta FS}{\delta y}\, dy$$

$$\text{but } Y = \frac{\gamma}{c} H = \frac{\gamma}{c}(1 + H_w/H_o)\, H_o$$

$$\frac{\delta Y}{\delta (H_w/H_o)} = \frac{\gamma}{c} H_o \text{ and } \Delta Y = \frac{\gamma}{c} \Delta H_w$$

$$FS = f(Y) \quad \therefore \quad FS = f(\Delta Y)$$

$$\Delta FS_{\Delta Y} = \frac{\Delta FS}{\Delta Y} \cdot \Delta Y = \left(\frac{\delta FS}{\delta Y}\right)(\gamma/c)\Delta H_w \; \ldots \; (2)$$

Similarly, by use of Hoek's (8) recommended X value equation, $X = i - \varphi\left[1{\cdot}2 - 0{\cdot}5 \dfrac{H_w}{H_o + H_w}\right]$.

$$\frac{\delta X}{\delta H_w} = 0{\cdot}5\ \varphi \left[\frac{H_o}{(H_w + H_o)^2}\right]$$

$$\Delta FS_{\Delta X} = f(\Delta X) = \frac{\Delta FS}{\Delta X} \cdot \Delta X$$

$$= \left(\frac{\delta FS}{\delta X}\right)(0{\cdot}5\ \varphi)\ \frac{H_o}{H_w + H_o)^2}\ \Delta H_w \ldots (3)$$

Thus, based on Hoek's Stability Chart recommendations, one concludes, for the overall change of factor of safety

$$\Delta FS = \left(\frac{\delta FS}{\delta Y}\right)(\gamma/c)\ \Delta H_w +$$

$$\left(\frac{\delta FS}{\delta X}\right)(0{\cdot}5\ \varphi)\ \frac{H_o}{(H_w + H_o)^2}\ \Delta H_w \ldots (4)$$

Incidentally, upon examination of the bases of the submergence corrections proposed by Hoek, it appears that the above constant (0,5 φ) was roughly established for rocky materials of unit weight $\gamma \approx 2{,}5$ t/m^3, and it would seem reasonable to adjust the constant 0,5 to the order of 0,5 $\left(\frac{2{,}5}{\gamma}\right)$ for various unit weights γ.

For the purpose of estimating the $\Delta FS_{\Delta Y}$ values sought through equation (2), (3) and (4), Fig. (20) has been prepared, based on Hoek's Stability Chart for circular failure. For every set of estimated values of (X, Y) the value δ FS/δ Y can be easily obtained from the chart offered. It may be of interest to point out, moreover, that the values of δ FS/δ Y satisfy exponential functions (δ FS/δY) $(Y)^n = C$, and that by statistical regression the function at $X = 25$ was found to be (δ FS/δY) $(Y^{1.37}) = 1{\cdot}24$ (Fig. 20).

2.2 *Slow excavation*

In accordance with Hoek's design chart, upon permitting the water table to drawdown, the Slope Angle Function becomes more nearly $X = i - \varphi$ $(1{\cdot}2 - 0{\cdot}3\ H_W/H)$. The corresponding conditions may therefore be very easily computed by analogy to the above equations (2), (3), (4), by mere substitution of the constant 0,5 by 0·3, or as suggested as possibly more appropriate, $1{\cdot}25/\gamma$ by $0{\cdot}75/\gamma$.

It has been repeatedly pointed out that one of the factors most frequently responsible for unstabilizing cut slopes is the speed at which modern excavation and hauling equipment deepens the excavation, with no time for pore pressure adjustments, and especially so when proceeding below groundwater, creating conditions similar to those of rapid drawdown of upstream faces of earth dams.

Problem 3. ***Problems of infinite slopes and of cut slopes within a natural slope.***

3.1 *Infinite slope stability postulates.*
Flownet parallel to ground surface

One of the conditions that has been found to be representative of the greatest number of slides along the steep 800 m high Serra of weathered granito-gneiss along the Brazilian coastline is the *typical infinite slope case of sliding*, as pictured schematically in Fig. 21. On the basis of records of

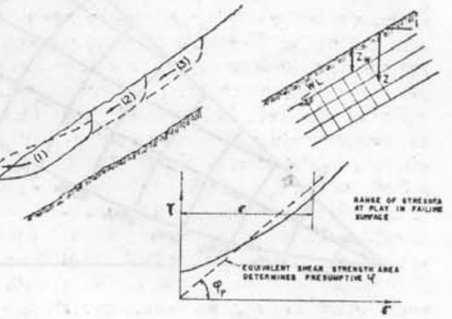


FIG. 21 SCHEMATIC INFINITE SLOPE STABILITY POSTULATES

sliding under heavy rainfall, wherein time and again hundreds of slides develop during a single heavier than usual rain, it appears to the writer that a fundamental principle of Nature may be postulated to the effect that *most of such steep weathered slopes*, that started out from a steep rock face submitted to weathering and successive sliding, are *at a Factor of Safety very close to unity under critical conditions.* Weathering provides for the gradual loss of strength, and such factors as occasional heavy rainfalls cause the infiltration, rise of water level, and loss of cohesion that once in a while reach, for a given zone of the continuous slope, the critical condition leading to failure.

Obviously, right around a given area that suffered failure under, let us say, a (1:100) once in a hundred years type of infiltration condition (somewhat related to a once in a hundred years condition of precipitation) there are any number of other similar areas that would be on the verge of failing (factors of safety slightly higher than one) but lost the opportunity of following the inevitable final destiny of peneplainization, until the next 1:100 rain occurs when the strength has been slightly more degraded, or a slightly higher infiltration condition, say 1:200 or 1:500 years, chances to occur. Truly the real picture is very complex, including statistical recurrences of a great many factors besides rainfall, such as rainfall continuity and intensity, seismic activity, etc, as superposed on the continuous long duration phenomena such as weathering, the gradual opening of cracks after prolonged surface creep movements or drying shrinkage, slow or rapid changes of surface cover, etc. But the principles applicable are exactly the same, and will herein be considered with reference exclusively to the rise of the water table due to infiltration, because in the case under configuration that is by far the dominant deleterious factor.

As a first step therefore, in such a geologic and hydrologic setting, it is suggested that representative natural slopes be analyzed for the purpose of determining weighted average shear strength parameters at play, upon assuming reasonably probable factors of safety 1,0 $\gtrsim$ FS $\gtrsim$ 1.2 on the slopes duly inspected. Obviously if there are slopes that have arrived at the condition FS = 1 within the past few years, an infinitesimal increment of geologic time, there must be quite a few other slopes, presently at FS $\ngtr$ 1, awaiting their geologic time to be ripe. The writer has often encountered the attitude, so apparently obvious as to be most obviously false, that the steeper the slope the smaller its factor of safety: and it would seem unnecessary to have to refute it. Most often along our mountain range it is the steeper slopes that may be more stable, involving the sounder virgin material that remains under a slide scar, whereas the flatter terrain usually signifies talus that is obviously at about FS = 1 under high W.L. conditions.

In short therefore, one is faced with three preliminary steps in interpreting existing natural slopes in the weathered material. First, *what approximate FS to attribute to the slope* under its probable critical conditions within short-term history. Secondly, *what critical unstabilizing conditions (e.g. elevation of groundwater table) to assume* to have prevailed associated with the FS = 1 of the slope in its short-term history. Thirdly, thereupon, *what presumptive shear strength parameters to extract from the above two hypotheses,* not for use per se in different engineering problems, but for closed-cycle analyses of changes of slope stability factors with imposed changed conditions for the self-same slope. Thereupon hangs a *fundamental design principle* that the writer recommends for prediction and decision on slope stability in residual soils.

As regards the assumptions on FS the writer, at the risk of appearing facetious and/or infantile, recommends nothing more sophisticated than the *observations of movements,* or of appearances of movements (25). The indications of inclined or upright trees (Figs. 22, 23) are as old as the hills (e.g. Taylor 1948, et omnia alia): the writer has used such indications with relative statistical success, since no slope gets close to FS = 1 without movements and inclinations of trees: and

Fig. 22 Surface inclinometers (trees) indicating slope instability in residual profiles

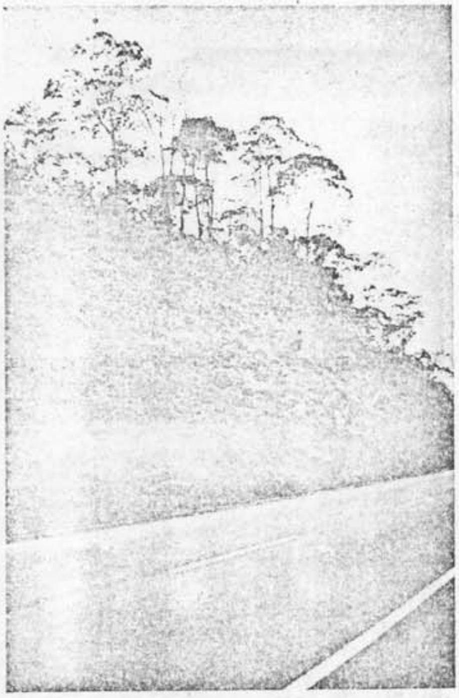


Fig. 23 Upright trees indicating slope not yet geologically ripened to factor of safety of one

no slope can reach FS $\approx$ 1 under a 1:100 infiltration without approaching, say FS $\approx$ 1,05 (and accompanying movements) under several 1:10 infiltrations, FS $\approx$ 1,10 under more frequent 1:1 infiltrations, and so on, in the decades of history recorded by trees. Note, however, that some discretion must be exercised in choice of trees for satisfactory indications, since some trees lean at the least provocation (wind, lebensraum, etc . . .) and most factors trend cumulatively towards downslope without thereby signifying instability of the slope (cf. Fig. 24).

Secondly, as regards the assumptions on C and φ it has already been stated that applicable φ values should be estimatable within a $\pm$ 5° error, and thereupon from the overall stability presumed, a likely presumptive C value can be determined. Without any difficulty three or more sets of possible (C, φ) values can be extracted, satisfying the assumed factor of safety. At any rate, as has been repeatedly demonstrated, most conclusions on changes of conditions are not so heavily dependent on the 'correct' choice of the presumptive (C, φ) set of values.

Finally, with respect to the water table and groundwater flow conditions taken to have been critical to the slope, an assumption has to be made. It has been stated (cf. Deere and Patton, 6) that 'It is the sequence of a low strength, low permeability zone overlying a high permeability zone,

Fig. 24 Type of tree must be chosen judiciously. Note upright large tree and inclined small trees

both of which are lying subparallel to the ground surface, that is the main unifying aspect of stability problems of slopes in residual soils' and '. . . the piezometric level of the water in the permeable zone of the weathered rock can extend above the ground surface'. Indeed there is no limitation as to what can happen: but the writer cannot find in his records or in his concept of the problem any evidence to ratify such assumptions as the general case. Along an infinite slope, failure does not occur merely or predominantly at the base as presumed by Deere and Patton's assumed artesian condition; anywhere along the slope a combination of slightly more unfavourable condition achieves the trigger failure. If the upper mature horizon is more impervious, it is so both upslope of the potential slide, where imperviousness is favourable by decreasing infiltration, and downslope of the slide, where such imperviousness would be unfavourable, by forcing artesian conditions. Or, if it is more pervious (even setting aside the weathered subsoil continuity with depth, for the purpose of argument), it would be so both upslope and downslope. A phenomenon that occurs indiscriminately over hundreds of areas along the infinite slopes is not likely to be due to artesian conditions. Instead, the writer proposes two basic postulates as more generally applicable: one, *that seepage within the mature and residual soil horizons is predominantly parallel to the infinite slope*; the other, *that the depth to groundwater surface varies with infiltration,* and *the critical condition will frequently correspond to the groundwater level rising to the ground surface.*

Under such hypotheses some basic parameters can be quickly established from infinite slope Mohr circle relationships. The stress obliquity

$$\tan \alpha = \frac{\tan \varphi\, p}{FS} = \left(\frac{\gamma}{\gamma - \gamma_w}\right) \tan i$$

from which one determines the weighted presumptive φ p

$$\tan \varphi\, p = FS_o \left(\frac{\gamma}{\gamma - \gamma_w}\right) \tan i$$

and, as shown in Fig. 21 if the range of normal stresses is evaluated from the assumed depth of slide, the average strength tan φ p can be redistributed into compatible sets of (c, φ) values.

The φ most likely to be applicable is the drained one, except in rather special cases in which an unusually rapid sliding movement anticipated and a relatively sensitive strain-softening stress-strain curve might justify development of rapid incremental pore pressures due to shear, in which case some anisotropic consolidated-undrained value would be more appropriate. The writer has recently witnessed an explosive slide of about 500,000 m^3 that was evidently associated with liquefaction, but it may be affirmed that such a case represents an unusual occurrence, of frequency far less than 1 in 100 of the cases faced.

Thereupon one must analyze the general case, in which the water table, parallel to the slope, occurs temporarily at a depth Z_w. One is now interested in examining new values of factor of safety FS_z at depths Z, in comparison with the assumed critical factor of safety FS_o of the slope with groundwater surface along the slope.

Above W.L. $\tan \alpha = \tan i$

$$\therefore\ FS_z = \frac{\tan \varphi\, p}{\tan \alpha} = FS_o \left(\frac{\gamma}{\gamma - \gamma_w}\right)$$

Below W.L. new obliquities are introduced because of change of effective overburden stress

$$\tan \alpha_z = \frac{1}{[1 - \gamma_w/\gamma + \gamma_w Z_w/\gamma_z]} \tan i$$

thereby one obtains $\dfrac{FS_z}{FS_o} = 1 + \dfrac{\gamma_w Z_w}{(\gamma - \gamma_w)Z}$

Fig. 25 has been prepared to indicate the variation of factor of safety FS_z with ratios of Z_w/Z for different values of unit weight γ of the soil, under the temporary assumption of a constant presumptive φ p value. Because of the hypothesis of a constant φ p value this graph is only applicable to the investigation of the variation of FS at a given hypothetical failure surface and depth Z (to be roughly estimated as shown in Fig. 27), as the water level changes (affecting Z_w and thus Z_w/Z values at constant Z).

The variation of unit weight of the soil with depth does have an effect somewhat greater than

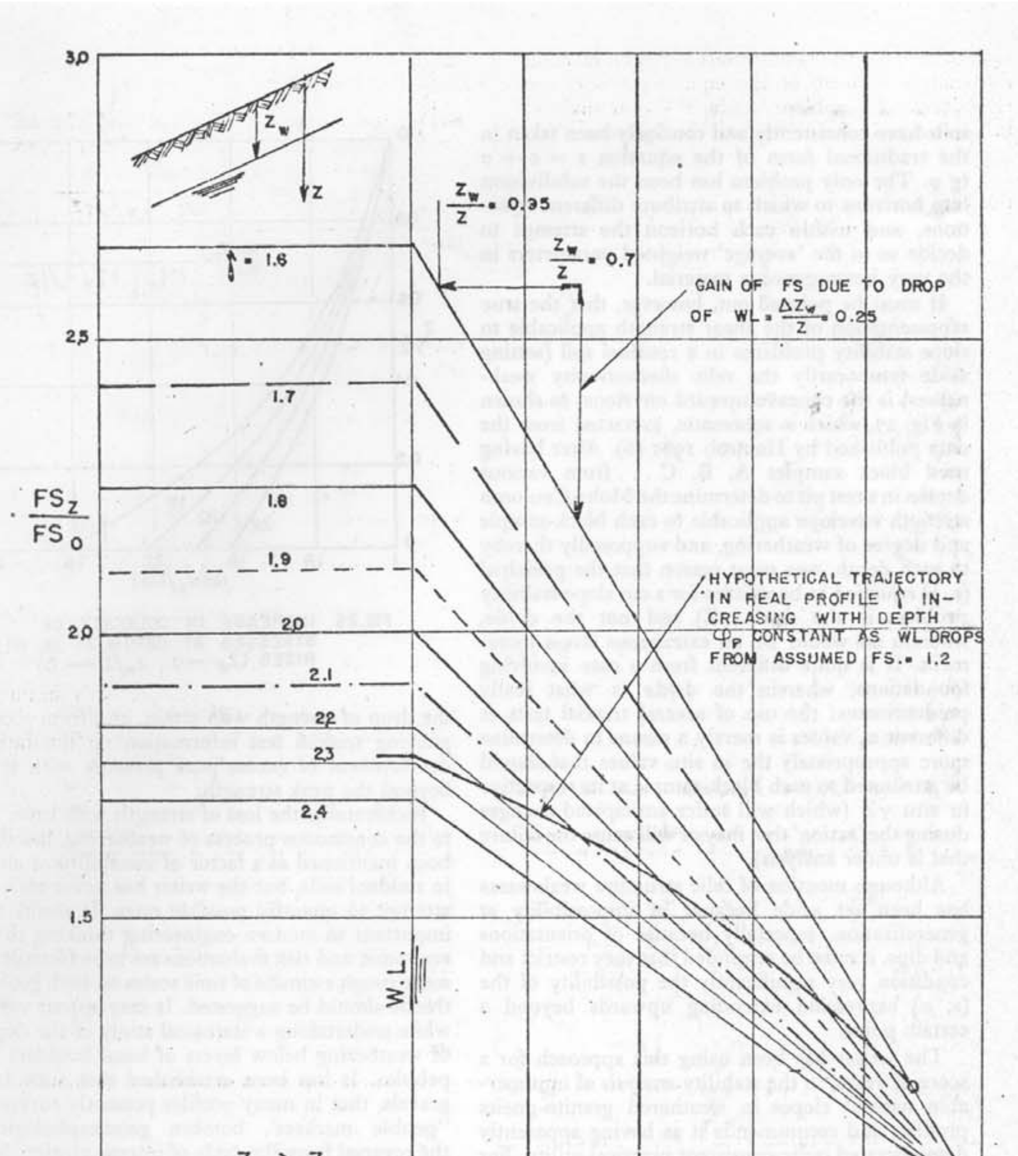


FIG. 25 INFINITE SLOPE VARIATION OF FS AT HYPOTHETICAL FAILURE SURFACE AT DEPTH Z CONSTANT, AS W.L. DEPTH Z_W CHANGES

would be suspected at first thought. A hypothetical trajectory has been included to show how one can easily adapt the use of the graph to the case of γ varying with depth, as is typical of weathered profiles. However, the more significant variation should be that of φ p (Fig. 27).

Since one of the principal concerns in stabilizing slopes is lowering W.L. (through providing impervious cover to cut infiltration, and introducing drainage from the toe or from tunnels) it is of interest to point out that the graphs permit one to extract directly (as shown in Fig. 25) for a given Z_W/Z change of WL condition, the FS_Z/FS_o change of condition of factor of safety.

3.2 *Adjustments on realistic strength vs. depth representation*

The shear strength equations applied to residual soils have consistently and routinely been taken in the traditional form of the equation s = c + σ tg φ. The only problem has been the subdivision into horizons to which to attribute different equations, and within each horizon the attempt to decide as to the 'average' weighted parameters in the very heterogeneous material.

It must be pointed out, however, that the true representation of the shear strength applicable to slope stability problems in a residual soil (setting aside temporarily the relic discontinuity weaknesses) is the concave upward envelope, as shown in Fig. 27, which is schematic, extracted from the data published by Hamrol, 1961 (6). After having used block samples A, B, C . . . from various depths in a test pit to determine the Mohr-Coulomb strength envelope applicable to each block-sample and degree of weathering, and supposedly thereby to each depth, one must reason that the principal (s, σ) equation to be written for a cut slope stability problem is the ds/d (γZ) and not the ds/dσ, wherein dσ would be an extraneous stress increment. It is quite different from a case involving foundations, wherein the ds/dσ is what really predominates: the use of several triaxial tests at different σ_3 values is merely a means to determine more appropriately the in situ values that should be attributed to each block-sample at its respective in situ γZ (which will suffer anticipated changes during the 'action' that may or will cause the failure that is under analysis).

Although mention of relic structure weaknesses has been set aside because of impossibility at generalization, especially because of orientations and dips, it must be reminded that they restrict and condition very significantly the possibility of the (s, σ) expression extending upwards beyond a certain point.

The writer has been using this approach for a score of years, in the stability analysis of innumerable natural slopes in weathered granito-gneiss profiles, and recommends it as having apparently demonstrated quite consistent practical utility. For instance, it allows one to estimate the most likely depth (and consequent probable volume) of slide that may develop due to the rise of water table upon increased infiltrations. Using the graphs from Fig. 26 one can plot the obliquity of stresses as they increase from the tan i of the drained slope, upon assumed incremental rises of the water table. The condition of tangency to the shear strength envelope would simultaneously indicate roughly how much of a rise of water table would cause failure, and at roughly what depth the critical obliquity would occur. Fig. 27 exemplifies a case. Relying on the estimate of most likely depth of slide as above suggested, one may establish a rough probable volume of slide debris, as is required for the design principle of preparing to face possible failures (22). As regards the possible speed of sliding, only very rough indications can be obtained, from the stress-strain curve, from a Brittleness Index (3) indicating

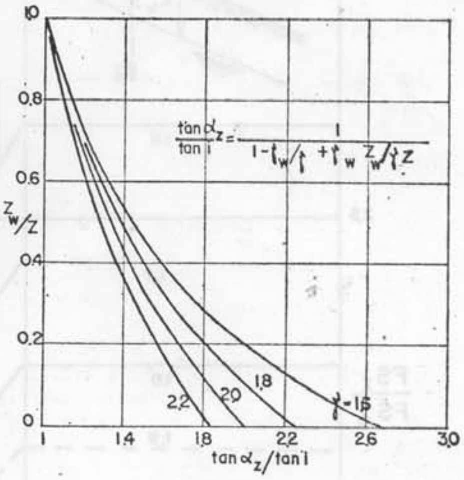


FIG. 26 INCREASE IN OBLIQUITY OF STRESSES AT DEPTH Z AS WL RISES ($Z_w \rightarrow 0$, $Z_w/Z \rightarrow 0$)

the drop of strength with strain, and from accompanying triaxial test information on the rate of development of excess pore pressure with strain beyond the peak strength.

Incidentally, the loss of strength with time, due to the continuous process of weathering, has often been mentioned as a factor of instability of slopes in residual soils, but the writer has never seen any attempt to quantify possible rates. It seems very important to modern engineering thinking that if economic and risk evaluations are to be formulated, some rough estimate of time scales on such geologic trends should be suggested. It may appear worthwhile undertaking a statistical study of the depths of weathering below layers of basal boulders and pebbles. It has been established that such basal gravels, that in many profiles presently survive as "pebble markers', betoken geomorphologically the reversal from the cycle of intense erosion down to sound bedrock, to the first-phase of depositions from flow velocities gradually dropping. Since the geologic era or epoch when the significant change of hydrologic conditions occurred may frequently be dated, the time required to develop the depth of weathering to present bedrock level may be estimated. If enough cases are collected for statistical analysis, the time law (probably exponential) for best fit may be tested, and thereupon some first order estimate of the progress of weathering during the life of the engineering project may be roughly obtained. The loss of strength could be estimated on the basis of an extrapolation of the strength vs. depth axis (Fig. 27) to the left of the zero depth axis.

3.3 *Cut slope within a natural slope*

This is indeed a case of interest to a great majority of practical problems, such as have been

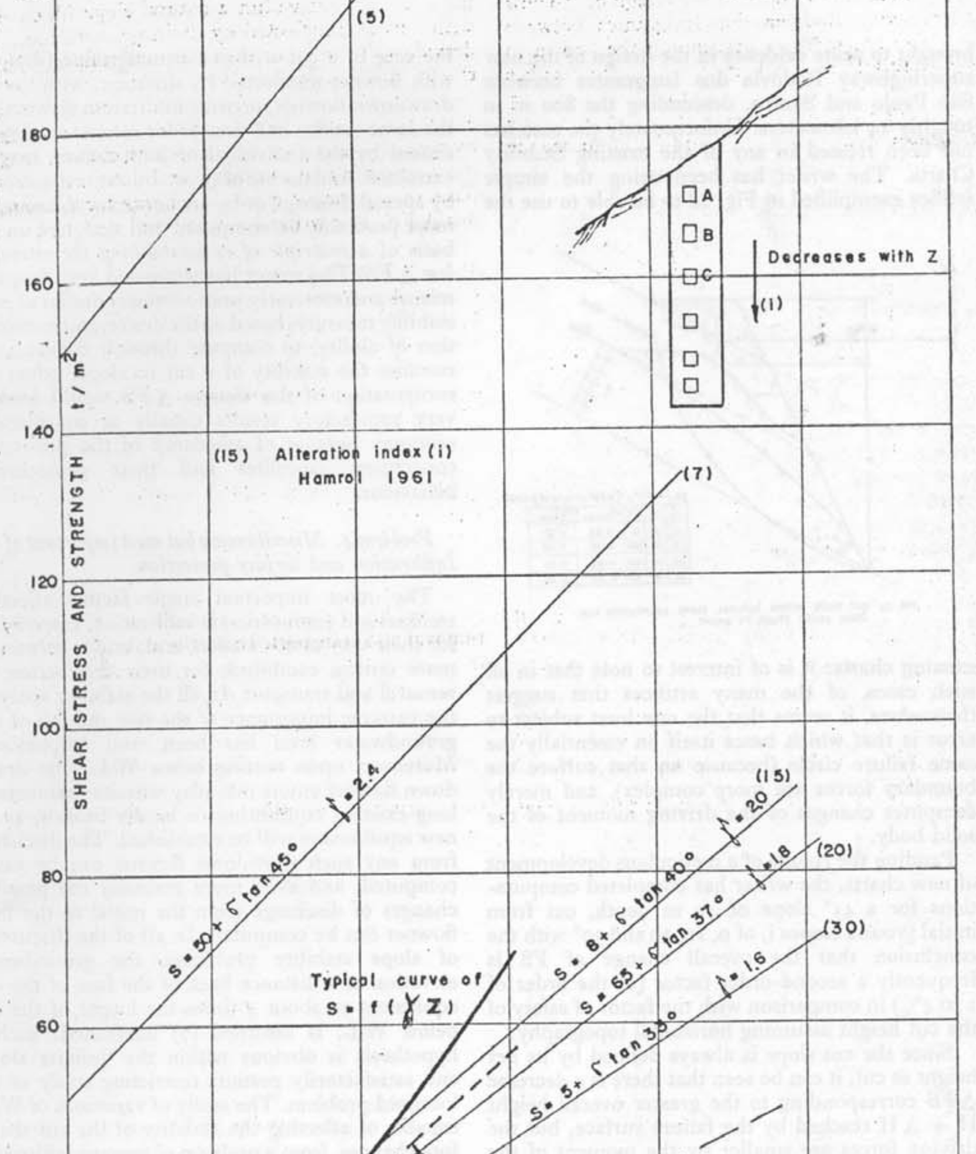
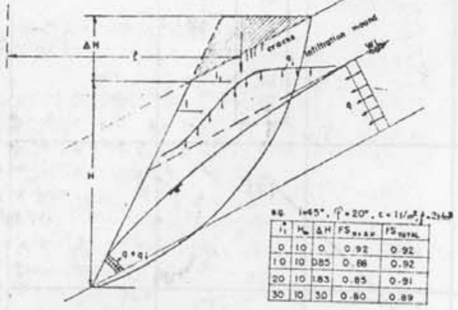


FIG. 27 APPROPRIATE REPRESENTATION OF STRENGTH EQUATION S = f(σ, Z) IN TYPICAL SAPROLITE PROFILE AND INDICATION ON SLIDE DEPTH AND VOLUME.

brought to acute evidence in the design of the new superhighway Rodovia dos Imigrantes between São Paulo and Santos, descending the 800 m in roughly 14 kilometers. Unfortunately the case has not been treated in any of the existing Stability Charts. The writer has been using the simple artifice exemplified in Fig. 28 to be able to use the



i_1	H_m	ΔH	$FS_{H+\Delta H}$	FS_{TOTAL}
0	10	0	0.92	0.92
10	10	0.85	0.88	0.92
20	10	1.83	0.85	0.91
30	10	3.0	0.80	0.89

FIG. 28 CUT SLOPE WITHIN NATURAL SLOPE. ADAPTATION FOR USING HOEK'S STABILITY CHART

existing charts: it is of interest to note that in all such cases, of the many artifices that suggest themselves, it seems that the one least subject to error is that which bases itself on essentially the same failure circle (because on that surface the boundary forces are more complex), and merely computes changes of the driving moment of the solid body.

Pending the results of a meticulous development of new charts, the writer has completed computations for a 45° slope of 10 m depth, cut from initial ground slopes i, of 0, 10, 20 and 30° with the conclusion that the overall change of FS is frequently a second-order factor (of the order of 1 to 4%) in comparison with the factor of safety of the cut height assuming horizontal topography.

Since the cut slope is always defined by its net height as cut, it can be seen that there is a decrease Δ FS corresponding to the greater overall height H + Δ H reached by the failure surface, but the driving forces are smaller by the moment of the non-existent triangle of soil above the crest of the cut, and therefore the final decrease Δ FS is somewhat smaller than by working with the overall height of the failure circle and hypothetical horizontal topography.

In the same Figure the phreatic line of a typical drawdown flownet has been sketched-in, to show that in an infinite slope with groundwater table originally parallel to the slope, if the excavation is not too rapid, the very process of cutting a steeper slope within a slope can tend to lower the groundwater immediately behind the cut, thereby permitting the steeper slope without a significant decrease Δ FS.

All of the cases above treated with respect to cuts from horizontal topography can be adapted to the case of a cut within a natural slope (drained, with flownet unaffected by drainage, with normal drawdown flownet, or with infiltration flownet) by the same artifice and first-order estimates of Δ FS caused by the individual or joint factors may be extracted. And the use of any stabilization treatment, by special drainage or by anchor ties, etc, can much more profitably be computed and designed on the basis of a *principle of re-establishing the estimated loss Δ FS*. The writer has witnessed very frequent misuse and extremely uneconomical design of such stability measures based on the desire, and presumption of ability, to compute through conventional routines the stability of a cut or slope, when the computation of the change Δ FS would lead to very satisfactory results usually at considerable economy because of avoidance of the pessimism concerning saprolites and their geotechnical behaviour.

Problem 4. Miscellaneous but most important of all. Infiltration and surface protection.

The most important single factor affecting residual soil engineering is infiltration, responsible for their very slow creation, and, under occasional more critical conditions for their destruction by removal and transport. In all the stability analyses the extreme importance of the rise and fall of the groundwater level has been well emphasized. Moreover, upon cutting below W.L., the drawdown flownet enters into play with its drainages: a long-existing equilibrium is locally broken, and a new equilibrium will be established. The discharge from any such drawdown flownet can be easily computed, and even more precisely the possible changes of discharge from the initial to the final flownet can be computed. In all of the discussion of slope stability problems, the groundwater elevation at a distance back of the face of the cut equivalent to about 4 times the height of the cut below W.L. is assumed (7) unaffected: such a hypothesis is obvious within the 'infinite slope' and satisfactorily permits restricting study to the localized problem. The study of variations of W.L. capable of affecting the stability of the cut therefore changes, from a problem of average infiltration rates on an infinite slope and corresponding rise of W.L. to accommodate the new equilibrium of increased outflow equal to increased inflow, to a problem of creation of an 'infiltration mound' within the immediate vicinity.

Theoretical analyses are available for the problem (e.g. 1), but absolutely no worthwhile data have been located that may presently permit design estimates. The subject appears to be of interest to more than one specialized professional field (e.g. hydrology, agronomy, geohydrology, soil mechanics) and possibly for that very reason has not come under the focus of attention of any of them.

The decision principles are clear and very repeatedly cited in qualitative terms. It is important to eliminate or reduce infiltrations. Not only on a cut slope but emphatically also *immediately back of its crest*. This is more accentuatedly true in residual soils, derived from infiltrations. And it can also be easily understood why despite the Soil Mechanician's demonstration that flatter slopes are stabler, it is often true in residual soils that the Civil Engineer will observe that by flattening a cut beyond a certain degree one creates new problems of shallow sliding if infiltration protection is deficient.

As regards design principles on risks of failure of slopes, it is also obvious that some frequency of recurrence of critical infiltrations is at stake. Horton showed that the infiltration versus time curve is of the exhaustion type, that approaches a constant value, usually after one to three hours. What is needed, therefore, and badly so before any rational design studies can be attempted in this connection, is a systematic set of infiltrometer tests to determine infiltration rates with various inclinations and covers.

Incidentally, it must be emphasized that the Civil Engineer's task is quite general and specializations can only be tackled correctly after due appraisal of the overall problem. As an example, mention may be made of the question of surface cover of cut slopes for simultaneous protection against infiltration and erosion. On steeper slopes, on which no grass cover will take hold, it has been observed that a natural millimetric growth both of cementitious agents and of lower plant life develops as a very efficient protective cover: studies are being undertaken to determine what the chemical and biochemical concentration and growth is, so as to evaluate how to foster (by humidity, fertilization spraying, etc) such a natural and cheap solution. Meanwhile, it has been observed that the principal problem with drainage ditches on berms (designed to intercept the runoff flows before velocities become erosive) has been the maintenance requirement of cleaning them of deposits of eroded debris; a recent successful design principle is obviating the problem by dimensioning the hydraulics of the ditches such that runoffs of about once or twice a year will achieve velocities capable of maintaining the ditches self-washed.

All of the design principles and procedures above summarized under the captions of 5·2 Problems 1 through 4, have been in development and use by the writer in his consulting activity on many of the major superhighway and railroad projects under construction in Brazil.

6. *Conclusions*

It has been the aim of the presentation, to catalyze new thoughts and action in connection with soil engineering in residual soils.

The writer feels that in some respects there is a significant distinction between the approaches which through natural historical factors arose in tackling 'conventional soil mechanics' and the problems of residual soils. Civil Engineers of broad and brilliant perspective created Soil Mechanics approaching it from Civil Engineering needs, and developing the specialization as a means to tend to those needs. Subsequently it has happened that Soil Mechanics Specialists have been trying to approach civil engineering needs in residual soils in the reverse direction from the specialist to the generalist: thereby one has run the risk of attempting to cut the customer to the Procrustean bed, losing sight of what are the aims and which are merely the means, as happens when tests and theories are pre-established, and the use of them is subsequently forced into adaptations to assumed realities.

Repeating a statement that belonged to the first paragraphs, the writer proposes no certainties and no proven solutions, except the exhilarating conviction that the field of residual soil engineering is wide open to thinking and action.

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