Rates of Consolidation Settlements Affecting Acceptable Building Performances

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Abstract : The problems of settlements of buildings in Santos, Brazil, are used to summarize some of the many complex important unknowns for theoretical predictions of nominal "final settlements" and time- settlement curves, for decisions of professional practice. Considerable programs of research would help, but only if directed towards statistical treatments yielding % Confidence Intervals CIs on successive intervening parameters, including corresponding comparisons under different sampling, testing, interpreting practices in vogue. The CIs point to needed Factors of Prudence FPs, for maximizing causes and maximum settlements, while concomitantly minimizing resisting factors and minimum settlements. It seems indispensable to use field loadings and Bayesian retrofitting procedures for future buildings: but the crucial question remains of designing the features prepared for adjustments, if settlements, total and differential, begin to exceed acceptable values. For existing buildings with significant settlements already suffered, creative foundation adjustments include joint use of recognized concepts of precompression and partial floating. Many theoretical and practical obstacles suggest shying away from piezometric point monitoring of prototypes, as appears inviting from improved consolidation theorizing and C_v determinations.

1. Introduction

The subject of saturated clay **consolidation,** and of so-called **secondary compressions** constitutes an important threshold and conceptual example, both because it was the baptismal topic of Terzaghian pioneering "effective stresses", and because it exemplifies how engineering solutions backed by scientific research and mathematical derivations have to undergo progressive adjustments, and also because it alerts us to dangers presently assailing geotechnique. On the one hand there is *the great proportion of professionals that have* clung to the erstwhile solutions as definitively correct, *philosophically an untenable premise.* On the other hand there are the practitioners who are satisfied with "prescriptive solutions" without realizing how far they may be gradually consuming the tolerance given by Factors of Safety. And in yet another sector there are the concerned academic bifurcations that have *swerved from the professional end-product, and* principally fall prey to mathematical idealizations *difficult to confirm or refute.*

So great was the impact of first-order successes of the mechanistically-rheological mathematical formulation of **primary consolidation,** and of consequent settlement analyses, that it produced a noticeable inertia regarding needed research and development, on discrepancies and statistical dispersions and **Confidence Intervals, CI.** Simultaneously there were also some points of logic, and of "observ(ed)(able) reality" vs. mathematical equations, that suffered biases not yet sufficiently debated, or deleted. As a result there arises a strong pragmatic undercurrent of building investors and structural generalists tending to prefer resorting to more expensive deep foundations (traversing the compressible layer) *in lieu of shallow foundations subject to the* settlements, varying in differential magnitudes and rates.

It need hardly be reminded that whenever an allowable behavior is dependent on differences, the engineering decisions have to guard against the reasonably maximized width of the confidence bands, e.g., the differential of the *maximum probable bigger settlement* as

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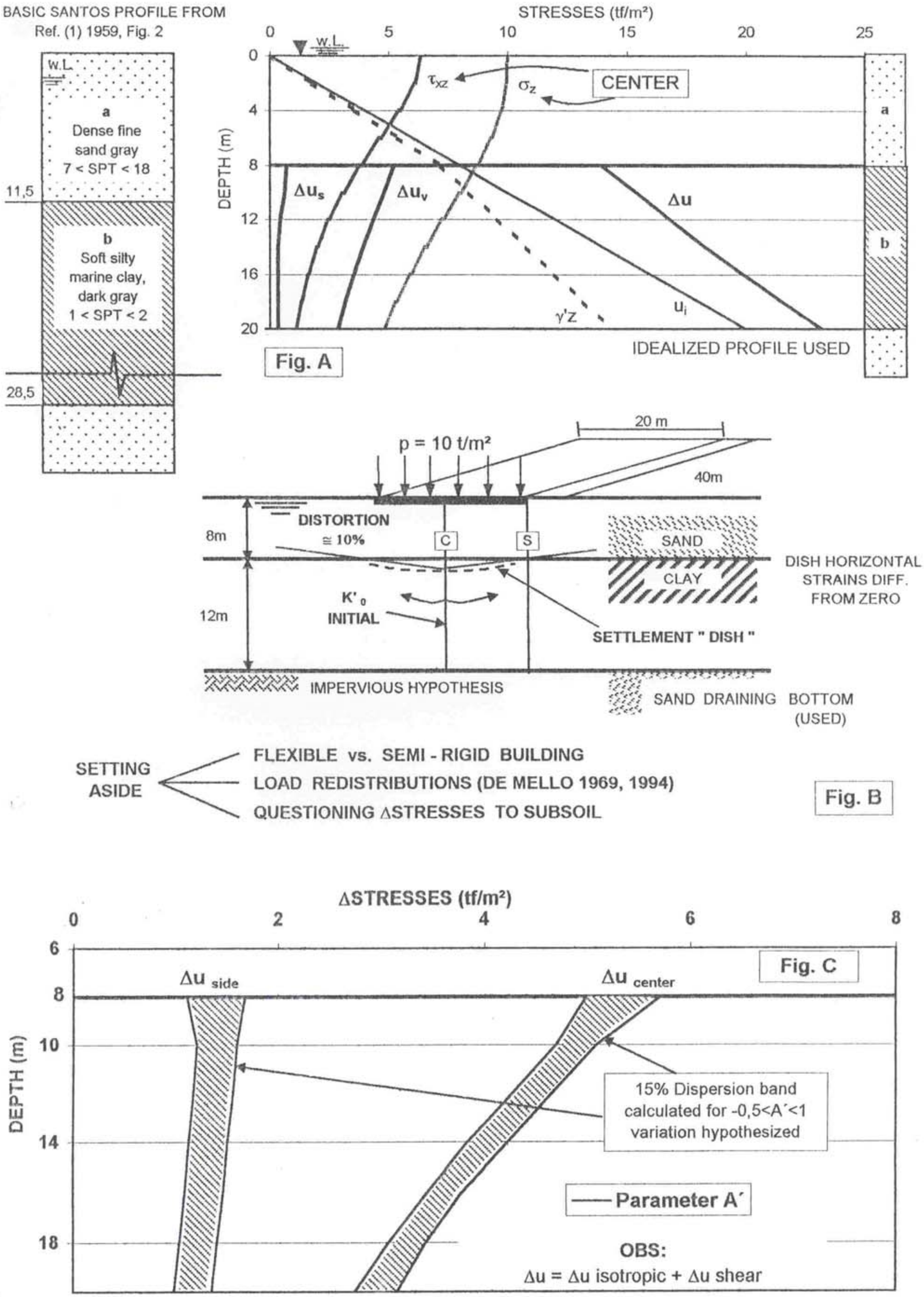


Fig. 1 - Representative conditions for buildings on shallow foundations, Santos, Brazil.

compared with the minimum probable nearby smaller settlement. The breadths of the levels of uncertainty thereupon become more nevralgic. The case is herein presented, Fig.1, as referred to hundreds of buildings of the city of Santos, SP, Brazil, and employs realistic data partly summarized in schematic form. The foundation conditions of the Santos beachfront received much first-order attention in the period 1948´-63 (cf.(1)(2)) and, because of seriously accumulated poor performances, are now under very critical scrutiny and action without any benefit of updated parameters and theories. The resulting portent of geotechnique's terminal ailment is reflected in the ongoing number of underpinnings with very deep root-piles, alongside with structural reinforcements by shotcreting: a presumed panacea not without important foreseeable risks.

Among the many problems yet left untackled, it is understandable that there are intuitively differentiated degrees of difficulties, as well as of likely significances, that dictate the benefit / cost ratios of developmental efforts. For instance, the "precise" confirmatory measurement of in situ stresses and Δstresses would probably constitute one of most difficult alleys: yet it is indispensable *because of the fundamental hypothesis of* **"in situ at-rest lateral pressure" as the zero lateral strain condition in the oedometer**, and for confirming the "elasticity" solutions for transmitted stresses, now automatic. It is for such cases that the first step should profit from parametric numerical analyses under different *hypotheses for aid in prioritizing. For instance,* right from the beginning the experimental oedometric data exposed the continuation of settlements after the baptized "primary compression" of Terzaghian consolidation theory, and reactions to so-called "secondary compressions" were really relegated to "*secondary" interest under the interpreted* premise that their time remoteness ("secular" cf. Buisman), and anticipated small magnitudes and rates, permitted leaving them dormant. The profession thus failed to advance into significances of magnitudes and rates of compression settlements that should be equally consequent independently of X mm/yr being due to a primary consolidation of a low plasticity clay of Compression Index $C_c \approx 0.1$, or to the early secondary compression of a very plastic and Sensitive clay of $C_c \approx 1.5$ and $C_\alpha \approx 0.1$. (a plausible ratio, cf. (3) 1984, although with the superposition of primary and secondary not yet cleared). The fact is that almost all of the early research was dominated by determinism, and by the impression that theories on clay behaviors could be established through single-parameter comparisons on single homogenized clays: we are thus continually frustrated by lack of core-data when attempting to resynthesize old milestone studies.

2. Intent.

This presentation's primary intent is to reemphasize that the behavior of real consequence to buildings, associated with damages from magnitudes and rates of *differential settlements and deformations*, is much more complex than hitherto postulated via added sophistications on the mathematics of flows of extruded pore-waters. And the difficulties of really improving the equivalence of PREDICTIONS vs. PERFORMANCES are so great, that foundation engineering should abandon the practice (used with starry-eyed daring) of **designing on prospective quantified settlements of some to several decimeters**. If CIs cannot be reduced to less than, say, 20%, (cf.Figs.5,6,7) the only way to achieve acceptable behaviors is to maintain the computed averages low enough so that the ± 10% margin on that low value proves satisfactory. For all buildings, industries, and important superstructures, the *architectural and operational requirements have* considerably tightened the professional needs, **specially for deferred settlements affecting expensive finishes and precise industrial equipment in operation.** And delayed damages tend to become progressively less tolerable.

Of course, the optimization of foundation solutions for average settlements of, say, not more than 20 cms, *still* leaves ample room for challenging **geotechnical solutions** (such as by floating or precompressed foundations,[1]etc.)

[1] Considering the great settlements (about 0.8 to 1.5m, *roughly 5 to 12%H*) already suffered by the questioned buildings, a combination of the two design *principles (precompression and floating)* already incorporated, added to the condition of GUARANTEES on proven behaviors (FG, cf. (4) 1987, 1988) should easily prove to be by far the best technical and economical **rehabilitating foundation** (alongside with the structural overhauling) for the problem faced.

before conceding defeat by resorting to expensive deep foundations. Some dominant problems are discussed with regard to Fig. 1 that idealizes the subsoil's upper quaternary highly compressible ($w_L \approx 130\%$) clay layer.

Terzaghi laid the basis for an excellent historic **engineering solution,** sufficiently founded on scientific hypotheses and mathematical idealizations to leave open avenues for advances. It seems that the avenues taken suffered from biases: academic research and theorization to correct Terzaghian simplifying assumptions on purely vertical consolidation of a thin homogeneous stratum seem to have left by the wayside the finite loaded area buildings on thick compressible strata.

Countless are the topics regarding oedometric settlements of buildings that have been laid dormant because of the erstwhile "adequate" successes of the soil **mechanics** oriented devoid of risk and reliability constraints. It is thus futile to attempt a minimally reasonable coverage in this modest paper. In fact, it is because of the multitudes of papers on partial views and results that confusions and frustrations abound. A few summary comments will be incorporated as minimally needed, in passing. Meanwhile, attention is concentrated on the wide-open questions for rates of total and differential settlements of consequence to buildings in Santos. Boundless indeed are the creativities inexorably unleashed by the start from physical intuition leaning on mathematical first-order idealization on priority cause-effect observation, to be followed by the zest of the unending quest on what becomes perceived to really matter.

For obvious reasons I must abstain from some topics, thoroughly recognized and discussed. Whose fault is it if Society spends and invests million times more in research on nano-subatomic or macro-galaxial quests than on crucial civil engineering effects on humanity's quality of life? We need concerted stepwise group efforts, advances, and DISCARDING. Three examples suffice. (1) The realities of column loadings, and their structural redistributions because of differential settlements (cf. (5) 1969, (6) 1994); (2) In situ stresses, and stress transmissions compared with presently derived solutions, elastic, elasto-plastic, etc..; (3) Evaluation, at engineering-level precisions, of the interferences of thin sand lenses within sediments, Fig. 3, either if fully draining laterally, or if only as pore-pressure equalizers as "non-exiting drains" (cf.(7) 1977). Instrumentation difficulties are undeniable, but incomparably smaller than resolved in all other scientific fields. It is important to develop transparently to the point of exposing probable CIs and consequent Factors of Prudence (FPs), and even of conceding defeat if necessary, in order to redirect the profession to viable responsible solutions. It would be regretable if an early deterministic mathematical success should become the stumbling block to further advances for new levels of challenge.

3. Critical overview.

After the acceptance of the simplified "model test" of the oedometer as firmly established, the advance from "perfect homogeneity and small-strain constancies" called for a real hop (in the late 1950's) to adjustments for leaning on the **triaxial tests** which include the convenience of easy pore-pressure measurements. Needless to mention the added advantage of permitting experimental checks on specimen and parametric variations of horizontal drainages. But practical inertias (including commercial and psychological), plus test variabilities accepted as "of natural diversity" and not subject to logic (bounded by errors and dispersions) interfered in diverting energies. Mesri and Choi (8) 1985 ably employed triaxial tests, summarizing many advantages, and not exceeding practical durations on typical size specimens. Most finite element computations employ assumed stress conditions, and not fixed (improbable) zero-strain states.

Respectful mention must be made of the pioneering proposal by Skempton and Bjerrum ((9)1957). And in some contrition I reproduce Fig. 2 ((10) 1969) which was well intended towards alerting against oversimplifications (of single-parameter and linear correlations, such as of the B, $\bar{B}$ and A pore-pressure coefficients of Skempton and Bishop, ((11) 1954), but may have deterred priority engineering progress by trying to be "better than necessary" at that milestone moment. The fact is that a long hiatus intervened before foundation engineering began to relinquish being quite content merely with vertical stresses and their increases. It stands to reason

Best fitting surface,
from (10) 1969.

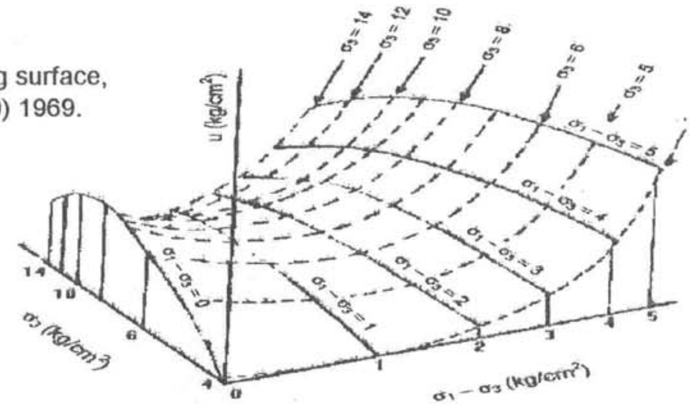


Fig. 2 - Statistically derived functions for $\Delta u = f\,[\,\Delta\sigma_3\,,\ \Delta(\sigma_1 - \sigma_3)\,]$

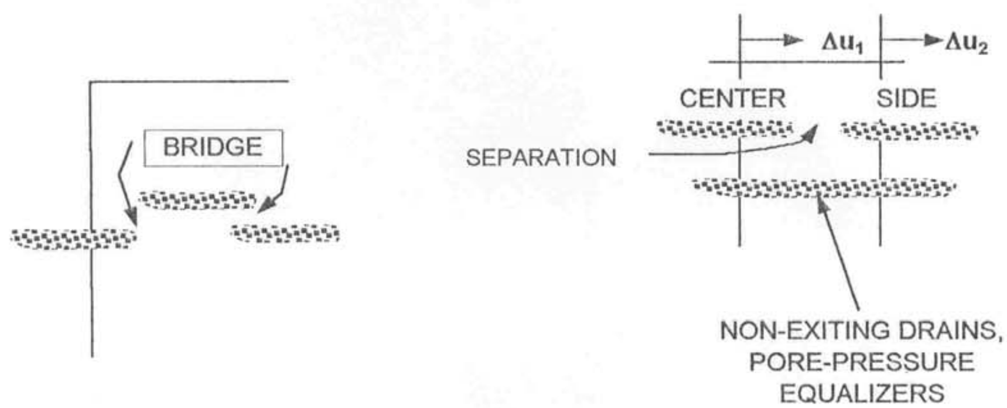
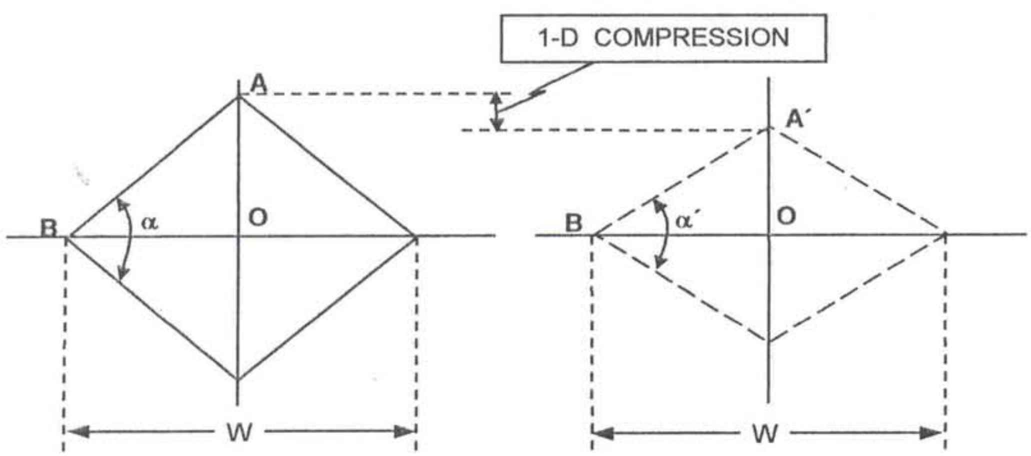


Fig. 3 - Schematic black-box interferences in horizontal drainage lenses, considering decameter distances between best possible boring profiles.



ADAPTED FROM FIG. 36, TAYLOR (15) 1942.

Fig. 4 - Pure shear distortion in oedometric 1-D compression.

therefore that a common verdict has been (cf.(12) 1996 p. 224) that "procedures were also described for adequately predicting the total magnitude of EOP (end of primary) consolidation **but not of the rate of its development":** and under the presumption of very modest errors in later stages of settlement, the hypothesis is that professional needs are being met. If considered resolved, in comparison with what? The directly proportional model-prototype comparison had been confidently taught. But in no less important a presentation than Burland's milestone Rankine Lecture ((3)1990) the data (unfortunately summarized, without piezometric observations) on the Surabaya (Indonesia) case surprise us with a very wide discrepancy between observed and predicted vertical compressions. Among the many other publications citing such discrepancies I limit my referencing to ((4) 1981). Thus in sheer judgment it must be emphasized that the many points of lack of logical cognizance of intervening parameters and confidence bands continues to oblige foundation engineering to avoid above-modest settlements in order to avoid undesirable performances.

The paper is concentrated on two points:

(a) a greatly summarized (with due apologies to those not cited) **subjective** interpretation of the historic, rather biased or randomic, efforts and partial solutions meritoriously contributed by eminent colleagues gratefully respected;

(b) secondly, a brief schematic submission on the case of Santos, concentrating on the present state of poor elucidation on questioned phenomena and parameters, some CIs reflected in the latter, and to what extent professional decisions should suggest required FPs for flexible load behavior, Figs. 1, 8.

I mention briefly the influence of "thin sand lenses", Fig. 3, possibly 10^3 to 10^5 times more pervious than the clay, which inexorably affect lateral drainage, as has been well recognized if they prove reasonably continuous. The early CPT profile's zig-zagging of point resistance alerted the profession. Over the past score of years the CPTU profiles with u dissipations reinforced the admonition and knowledge, with **point results**.

As schematically shown, the professional questions that arise are: (1) what might be the rough similarities between adopting a k_h/k_v anisotropy throughout, in comparison with different sets of lenses (?), considering the latter's localized dissipations on non-constant Δu profiles? It can be resolved by computers, for sufficient parametric variations to aid in judgment. (2) At least 2 idealized different conditions are visualized regarding "impaired continuities" of such sand lenses: (2.1) Apparently and really interrupted, but with possible "bridging" across very short distances or elevation differences (2.2) Really "enclosed lenses" serving merely as "non-exiting drains, pore-pressure equalizers". Many hypotheses can be modeled for parametric computed comparisons of CIs.

Where lie the nevralgic engineering unknowns? It is first obviously in the impossibility of sufficient subsoil profiling for reducing extreme CIs on horizontal dispersions of significance. Thus we immediately conclude that (a) running consolidation tests (point results) even with separate vertical and radial drainages, and laboratory sophistications will be of little avail except as rough indices for insertion in the above parametric comparisons. (b) thus we will never dispense with (b1) expecting and designing for wide ranges of discrepancies (b2) working with ample FPs (b3) relying much on PROTOTYPE-FIELD MONITORING, AND WHAT CAN EFFECTIVELY BE DONE once the monitoring begins to come in. (c) thereupon, considering the need of prototype monitoring, and in the present case both of the soil mass settlements near foundation level and of the column bases, one bifurcating decision is whether to prioritise "lumped-effect parameter" (settlement) or to concentrate on causative parameters (u dissipations, etc., much more kaleidoscopic and complexly interconnected) (d) finally, once monitoring has to be programmed, in first priority it is imperative to forecast what is to be **done for the specific prototype on hand**, and as second goal comes the acquisition and provision of **data for future prototypes**.

With no hope or intent of achieving adequately extensive and intensive coverage, some determining points of widespread practices may be summarized as indicating trends.

4. Oedometer tests and continuous revisions introduced. Unconvinced (?) and insufficiently convincing (?).

The M.I.T. systematic research, Taylor 1942 (15) merits starting attention because it established the conventional test and its bases, despite the perceptible and emphasized scatters (CIs). Observational procedures of 60 years ago may have been an iota less precise, but great care and fervour of the research are unquestioned.

An immediate observation arises on viewing the important effect of the INCREMENT LOADING IL ratios on the C_V values for each loading stage. For $\Delta p/pi$ varied between 0.4 and 2.4, the SQUARE ROOT SR C_V values varied roughly between 0.5 and 1.4 times the conventional for the adopted IL ratio = 1. The practical reasons for the 24-hour intervals were understandable. The IL=1 was for equally spaced points on the $\log\sigma$ scale of the graph, a reasonable first intuition. But for most buildings the loading conditions should imply very much lower C_V values. Presumably it was hoped that having, from earliest days, a standardized conventional test, the specimen-to-field ratios would be establish(able)(ed) by adjustment factors via case histories (a very questionable point from both ends). How is it that the visible scatters (Figs. 5, 6) did not immediatelly raise the provocative self-analyses regarding errors of reasoning and/or testing ?

The reality is that for various reasons a great number of different "adjustments" have been steadily introduced by Academia, in the laudable intent to improve the extraction of adequate values for the needed parameters; and, also surprisingly for reducing the time of testing (economically a minute issue). With the advent of very sensitive transducers for pressure measurements the range of possibilities widened, duly improving some features. The principal parameters extracted from the oedometer are the C_c and σ'_c for "final" settlement calculations, and the C_V and C_α for time-settlement estimations. Both these seemingly routine aims continue to cause major frustrations, partly in routine conditions, and mostly when unexpected in "special clays".

The contributions to errors are many, and repeatedly emphasized, such as: (a) problems of "intact sampling" affected by Structure, Sensitivity, remoulding disturbances, with "effective quality" of samples and specimens yet poorly defined and quantified; (b) exaggerated transient pore pressure gradients; (c) need for closer points to define the "yield point" (break of preconsolidated to virgin curves, near-straight lines) and probable specimen-to-field correction thereof; (d) and so on. No attempt is herein made to cover the plethora of well-intentioned recommendations. Mesri and Feng 1992 (16) summarily list, besides the IL test, the Controlled Gradient (CG), the Constant Rate of Strain (CRS), Constant Hydraulic Gradient (CHG), Constant Pressure Ratio (CPR) and Constant Rate of Loading (CRL) tests, and one can find even other references, such as the Single Loading Oedometer (SLO) test (17) 1986. Several apparent practical reasons may explain why the conventional (IL=1.0) oedometer has not been perceptibly substituted. Inertia. Weight of "authority". Complexity. Investments needed for revamping. Shying away from too many proposals, offered piecemeal. Lack (always unavoidable) of statistical data for tying past and conventional (however premature, with multitudinous more data of routine qualities) to the new, and to dismally few prototypes. And so on.

But the important questions seem to lie in the conceptual area, and in some unnecessary individualisms, quite apart from the dispersions and CIs discussed below. The concept of oedometer specimen as a good model of the homogeneous clay layer under uniform loadings had been abandoned in the IL conventional test by its IL=1. The CG, CRS, CRL tests apparently aimed at reducing some obvious brutal disturbance factors. However, the original premises were maintained, of assuming K_o lateral stress (routinely the normally consolidated NC value), forgetting its adulterations by non-snug fit and other factors, and especially, forgetting the incidence of shear distortions while preserving zero lateral strain. Fig. 4, adapted from Taylor (15) 1942, reminds us of the inevitable shear distortions in the 1-D oedometer compression (analogously noted by a few other authors). One should refer to Skempton (18) 1961, informing of the horizontal in situ stress of the order of 2.5 σ'_V in London clay [2], to recognize

[2] The overconsolidation is expressed distinctly either as a ratio OCR, useful for some purposes, or as a difference $\sigma' - (\gamma z)'$ on the profile, better for other needs.

quite a range of possible lateral stresses between the NC condition and the OCR states. And in the Santos case we cannot overlook the reality that the extension (Fig. 1b) from a horizontal chord length to the arc of the trough, inevitably implies extensions altering the presumed K_o condition. Finally, moreover, geotechnique is reminded of the Schmertmann (19) 1983 "challenge" regarding the possible influence of secondary compressions in changing K_o, and the ensuing debates and research efforts of yet incomplete coverage and convincing evidences. This despite such reassuring authoritative acceptances as (20) 1985 "Data...(8) 1985, **support the hypothesis that creep occurs only after the end of primary consolidation, i.e. after dissipation of excess pore pressure**" because of lacking broad mental models to incorporate the very wide heterogeneities of soils, stress-strain-time conditions, and experiences. Surely DESTRUCTURATIONS depend on Structure, Sensitivities, etc. wherein intervene many colloid-chemical factors of particles and pore-liquids (e.g. (21) 1982) and also loading-deformation conditions (as in the IL ratios etc.) of magnitudes, rates, durations etc. Poorooshasb, H.B. et al (22) 1981 repeat the denial of separation of primary and secondary consolidation process "for highly sensitive clays" and develop a mathematical solution for a joint rheology (applying the principle of conservation of mass), but, unfortunately, fail to associate (even in mental model) the parameters with **different degrees of sensitivity.** LOGICALLY (?), unless proven otherwise (?), with CI data, in special soils, **destructurations should begin, and increase, as effective stresses increase within each loading stage,** as u dissipates. Or, would a very special coincidence dictate that as σ'_v, σ'_h increase, a **decreasing shear destructuration** from $(\sigma_1\text{-}\sigma_3) \rightarrow (\sigma'_1\text{-}\sigma'_3)$ should **exactly compensate** for the increasing $(\sigma_v'+\sigma_h')/2$ destructuration? Barring conceptually improbable exactness, **maybe a practical engineering similarity** might occur, with adequate CIs for a certain range of Plasticities, Sensitivities, Stresses, etc.? At any rate, for possible promising generalizations everything points to the use of pore pressure coefficients analogous to those of Skempton and Bishop (11) 1954, as described schematically further down, Figs. 1a, c.

In short, the crucial point is that the "strain-conceived oedometer" headed for a dead-end, going against the current opened by triaxial tests with pore pressure measurements, and the ever-growing use of stress analyses via elasticity and sundry finite element solutions. There is now no practical reason why triaxial consolidation tests (permitting various σ_1/σ_3 ratios, and routinely speeded up by draining filter strips etc..) should not have completely substituted the oedometers, for ulterior estimates of Δu including $\Delta\sigma_V$ and $\Delta\sigma_h$ values at will, and including inquisitive parametric variations.

An example of a second-order individualism arises from the starting notion of the preferred "end of primary EOP" test result idealized to "exclude" secondary compression upon reaching dissipation to a "zero pore pressure" (16) 1992. Irrespective of measurement precisions, there cannot be any continuing expelling of pore water without a pore pressure and gradient. Thus, adequate mental modelling (as also reflected in the mathematical formulations, to asymptotic infinity) should have irrevocably excluded unreflected intuitions of a true EOP: therefore, if EOP is nominal at "near zero pressures"[3], it should have been possible to obtain some comparisons of the errors depending on the numerical values taken to establish the systematic "near zero". These have been taken at 1,2 to 5,2% (23) 1971, about 2% (8) 1985, and 3 to 15% (16) 1992.

And so on, many a second-order difference occurs. In a hope of retrieving as much as possible of the plethora and **welter of individualistic efforts** that represents so much well-intended **expenditure,** GEOTECHNIQUE should be enjoined to an **investment** effort, to stitch together as much as possible of different viable proposals, which may well be retrievable and preservable if: (a) they imply **second– order**

[3] Many collateral theorizations, research experiments, and data, exist on slurry consolidation both for the cases of tailings and in the hope of reproducing geological sedimentations. No attempt can be made to broach these additional avenues, although they also suggest many issues of interest. Passing mention is made of recent laboratory consolidation experiments (24) 1999 which report achieving 19% strain without "measurable dissipation of residual pore pressure". Measurability is an issue: and so also flow under infinitesimal gradients, etc. Are they engineering or scientific pursuits?

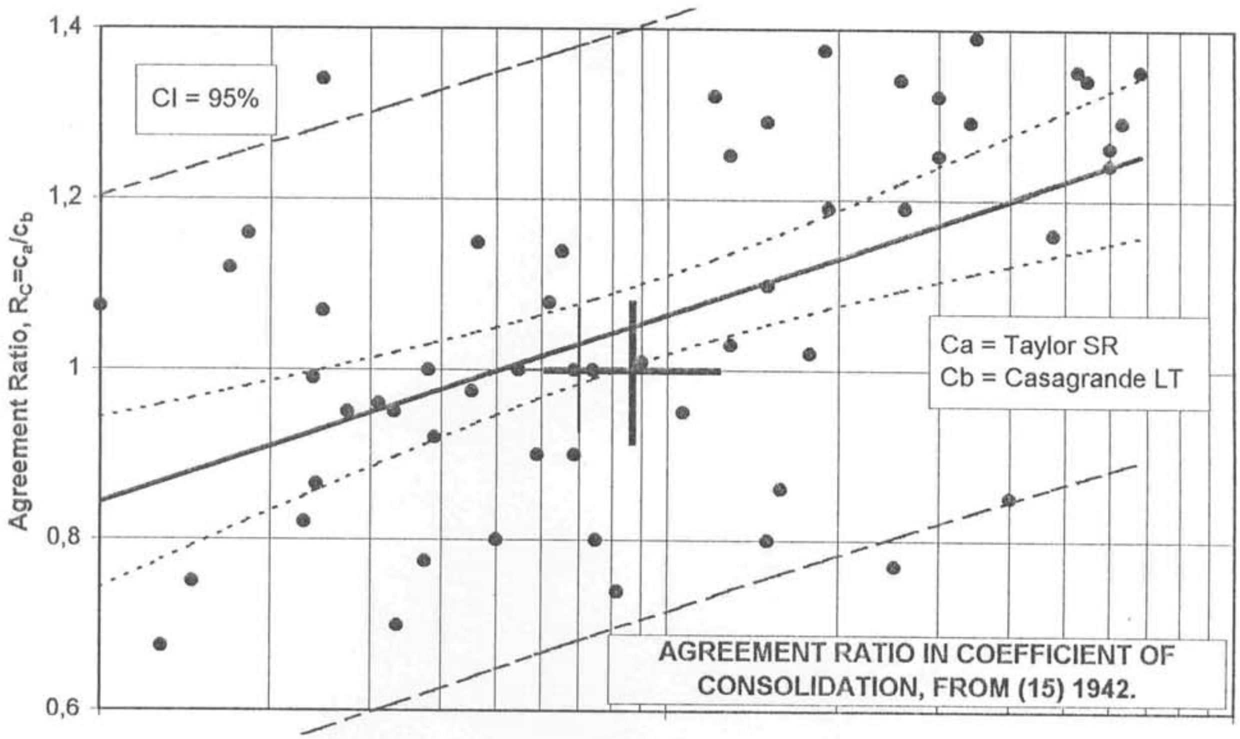


Fig. 5 - With higher pressures progressively higher LT / SR Cv accelerations.

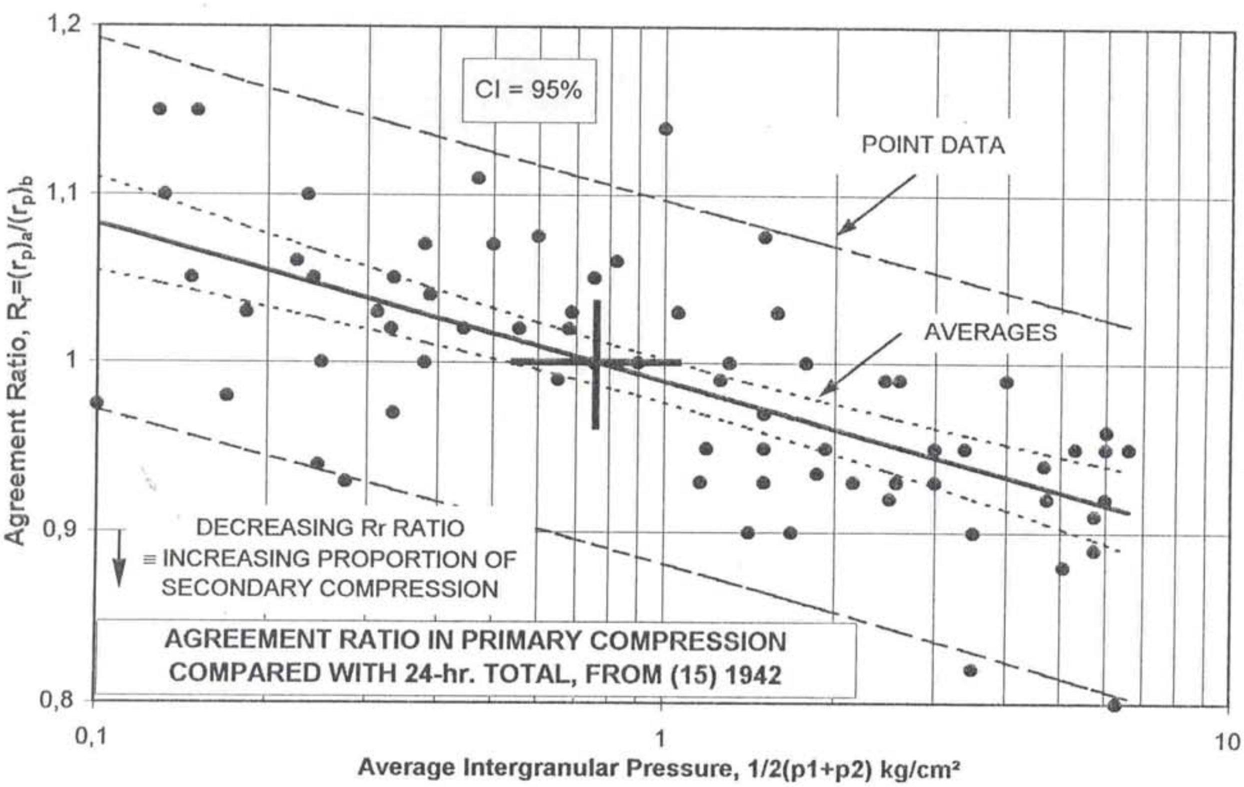


Fig. 6 - Proportions of primary to 24-hr. total, decreasing with increasing pressures.

scatter, quantified; (b) statistical regressions with CIs are deduced to pass the baton between different schools. All the practical procedures in widespread use and embodying essentially the same correct and wrong principles should be compared for usefulness in predictive decisions.

5. Insertion of statistical-probabilistic assessments, employing (15), 1942, Taylor's data, as mere examples.

Figs. 5,6,7 indicate dispersions, even for 95% CI on averages, with very wide CIs on point data. The 95% CI is arbitrary, is used throughout the example: it may be changed at will, depending on the resulting Factors of Prudence FPs felt to be reasonable " by experience". Figs. 5 and 6 can be summarily discussed as indicating the *proportional component of secondary* compression in the IL 24-hour compression. This should have had direct pratical implications to the ADJUSTMENT FACTORS from laboratory to prototype, a directneed and hope of the profession. Both indicate progressively increasing secondary participation with *increasing pressures, compatible* with the intuitive conclusion (variously documented in part) of *secondary compressions* **associated with destructuration**. It is pointless to delve into any discussion on the directly aimed comparison, since the **square-root SR fitting method** of Taylor can be taken as a **nominal reference**, and the log-time LT fitting method of Casagrande should have been readily **discarded on first principles** of scientific method, as well as of practical use. Errors being inescapable, and inescapably greater at " initial and final" conditions (0%, 100%), the log-fitting method has no merit in theory or in conventional laboratory application in highly impervious clays. Above all, for **field use by prototype-to-model concepts** it *is a sterile black-box of monitored data, almost* always inevitably lacking in satisfactorily long duration: when effectively documented, it is tantamount to shutting the stable doors long after the horses have fled. Fig. 7 further shows the wide CIs but is set aside from profitable

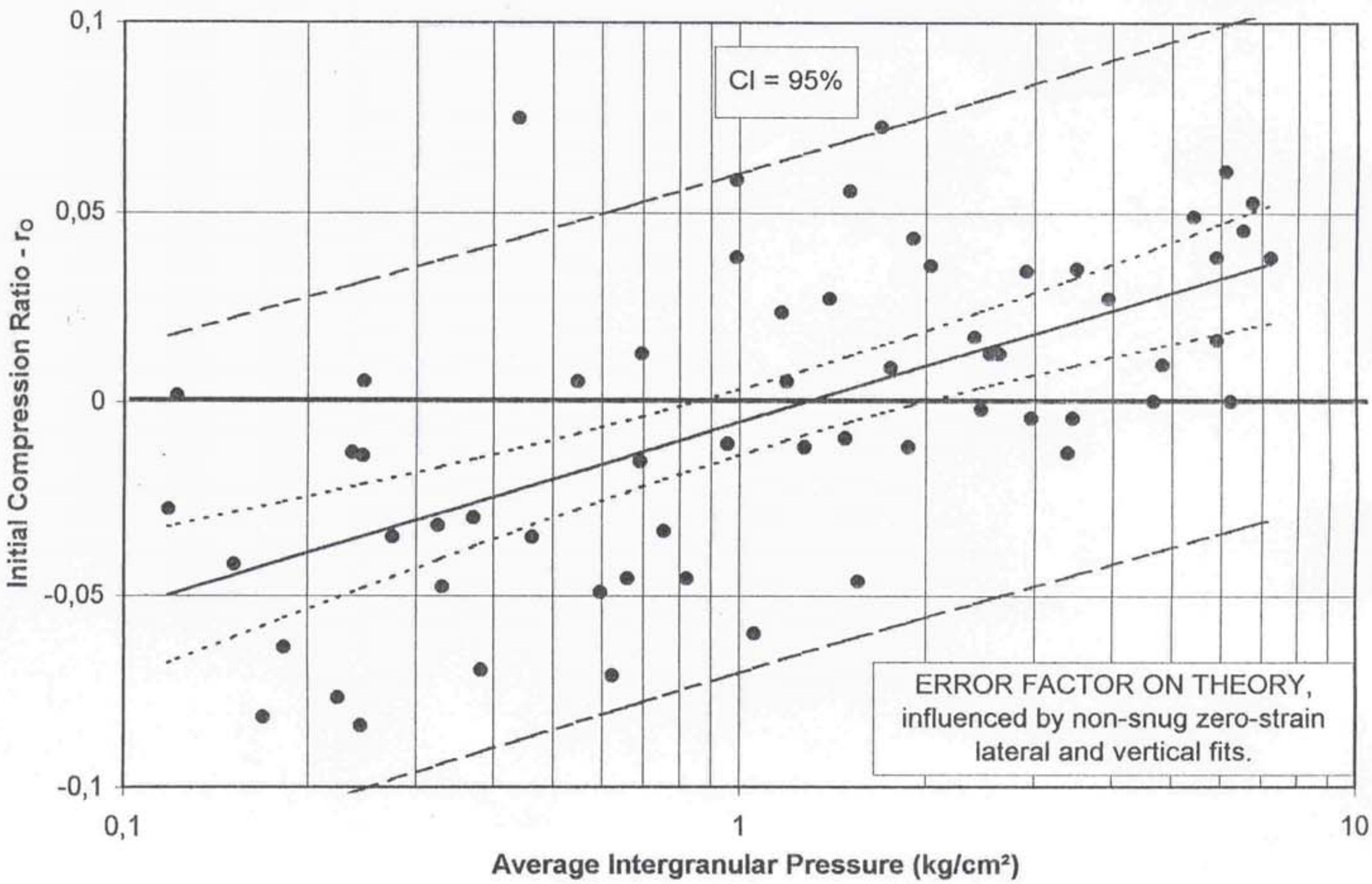


Fig. 7 - Initial compression ratio to 24-hour. Spurious, not considered.

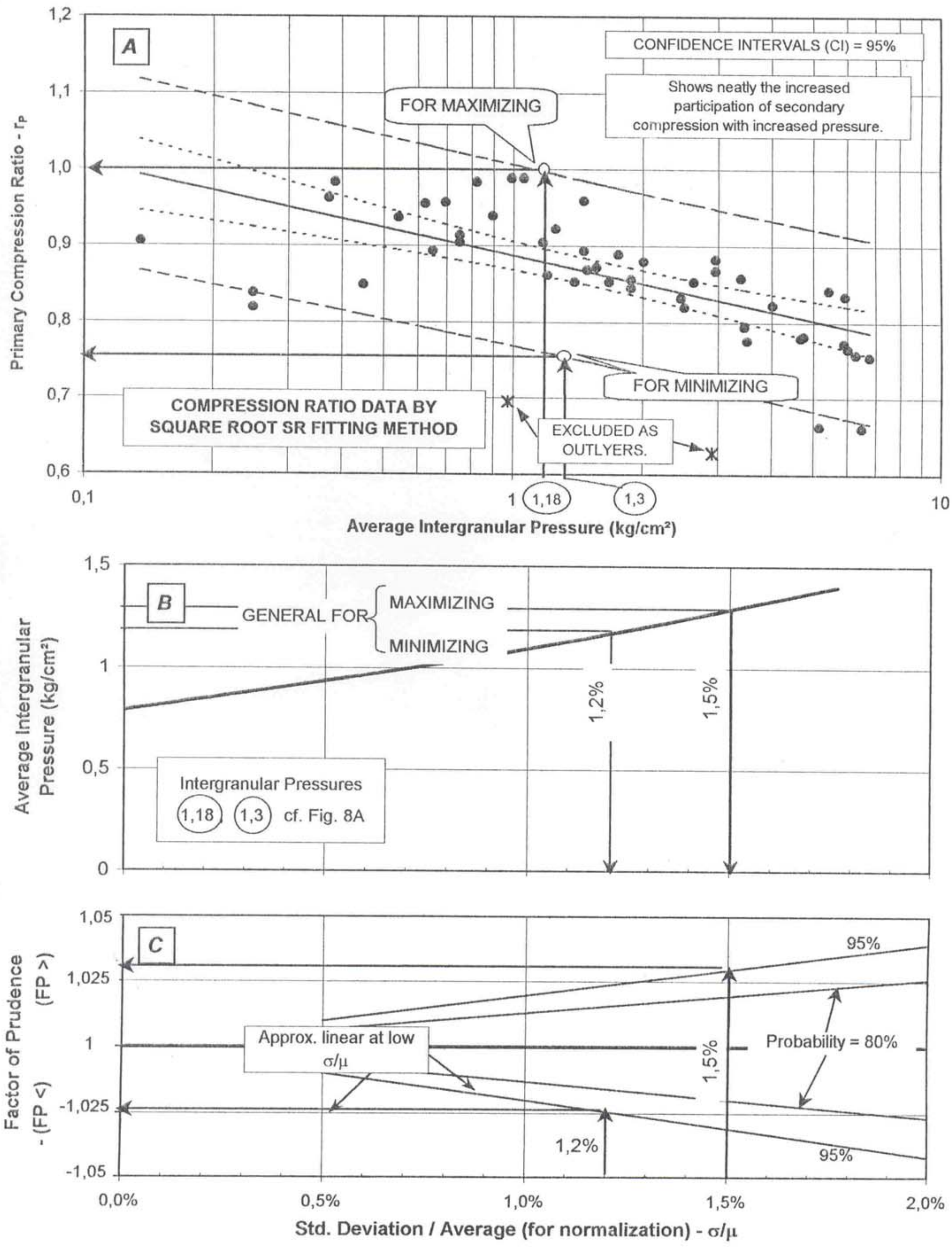
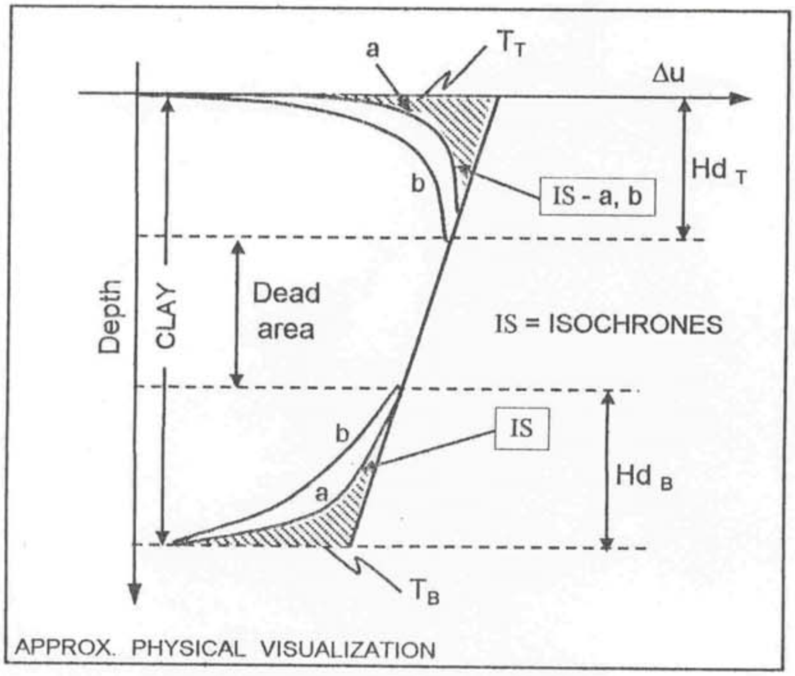


Fig. 8 - Exercise on partial confidence probability evaluations.



ADDITIONALLY CHANGING k and Cv values during consolidation not considered.

SIDE		CENTER	
a $T_T = 0{,}00125$	**b** $T_T = 0{,}01$	**a** $T_T = 0{,}00125$	**b** $T_T = 0{,}01$
$k_T = 1{,}5.k_B$	$k_T = 1{,}5.k_B$	$k_T = 1{,}5.k_B$	$k_T = 1{,}5.k_B$
$Cv_T = 1{,}4\ Cv_B$	$Cv_T = 1{,}4\ Cv_B$	$Cv_T = 1{,}08\ Cv_B$	$Cv_T = 1{,}08\ Cv_B$
$T_T = 4.T_B$	$T_T = 2.T_B$	$T_T = 4.T_B$	$T_T = 2.T_B$
$Hd_T = 0{,}6.Hd_B$ **c**	$Hd_T = 0{,}84.Hd_B$ **d**	$Hd_T = 0{,}52.Hd_B$ **e**	$Hd_T = 0{,}73.Hd_B$ **f**

a → **b** - ADVANCING U DISSIPATION, INCREASING T and t

c → **d** and **e** → **f** - DRAINED AREAS MOVE INWARDS, BUT DIFFERENTLY, CHANGING THE (ROUGHLY MID-HEIGHT) POINT OF " NO - RETURN ".

TABLE - Summary of approx. results calculated.

T = Time for given Udissipation

$T_{top} = T_T \qquad T_{bottom} = T_B$

Cv = Coef. of consolidation

Hd = Effective drainage distance for dissipation assumed by $T = Cv.t / Hd^2$ for equal times t

k (cm/s) - different, top → bottom, from log k vs. ε straight line

Fig. 9 - Thick Layer double drainage asymmetrical from start, and with point of bifurcation (ZERO du/dz) having to change with time.

discussion because it is extraneous to the concepts and theory, spurious.

Proceeding to Figs. 8 (a)(b), we stop to deepen the extraction of statistical-probabilistic interpretations of meaning for professional orientation. Firstly, the use of CIs on **point data**, as compared with **averages,** is another basic question of judgement. In the use of cumulative behaviors, such as compression settlements and drainage volumes, the averages are accepted as more reasonable. In the present case, however, involving carefully homogenized meticulous laboratory research results, and since the results (differing with pressure intervals) would tend to be applied as valid at the distinct pressures, the example is pursued using point data CIs, merely as an example, purposely for exaggerated illustration.

Many other publications provide data permitting similar methodological interpretation, and, in fact, for the profession's benefit it will be imperative to employ as many of them as possible, principally to cover varied clays and conditions, and more modern test and interpretation methods proposed.

6. Exercise on Confidence Probability evaluations as per typical modern CODES, magnifying "loadings" and minimizing "resistances".

A few examples occur in publications wherein despite questionable logic, theories, equations, and deterministic parameters, there is a claim of good performance vs. prediction compatibility. Realistically we should much prefer a good number (sample of universe) of cases within a CI of performance/prediction ratio, to few individual cases of coincidences of really "good equivalence". Inevitably a somewhat biased universe, since cases of less successful predictions would tend to be kept in the unpublished files. Moreover, even for coincident data at the time of publication submission, there is rarely a presentation in statistical terms of CIs, including the variation of CIs with time that is the crux of our professional problem: the designer's hope is that the **delayed behavior should become progressively more confidently coincident.** Finally, a frightening hurdle, in the cases of buildings the rule has been to monitor and publish the column-base settlements, introducing other complex interventions of building stress redistributions: whereas, for elucidation on geotechnique we should desire indications on effective loads, and their reflection on the settlements of the subsoil mass at footing level.

A statistical treatment is imperative, to establish Factors of Prudence FPs for confidence on limiting the probability of unexpectedly exceeding the acceptable or tolerable limits of performance. Modern Codes rationally advanced to establishing **Factors for maximizing** the "loadings", in generalized terms, the **active factors causative** of the problem to be avoided: concomitantly, **Factors for minimizing** the "resistances", that is, in principle the **factors participating at the passive end.** In the present case we are concerned with differential settlements, for which I postulate that we can reason towards maximizing the maximum *settlement calculation, and minimizing the* minimum settlement position.

Many parameters enter into the final FP, a product of FP_1, $FP_2 ... FP_n$. For instance, the CIs on thickness of compressible layer, its σ'_p and C_c, the initial effective stresses, the Δstresses *transmitted, etc. This presentation is limited to a* single intervening parameter, Fig. 8A derived from Taylor (15) 1942, *for an example of a* routine procedure of statistics/probability, and coupled with a query of concept and logic on the conventional IL oedometer test result used, in comparison with the postulated preference for the EOP result.

Fig. 8A really gives an indication on a set of **nominal EOP** compressions to be compared with the 24-hour IL = 1.0 test (disregarding adjustment, unknown, for the specimen's structural change, cumulative on successive loadings, because of the additional compression between EOP and 24-hour). Let us avoid these and other additional complexities since the purpose herein is merely to exemplify how to employ a routine method, hopefully for redeeming the vast store of different data. Since presumably by far the greatest percentage of professional "final settlement" computations have used the e-logσ data from the conventional test, the first conclusion that in passing we must signal, from the change of r_p with pressure, is that the C_C slope routinely used should be steeper *than the nominal EOP (a roughly 20% decrease* of r_p in moving from 0.1 to 7.0 kg/cm^2 of effective stress).

However, there are dispersions and CIs from the statistics, to incorporate in probabilistic computation, for PRUDENCE IN DECISIONS. By random choice we shall work with the 95% CI, which means that there will be only 2.5% probability risk of the numerical value deduced being "exceeded", to above in maximizing, or to below in minimizing. Other CI values are frequently used (e.g. 90%, 80% etc..) and cited by statisticians, without any judicious bridging to the professionally meaningful problem, because that is a task that befalls us. The CENTER (Fig. 1A) initial and final effective stresses are roughly 1.1 and 1.5 kg/cm^2, average 1.3 kg/cm^2, while the respective SIDE ones are 1.1 and 1.26, average 1.18 kg/cm^2.

Along the graph of Fig. 8A and its CI one must compute different (slightly varying) normalized values of the desired cutoff probability (2.5%) as a function of the respective standard deviation μ at that position. Routine calculations yield the curve of Fig. 8B. Finally, in the graphs of Fig. 8C we return to what FPs have to be used for maximizing the CENTER settlement, upper half of the graph, and for minimizing the SIDE settlement, lower half. Thus if the deterministic differential settlement had been computed center-side as 30-12cms, the probabilistic decision should be based on (30) (1.03) - (12) (1/1.015), 30.9 -11.7, 22.2cms in lieu of 18. Whether it matters or not is quite a separate issue.

Problems and solutions of the realms of soil sciences and mathematical idealizations are to run in parallel, duly respected and often profitably followed. The professional problem of concern for extrapolations to the future is herein emphasized, principally in view of the premonition that the **only promising avenue lies in prototype-monitoring**. Such proposals, seemingly obvious, always imply considerable expense and annoyance, and not infrequently include surprises and frustrations. We recall that the elucidations of presumed different phenomena of primary (Terzaghian, slightly corrected (28) 1967) consolidation, and longer-term compressions do not seem "finaliz(ed)(able)" in comparing oedometers and thick clay layers. Conceptually some differences of phenomenologies, and proportions thereof, will ultimately persist, and therefore the professional decision centers on ***how good is good enough.*** In this light the gross success of the Terzaghian theory (as well as some comparative facilities of instrumentation) led dominantly to piezometric and occasional inclinometer monitoring. Delving into the implicit concepts, it seems clearly preferable to reinforce direct settlement/deformation monitoring, so as to avoid falling into vicious circles of begging the question, acceptance of what is questioned, which is the idealized erstwhile theory.

7. Velocity formulations of settlements and of drainages and pore pressure dissipations.

Very many, essentially all, problems of geotechnical engineering are tackled via **the complex global affecting parameter**, while the scientific quest pursues the complex **component causative parameters**, attempting analytic *explanations of the how and the why*, for purposeful predicting and design. Take instances from Meteorology (winds, waves etc.), Hydrology, Seismology, etc... In recognized principle the two are inseparable, as stride after stride of one´s feet. But depending on the apparent (especially if very difficult to confirm/deny) success of one, it may be left aside as "established", and efforts concentrated on the other. It seemed that the *homogeneous model-to-prototype* total settlement calculations could be set aside as "reasonably certain". And *time-settlement queries* diverted to improved evaluations of C_v (and piezometric dissipations), in tune with the mathematical derivation and its progressive sophisticated perfectionings.

The **affecting global parameter** of our need is settlements, "total" and velocities, because a building´s deformation-secondary stress reactions cannot care about what caused the causative settlements. Thus, assuming that the ***total final settlement*** *could be* "established with certainty" (a presumption fraught with erraticities, but a few of which above signalled, and not yet confirmed, **within acceptable degrees of statistical** CIs in prediction-performance realities) attention concentrated on methods for extracting C_v values, from laboratory and prototypes.

Can one ever extract profitable lessons from *observed data without either a phenomenological* intuition and "law", or statistical CIs, ... and in truth, without both orientations jointly as essential? At the present juncture, since all the intents suffer from some question or other, and it might be that other interfering parameters and factors really override, one should commend an attempt to digest in statistical CIs all available data with all comparative methods. For instance, can one find analyses for professional conditions of Fig. 1 with significant variations from top to bottom of layer, or/and incorporating erraticities of Fig. 3 as minimal nevralgic dispersions?

Terzaghi himself early (1927) recognized that modifications would be necessary because of the effects of secondary compression, and different hypotheses and proposals began (cf. (15) 1942). But the erstwhile dominant faiths were of "certainty" (shocking?!) **on time-settlement laboratory determinations**, *and need to extract* from them (refer to the admonition of Fig. 6). Although impossible by logic, and even by the very mathematical derived "law" asymptotic to infinity, the pervading mythology was that secondary compression started after the end of the primary.

Present aim is essentially to consider the *prevailing practices of settlement-time interpretations*, especially as possible aids to the Santos problem. Important issues are broached only in passing: for instance, Brinch Hansen (25) 1961 emphasizes "It · is evident... that secondary consolidation must start as soon as an effective stress is developed " (already signalled by Taylor (15) 1942, but possibly in humble recognition, conceptually applicable at neither extreme, *neither* **as soon as**, *non only* **after the end**, extremes how definable?).

Brinch Hansen set about matching an "*approximate model law for simultaneous primary* and secondary consolidation" accompanied by the graphical procedure of subdividing the time axis abruptly into a $\sqrt{t}$ start, and the log t extension, an artifice of reproducing observed laboratory data irrespective of rheologies that could greatly differentiate model-to-prototype. Seemingly this method of direct use of the time-settlement curve (without normalized weight *adjustments) did not spread into professional* practice, for practical reasons readily interpretable. It is mentioned, however, because other methods of widespread unquestioning use embody the same principles, of blind and bland acceptance of a compression-time graph for analysis by the Terzaghian primary equation[4] despite its adulteration by secondary compressions of greatly different and varying interveniences.

Having discarded the LT method, and thus temporarily retained only the SR fitting method as widespread, it now seems appropriate to begin by mentioning the very promising avenue opened by Asaoka (31) 1978, principally for a conceptual discussion of the basic principles, **less shackled to consolidation theory on u dissipations.**

It is inappropriate and impossible to bro..cn here the "method based on the Bayesian inference of the non stationary stochastic process for predictive probability distribution of future behaviors". It is a proven procedure for progressive adjustments of posterior probabilities using (n+1) data on a prior (postulated, or already reached) probability based on the (n) data. It is widely recognized that any judicious estimated prior probability serves as an acceptable start, and ulterior precisions (CIs) at revised probabilities inevitably improve even if the phenomenologies change, gradually. Despite all questionings on the Terzaghian theory, Asaoka's start with the prior probability based on that theory is logical, difficult to set aside. And his direct use of settlements (recommendably extendable to settlement rates) is in accord with my concern, of concentrating on what really matters, and leaving the rest to "unknown erraticities".

What merits questioning is the use of the straight line (especially if graphical) as proposed because of being simpler, but to be dismissed) aimed at the deterministic 100% primary (inexistent). What we really need is the basic rough starting design of probable settlements and rates, and then the Bayesian statistical methodology for continually

[4] Regarding **this primordial model**, with all due respect for the intents and brilliance of many improved mathematical solutions derived for special conditions, one questions whether or not they should really belong as second-order sophistications. The question must be estimated by reference to the many dominant parameters and CIs involved for reaching the professional end-product sought, of full time-settlement curves, to an operational life of some 25 to 50 years: therein the primordial obstacle is that of reasonably confident prediction of the "practical final settlement". Only few references are listed, as examples, all of which represent limited "corrections" (of ≤ 10% ?). One clearly concludes that it persists as an incessant effort (26) 195, (27) 1961, (28) 1967, 1995, rather to the detriment of professional practice which depends on confident decisions, even if always conditioned by Factors of Prudence FPs, determinable from CIs.

improving and adjusting, as early and fast as possible, the predictions of **probabilities of future settlements and settlement rates,** with their CIs, for anticipating FPs. If phenomenologies change the best regression will duly curve.

This methodology is applicable everywhere, both in the laboratory and in the field, and should be applied with all proposed methods, in parallel, and for all possible documented cases. I must repeat the emphasis on these points, because I take the liberty to extract from the following proposals a general concept that will be freed from impossible determinism, heralded by Terzaghian gross success. Discussions on the scientific/rheological/mathematical composite vs...distinct primary vs. secondary consolidations will continue, extenuating: vast complexities will not favour us with single unified theories or simpler case histories and idealized professional challenges. Thus, what is needed is a procedure for assessing statistical reality (and its adjustment potentialities) as early as possible. If such a stance be accepted, the profession should examine all proposals regarding a) behavior(s) really sought b) determinism or not c) how far implicity dependent on Terzaghian idealized consolidation mathematics d) comparable applicability in laboratory and field e) practicability of early application, and continuing, as belonging to an inseparable statistical universe. The relative merits will show up, first on logical concepts, next on CIs and FPs, finally on viability in professional practice.

As mere examples singled out for such reflections: - The **velocity method** (32) 1978, for oedometer only, should have been used (possibly with software readily developable) for compressions with SR and EOP results of laboratory research: but its extension to cover secondary consolidation (32) 1984, including settlement rates and pore pressure dissipation rates, might merit improvement. The **rectangular hyperbola fitting method** (for 60<U<90%), (34) 1981, and its relatively successful uses discussed for clays treated with vertical drains (e.g. (35) 1995, (36) 1996) would appear less inviting for buildings. It is significant that in (36) 1996 the ASAOKA observational method confirms the trends towards prototype monitoring: the comments regarding progressive Bayesian treatments with regressions and CIs persist. In (37) 1999 Baguelin further advances Asaoka´s avenue, giving a more generalized expression on the primordial Terzaghian, and applies regressions, and recognizes that the intersection (of presumed final settlement) is *greatly affected by dispersions: but a step further* will come with the quantifications of the CIs.

Having reflected on the many unknowns still to be reasonably resolved regarding rates of settlement as dictated by piezometric monitoring of rates of pore pressure dissipations, we finally advance to our professional problem in which the *double drainage starts as significantly asymmetric,* and the point of bifurcation of flows to top and to bottom should progressively change.

8. The professional case.

From all the foregoing discussions, facing so very many and complex interacting parameters, it seems that the case from the professional geotechnical perspective points to:

(1) using in situ stress and Δstress estimations;

(2) resorting to triaxial consolidation tests for estimation of the composite compressions due to isotropic ΔV_v from $\Delta\sigma_1$, and ΔVs from shears $(\Delta\sigma_1 - \Delta\sigma_3)$ and later $(\Delta\sigma'_1 - \Delta\sigma'_3)$;

(3) monitoring movements (principally top settlements) of the soil mass, for optimized Bayesian regressions of predictions of probable developments, with their CIs;

(4) decide on design, and prepare for eventual timely actions in re-steering the projections depending on the FPs judged necessary.

In Figs. 1A, C we present the approximate data calculated for stresses. The stresses transmitted to the clay layer were taken simply from the *elasticity Influence Factors given by Poulos and* Davis (38) 1974. The Δu profile was estimated on the basis of the A, B coefficients, with the following adjustment and estimations for lack of data. It is reasoned that for normally-consolidated NC clay the vertical stresses, initial and transmitted, are **major principal stresses** σ_1: therefore a parameter A' was postulated, as a function of σ_1, obtainable by algebraic reshuffling of the A, B equation given in terms of σ_3[5]; A' would become A - 1. Thereupon an estimated range of plausible

A' values was taken, -0,5<A'<1.0, extracted from some tests simply assumed reasonable. The Δu_s does not result significant (15% dispersion band to add to the Δu_v) but it is maintained merely for elucidative principles.

The final inclined graphs for CENTER and SIDE of Fig. 1C are recognized by geotechnicians as obvious, dismissing the homogeneous and symmetrical double-drainage hypotheses. Also the perceptible differences between CENTER and SIDE (and intermediate Δu profiles) are obvious, indicating a recognized horizontal drainage: not so salient, but physically obvious, is the fact that the side drainage vectors, controlled by du/dz, should also have complicating inclined components. Additional contributions to marked asymmetry arise, obviously, from the top vs. bottom differences in: (1) needed $\Delta u \rightarrow \Delta \varepsilon$ along the assumed virgin compression C_c line, higher ratios of p_f/p_i at top than at bottom; (2) modestly smaller permeability k of the bottom, by the e vs. log k straight line; (3) consequent different vertically draining C_v values for top and bottom areas between isochrones; (4) estimated different weighted drainage lengths H_d, bottom and top, while areas between isochrones are equivalent at successive times t = 1, 2 ...n. Roughly estimated numerical iterative comparisons were made, using two successive isochrones for time FACTORS T = 0.00125 and T = 0.01.

Fig 8A and the accompanying Table summarize the principal indications. In a thick NC clay layer under a finite-area loading, the postulated double drainage is asymmetrical from the start, and the point of bifurcation (not mid-height, but with ZERO du/dz gradient) has to keep changing gradually with time[6]. Such numerical comparative results, however crude as recognized and undeniable, are merely presented as a further strong argument against piezometric monitorings to confirm the advance and "stabilization" of settlement problems via consolidation theories' mathematical sophistications.

[5] It would seem correspondingly logical that for high OCR clays the σ_h could be the σ_1, in which case the vertical stresses would be treated with the conventional A values.

[6] It seems indicated that these discrepancies add up to greater effects than the purity of the Gibson (28) 1967 correction to Terzaghi's omission of the relative descending movement of the solids (top) against the upward flow of porewater.

9. Messages of apparent professional consequence.

It seems that the early success of consolidation theory and idealized mathematics blurred the reality of multitudes of complexities, of difficult "ultimate clarification". Efforts have been predominantly towards the illusive understanding of pore pressure generations and dissipations, an important but distant aim, regarding only one of the many causative components to settlements of buildings.

There should be pointed preference for working with stresses and Δstresses, rather than with zero-strain hypotheses. It is indispensable to use Bayesian progressive probabilities, deducible by regressions with CIs, in order to prioritize parameters and procedures of greater consequence. Above all, for the **effects** on performances of buildings the **causative factors** are soil mass settlements and settlement rates: and these must be monitored for effective decisions. Regressive research on **causative** factors for the settlements themselves constitutes very broad a quest, one step further removed from the end purpose needed.

Geotechnique must be imbued with the intrinsic acceptance that knowledge will forever be statistical, the culmination in perfection being unachievable.

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