Closure

VICTOR F. B. DE MELLO[14] M. ASCE. -- The writer's first and pleasant obligation is to acknowledge with sincere appreciation the fact that Dr. Ricardo Dobry prepared the Spanish translation of the Report. Further, it behoves him to correct the more flagrant erratum of the report: in Fig.2 the insert σ_{max} should read $\sigma_{max} = \propto (E\ Vh/V)$. Finally, the writer wishes to profit of the occasion to present the data extracted from Vesic 325 (Figs. 68, 69, 70) in a more convenient form than as shown in Fig. 13: Fig. 27 below shows more clearly that the SPT is a cruder index, more fraught with scatter, at shallow depths,

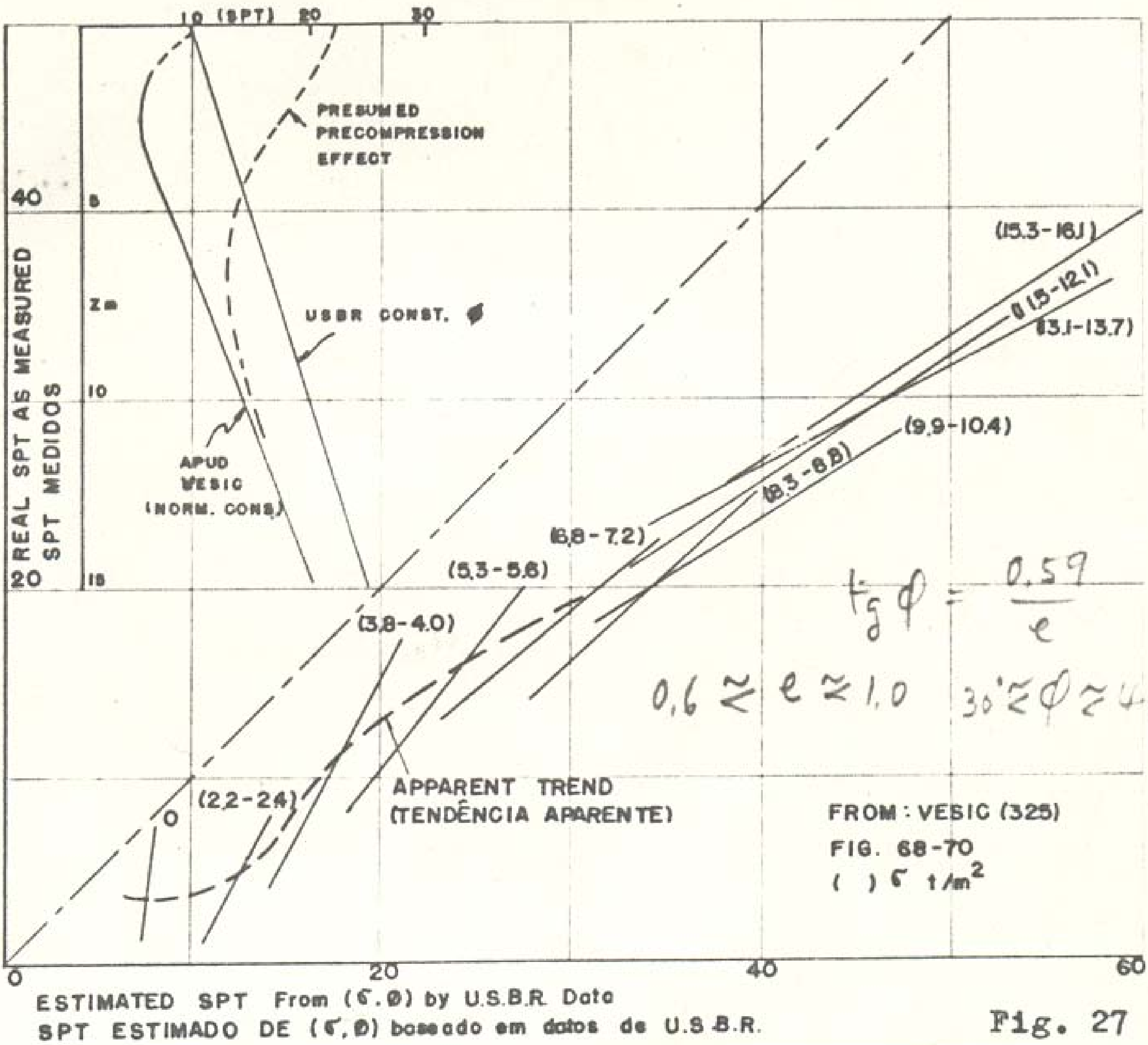

Fig. 27

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that there appears to be a trend of variation with depth including a locally optimized penetrability at a few meters, and that all results (up to depths of about 15 m) interpreted directly from the USBR data in a normally consolidated sand of a given density would lead to considerably higher estimated SPT values than are really measured. Note that precompression effects may, as discussed in item 3.5 (Table) on P.44 of the report, push the SPT vs. z curve more to the right near the surface than at depth, if the Ko effect of precompression depends on ratios OCR.

Brief mention must be made, both of the papers presented to the Conference (Vol. II) and of the specific discussions received to the Report (which was completed before receipt of any of the Conference papers) in stimulating discussion to the topic. The writer will attempt to group the contributions within the following general subdivisions.

1. Preference for pseudotheoretical treatments connecting SPT with resistance, through impact energy.

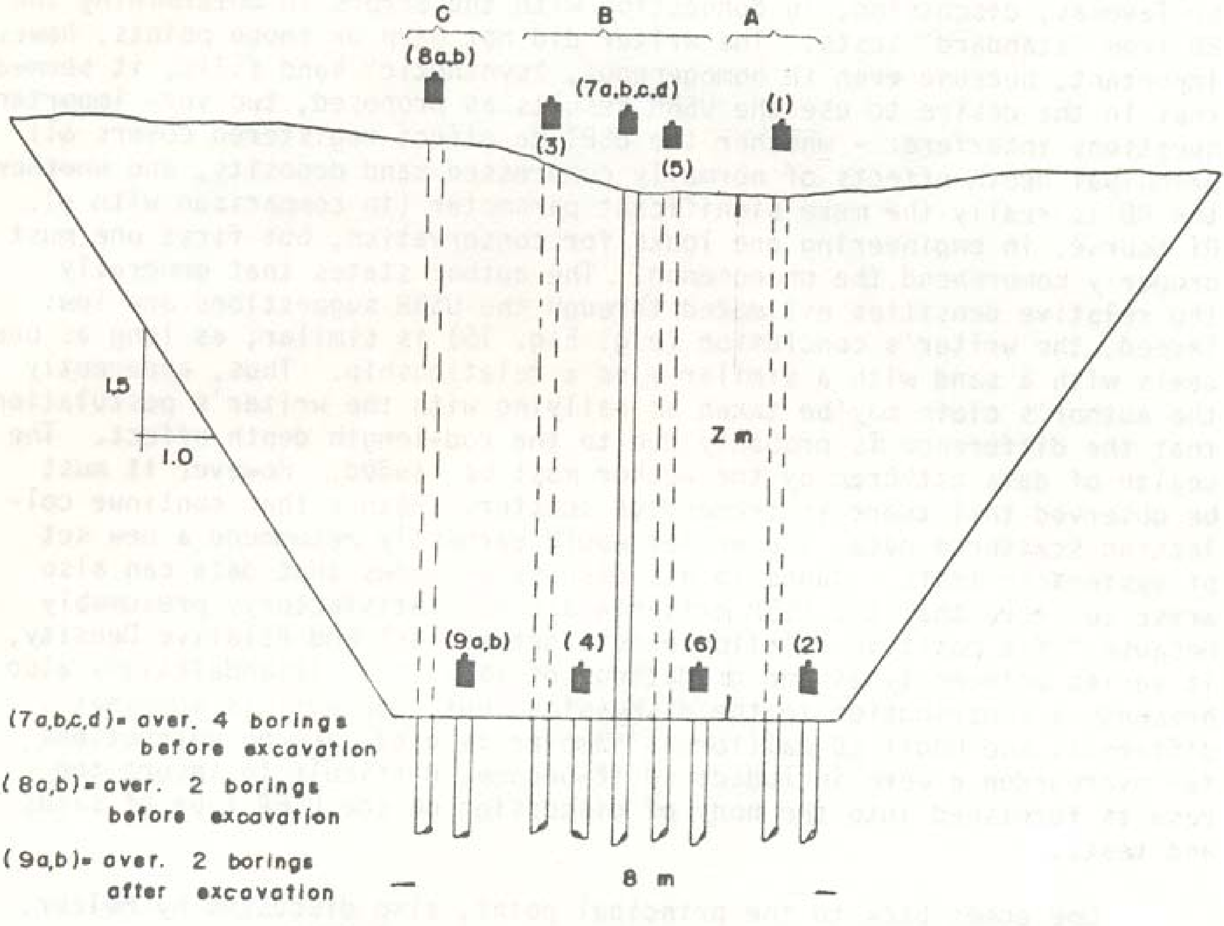
This was one of the main points of the Report, and apparently many of the pronouncements follow the same direction. Dobry (II. p.1) in his paper on the dynamic cone uses similar considerations to show that in comparing its index Nc with the SPT, the difference Nc-SPT is a better index than the ratio Nc/SPT. Indeed one notes that the author includes consideration of strata wherein it is presumed that the steady increase of SPT with depth would connote a constant RD, in accord with the USBR data: the writer would appeal to the author to reexamine his data in the light of the suggestions brought forth in the state-of-the-art paper (e.g. Fig. 27 above) hoping that assumptions on relative densities of strata may be substituted by bonafide test values, including in situ values, for closer appraisal hopefully more fruitful. Nowatzki (II p. 63) follows the same trend purporting to establish "a theoretically correct three dimensional static analysis of the SPT, using plasticity theory and the Coulomb failure criterion": the writer feels that the trend is very promising, and that modern computational facilities lead to early prospects of optimizing spoon penetrometer design for the purposes suggested in the report. It must be noted however that since the SPT is really a measure of penetration energy and not of force, the static analysis is only a small part of the problem, and the realistic assumption concerning the friction forces acting on the inside wall of the sampler (and the soil plug formation) may be far from the author's assumption that led them to be neglected. Schmertmann in his discussion strongly supports the same trend, showing the importance of side-wall friction in determining the resistance to penetration of the sampler. Incidentally, considering the research facilities and enviable abilities the author musters, the writer ventures to hope that the pseudotheoretical correlations between the SPT and the cone penetrometer Rp may be further investigated under especially controlled conditions: in insensitive s = c clays if we accept roughly Rp $\simeq$ 2 SPT (would depend on depth) there is a patent discrepancy with the presumed rough correlations SPT $\simeq$ 16 c (depends on depth) and Rp $\simeq$ 10 c; in sands it is not known for certain that Rp measures the constant "deep" point resistance representative of the stratum's density and ϕ, depending on how many diameters of sinkage of the cone are required, at any position of testing, to establish such a limiting value.

Reginatto's paper (II p. 77) although connected with a special case such as collapsible soils, also concludes that despite difficulties in interpretation, the SPT "is related to the in-place shear strength of loessic collapsible soils, but only for the values which correspond to the moisture content at the time the test is made."

Peck in his discussion supports the same reasoning directly, and indirectly applies it upon emphasizing the direct correlation by Hendron, between SPT and shear modulus: however, the reference to the direct correlation by Gibbs (II p. 27) between SPT and tendency toward liquefaction would already extend far beyond the writer's reasoning since it was anticipated by the writer that liquefaction should be influenced, quite as significantly, by factors other than merely the sand's ϕ value or density. As regards energy corrections for depth effects the writer fully agrees with Peck that other more realistic and refined analyses (such as wave equation etc.) will be much more fruitful than the simple Newtonian impact equation used by the writer, conjuring the pile formulae soil engineers are so familar with: indeed, the writer's function was presumably to report on published information and it was not without dismay that it was discovered that not even the crude demonstration of the inevitability of depth effects had been reflected in the prescriptions on consistency and denseness evaluation through SPT. The writer's function, unfortunately, could not at the time be extended to include the development of a paper on energy corrections: his aim and conclusion, in attempting to sort out the Babel of discussions on SPT, was summarized in the statement "pending further and more meticulous investigations ..... one must assume in all statistical comparisons, etc., that there should be a significant effect of length of rods ... (and) special attention must be paid to the very rapid changes (and therefore erratic results) over (shallow) depths". Incidentally, since pile formulae were mentioned, and a vast context of criticism may be conjured by association, the writer wishes to emphasize that the analogy may indeed be rather small: in the case of piles one is interested in energy-penetration equations when the pile is near refusal (and thereupon lies the bulk of the criticisms) whereas in the case of SPT one is generally interested in energy-penetration functions when the penetration is significant, in loose and soft soils. Finally, since Peck qualifies some of the writer's data on sands, on the claim that usually clayey sands are concerned (which is true as regards, for instance, Fig. 26) it must be explained that the Usiminas sands (Fig. 22) on which depth effects were discussed are pure sands.

Melzer's discussion on below vs. above water table corrections, both for the SPT and for static penetrometers, is indeed disconcerting to the writer. The writer's concept (in the statement quoted by the author) was (and is) that when the field of use of one crude test has been explored, the only appropriate solution is to set it aside and to resort to another type of test that precludes the limitation (e.g. when the moot question is dynamic effects, a static test may be employed). But the author claims that the static test also suffers from a significant change from above to below groundwater level: which, in the writer's opinion, merely serves to enjoin the author to postulate, develop, and confirm, minimal theoretical explanations for the discovery, rather than merely to obtain random linear single-parameter statistical regression, so that the phenomenon may be understood, and thereby stoodover.

Two discussions (Godoy, and Videla and Nadeo) have been put forth in immediate consequence of the state-of-the-art report, in attempts at confirming the depth effects as postulated. The writer hastens to re-emphasize that the real energy and rod corrections may be quite different and more complex (cf. Peck, above), and that an important rejoinder is that in order to confirm variations of smaller order one would probably have to use carefully controlled "laboratory" test conditions. The writer summarizes in Fig. 28 the data similarly furnished by a local railroad which in the course of relocation work comprising significant cuts under



EFFECTS OF EXCAVATION AND ROD LENGTH

EFECTOS DE ESCAVACION Y DE COMP. DE BARRAS

Fig. 28

SECTION	PERFORATION;m	1	2	3	4	5	6	7	8	9	10	11	12
A Z = 10	RATIO (1) : (2) DEPTH ALONE	0.86	1.33	1.14	1.50	0.75	1.33	1.31	1.12	0.96	1.16	1.36	0.93
B Z = 12	RATIO (3) : (4) DEPTH ALONE	2.00	1.43	1.37	1.37	1.25	1.17						
	RATIO (5) : (6) DEPTH ALONE	1.43	1.33	1.15	1.20	1.07	1.33	1.87					
	RATIO (7a,b,c,d): AVER,(3),(5)	1.10	1.22	1.37	1.54	2.08	Possibly excavation release alone						
	RATIO (7a,b,c,d): AVER. (4),(6)	1.83	1.69	1.73	1.94	2.36	Depth and excavation release						
C Z = 12.5	RATIO (8a,b): (9a,b)	1.79	1.59	2.33	1.71	1.90	1.69	Depth and excavation release					

existing highways (substituted by viaducts) was able to repeat borings under conditions indicative of stress-release and rod-length depth effects: it concerns a fine sand, very slightly clayey (weathered sandstone) above water table, with $6 < SPT < 25$ using rods 3 kg/m.

2. Preferential significance of ϕ vs RD in SPT of sands. The paper by Gibbs (II, p. 27) supports the use of the USBR correlations for direct determination of RD of sands. Of course there are serious criticisms to the use of RD as explained by Peck in his discussion in considering important aspects of sand deposit formation, and as further discussed by Tavenas, discussion, in connection with the errors in determining the RD from "standard" tests. The writer did not harp on those points, however important, because even in homogeneous, "synthetic" sand fills, it seemed that in the desire to use the USBR results as proposed, two very important questions interfere: - whether the $dSPT/d\sigma$ effect registered covers all principal depth effects of normally compressed sand deposits, and whether the RD is really the more significant parameter (in comparison with ϕ). Of course, in engineering one looks for conservatism, but first one must properly comprehend the phenomenon. The author states that generally the relative densities estimated through the USBR suggestions are low: indeed, the writer's conclusion (e.g. Fig. 15) is similar, as long as one deals with a sand with a similar ϕ vs ε relationship. Thus, apparently the author's claim may be taken as tallying with the writer's postulation that the difference is probably due to the rod-length depth effect. The wealth of data gathered by the author must be lauded. However it must be observed that there is tremendous scatter. Rather than continue collecting scattered data, the writer would earnestly recommend a new set of systematic tests. Janes in his discussion shows that data can also arise to prove that the USBR criteria are not satisfactory, presumably because "if a positive relation exists between SPT and Relative Density, it varies between types and gradations of sand ..." Triandafilidis also presents a contribution to the discussion, but the soil is somewhat different, the Modified California sampler is used, and no corrections for overburden σ were included; so it becomes difficult to insert the results furnished into the body of discussion on the USBR type of sands and tests.

One comes back to the principal point, also discussed by Melzer, regarding the choice between direct correlation of (SPT, σ) vs. ϕ or (SPT, σ) vs RD. To begin with, the question as to which σ to use affects both procedures in exactly the same manner: so the eventual or real difficulties in estimating which σ to use obviously do not affect the choice between RD vs. ϕ as the preference. It seems most practical to retain the overburden σ as the basic reference, and to obtain good empirical statistical correlations for various conditions of precompression (OCR values). If ϕ is the more directly significant parameter in affecting SPT as a function of σ, it is conceptually impossible to accept any other sequence than (SPT, σ) $\rightarrow \rightarrow \phi \rightarrow$ RD for the general case, since Fig. 14 well shows that there is no general relation between ϕ and RD within acceptable confidence bands. Incidentally, proposals such as Melzer's (disc.) may find comfort in recognition that the desire to proceed directly (SPT, σ)$\rightarrow$ RD is not thwarted, since all that it means is that a single average $\phi = f$ (RD) relationship has been assumed, therefore inexorably carrying the errors pertaining to such an assumption: incidentally, in engineering one may frequently fall back to such a decision, but it would be a gross mistake to illude oneself with the supposition that an appropriate theoretical concept has been embraced.

Melzer's paper (II p. 37) suggests as an interesting new concept the relevance of the parameter of "compactability": it would be necessary to conduct systematic tests to confirm the significance of the suggestion.

3. Direct correlations with settlement.

Peck in his discussion seems to indicate, with reference to the Terzaghi-Peck "design curves for proportioning footings on sand" that there may be a "direct correlation between the standard penetration resistance of a sand and the soil pressure required to produce a footing settlement of approximately 1 inch." Indeed, if distinct individual correlations are visualized for essentially each sand deposit, the writer fully agrees; but if there is any risk that it be concluded that a usable generalized relationship may prevail for all sands (as has hitherto oft been glibly stated), the writer reemphasizes his strong recommendation to guard against such a pitfall. Once the pseudotheoretical approach has been accepted to indicate that SPT is fundamentally related to (σ, ϕ), one cannot fail to comprehend that settlement correlations with SPT cannot prevail within acceptable confidence limits. To this purpose was Fig. 26 developed from published information, presumably on pure sands and on compatible SPT determinations, to show the wide scatter of "individual cases." Indeed, as pointed out by Peck, the writer's own data inserted in this specific figure concerns clayey sands (skip-graded "dirty" loose sands, but probably mostly precompressed), but this was so done not to imply any connection with pure sands, but to show that data on a given eminently sandy material may be developed within satisfactorily narrow confidence limits.

Langfelder and Johnston (II p. 15) present a case concerning poor predicting capacity of settlements of tanks on the basis of published "prescriptions." Incidentally, some of the details within the paper may be qualified; for instance the authors mention that water table corrections were used, and one notes that more recently this has been considered unwarranted (and probably justifiably so), since any submergence effect on settlements, through overburden effective stresses, should also have made itself felt in an effect on undrained resistance indices of SPT blowcounts.

The basic problem is conceptual. When one wishes to investigate the precision associated with a test and procedure, the best way is to check how closely it reflects specific parametric changes of the behaviour one is interested in. Preloading of tank foundations are the most truly relevant cases. The writer had hoped to be able to present herein one such case, but unfortunately the data havenot yet been formally provided. Through SPT and static cone Rp investigations it was estimated that a set of 40m diameter reinforced-concrete water tanks would settle 20 to 30 cm, because of loose micaceous residual sandy silts. A preloading solution was adopted; settlements were measured, were rapid, and essentially of the order of magnitudes estimated, but upon repeating the borings after the precompression, one had to concede, humbly, to the fortunate interference of a degree of luck and eventual intuition. The tanks as finally constructed and loaded settled, in recompression, but a centimeter or two. And, honestly, it could not be hoped to be otherwise, because there is an important difference between a direct treatment effect and an indirect index to evaluate it. Very small differential effects of $\Delta\varepsilon$ and $\Delta\sigma_m$ in a multitude of soil elements accumulate to absorb the compressibility and deformability potential that results in settlements: how precisely can present penetrometer results record such small differential effects without being completely hampered in their basic task of subsoil exploration? Obviously, therefore, precompression effects should be statistically incorporated through adjustments in the "constant" used in the Buisman-type formula for computing settlements from Rp.

4. Miscellaneous.

Papers and discussions have continued, and will continue, to come forth on miscellaneous topics that the writer could not possibly broach in greater detail. A concomitant arena for such pronouncements has been Geotechnique (e.g. Vol. XXI, nr 2, June 1971) as a result of ref. 126. Hedges (II p. 63) presents recommendations on the use of drilling muds; Napoles Neto (II p. 85) repeats earlier discussions and appeals; Arce et al. (II p. 95) would extend the scope of the writer's directed preoccupation to include the dynamic cone (incidentally, the so-called Burmister equation may not be used for changing from one energy and sampler to another, as should transpire from the Report and discussions - e.g. Schmertmann). Schnabel (discussion) makes proposals with respect to seating: the writer feels that the penetration phenomenon is continous, and therefore any "arbitrary seating" can be applied and adjusted, the problem being to avoid "erratic errors." Finally, Moretto (discussion) repeats earlier pronouncements recommending the "different and improved sampler" often used in Argentina. The writer could not embrace other penetration test samplers into the scope of the already heavy task entrusted to him. Indeed the writer strongly favors the development of more appropriate samplers, but only if such development is pseudotheoretically oriented (cf. Report). While such development does not come forth (and the Moretto suggestions do incorporate interesting suggestions, but do not cover the range of revisions really necessary) it is conscientiously correlated through judicious statistics, with the crude but well-known SPT so that temporarily a common minimal language of reference may be used.