54.1

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The paper is to be commended as another
attempt at providing the foundation engineer
with a single test procedure for the impor-
tant evaluation of the coefficients of sub-
grade reaction of cohesionless soils, and
because some probabilistic reasoning is
introduced.

However, I find it necessary to raise some
points that to me appear crucial, as discus-
sed in my State-of-the-Art presentation
"The Standard Penetration Test", Vol.1 p.1,
Fourth Panamerican Conference SMFE,
Puerto Rico 1971, as well as the closing
discussion on Vol.3 p.104. I excuse my-
self for repeating my own earlier work:
I would have been quite satisfied if the
authors had quoted it as a reference (the
most recent references cited are roughly
1967!) and had curtly stated their dis-
agreement with such and such. I am sure
that there is much room for disproving and
correcting some of my preliminary claims:
but, since they were offered with the best
of intentions and were aimed at avoiding
unnecessary effort and pitfalls, the thing
that I find quite disconcerting is that four
years can pass without agreement or contes-
tation but only a disparaging silence!

The main points I should like to summarize
as pertinent to the present paper are:

(1) It appears that the SPT as applied at
    very shallow depths (plate bearing test
    and footing pressure bulbs) may be sub-
    ject to rapid energy (and penetrability)
    variations, making it an unfortunately
    erratic index for such use. At any
    rate, it is doubtless overdimensioned
    and therefore crude for such use.

(2) The best possible correlations that
    could be sought would be directly
    from one parameter to the other, with-
    out going through intermediate ones:
    so, for instance, direct correlations
    of K with SPT rather than Dr with SPT
    and thereupon K with Dr.

    However, in any attempt at correlating
    K with SPT, which is really an attempt
    at correlating strength with settle-
    ments, no very close correlation can
    be expected. Precompression is one
    factor that causes profound variations.
    By precompression the settlement
    (recompression) may be reduced so as to
    be imperceptible: indeed, concomitant-
    ly there is a void ratio decrease
    that should be reflected by an increase
    of strength, and thereupon SPT should
    also change; the problem, however,
    is that settlement is a cumulative
    effect of infinitesimal individual
    compressions, and meanwhile the local
    individual compressions may be too
    small to reflect any perceptible change
    of strength.

Such precompressions have been dis-
cussed with reference to presumed
"locked-in" lateral stresses (affecting
strength and SPT), which is unfortu-
nately another instance of reasoning
indirectly through stresses when we
are concerned with strains for settle-
ment: the problem is that in decom-
pression very small strains may be
sufficient to release the presumed
stresses (especially in sands at very
shallow depths), of which we really
know nothing.

In my experience, with many cases of
very successful preloading for tank
foundations, etc. I may say that set-
tlements were reduced to "zero" but
never have we been able to detect any
change of SPT in the after as compared
to the before state (nor, for that
matter, of the much more sensitive
static cone penetrometer R_p).

(3) The third very important concep-
    tual problem concerns the
    definition of the Relative Density,
    Dr, parameter. Whereas it appeared
    to have arisen from a valid concept,
    similar to the Consistency Index IC,
    one should note the very important
    difference, that is that it is depen-
    dent on the difficulties of defining
    and measuring extreme values: so
    the problem is not merely one of test
    precision and errors in concept of
    statistics of averages (cf. Tavenas
    and others, Canadian Geotechnical
    Journal) but rather one of statistics
    of extremes. The parameter must be
    considered badly defined.

(4) Finally, it is rather unfortunate and
    incomprehensible that such a prelimi-
    nary suggestion with regard to cate-
    gories of Relative Densities as made
    by Meyerhof twenty years ago, should
    not have been gratefully received as
    a first hint, and duly revised sub-
    sequently. The Civil Engineer's
    trend towards linearization cannot be
    submerged! How can there be any
    meaning to a classification of relative
    densities varying linearly as very
    loose (0-20),loose (20-40),medium
    (40-60) dense(60-80),and very dense
    (80-100), when most properties would
    change exponentially such that, for
    instance, the more appropriate quali-
    fications could well be (0-50),(50-75),
    (75-88),(88-95) and (95-100)?

In short, the concepts behind the
effort and subsequent paper lead me to
suspect that big dispersions and errors
continue to be inevitable, and the
results will only prove applicable as
prescriptions, and not as correlations
for predictions.