VICT[illegible] MELLO

International Symposium on Engineering Geology and Underground Construction. Lisboa, 1983. Theme 3: discussions. v.3,p.74-75; 77-79; 80 e 83.

— Comment V. de MELLO (BRAZIL) - General and Panel Reports

We are all convinced of the importance of case histories for developing our reasonings, and also of the intrinsic value of further and further geologic studies towards reducing unknowns, risks of unknowns, and costs of such risks. And we all recognize the importance of geology, the more the better: that is why we are gathered at such a conference! Let us not make the mistake of speaking within our closed circle, to our ownselves: it is to our clients that we must speak, and convincingly. In order to do so we must have the courage to separate some of the adulterated data that most often surround us.

The problem lies in how to develop quantifiable data, for adequate comparisons. It is sometimes doubtful that one may treat real case histories in such a manner as was done by Rodrigues de Carvalho and Conceição David in their otherwise excellent paper, without being conditioned by some inevitable variations that occur with time. Borings carried out in 1970 and in 1980 are not similar, do not belong to the same statistical universe, because we are forever purposely changing, improving; designs conducted in different years may not have followed the same design criteria.In order to reach adequate comparative conclusions we must maintain factors of safety, design criteria, and often, even subjective individual judgement factors, sufficiently fixed, as is the case of analysis of partial differentials $\partial X/\partial c$, when a function X is very complex and dependent on multiple variables, a, b, c, d, e.. .. y, z.

Therefore, in order to carry out convincing analyses of benefit/cost ratios or incremental benefit/cost comparisons on case histories, we have to work with mental models of "partial stages" of investigation belonging to a real case history as "finally resolved". And we have to apply nominal (fixed) conditions rather than "real" conditions, because in reality there are deterministic contractual interferences that do not allow an objective comparison: for instance, a given change of design should be favourable, but a contractor can claim all kinds of reasons to make it very much more expensive; multiple are the similar real conditions that viciate the adequate analysis of a real case if it progresses stepwise.

On the other hand, a case already completed with, say, 100 borings and all internationally applicable "nominal prices", can well be subjected to analyses, by computer (objective, using objectively constant criteria), under the hypothetical first 50 borings, further 20 borings, and further 15 borings. Moreover, such analyses can well be conducted under statistical and probabilistic procedures, selecting the sequence of borings at random.

The point is that although we emphasize the importance of analysing case histories, in order to avoid chaotic conclusions, or conclusions dominated by subjective and/or wishful thinking, it is even more important to run such case-history back-analyses in a nominal manner, objectively, expurgating the inexorable subjective and deterministic reasonings.

(H. Dias - Portugal)

— Comment - V. de MELLO (BRAZIL)

When we discuss insurances with respect to contingencies, I believe we must honestly recognize that if insurance companies are shying more and more from accepting insurance risks on underground projects, it is in large part because of a disservice that we are doing to ourselves in allowing society ~~of~~ to labour in misconceptions regarding failures vs. misbehaviors, and engineering vs. engineering science. We have already accepted that there is nothing deterministic, no clear-cut right-wrong in engineering: we face problems of statistics. But we have to guard against spurious statistics. For the sake of statistics, which is the backbone of conscientious insurance policy, it is necessary to develop the histogram of the continuum of reality: that applies to repetitive conditions, of different degrees of insufficiently satisfying behavior, or of misbehaviors. For this histogram to be reasonable we have to exercise the right and obligation to recognize and exclude the extreme outliers, the accidents, the catastrophic failures, the "acts of God", the occurrences which humans accept with humility, because to try to face them would be too difficult, too expensive, ever frustrating, to some minimal degree, however minimized.

Secondly we must recognize that with a histogram well determined we shall define adequately the average behaviors, and our abilities to predict what should happen: that is the noble aim of engineering science. But engineering is Decision, the discontinuity of yes-no, of accept-reject: and it is much more based on the ability to predict and guarantee what will not happen. All I need to know is that the displacements will not be higher than x cms, I do not really need to refine precisions regarding their being 0.3 x or 0.5 x. In other words, engineering decisions are taken on the basis of "limit solutions", that is, bands of confidence limits (statistical). Thus, after exclusion of extremes that are so singular or rare as not to permit establishing repetitive frequencies, our efforts have to be towards incremental benefits in narrowing the width of confidence bands; and towards judging such efforts on the basis of the incremental costs taken to effect such narrowing. Mind you, hopefully such narrowing will really revert into a profitable ch... (improvement) of design decision: but such gains are definitely not continuous. We may well push forward from 50 borings to 100 without uncovering any "new information" such as to force us to change a design decision, and yet in proceeding from boring nr. 100 to boring 101, there may be a sudden (discontinuous) change of design decision.

All such concepts may be statistically formulated, to permit insurance companies to accompany our efforts and failings: but the first indispensable step is for us to clarify such concepts within our own profession.

(Mineiro, A. - Portugal)

(Pistone, R.S.)

— Question - MELLO, V. de (BRAZIL)

We recognize two distinct phases of study, of greatest interest and need. Firstly the adjustment of parameters and computational models and methods, so as to be able to predict deformations (or other behavior) reasonably: apparently your paper claims to have achieved success in this first step, since the observed displacements were of the order of 2 mm, as predicted. The second problem is one of decision: how acceptable are the displacements predicted and observed. In other words, if the displacements had resulted 0.1 mm, or 5 mm, or 20 mm, instead of the 2 mm, would you kindly comment on what design decisions would have suggested a need to change ?

(Pistone, R.S.)

(De Freitas, M.H. - United Kingdom)

— Comment - de MELLO, V. BRAZIL)

In partial reply to Dr. De Freitas, before putting his question to the Panel and the floor I take the liberty to mention that maybe such a question put to the client will get a positive reply if he has been satisfied. But it is intrinsic to human nature that we ourselves are forever trying to push forward the frontiers of impunity. Therefore after a project has been successfully completed, if we ask our own inner selves if we would repeat exactly the same solution, I am sure most of us would reply with reservations. In my own 35 years of professional experience, fortunately satisfactory and satisfying, I have never found a single project that I myself would not have solved with some slight improvements if I were to face the very same problem again. That is the reality of the forces that impell us forward.

Will anybody else offer further comments, please ?