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Co-Reporter's Remarks — Session III

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1.0 General

The papers fall into three categories, namely, those which are essentially case histories (30%), those which set out design/analysis procedures(40%) and those of a research nature which afford insights into the phenomenon of liquefaction (40%).

There is an increasing awareness of the uncertainties involved the predictive route and the need to introduce risk concepts. There is also interest in estimating post liquefaction settlement. However the recent comments of Lomnitz(1994) on the ubiquity of liquefaction in particulate systems does not seem to have provoked new ways of thinking in Session III although there could be some reactions in Session I.

2.0 Specific comments

My specific comments are restricted to a selection of the papers. In the paper by Armijo et al (p4) a somewhat questionable statement is made: " From a practical perspective, this means that the problem has to be dealt with by real specialists, or it is necessary to use a computer program of the expert system type, such as the one proposed by Armijo et al.(1994b)". I would not like to rely on **any** computer system, expert or otherwise, in any geotechnical process, without the guidance of a specialist in the process itself. As regards the value of ground improvement by deep compaction it would be useful if Mollah (3.38) can comment on the behaviour of the deep dynamically compacted hydraulic fill at the Ashuganj Fertiliser Plant on the Meghana River, not far from the location of the massive liquefaction he reports. In 1976-1977 following Lomnitz and Cornell seismic hazard modelling I estimated that for a plant life of 24 years the PGHA at the 20% risk level would be 0.16g. Deep compaction with 5-10 x 10^6 Joules per blow on a 10m grid with 8 passes gave typically an improvement of q_c of from 40 to 80bars at a depth of 10m.

Keeping with the theme of ground improvement, the paper by Raison et al considers a model for the behaviour of a piled foundation when liq ufaction does occur. Some more details on the "appropriate" computer programs used would be helpful. There is an important generic issue here in that the analysis and **design** of piles in ground, which in its upper zones suffers severe stiffness degradation and even liquefaction, is very uncertain territory, more so if the the piles become pile groups or large diameter caissons which cannot be assumed to deform with the free field soil profile. There is a dearth of observational evidence on the behaviour of piles where the upper 10-20m is vulnerbale to stiffness degradation and Nigata stands out still as the best example of the effects of lateral ground displacement.

Miura(3.06) by investigating the effects of fabric anisotropy opens up an interesting issue. Anisotropy is always an acute embarressment for the geotechnologist as it is for the geophysicist. We know it exists in nature but very often we are forced to ignore it or treat it simplistically. Recognition of fabric properties in natural deposits is now an active issue among sedimentary petrologists using SEM techniques. The improved methods of coring in soft sediments may help proper recognition of such fabric. State of the art wireline rotary coring with polymer muds can capture disrupted fabrics and so open up the possibility of better palaeoseismic studies.

Pelli et al make the useful suggestion that reconstituted specimenns in the laboratory should have Vs values matching those measured in situ. This is in accord with the increasing weight being placed upon in situ Vs determination. The in situ assessment of anisotropy by polarised shear waves presents formidable problems in practice. Should it be carried out? Is it significant against all the other uncertainties?

The continuing work on soil models (Hwang et al,Saitta et al) is leading to better matching with the results of laboratory tests and tests on research sites. However the utility of such refined models for design engineers, even with the reduced number of parameters is worth consideration. In most cases they serve to inform expert judgement on the bounds of real soil behaviour but several important matters do not seem to be addressed in current models which inevitably concentrate on failure conditions. Such questions as the deformabilty under excitation which is random in time and orientation and with principal stress reversals, are not explictly dealt with. And there is the whole issue of the ubiquity of soil structure, which embraces early diagenetic fabric and age which would be expected to be significant in the small strain stffness and the nature of a transition from a contuuum supporting elastic wave propogation to a quasi-fluid body without support capabilty.

Much of this will be covered in Session I but the onset on non-linear behaviour and the nature of the transition to a "liquified state" and back again has signifcant implications for safe and economic design of deep foundations.

I was especially interested in the stochastic approach in Popescu et al noting that the choice of the distribution function for the underlying random variables is stated to have a signifcant influence on computational results. They show in their Fig 2 a validation of the Gaussian distribution on the particular site. In view of underlying log normality in thedistribution of particle size in comminuted natural cohesionless deposits (see also Kolmogurov 1941) does this same Gaussian distribution hold for other sites?

A deterministic appreciation of inhomogeneity is embedded in the paper of Holchin and Vallejo. The linear elastic fracture mechanics approach seems promising but I would expect that the use of FLAC type numerical models to be more appropriate. In engineering practice the matter of tensile strength of earth material often arises in both drained and undrained situations. Was this parameter explored in the laboratory studies?

Goh with his neural network points to an attractive route which can only be made smoother by more relevant observational information. The question which must be asked is: In a training set are all the variables appropriate and sufficient? In view of the sigificance of fabric, diagenesis, inhomogeneity and age, do the 8 input variables cover these factors?

Kaya and Fang are proposing a novel geophysical method which could be useful in assessing the cementation factor. A suite of new hybrid seismic/electrical surveying methods (eg electro-kinetics) are being actively developed. Such methods of which TDR is an example need to be be carefully evaluated for their potential contribution to the better characterisation of potentially liquifiable soils.

3.0 Points for discussion

a) How far is our model of soil liquefaction which invokes excess pore pressure robust and fundamental?

b) How far can be go with expert systems?

c) Are we coping with uncertainty?

d) Are we properly and sufficiently describing and characterising soils for their liquefaction vulnerability?

e) In the field are we still looking out for phenomena we cannot explain and giving good descriptions?

f) How should we use soil models in design ?

f) How should be deal with the transition from degraded soil stiffness to liquified zones in the design of deep foundations.

4.0 Additional references

Lomnitz C 1994 Fundamentals of Earthquake Prediction, John Wiley & Sons Inc.

Kolmogorov A.N. 1941 Uber das logarithmisch normal Verteilgungsgesets der Dimensionen der Teilchen bei Zerstuckelung Dokl. Acad. Nauk. SSSR, 31, 99-101

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