

Editorial

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Geotechnical engineering covers a wide range of skills and a correspondingly wide range of journals caters for them. These range from *Geotechnique*, at what one might consider the ‘academic’ end of the spectrum, to *Ground Engineering* at the ‘practical’ end. The *Quarterly Journal of Engineering Geology and Hydrogeology* caters for those particular aspects of our discipline; and somewhere in the middle we find this journal, *Geotechnical Engineering*. In his introduction to the first issue, published in 1994, Professor Chris Clayton describes how, when in 1992 the Institution of Civil Engineers (ICE) originally decided to split its *Proceedings* into a number of specialist journals, geotechnics was not among them, because it was felt that it was covered adequately by *Geotechnique* and *Ground Engineering*. However, the ICE subsequently decided that ‘the *practical application* [my italics] of soil and rock mechanics – geotechnical engineering – was now a major part of civil engineering, and that such an omission was surprising.’ Professor Clayton continued: ‘The object of *Geotechnical Engineering* ... is to bring practical aspects of the art and science of geotechnics to as wide a civil engineering audience as possible’.

In this Editorial, I try and assess how we are fulfilling this objective. What sort of paper is going to help practicing engineers to design or construct works that fulfil their purpose and are safe and economical to build? One might hope for some case histories, but in a quick scan of recent issues I found very few. In this issue, the paper by Powrie *et al.* (2020) demonstrates the lessons that can be learned from a case history. An important aim, I consider, should be simplicity. It should not be necessary to use finite elements to analyse every problem; indeed, the use of complex software can militate against understanding, which can sometimes be better obtained by simple calculations. However, if one oversimplifies, one risks losing some essential feature of a problem.

Geotechnical engineers in practice do not generally have the time or the inclination to master in depth the more academic aspects of the discipline, but they do need to use and have confidence in its findings. One way in which they do this is by using sophisticated numerical software (such as finite elements) to analyse complex problems. The paper by Giardina *et al.* (2020) aims to assist engineers who may be uncertain as to the degree of sophistication that they need to employ in their

choice of soil model. The authors analyse the effects of tunnel construction using three very different conceptual soil models and compare the analysis results with the results of a centrifuge test. Different aspects of the centrifuge behaviour are modelled best by different conceptual soil models. I am sure that engineers having to assess the effects of tunnel construction will find these results very useful. I would have appreciated more of the authors’ opinions of *why* the different models gave different results, in terms of relating their mathematical formulation to the physical behaviour observed or modelled. They comment, for example, that the large horizontal displacements exhibited by the Mohr–Coulomb model next to the tunnel result from failure of the ‘soil’ at very low stress levels, but not why the real soil does not likewise fail. I also found it disturbing to think that none of the models used were able to model reasonably accurately all aspects of the real soil behaviour; although it is now more than six decades since Roscoe *et al.* (1958) selected Leighton Buzzard sand for their study of the fundamental behaviour of soil, on the grounds that it is a comparatively simple material.

The paper by Moore *et al.* (2020) addresses another important question in practice. The undrained shear strength of a soil can be used effectively in simple upper and lower bound calculations in design. But the choice of an appropriate value of strength to use can be very difficult, as it is known that many aspects of sampling and testing can affect the result. The authors have developed an artificial soil to enable them to quantify one particular aspect, that of unloading and reloading, which cannot, almost by definition, be addressed by means of laboratory tests on natural soils. But it would be useful, I think, to compare the authors’ findings with a similar study of real soils carried out by comparing the results of laboratory and in-situ strength tests.

The paper by Loli *et al.* (2020) seems to me to admirably fulfil the aims I set out above. The authors have taken a problem, that of assessing movements resulting from the thawing of frozen soil, which is difficult to assess conceptually and often, as they point out, complex geometrically. They have addressed the problem by one of my favourite techniques: breaking it up into its constituent aspects, each of which is susceptible to simpler analysis, and then bolting the various aspects together again. This approach does have the danger (as the authors

acknowledge) that the various aspects of the problem may interact in more ways than the analysis allows, but such interaction can often be allowed for, at least qualitatively. And the approach does have the advantage that it can be easier to identify the important factors affecting the problem than if they are hidden inside an all-encompassing numerical analysis.

The paper by Powrie *et al.* (2020) is an admirably honest account illustrating the point I made above that sophisticated software can hamper understanding. The authors describe how the program Wallap (whose purpose is the design of long retaining walls in plane strain) was used for the design of piles under lateral loads (for which three-dimensional effects apply), resulting in the piles being up to 50% longer than necessary, and contributing to substantial cost overruns. More reliable designs were obtained from an empirical method, based on full-scale tests, dating from 1957 and a limit equilibrium analysis method dating from 1961 and included in all the standard piling textbooks.

Finally, we have two papers that report on practical and innovative techniques, both of which should prove interesting and useful to specialists in their respective fields. The paper by Faizi *et al.* (2020) describes tests on a new type of suction caisson with wings. As the authors acknowledge, the tests are very preliminary, having been carried out on very small-scale models at 1g. They therefore need to be supplemented by centrifuge and full-scale tests, and by appropriate analysis. However, small 1g tests do have the advantage that it is possible to study the effects of a wide range of parametric variation comparatively quickly.

The paper by Thomas (2020) is not, strictly speaking, geotechnics at all, but it does deal with the design of an item, the rock bolt, which is widely used by geotechnical engineers; and the paper is written in the language of materials science, which

geotechnical engineers should understand. This language is used in relation to a new analysis method to give confidence in the behaviour of glass-fibre-reinforced polymer bolts in shear, an aspect that engineers had previously had concerns about.

We have then in this issue six papers, all of which contribute in different ways to the practice of geotechnical engineering. I think that the Institution should be pleased that its original aims set out more than quarter of a century ago are still being fulfilled.

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