

# SPECIAL REPORTS

## THOUGHTS ON THE FUTURE OF GEOMECHANICS RESEARCH IN AUSTRALIA

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The previous generation of geomechanics researchers in this country operated under conditions which differ significantly from those which apply today. In the past:

- (a) geomechanics was then an emerging discipline, with a wide variety of problems awaiting solution.
- (b) researchers had substantial freedom to choose a topic which interested them, and to pursue it as they saw fit.
- (c) they could work either as part of a team, or as individuals.
- (d) geomechanics research was relatively cheap to undertake, provided that the research infrastructure (such as a laboratory or a computer) was available for the researcher's use.

Today's circumstances are vastly different, and tomorrow's will be even more so.

Geomechanics is a maturing, if not entirely mature, discipline, and much of the primary research has been done. We are now in the secondary phase (or in some cases, even the tertiary phase) of research refinement in many of the issues related to traditional soil mechanics, rock mechanics, and foundation engineering.

Some freedom of choice of research topic remains, but researchers are increasingly constrained to seek topics which are "relevant to industry", and which will attract funding from grant-giving bodies. Such finding is now essential, since research is no longer "cheap". We are now very much in a "user-pays" mode of operation, and many of the overheads which used to be absorbed by the researcher's host institution are now costed and charged for. Increasingly, it is difficult to do individual research, as the increasingly complex requirements of computer programming, laboratory testing and field testing, make it almost mandatory to have some research assistance, either in the form of fully-qualified and highly-paid research personnel, or research students, who are also becoming increasingly expensive to support.

What then must the geomechanics researcher of tomorrow do if he or she wishes to pursue serious research and hope to be successful? I would suggest that there are at least four issues which must be addressed:

1. the selection of a suitable research area
2. the possibility of involvement in multi-disciplinary or inter-disciplinary research, rather than being confined to the traditional "furrows" of soil mechanics, rock mechanics, engineering geology, or foundation engineering.
3. acquisition of funding
4. participation within a research team.

The selection of a suitable area of research often poses less of a problem for established research groups than for individuals or groups who do not have a "track record". Many of the more successful groups have taken an evolutionary approach to their research, extending and building upon their past work in order to solve new problems. In such cases, the continuity of the research process produces rapid incremental advances in research output and a burgeoning body of publications. However, in some cases, it may also tend to inhibit the tackling of new and different problems.

The Australian Geomechanics Society has identified three broad areas of research which are considered to be of economic relevance to Australia and which offer potential for significant advances in knowledge to be achieved through research. These are:

- (a) problematic soils of Australia and the Pacific Rim
- (b) mining geotechnics
- (c) environmental geotechnics.

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These are broad areas which have global as well as local importance, and which can be pursued for both

short-term and longer-term objectives. While such a listing of research areas does not preclude other topics being considered, what we do not need is a third- or fourth-generation revisiting of tired topics for which the potential for new knowledge, new understanding, and economic benefit is minimal.

All of the above-listed research areas have inter-disciplinary components, and the old divisions between soil mechanics, rock mechanics and engineering geology should be set aside. The success of the Australian and New Zealand Geomechanics Societies is some evidence of such barriers becoming less rigid. We should however be prepared to work outside even these broader boundaries of geomechanics and embrace disciplines such as chemistry, biology, seismology, geography, soil science and agriculture into our research thinking and our research teams. Indeed, in an area such as environmental geotechnics, such a broadening of the research skills base is mandatory.

We must also recognise that at least some of our research in the future will need to be focussed on geological and geotechnical conditions and problems in specific geographic areas. Such research may certainly be more applied than fundamental, but is still very important and can be validly classed as research.

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Research funding is available (at least nominally) through Government and private sources. Government funding, such as that through the Australian Research Council, is increasingly competitive and difficult to obtain, as evidenced all too clearly by the mere 19% success rate of new applications for 1993. Industry in Australia has been reluctant to fund research, except for limited projects of immediate relevance, and it is likely to be some time before a more research-oriented culture is developed. Consequently, the leader of a research team will perforce have to spend time and effort in seeking appropriate funding, and less actually doing the research.

***"Basic research is when I'm doing what I don't know I'm doing".***

This then leads to the question of research teams. Few individuals can expect to obtain consistent research support in the future unless they become part of a coherent research team. Indeed, because of the funding constraints mentioned above, even

established research teams may have increasing difficulty in obtaining funding for their activities. Consequently, it may well be necessary in the future for both individuals and groups to "joint venture" in research projects and combine their resources and skills in order to further their research aims. The Co-operative Research Centre scheme is a mechanism available for such an approach, and also for extracting industry funding for research. Its long-term viability, however, remains to be proven, since research partners are often separated by vast geographical and ideological distances. Nevertheless, joint-venturing, if carefully and rationally planned, should be a means of undertaking larger-scale projects and enabling groups to pool their resources and "think big" (or at least bigger) in terms of developing both laboratory and field testing programmes.

In summary, we must recognise that, in the foreseeable future, geomechanics research will need to address subjects which have at least some measure of short-term relevance to Australia and to the Asia-Pacific area. Increasingly, we must be prepared to amalgamate, joint venture, and form teams which include both traditional geomechanics researchers and those in peripherally-related disciplines. We must be prepared to engage in more coherent and focussed research programmes rather than individual (and often unrelated) research topics. At the same time, we must not completely inhibit the individual researcher who insists on doing his/her own thing, or the person or group doing fundamental research which may not have an immediately foreseeable application. The famous scientist Werner von Braun defined such research as follows:

***"The outcome of any serious research can only be to make two questions grow where only one grew before".***

"Basic research is when I'm doing what I don't know I'm doing". One of the challenges we face in geomechanics research is to make provision for such efforts to continue within the more short-term result-oriented environment in which the majority researchers must operate.

Finally, how can we measure our success as researchers? Clearly, one way is to obtain quick answers to immediate questions, and this appears to be the measure by which contemporary research is being judged. However, a more satisfactory measure, and one which will ensure the survival of meaningful research, is provided by the 19th century American economist and social philosopher, Thorsten Veblen, who wrote:

"The outcome of any serious research can only be to make two questions grow where only one grew before".