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Dear Sirs,

Deformation moduli and rock mass characterization

A recent article in your journal by Palmstrom and Singh (16, 2001, 115-131) drew attention to the difficulties of interpreting the results of plate jacking and plate loading tests. Although one may have reservations, the article is a useful contribution to the literature. The authors compared some of the earlier and more recent classification methods e.g. RMR, Q, and RMi (the latter developed by Palmstrøm) and the degree to which they correlated with the measured results from the reviewed loading tests. A potential weakness of course is the correctness of the rock mass characterization at each test site, but collection by mostly one organization may have minimised this source of error.

Two of the older correlations between deformation modulus (which we can refer to as M) and RMR and Q, date from Bieniawski, 1978 and Barton et al. 1980. These were specifically for rockmasses at the higher end of the quality scale, namely RMR>50 and Q>1. Naturally their development was limited to the data base that was used at that time. The error introduced if attempts are made to use such correlations outside the intended range of the data base are clear, and hardly need to be emphasised. The authors' Table 3, showing the effects of varying uniaxial strength from 4 MPa to 200 MPa was a clear example of the inadequacy of the 20 year-old Q-relation as a *general formulation* for all strengths of rock. This is because uniaxial strength did not appear explicitly in the 1980 formulation, as it was only designed to estimate moduli for rock masses with Q>1. As a basis for distinct element modelling of medium strength rock masses, it has proved very successful.

The Norwegian first author is aware of the published improvements and generalizations made between the Q-value, deformation modulus, and seismic velocity. He was a guest, and seminar participant at NGI in the same period as their development. Unfortunately, these widely published developments, including two ISRM congresses and an international symposium in India, were not included in the Palmstrøm and Singh

2001 comparison of classification methods. The first author has recently published a book that includes this topic, but he has ignored these developments.

The improvements and generalizations of the Q-value, to help it correlate with other features than rock bolt and shotcrete quantities, were first published by Barton, 1995. These included a simple uniaxial compression strength-based normalization of Q to the form Qc, and allowance for the positive effects of depth or stress on modulus (and Vp), and for the negative effects of porosity (on both M and Vp). Some of your readers may not be aware of this integrated method, so the first reference to its development is appended.

This topic raises something more fundamental about rock mass classification methods. Should 'internal' and 'external' so-called boundary conditions (i.e. water and stress) be *included* in rock mass *characterization* of a pre-excavated, virgin rock mass? It seems to be generally agreed that their inclusion *is* needed when performing rock mass classification for preliminary design of reinforcement and support in an underground opening – which will often be significantly affected by both water and stress.

Because Palmstrøm's RMi method, presented several times in your Journal, does not include these internal and external boundary conditions in the derivation of RMi, he is promoting the belief that this is the best approach. A recent report in the ISRM News Journal from a rock mass classification workshop in Australia in 2000, for which he was a co-reporter, may also leave some readers with the impression that the 'accepted international opinion' is for exclusion of water and stress in rock mass *characterization.* This is far from what is desirable for the following reasons.

When rock mechanics engineers back-calculate a deformation modulus (M) from measured tunnel or cavern deformations, or when they do the same beneath an instrumented plate jacking or loading test, in each case using an MPBX, there will usually be the effects of both water and redistributed in situ stresses within the interpreted result. The water may have a dual role in softening joint coatings or discontinuity fillings where stresses are not too high, and any water pressures will reduce the effective stresses.

If the deepest MPBX anchor points are sufficiently far from the excavation, perhaps the (almost) undisturbed deformation modulus for that particular depth and rock unit can be recorded. This depth obviously includes the full internal and external boundary conditions referred to above. Likewise, when a geophysicist calculates arrival times and seismic attenuation, perhaps from VSP or cross-hole seismic velocity tomography, he is not asked to somehow exclude the effects of water pressure and rock stress, i.e. the all-important effective stress. The Moho velocity discontinuity is not corrected for stress or fluid pressure either. In each case the fully coupled condition is being recorded, via times of arrival or measured strains, as the case may be. To be successful, classification and characterization schemes must also be fully coupled, if they are to correlate in a simple manner with measured, fully-coupled phenomena. RMR includes water, but not stress, while RMi seems to exclude both.

In order for rock mass classification and rock mass characterization methods to correlate readily with such measurements of modulus and velocity, both close to and distant from an excavation, which must surely be an important goal in our subject, the adverse effects of water and the positive effects of rock stress on such measurements must be accounted for. (Of course when crossing the water table the presence of water will first have a positive effect on Vp.)

In the recent Q-system correlation routines described by Barton, 1995 and 1999, it is shown how depth of measurement (or effective stress) alters the Q – M and Q – Vp correlations. However, it is absolutely necessary that intelligently chosen ratings for Jw and SRF are included in these correlations, since the first four parameters are giving an incomplete, uncoupled description of the rock mass, as emphasised by Barton, 2002. For pre-excavation *characterization*, existing SRF values of 5, 2.5, 1.0 and 0.5 will be used as depth increases, unless 'over-ridden' by a fault zone. Appropriate values of Jw may be 1.0, 0.66 or perhaps 0.5 as depth increases, but this choice will also depend on local permeability and pressure. Different values of these parameters may be needed for support design and for input to continuum modelling of the near-field effects of excavation. However, a fully coupled distinct element model would account 'automatically' for many of the potential changes due to excavation, due to its non-linear response.

Water 'softens' clay-bearing discontinuities, and clay-bearing faults 'soften' the rock mass, both of which have greatest effect when excavation occurs. Our Vp and M measurements will tend to reflect both these effects, whether carried out from within the excavation disturbed zone, or from outside the EDZ. Values of Vp and M will usually be distributed as knee-shaped curves close to the openings. The shape and depth of 'penetration' of the EDZ will depend on many factors, not least the efficiency of the excavation methods, which are probably not always as might be desired in site investigations, where a less well-equipped sub-contractor may be engaged for the preliminary site investigation project.

The 'unexplained' low moduli presented in your Journal by Palmstrøm and Singh, 2001 are largely due to loosening caused by blast damage as they pointed out. They are also specifically due to radial stress relief and joint-voidage effects, which would not necessarily have been fully accounted for in the classifications performed. Measurements of Vp in the same near-excavation zone would also be reduced by this joint-voidage, and this would affect the velocity due to the changed proportions of travel times through air-filled or water-filled joints or blast-induced fractures, and through a less radially-stressed rock mass.

In conclusion, those who call for truncated *characterization*, with coupled behaviour excluded, as with RMi, are doing the profession a disfavour, as they will not be able to correctly correlate with the real-world response of rock masses, which is highly coupled and which therefore influences many of our in situ tests. Deformation moduli and seismic velocities do not correlate with jointing and discontinuity parameters alone, but with the complex response of these 'structural' parameters to the 'internal' and 'external' boundary conditions, which often give anisotropic, depth-dependent properties, as emphasised by Barton 1999 and 2002. It is therefore illogical not to include both water and stress, both in characterization *and* classification. Further improvement in all the authors' referenced techniques for modulus estimation will be needed for this reason, and RMi will also require more parameters, so that stress effects and water *are* included.

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