Comment on the paper 'Strong ground motion and site response in Deep South African Mines', by S.M. Spottiswood and A.M. Milev

in the Journal of the SAIMM, vol. 105, no. 7, pp. 515–524,

by W.D. Ortlepp*

Comments

In their excellent paper, Milev and Spottiswoode have provided quantitative and definitive descriptions of an important phenomenon that hitherto has been largely the subject of conjecture.

They convincingly show that 'site-response' can amplify the strong ground motions on the rock-wall of a stressed tunnel by a factor of 10 to 12 compared with the PPV in the solid rock mass (at the same distance from source). This knowledge gives us a firm foundation from which to begin to build an understanding of the mechanism of damage resulting from a rock burst. Regrettably this understanding is still far from adequate, in my opinion.

The authors of this paper, however, have made some statements that are too reassuring and which tend to reinforce the sense of complacency that prevails among many members of the rock engineering community on the deep gold mines.

This complacency is unfortunate, because there already exists a view that 'mesh-and-lacing' is a perfectly adequate method of support for tunnels in rockburst-prone mines. Many practitioners believe that, in any case, it cannot be improved on without incurring great extra expense. There is a mind-set that too easily rejects proven innovative technology and accepts the view that improved performance is not worth striving for, because it costs too much. Such a mind-set is not easy to reconcile with the claims made at the highest level of management, that mines pursue a policy of zero-tolerance towards anything that compromises safety underground.

As the authors point out, a PPV of 3 m/s is routinely used as a criterion for the design of support systems in rock-burst prone mines. Precisely because it is used routinely and uncritically, and because serious tunnel damage still occurs too frequently, the question has to be asked whether or not 3 m/s is an acceptable criterion. I would like to suggest that the conclusion reached in the paper that it is adequate is debatable, if not actually erroneous.

With the exception of Driefontein all of the observed data plots show less than one instance per year where a PPV of 3 m/s has been recorded. Is one to infer then that less than one damaging seismic event (rockburst) occurs each year on each of these mines? If that inference is correct and if it can be shown that significant damage occurs only if the PPV substantially exceeds 3 m/s, then it could it be argued that this value is an acceptable criterion for design.

It would have been extremely valuable if the authors had indicated how many times higher values occurred but were not recorded because the instruments had become detached from undamaged rock walls or had been lost in rock debris resulting from rockburst damage to the excavation. Instead the reader might now gain the impression that a higher value for the PPV criterion is not appropriate because such a high value will never occur anyway. While conventionally supported tunnels continue to suffer serious damage albeit even occasionally, such an impression would be incorrect and unfortunate!

In a way the authors actually concede that 3 m/s is not a sufficiently high value to use as an effective criterion. In the case of the simulated rockburst experiment where a PPV of 3.3 m/s was measured and 'high intensity damage' was observed they note that '... not even a single rock bolt failed ...What value of PPV or ejection velocity do the authors suggest would have been high enough to break rock bolts?

Since rock bolts or even cable anchors are commonly observed to have been broken as a result of severe tunnel rockbursts (as exemplified in Figure 1) an adequate criterion would be one that specifies a PPV value high enough to ensure, unequivocally, the expectation that non-yielding rock bolts would fail.

Such rigid rock bolts would then be deemed, by definition, not to be adequate for the support of tunnels in rock bursting conditions.

The reason why the rock bolts were not damaged in the simulated rockburst experiment was simply that they were subjected to a relative low intensity of dynamic loading. The phrase 'severe dynamic loading' (last para of p. 515) and the label 'high intensity damage' on Figure 13 p. 523 (which is reproduced here as Figure 2) are seriously misleading. In reality the damage caused by the experimental blast was so slight that it would hardly have been noticed by the miners and certainly would not have been reported in an operating mesh-and-lace supported tunnel.

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Figure 1—Severe rockburst damage in a deep tunnel approximately 60 m from its intersection with a fault on which a seismic event of M_L = 3,1 occurred

My observation is not intended to trivialize the original experiment, which is so comprehensively documented in the special edition of the SAIMM *Journal* of August 2001, but to put a proper perspective on the severity of the rock burst damage it produced.

The experiment made an important contribution to our knowledge inasmuch as it set a lower limit for the size of a rockburst that would cause some damage to a sparsely-supported deep hard-rock tunnel. That minimum event would be about ML = 1.3 (Milev *et al*., 2001, p. 258) provided that the source was about 6 m away from the tunnel walls and the fracture surface or the fault-slip surface, which caused the release of the seismic energy, did not itself approach closer to the tunnel.

To make it easy for the reader to confirm how low the intensity of damage actually was, Figure 4 from p. 249 (Haile and Le Bron, 2001) is reproduced here as Figure 3. This shows that the most severe damage occurred 13 m along the tunnel where 0.155 m^3 of rock per metre length of tunnel, was displaced from the sidewall. This equates to a mass of 410 kg which, if uniformly distributed over the area tributed to one rock bolt, say 1.5 m^2, would represent a slab of rock no more than 100 mm thick. The maximum volume of a single block was estimated at 0.07m^3 or about 190 kg. The photograph in Figure 3 on p. 248 (which is reproduced here as Figure 4), shows clearly that the damage was slight and hardly of 'high intensity'.

All the evidence indicates that there was only minor damage, which contrasts strongly with the assertions of high intensity damage and severe seismic loading. This contradiction suggested to me that the basis of the statement '… not even a single rockbolt has failed despite the severe dynamic loading' should be examined carefully.

Consider a 'key-block' of 200 kg mass in the tunnel sidewall which is separated from the surrounding rock by an unfavourable set of joints with no cohesion and which is to be retained by a single rock bolt. In order to determine the worst-case dynamic effect on the rock bolt we need to compare the energy demand imposed by the threatened ejection of the block, with the dynamic capacity of the bolt to restrain or prevent the block movement.

If the strong ground motion of 3.3 m/s thrusts briefly against the cohesionless interface between block and rock mass and the only inhibiting factors are inertia and friction, it would tend to eject the block at a velocity of nearly 3.0 m/s (a maximum ejection velocity of 2.5 m/s was actually measured on the tunnel wall – p. 515).

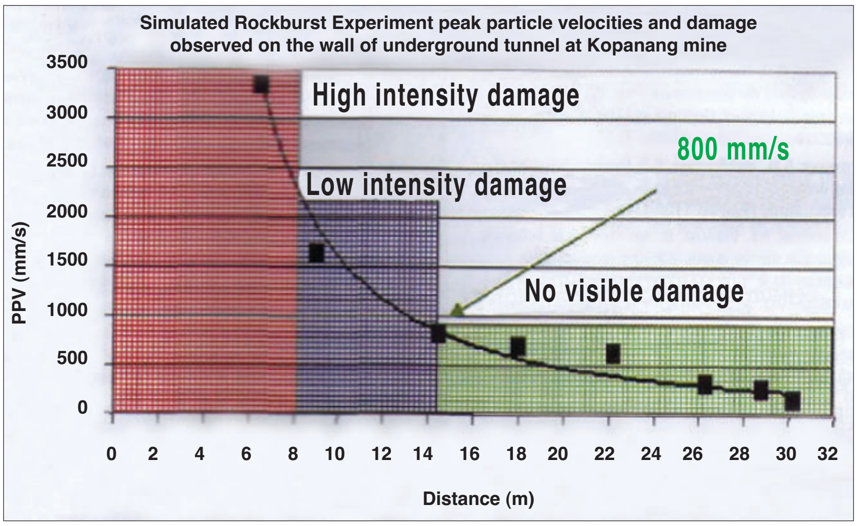

Figure 2—Damage intensity in relation to PPV

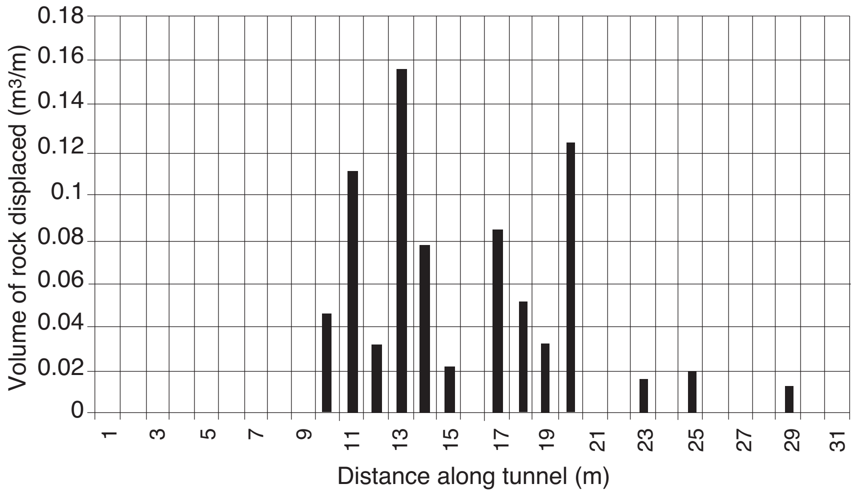

Figure 3—Distribution of rockburst damage along the length of the experimental tunnel

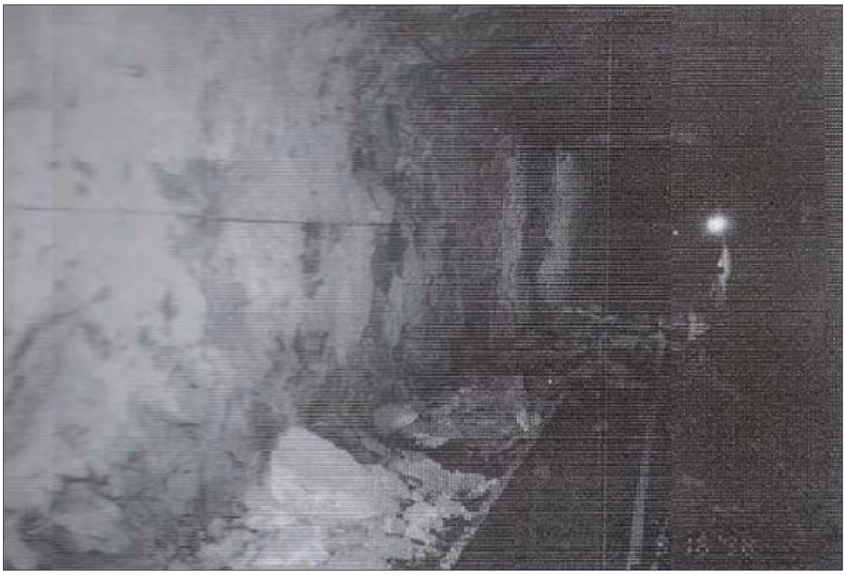

Figure 4—Photograph of 'high intensity damage' (sic) along experimental tunnel

The kinetic energy would be

$$KE = \tfrac{1}{2}\, m\, v^2$$

where m = mass of block
v = ejection velocity

$$\text{i.e. } KE = \frac{200}{2} \times 3 \times 3 = 900J$$

The dynamic capacities of three types of fully grouted 16 mm tendons are shown in Figure 5 (from Ortlepp *et al.*, 2001). Brittle failure of the re-bar typically used in mesh and-lace support occurs after some 50 mm elongation across the separation surface. However, at less than one-half of this elongation value, its elastic strain limit will have been exceeded and, in an engineering sense, the unit will irreversibly have entered the failure regime. With its elastic limit load being about 120 kN, this amount of strain would represent a maximum dynamic capacity of 120 kN x 0,025 m = 3 kJ, for a 16 mm re-bar. This can hardly be regarded as a significantly large capacity compared with the 25 kJ/m2 suggested by Jager *et al.* (1990) as the dynamic resistance required to contain the effects of a severe tunnel rockburst. However, it is more than three times greater than the energy demand imposed by the largest block ejected by the simulated rock burst and thus adequately explains why the rock bolts were not broken.

It is of more than academic interest to determine, using the same energy calculation, what would have damaged the rock bolts in this situation.

If the mass of the block had been 665 kg, the rock bolt would have broken, probably in a brittle fashion. Alternatively, if the ejection velocity had been 5 m/s the kinetic energy of the smaller block would have been sufficient to break the bolt. Whatever way one looks at it,

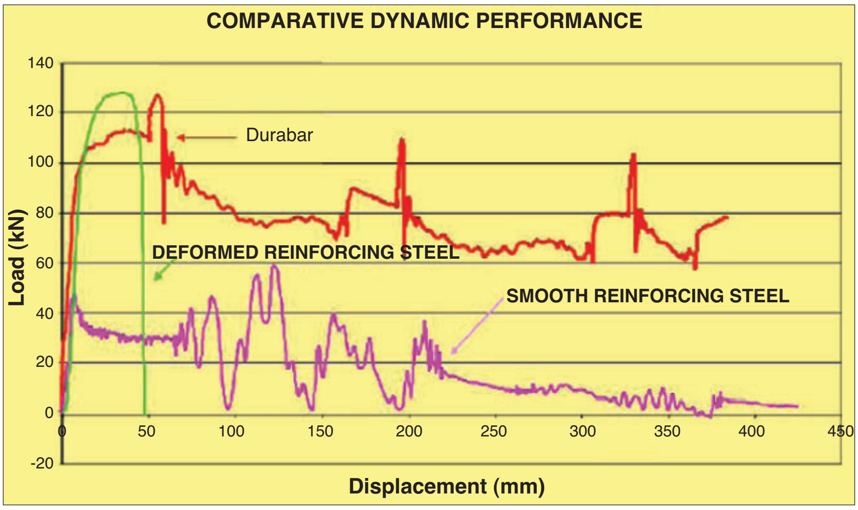

Figure 5—Dynamic load-displacement characteristics of fully-grouted 16 mm rockbolts

this example does not support the view expressed by the authors that a velocity of 3 m/s is an adequate criterion.

In my view this simple argument highlights the following important conclusions:

- It is absolutely necessary to have a valid criterion on which to base a proper engineering design for tunnel support
- Determining what is the correct criterion is not a simple matter. Certainly the mass or thickness of rock must be taken into account, also the extent to which the rock surround can be further fractured, even fragmented (as in Figure 1), during the damage process. Importantly, as the authors have so clearly shown, the site effect is a key consideration
- The value of 3.0 m/s as a criterion, by itself, is not adequate. Consideration must be given to those cases where the restraining tendons fail in tension and/or the rock is further fragmented, features which indicate that more extreme phenomena may be involved in the damage process.

I believe that there is crucially important information which is lost in the few incidents where damage was so severe that the authors instrumentation would not have been able to survive. The rock mechanics community must not metaphorically shrug their shoulders and express the belief that '… it is not possible to do anything anyway, in the case of such extreme events'. The truth is that nowhere has the available technology been used yet in a properly designed way and put to a stringent test. Experimental procedures for subjecting tunnel support systems to severe testing have been described by Ortlepp (1992 pp. 675–682).

What is the potential improvement that can be achieved if yielding tendons are employed sensibly in a really challenging burst-prone environment? Based on existing and published technical information, together with some reasonable and realistic assumptions, I offer my submission of their potential benefits.

A recently-developed cable anchor can survive an impulse of 11.0 m/s initial velocity and not lose its dynamic capacity (Ortlepp and Erasmus, 2005). Yielding 16 mm rock bolts utilizing the same principle of operation, can maintain a resistance of 80 kN for some hundreds of millimetres of movement after an initial ejection velocity of 3.0 m/s—Figure 5. Like the cable anchor, these rock bolts will also survive considerably higher ejection velocities.

Let us compare the capability of such yielding bolts with the 'rigid' conventional fully grouted re-bar of the same diameter, operating under similar conditions:

Let the support units be spaced 1.4 m apart to support a 'tributary area' of 2,0 m^2. If a wall rock thickness of 1.0 m was incipiently 'unstable' it would exert a static demand on each unit, as follows:

$$\begin{aligned} \text{Static load} &= \text{volume} \times \rho\, x\, g \\ &= 2.0 \times 2.65 \times 9.8 \\ &= 52 \text{ kN} \end{aligned}$$

where ρ = rel. density = 2.65
g = 9.8 m/s^2

With conventional 16 mm re-bar having a yield load of about 120 kN, the static 'safety factor' would exceed 2.0 which would be quite adequate. With a quasi-static yield point of 110 kN, the yielding bolt would also have a capacity exceeding twice the demand. The conventional safety factor concept, however, is not strictly appropriate because yielding bolts do not fail, in tension.

If the 665 kg mass was ejected at a velocity of 3.0 m/s, as was discussed earlier, its kinetic energy would have been $\frac{665}{2} \times 3 \times 3 = 3000$ J. This would cause failure of the rigid bolt, (the dynamic capacity of which is 3 kJ).

The volume of the rock would be $\frac{665}{2650} = 0.25$ m^3 (rel. density of quartzite is 2.65).

When distributed uniformly over the tributary area this would represent a thickness of 127 mm. Thus, given adequate containment between the bolts, the capacity of a stiff system, expressed as thickness of wall rock that could be contained in a rockburst, would be 0,13 m at a 'safety factor' of 1.0. This implies that some 50% of the bolts would be broken.

In a moderately severe rock burst, if a thickness of 0.5 m of wall rock was potentially vulnerable to ejection at an initial velocity of 3.0 m/s, the kinetic energy KE transferred to the support unit (i.e. the seismic demand) would be:

$$KE = \tfrac{1}{2}\, mv^2$$
$$= 11.9kJ$$

where $m = 2650$ kg
$v = 3.0$ m/s2

By estimating the area under the appropriate curve in Figure 5 it can be seen that the yielding bolt, by being forced to slide through 150 mm of displacement, would consume about 12 kJ of energy which is enough to quickly stop the movement of the rock. Simply stated, a thickness of rock four times greater would easily be arrested and the belts would be undamaged.

It should be noted that this somewhat simplistic damage mechanism of a single 'block' of rock moving freely (apart from the sustained resistance of the rock bolt), represents the worst case. In reality, frictional contact between contiguous blocks and wedging and jamming interactions will substantially reduce the inward movements of the fractured walls. Various possible mechanisms for rock burst damage in tunnels have been proposed by Ortlepp (1992, pp. 593–609).

Most importantly it should be emphasized that although the available seismic energy may be extremely large, its impact on the tunnel is very transient—lasting only a fraction of a second. While the resistance of the yielding bolt is sustained throughout the duration of its sliding movement the imposed seismic force is not. It is maintained for only some tens of milliseconds. The stiff, conventional rockbolt will fracture almost immediately, if its dynamic strength is exceeded. The yielding bolt, on the other hand, will survive unscathed with its ability to contain a further rockburst essentially unimpaired.

The ultimate 'bottom-line' question is how much additional cost is it worth spending initially, on a support system that will have the following benefits:

- It will contain at least five times greater thickness of rock in a rock burst
- It will survive ejection velocities considerably greater than those that will destroy a conventionally supported tunnel
- It will require far less rehabilitation cost and reopening effort after a rock burst
- It will significantly reduce the potential for loss of production and loss of life.

It is worthwhile to reflect on this important matter of cost. At present low levels of local consumption, the per unit cost of a yielding bolt is about 50% greater than the equivalently sized re-bar. When the complete costs, including labour and energy, of drilling the hole and filling it with grout are taken into account, the difference per installed unit is reduced to no more than 10%.

If one wishes to compare the effectiveness of a yielding system against a non-yielding system on the basis of equal cost per m2 supported, the linear spacing between the marginally more expensive yielding bolts will need to be increased by $\sqrt{10\%}$. The unbreakable yielding bolts would thus have to be spaced 3.2% further apart not to increase the cost of the support system.

Using the increased value of 1.45 m for the spacing instead of the 1.4 m of the original example, the simple arithmetical procedure outlined previously can be repeated to find out by how much the supported thickness would be reduced. For the example quoted earlier, for the same cost, the supported thickness would be reduced from 0.5 m to 0.47 m. This is still 3.6 times better than the 0,13 m capability of conventional stiff support.

To conclude a lengthy but, it is to be hoped, a worthwhile contribution to the paper by Milev and Spottiswoode, I would like to commend them for the sound science and excellent presentation of solid data which has focused attention on the shortcomings that still exist in our understanding of the mechanism of severe rockburst damage in tunnels.

My final plea is that the deep-level gold mining industry should, urgently:

- increase the amount of funding available for research into the mechanism of rockburst damage, and
- re-examine their cost-based justification for not using yielding support for all deep-level development. It is an argument that clearly has no validity and no harmony with the phrase 'zero tolerance'

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Reply to the comments made by W.D. Ortlepp on the paper 'Strong ground motion and site response in Deep South African Mines', by S.M. Spottiswood and A.M. Milev

The contribution by Ortlepp makes a powerful case for more widespread use of yielding tendons to support tunnels subject to rockbursts. Ortlepp shows that the use of such tendons will work much better in absorbing the kinetic energy of rock ejected from tunnels sidewalls than the non-yielding tendon in general use. Unfortunately it is possible that even yielding support would not have prevented extreme damage such as is shown in Figure 1 of his contribution, but then this truly extreme degree of failure is rare.

Mr Ortlepp correctly indicated that the Milev and Spottiswoode (2005) paper did not provide a complete understanding of the mechanism of damage: it was not our objective to attempt such an exercise but to report on several instrument-year's worth of observations in stopes and to provide some analysis of these observations.

The main message of our paper (Milev and Spottiswoode, 2005) was that a peak particle velocity (PPV) of 2 m/s or 3 m/s can be expected to occur each year in many panels of each of several mines in the Far West Rand. Rock burst support in stopes is typically designed to absorb the kinetic energy of the entire hangingwall of each stope up to a pre-defined height: values of ground velocity are required for support design. The fact that not all of these panels experience severe rockburst damage every year is due to some 'over-design' factors such as the choice of this predefined height, the self-supporting behaviour of the hangingwall, the recommended practice of ignoring the supporting effect of the face and the widespread use of backfill. The fact that many panels do experience rock bursts that are sufficiently severe as to result in injuries and fatalities shows that the support systems, as installed, have failed to achieve their stated objective of providing a reasonably safe working place.

McGarr (2001) pointed out that PPV on faults with weak infilling is limited to a maximum value of about 1.5 m/s. The PPV could be as high as 4.1 m/s in areas of intact rock that are stressed close to failure. Fortunately, lower values of stress change and therefore PPV occur even within the source region as part of the process of stress transfer (Ryder and Jager 2002). In addition ground velocity reduces (attenuates) with distance from the source in a generally well-behaved fashion until the interaction of the seismic waves with the stopes results in the site (amplification) effect that is reported in our paper. The upper bound of 3 m/s for the PPVs measured in our study is consistent with the values given by McGarr.

It was unfortunate that our paper was interpreted by Ortlepp as tending to 'reinforce the sense of complacency which prevails among many members of the rock engineering community. This was not our intention. The section in the introduction of our paper that might have 'reinforced complacency' was reference to an analysis by Haile and Le Bron (2001) of the performance of rock bolts in a simulated rock burst experiment. Ortlepp's contribution consists mostly of an analysis of the work of Haile and Le Bron (2001) rather than of our paper (Milev and Spottiswoode, 2005). He takes issue with the implication in Milev *et al.* (2001) and in Haile and Le Bron (2001) that non-failure of the tendons at the site was evidence in favour of widespread use of such non-yielding support in rockburst conditions. We agree with Ortlepp's analysis that the kinetic energy of a loose sidewall with a thickness of only 127 mm moving outwards at 3 m/s would have resulted in failure of the tendons that were installed. Then why did no tendon actually fail when there was enough kinetic energy for them to fail?

The limited amount of visible damage to rock between support units at the site of the simulated rock burst experiment can be attributed to the good condition of the tunnel and to the relatively low stress regime in the vicinity of the tunnel. Reddy and Spottiswoode (2001) pointed out that the degree of fracturing was commensurate with the estimated field stress of 50 MPa. It is probably true to say that severe rockburst damage such as that shown in Figure 1 of Ortlepp (2006) occurs almost exclusively under conditions of high field stress and/or loose sidewall when driven by high values of ground velocity with accompanying dynamic stresses.

Is the focus on using PPV as the only dynamic parameter that controls rock burst damage valid as is suggested by Ryder and Jager (2002)? Other factors can also play a role. For example, Reddy and Spottiswoode (2001, p. 271) found that the ejected blocks were mostly bounded by pre-existing fractures but also by recent fractures that were probably caused by the blast. As can be seen in Ortlepp's Figure 2 (from Haile and Le Bron, 2001) some areas of newly exposed sidewall are close to vertical and not bounded by bedding planes. Figure 8 of Reddy and Spottiswoode (2001) shows a clear example of such a near-vertical region of newly exposed sidewall. Considering that no fall-outs occurred behind the installed washers through which tendons restrain the sidewall and that the sidewall would have been made safe by barring any loose material before the support was installed, the pre-blast sidewall was held together, at least in some places, by rock strength and not by friction. It is not velocity that breaks rock but stress caused by differential movement. Dynamic stress changes normal to the sidewall are caused by differential acceleration between the sidewall and the material immediately behind the sidewall and not by ground velocity. Field stresses and ground accelerations should also be considered when studying rockburst mechanisms.

Dynamic skin stress is another factor that is generally ignored: see Milev *et al.* (2002) for an analysis of the likely effect of Rayleigh waves on stopes.

Most mining-induced seismic events take place in the fractured rock ahead of the face or on geological structures when they are intersected by mining (Ryder and Jager, 2002). In our study the measurements were taken on the surface of the hangingwall and not within these source regions. Very few direct measurements of the PPV in the source region of damaging events have been obtained. In three cases our instruments were buried or irretrievably lost due to rock burst damage and direct measurements of the ground motion were not possible (Milev and Spottiswoode, 2005, p. 522).

We agree with Ortlepp's opinion that the current understanding of the mechanism of damage resulting from a rockburst is still far from adequate. Our concern is principally that the underlying physics of damage has not been sufficiently explored. To use a phrase favoured by the legal fraternity, we in the rock mechanics business have not sufficiently 'applied our minds to the problem'.

We concur with his concluding plea for more funding for research into the mechanism of rock burst damage as long as this includes a more thorough consideration of the physics behind the process. The Rockburst Management Project (MHSC, 2006) is planned to run until 2010. Hopefully it will create a viable platform for a more fundamental understanding of rock bursts.

In conclusion we would like to express our appreciation to Mr. W.D. Ortlepp for his worthwhile contribution and for the opportunity for further discussion.

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