VIEWS

We Must Change Exploration Thinking in Order to Discover Future Orebodies

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(SEG 2010 F)

INTRODUCTION

Many of our Society's members work in mineral exploration and many student members probably hope to find employment in this or a related field. While discovering ore is the primary wealth-creating part of mining, exploration rarely is seen as crucially important by mining company boards and CEOs or industry analysts when economic conditions are tough—it is inevitably the first casualty of hard times. It has always been this way and is probably never likely to change.

I recently published a note in the Geological Society of Australia newsletter (Wood, 2015) responding to a letter by a geologist colleague under the heading "Resource doldrums" (Burban, 2015). Burban put forward a number of reasons for why he thought mineral exploration is in its presently parlous state.

The following is an expanded version of my note, which generally agrees with Burban's thesis. I hope it may assist SEG members trying to make sense of the challenging world they presently face in seeking or continuing employment in mineral exploration or mining, and contribute to discussion within the Society about the most serious issue members have faced in the past 30 years.

My comments are focused on porphyry copper and copper-gold deposits which are mined on a large scale, but I suggest from experience in discovering low-sulfidation epithermal gold deposits that some of my suggestions have relevance to the future discovery of this and other types of ore, recovered using a narrow-vein mining technique.

PAST AND PRESENT EXPLORATION APPROACH

Burban (2015, p. 8) proposed that much of exploration's present pain is self-inflicted and, in his words, is partly "the result of what happens when grandiose plans based on magical thinking don't pan out"; or, put less critically, is partly the result of ill-considered exploration strategies and tactics. Those of us employed in mineral exploration need to shoulder some of the blame for this pain, and Burban listed reasons for why we need to do this, such as "spending hard-to-get funds on projects that are unlikely to be profitable for a very, very long time ... if ever." He also suggested these projects "tend to be concentrated at relatively shallow depths and are being recycled again and again." No wonder investors are questioning the "perennial game of 'smoke and mirrors' that has been played by the exploration industry since the early 1970s," as he put it.

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I'm not as cynical about the conduct of exploration since the early 1970s, although I believe it started to lose its way sometime in the 1990s, as opportunities to discover shallow orebodies diminished significantly throughout much of the world. Yes, shallow orebodies were still discovered in the 1990s, and they were not necessarily in exotic and difficult locations. I also do not believe that every shallow orebody has been discovered; they are just likely to be more difficult to discover in many countries because of masking cover material, etc. However, I do believe there was a need, 20 years ago, for major change in how we conducted the search for new orebodies, particularly in recognizably mature exploration jurisdictions in Australia, Canada, and the USA, and this did not happen.

I see little evidence that the need for change has been recognized and acted upon substantially in any of these three countries in the past 20 years. Exploration geologists have to take a significant share of the responsibility for this lack of action, as only we have the necessary knowledge to argue the need for change. Company management and geologists (where they were in a position to influence strategy) in these countries often elected to basically tilt at windmills and invariably waste shareholders' money by riding off to ever more exotic and high-risk locations, rather than adjust their local discovery strategy and tactics.

Decline in exploration

Richard Schodde from MinEx Consulting compiles data on mineral exploration in various regions of the world (Schodde, 2013, 2014), and his data since 1975 led to three disturbing observations that are relevant to future exploration, particularly in Australia and North America.

First, while 60% of world exploration expenditure was spent collectively in Australia, Canada, and the United States in 1975, by 2014 this figure had fallen to 30%. One of the reasons for this is the increasing "red tape" of bureaucracy companies have to deal with in these countries. However, there is undoubtedly a strong perception that the chance of discovering an orebody has been diminished substantially in the decision by many companies to significantly reduce exploration expenditure in Australia, Canada, and the USA.

Second, Schodde shows that world expenditure on base metals plus gold exploration fluctuated in the US$2 to 5 billion range (in 2012 dollar terms) from 1975 until 2005, when it rose rapidly to peak at ~US$18 billion in 2012. It then fell quickly to ~US$10 billion in 2014, with more reduction probably to follow, possibly returning to the US$2 to 5 billion range of 1975 to 2005.

Third, many of my generation of exploration geologists expected a technological (geophysical) revolution would come to dominate discovery, but this hasn't happened. As Schodde's compilations show, generous attribution would only credit geophysics as the leading factor in selecting suitable base metal exploration projects, for example, in at most 30% of cases, and in <50% of cases when deciding where to drill the first hole. For gold projects, the estimates are <20% and <10%, respectively.

So, what do these observations foreshadow for the future of exploration, as well as employment of our younger members? Timing is important in exploration and, while it is no consolation for those about to graduate, at some time during the next five years the mining cycle should turn up, provided past experience is a guide, world demand for metals continues to grow, and the present imbalances even out. For those members presently without relevant employment but determined to pursue a career in exploration, the challenge is to find a productive way of maintaining contact with the mining industry during this slow period, however long it lasts—which will not be an easy task (Wood, 2014).

The reason for suggesting there may be opportunity at the end of the downturn is because of exploration's aging geologic workforce—much of which probably will leave the industry over the coming decade. When the demographic bubble of older geologists retires, there may not be adequate numbers of locally trained geologists to supply the resulting exploration demand in many parts of the world. The value of this proviso depends, however, on exploration companies adopting a progressive business approach to discovering ore over the next decade, thereby restoring investor confidence in financially supporting exploration; neither of these outcomes is by any means certain.

EXPLORE DEEPER NEAR MINES

The relative halving of expenditure in Australia, Canada, and the USA started in 1995, and this is when I suggest exploration in these countries should have made the transition to a different way of exploring. There was then an obvious and compelling need to largely abandon a predominantly surface to near-surface target approach and embrace deep exploration.

The reason for this is simple and apparent if we consider two points: (1) the discovery rate of major orebodies had been falling from the early 1970s onward and (2) the depth of discovery was mostly 200 to 300 m or less (Schodde, 2013), and has largely remained the same up to now. The reason for the relative shallowness of discovery is not geologic—companies exploring during this period mostly were seeking ore amenable to open-pit mining, and most still are, if their present method of exploring is a reliable guide. Very few groups tested deep enough to discover ore in the next 1.7 km below surface, down to 2-km depth, for example. On rare occasions, a deep orebody was discovered.

During the recent boom, from 2005 to 2012, ~US$100 billion (in 2012 US$) was expended on gold plus base metals exploration, roughly at an average rate of ~US$12 billion per year. Given this, it is easy to accuse the exploration sector of squandering shareholders' money, as Schodde estimates this expenditure produced fewer than 13 tier 1 gold and copper discoveries (12 from 2004 to 2013, where the net present value of a tier 1 discovery is >US$1 billion). No wonder investors began to question the value of this expenditure. The mining industry has a major challenge to convince investors that the return from high-risk exploration justifies their funding support.

Burban (2015, p. 8) proposed that "blind deposits and underground block-caving mining methods are very much the way of the future," a forecast I endorse for some large orebodies. However, I don't fully accept his related comment that "finding these non-outcropping deposits is both time-consuming and expensive due to the forensic nature of the search." Based on my experience at Newcrest Mining, I suggest there is no compelling reason why discovery of an orebody mined by caving necessarily has to be greatly more expensive or time consuming than discovery of one mined by open pit, particularly if exploration is conducted with cave mining in mind.

Cave mining is a mass underground mining technique (Wood et al., 2010) and only can be applied economically to a deposit with a particular geometry—to be such an orebody, the deposit has to have a regular geometric shape and be relatively large. Its shape may be that of a rectangular block or a solid or annular ore-ring cylinder, for example. Its size means it will present as a relatively large target, both volumetrically and in plan area.

A deposit mined using caving will be measured in the 10s to 100s to 1,000s of millions of tonnes of ore. Invariably, the smaller the tonnage is, the higher the average grade requirement will be, but location will play an important role in determining required metal grade. A small, necessarily higher grade copper-gold orebody, for example, will generally contain in excess of 50 million tonnes of ore and will occupy a volume of more than 20 million m^3, with a plan area which, ideally, is at least 200 m square, and a vertical dimension of 500 m or more.

This is not a small target at any depth, even at 500 or 1,000 m, and is magnified greater than tenfold if a typical porphyry orebody is being sought. A 500-Mt porphyry orebody amenable to cave mining, for example, will have dimensions that are some variation of 400 × 500 × 1,000 m. Given that porphyry deposits almost always are flanked and overlain by barren to variably mineralized and altered rock, the plan area of the geologic target is very large and is easily a square kilometer or more in size for a 500-Mt deposit. Exploring for a target of this scale does not require close-spaced drilling and large expenditure during the discovery phase.

Neither does a successful search necessarily require an elaborate forensic approach. It does, however, require geologists conducting exploration to be aware of what will be encouraging indications of ore potential in deep, widely spaced discovery holes, and to have the confidence and opportunity to drill deep follow-up holes to investigate relatively meager or negligible geochemical results because of other evidence. They need to be guided by the possibilities that the geology offers up as a guide to ore, and they need to be supported by their management to drill the required holes. Above all, success requires geologists to be pragmatically creative and optimistic in their thinking. There is nothing new in this.

A willingness to drill a barren quartz-vein stockwork on the top of Mt. Fubilan in Papua New Guinea, for example, was the reason for discovery of the Ok Tedi orebody at shallow depth in 1969, nearly 50 years ago. International expert discussion about the significance of a possible leached capping exposed on top of Mt. Fubilan had resulted in indecision and procrastination up to that point. The simple pragmatic observation by Ken Phillips was that the only way to determine what occurred below the barren outcrop was by drilling a hole through it (Hope, 2011). He was rewarded when the hole was drilled and high-grade supergene copper mineralization was intersected only 40 m below surface.

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Exploration's "dismal success record over the last couple of decades" alluded to by Burban (2015, p. 8) possibly didn't need to happen, either. Nor should it be taken as necessarily predicting the outcome of future exploration. Exploring apparently greenfield projects also does not warrant being labeled as "too expensive" by investors, as he suggested is the case. There are ways in which deep exploration can be conducted that are cost-effective and efficient, in both brownfield and greenfield settings, provided the search is conducted in a permissive geologic terrane by exploration geologists who understand their target.

SUGGESTION FOR FUTURE EXPLORATION

It should be obvious by now to the mining industry that continuing to apply shallow-target exploration strategies and tactics will fail to produce an acceptable frequency of orebody discovery in the future, irrespective of the successes post-1950. These strategies also will fail to encourage speculative investors to risk their capital in funding junior-sector exploration. Companies must capitalize on the evolution of mass mining presently underway and move from seeking orebodies that will be mined by open pit to ones that will be mined using some type of mass underground mining technique.

Deep exploration raises significant challenges for junior companies with limited access to funding; however, these challenges may be overcome if progressive funding strategies are adopted and applied (Sillitoe, 2010; Enders and Saunders, 2011). Some major companies have already started to focus on deep orebodies, but these companies tend to be the exception rather than the rule. Other companies were doing this from the mid-1990s onward and the success of their foresight was demonstrated by the discoveries they made—at Cadia, Oyu Tolgoi, Rio Blanco-Los Bronces, Escondida, etc.

Although shifting focus from near-surface to deep targets may seem a daunting challenge to many exploration geologists, it is not a radical proposal, nor is it one that requires difficult changes in the way exploration is conducted. It does, however, require significant change in exploration thinking.

The first change in this thinking is simply to disregard existing wisdom about the orebody potential of previously explored mining districts and to revisit and reevaluate these, wearing a different set of glasses that look deep. Evidence of uneconomic mineralization at the surface and previous mining should be treated as an indicator of a district's metal fertility and a possible predictor of deep orebody discovery. There is nothing particularly original in this suggestion; it is basically common sense and an approach with a long history and track record of success—i.e., the "shadow of the headframe" approach (Meussig, 2014).

An orebody with its top located 500 to 1,000 m below surface is unlikely to exhibit an obvious sign on the surface saying "drill here"; however, it may display subtle surface and near-surface indications of ore potential if one has the skill and good fortune to recognize these (Wood, 2010). If discovery is to be achieved efficiently and cost-effectively, the ability to manage risk is necessarily embodied in this skill.

The second necessary change goes to the heart of the way most of us explore. It requires that we drill deep holes early in a project with the objective of collecting relevant geologic information, rather than in the way we have been taught mostly to do—(hopefully) intersect ore. This is not as heretical a proposal as it may seem. If there is deep ore to be discovered, building a reliable 4-D picture of the local geology as quickly as possible after beginning the search is crucial to efficient discovery.

I suggest there are five simple requirements that must be adopted to lead to the discovery of deep ore:

1. Exploration geologists must understand and accept that most of the Earth's crust below about 300-m depth is unknown, in an orebody sense, as was much of the upper 300 m of crust when modern exploration started post-World War 2.
2. Exploration managers must educate their company CEO and Board, as well as investors, about the unrealized ore potential of the 80% of the Earth's crust that is poorly explored in known mining districts and metallogenic provinces, from about 300 m below surface down to 2-km depth (and this does not include the potential of greenfield regions).
3. Exploration geologists need to become much less risk-averse when it comes to what evidence is required before drilling an early and deep exploratory hole.
4. In turn, these same geologists need to throw off the mental shackle that prevents them from using a drilling rig as nothing more than a "giant geological hammer" with which to sample rocks—developments in coiled tube drilling technology offer opportunity to significantly reduce drilling cost (Giles et al., 2014).
5. Exploration must return to its geologic roots and wean itself of the hope that a "black-box" technique of some sort will enable it to discover deep ore—technology should support, not drive, exploration.

Satisfying these conditions will go a long way to rewarding speculative investors who fund junior-sector exploration. After all, exploration is a high-risk endeavor and it is better to be spending whatever speculative capital is available on exploring for ore at depth than on continuing to recycle failed projects that "tend to be concentrated at relatively shallow depths" (Burban, 2015, p. 8) and mostly never produce a reward. High risk needs to be accompanied by the possibility of high reward. Applying capital to deep exploration means there is a chance high-grade ore will be discovered, as occurred with Newcrest's discovery of the Ridgeway orebody at Cadia (Wood, 2012).

I believe that if these conditions become the new norm for how exploration is conducted, the future for ore discovery will be improved, as may the chance of our student members gaining employment in mineral exploration. Failure to make the necessary changes to exploration strategy and tactics will likely delay a resurgence of exploration activity until the need to change is finally acknowledged and forced upon industry. Exploration is a business, not a lifestyle, and is no different from any other business where shareholders' capital is applied; it has to produce an acceptable financial return to investors or be modified so that it does.

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