# I Think, Therefore I Add Value – Philosophy of Science as a Practical Tool for the Mine Geologist

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## ABSTRACT

The philosophical context of many activities in mine geology can be an important lever in adding value to operations. For mine geologists (including resource and grade control geologists) we argue that the tasks of problem formulation, observation and data collection, interpretation and modelling invoke various philosophical considerations whether the practitioner is aware of them or not.

Constructing testable models to explain reality is the definitive aspect of any activity claiming to be 'scientific'. A primary goal of mining geology is to build models that accurately predict reality to an acceptable degree and these include litho-structural, stratigraphic, grade control, resource or reconciliation models. What's more, in mine geology we are often able collect additional drill samples or access new underground openings, for example, that can provide relatively rapid feedback on how predictive, and therefore useful, our models are. In this paper, we describe the key philosophical frameworks proposed for conducting scientific investigations, including falsification, the method of multiple working hypotheses and structural concepts of scientific theory. The strengths and weaknesses of these approaches are discussed and an attempt is made to relate them to activities undertaken by mine geologists.

Common perceptions of what constitutes the scientific method have been dominated by how scientists do experimental physics and chemistry in particular. There are very important differences in the types of problem confronted in these experimental sciences compared to the 'historical sciences', such as geology, where the processes studied are unique and only evidential traces of past events are available. The philosophical implications of attempting to validate numerical geological models, such as resource estimates are also briefly considered.

Some practical conclusions are drawn about how mine geology activities can be better implemented if practitioners are cognisant of the importance of philosophical considerations when attempting to gain new knowledge through a scientific investigation. We believe that the implications for creating additional value to a project or operation can be very significant when geology is applied by a practitioner with an understanding of the philosophical basis of the activities we think of as constituting a scientific investigation.

> *Remember that all models are wrong; the practical question is how wrong do they have to be to not be useful?* (Box and Draper, 1987, 74 p).
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> *The demand that theories be highly falsifiable has the attractive consequence that theories should be clearly stated and precise. If a theory is so vaguely stated that it is not exactly clear what it is claiming, then when tested by observation or experiment it can always be interpreted so as to be consistent with the results of the test.* (Chalmers, 1999, 67 p).

## INTRODUCTION

This paper is written from the perspective of two experienced mining industry geologists with an audience of professional geologists in mind. It is not intended to be an academic contribution to philosophy! While the subject matter may be somewhat unconventional for a mining geology conference, we are convinced that the role of key note papers is to stimulate new thinking, and hope that our efforts succeed in this aim. Furthermore, there are very practical consequences of being clear (or unclear) in the formulation of scientific work in our discipline, consequences which we believe have considerable value implications.

Although the target audience for this paper is the mine geologist, it should be generally useful to those working in other fields of earth science including climatology,

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oceanography and environmental geochemistry where the required assumptive frameworks are philosophically similar.

## Is mine geology 'scientific'?

If our work as mine geologists is to be justifiably labelled as 'scientific' we need to pose questions that are, in principle, falsifiable; subject these appropriately framed questions ('hypotheses') to testing ('experiments'); and design data collection for these tests in a manner that has the objective of falsification of a hypothesis. We also need to be aware that interpretations of geological data never exist separately to our assumptive framework ('theory').

## Why is this important anyway?

It is reasonable to ask 'why is following a scientific approach important for a *mine* geologist?' The answer, we argue, is that mine geologists are *practicing industrial scientists*. To simplify here, geologists working within the mining value chain are referred to collectively as 'mine geologists', and included in this category are those involved in day to day operational mine geology, grade control, quality control, resource evaluation, reconciliation, geometallurgy, near-mine and brownfields exploration. For all scientists the core professional tasks are problem formulation, hypothesis generation, data collection (design and conduct of experiments), interpretation, modelling and prediction based on these models. Following this, ideally, a feedback loop occurs whereby deviations from prediction are used to update or change the hypothesis. *These tasks collectively constitute practical implementations of the scientific method.*

Expressing this in a more concrete fashion, many of the key activities performed by a mine geologist require us to make predictions based on fragmentary data (for example predicting the location of a coal seam at a point midway between two drill holes in which a coal seam has been recorded). In the mining context, the outcome of these predictions usually has a direct (or indirect) economic consequence. We argue that the quality of prediction is likely to be materially improved if firmly rooted in the scientific method. In addition, if the predictions are framed and presented in terms of the underlying science, the quality of decisions made based on those predictions will likewise be improved.

Consequently, an understanding of what science is and how it is done is very important if our work is to be structured and implemented effectively, and, more to the point, if our profession is to sustain and increase our historically critical contributions to the mining industry.

One of the authors has conducted a 'straw poll' of more than 2000 short course participants over the past decade, over 90 per cent of whom were geologists (mostly trained in Australasia, but also in Africa, North and South America, Asia and Europe). The rough estimate is that much less than five per cent of geologists were exposed to philosophy of science during undergraduate university training. Since the great majority of geologists working in the minerals industry evidently received no training in philosophy of science, a paper outlining the key aspects of philosophy of science in the context of mine geology should be a practical and helpful contribution. Explicit understanding of the nature of the assumptions employed in designing data collection and doing interpretation and modelling should allow mine geologists to better structure their investigations. This in turn should flow on to development of more robust geological inputs to important (and risky) business decisions.

As an aside, we do not, in this paper, deal specifically with the impact of using probabilistic frameworks as the basis of making decisions or prediction in mine geology (eg geostatistics for kriging or simulation models). While this is a very important topic, it must involve detailed consideration of the assumptions specific to statistical models and will be dealt with in a subsequent paper. The interested reader is strongly encouraged to seek out the landmark monograph on this topic by Georges Matheron (Matheron, 1989), which is a comprehensive exposition of the philosophical and practical implications of building probabilistic models for unique phenomena such as mineral deposits.

## The structure of this paper

The structure of this paper is as follows: firstly we give a few definitions to set the context. We then consider the scientific method and scientific models and contemplate important differences in the types of problem confronted in the experimental physical sciences (in particular physics and chemistry) compared to the geological sciences. Common perceptions of what constitutes the scientific method have been dominated by consideration of how scientists do experimental physics and chemistry in particular. Geological sciences are different to experimental physical sciences in several important ways, chiefly that the processes we are studying occurred long ago under unknown conditions, rather than in a controlled environment in the present. Some key established philosophical frameworks for conducting science are then described. These include the concept of falsification, the method of multiple working hypotheses, and more recent 'structural' concepts of scientific theory.

We attempt to draw some practical conclusions about how the work of mine geology can be better implemented when practitioners are cognisant of the importance of the philosophical considerations. The implications for adding value to a project or operation can be very significant.

# PHILOSOPHY OF SCIENCE

## Background

A history of the philosophy of science is given by Losse (1980). The philosophy of science is a broad subject and its exact definition is not readily agreed upon by professional philosophers. Chalmers (1999) gives an excellent overview of the philosophy of science that is recommended for geologists wanting to familiarise themselves with this subject at an introductory level. Rosenberg (2000) provides a more in depth discussion aimed at philosophers and those already familiar with the subject. Frodeman (1995) gives a concise and very readable summary of the distinctiveness of geological modes of reasoning, which includes a review of contemporary philosophy of science as it relates to geology. Frodeman (2003) presents a thorough, enlightening and entertaining discussion of the way geologists approach their work in the context of the philosophy of science. A general exploration of the differences between experimental sciences (such as physics and chemistry) and 'historical sciences' (eg geology, as well as astronomy, paleontology and archaeology), where the emphasis is on explaining existing natural phenomena in terms of long past causes is given by Cleland (2001, 2002).

## Definitions

The Oxford Dictionary (http://oxforddictionaries.com/) defines philosophy as '…the study of the fundamental nature of knowledge, reality, and existence'. The two main branches of philosophy are epistemology, the theory of knowledge, especially with regard to its methods, validity, and scope, including the distinction between justified belief and opinion; and metaphysics which deals with the first principles of things, including abstract concepts such as being, knowing, identity, time, and space.

We use the term 'philosophy of science' in this paper as defining the subset of epistemology applied to the activity of science.

### The relationship between 'science' and 'philosophy'

All of the sciences (and various branches of mathematics) were originally considered to be parts of philosophy hence the archaic term for sciences was 'natural philosophy' (Rosenberg, 2000). The history of science is of successive breaking away of individual subject areas (subsets of natural philosophy) to form new disciplines considered distinct and separate from philosophy as such. Euclid did this for geometry; Galileo, Kepler and Newton did it for physics; Darwin did it for biology; and Lyell, Murchison and Hutton did it for geology. As an aside, there are many accounts of the origins of the modern science of geology – we recommend Stephen Jay Gould's book (Gould, 1987) on the discovery of 'deep time' as one of the best.

Each time a subject area broke away, the scope of philosophy was reduced with a shrinking set of residual questions left for philosophers (*sensu stricto).* These residual questions, including 'what is the activity called science?' and 'how can we tell whether something is, or is not, scientific?', are core to what is now called 'philosophy of science'. Such questions are important in defining the boundaries of science and to help protect it, for example, when non-scientific activities masquerade as science. In these cases the fundamental attributes of science need to be understood to identify what is *not* science. Understanding such distinctions is important when deciding how we must design and conduct our investigations and work as mine geologists in order to reasonably claim that they are 'scientific'.

By necessity the conduct of science involves philosophical (strictly, epistemological) considerations, whether we are aware of them or not, because in order to be sure that what we are doing *is* scientific we must work within a framework that separates 'scientific' from 'non-scientific' modes of acquiring and evaluating knowledge. Awareness of the philosophical context within which mine geologists work provides a significant advantage to *the practitioner* when attempting to create value, because it has the capacity to improve the quality, clarity and reliability of the work being done and thus the predictions made.

## METHOD AND MODELS IN SCIENCE

### Models

An important task of mine geologists is to generate geological interpretations of mineral deposits to be used as either inputs to the resource estimation process (including grade control and quality control activities) or to predict mineralisation geometry to improve extraction efficiency. Geological interpretations are key inputs to models used in the business of mining and such models are examples of *scientific models*. In addition geological interpretations are vital to the discovery of potential extensions of existing mineralisation or undiscovered occurrences of related mineralisation.

A scientific model is a set of 'statements' or hypotheses that are postulated to describe the nature of some phenomenon, in order to answer questions about that phenomenon. For example, in mining geology we are typically interested in the profitable extraction of some commodity, for example a metal. In order to plan and execute the mining of this commodity, we require predictive models of the key factors that will influence the value that can be achieved. While the concentration (ie grade) of the valuable component is a critical variable, geologists know that description of the geometry of key geological features of a deposit is essential to successful spatial prediction of key attributes like grade distribution. Today, such geometric models are usually summarised using computer software as three-dimensional (3D) 'solid models' (wireframes) or surfaces representing faults, stratigraphic boundaries, alteration fronts, etc. These solid models are scientific models in the sense that they represent or summarise hypotheses about the geometry, extent and character of the mineralisation. Other examples of scientific models in mine geology include genetic models of mineral deposits, spatial models of grades and other rock properties, models used to aid geotechnical or hydrogeological investigations.

### Scientific method

The essence of the scientific method is to generate hypotheses and compare them with observations of nature. This comparison is generally assumed to be in the form of experiments. Popper (1958) summarised the task of scientists as putting forward statements, or systems of statements, and testing them step by step. The scientific method is thus rooted in the generation and testing of hypotheses against observed results. Hypotheses may take the form of equations, verbal statements or other models or representations of reality, such as maps and wireframed solids.

The *value* of scientific models lies entirely in their use to generate predictions. Historically the usefulness of a scientific model depended on its success in predicting the outcomes of experiments or new observations. The evolution and refinement of a scientific model proceeds by making predictions, based on a model or set of hypotheses, and then comparing the outcomes of experiments or observations to those predicted by the model. A model is therefore always *interim:* it will be refined or even abandoned if it fails to predict existing or new observations.

## DISTINCTIONS BETWEEN APPROACHES IN HISTORICAL AND EXPERIMENTAL SCIENCE

Traditional concepts of science are largely based upon the modes of investigation followed by physical scientists studying phenomena that can be observed and measured in the laboratory *in the present moment,* for example in experimental physics, chemistry and biochemistry, to name a few. Popper (1958) used experimental physics as a type example in his development of arguments, as discussed in the next section of this paper.

There are important differences between geological sciences and the experimental physical sciences that arise from the temporal nature of the phenomena being investigated. Geologists investigate events that occurred in the past under uncertain conditions. In contrast to this, physicists and chemists are generally interested in observing phenomena that can be studied in a controlled environment in the present moment. Evidence about past geological processes (while possibly based on laboratory analysis of remaining rock materials) is *evidence of a past event; ie it is historical.* The option of seeing that exact event repeated now is generally not available in most cases in the way it is to a physicist or chemist.

Cleland (2001, 2002) discusses the contrast between 'historical sciences' (such as geology, palaeontology, archaeology, astronomy, etc) that are mostly concerned with evaluating hypotheses about past (and often *long past*) events with 'real-time sciences' such as chemistry or physics. Scientists working in a historical context cannot usually reproduce in a laboratory today the events they study, though some very limited aspects – often under highly constrained conditions and assumptions, may be studied in this way (eg the study of melt phase chemistry in experimental petrology; or study of failure modes in shear box experiments). They can, however, look for preserved relicts of features formed by the events that are diagnostic of specific conditions, in what Cleland (2002) calls 'the search for smoking guns'. Sometimes these signs may be clear and unambiguous, for example the presence of a trail of footprints provides very strong evidence that some creature walked across that exact surface at some unknown time in the past. We are confident to draw this conclusion because modern analogues (footprints in mud left by the passage of a creature) can be observed today. At times we may be happy to extend the observation to make further inferences of varying degrees of certainty – for example, we can be highly certain that at the time the footsteps were imprinted, the horizon lay at the earth's surface, we can be less certain that the environment was terrestrial and we are less certain still that the creature making the footprints was a dinosaur.

These sorts of extensions are familiar to geologists, most of whom would have been taught the Principle of Uniformitarianism (Hutton, 1899; Lyell, 1837) and learned that 'the present is the key to the past'. While using modern observable earth processes as an analogy to ascribe similar origin to similar features preserved in the geological record has been invaluable in developing an understanding of the history of our planet, in many cases, there is either no modern analogue (komaatiites, flood basalts, diatremes) or we simply don't have access to the inferred environment (ophiolites, deep crustal environments, mantle processes). The present is a small and biased subset of all environments that have existed over geological time.

In historical sciences like geology, it is usual to proceed by the examination of 'evidentiary traces' and not direct examination of the actual phenomenon being investigated (Cleland, 2001, 2002). For example, the geologist may be interested in a hypothesis about whether a fault predates or post-dates a mineralisation event; however, what are actually investigated are *traces of the event*, not the event itself. Evidence is sought that can distinguish one hypothesis among a set of possible explanations as 'the best'. Such evidence has high value in geology, and the imaginative work (including thought experiments) required is an important (and arguably distinctive) mode of geological scientific thinking. In many cases, evidence that eliminates (or renders less likely) certain hypotheses has at least as much value as evidence that directly supports a hypothesis.

Historical sciences also differ from experimental science in that they are studying *unique occurrences* whereas the latter may observe the same (or essentially the same) phenomena repeatedly. While there may be families of deposits that share many features, the circumstances of each instance of mineralisation has such a multitude of parameters and boundary conditions that no two deposits can be considered to be identical in the way that two chemical reactions involving the same compounds, run under identical conditions, can. It is an impossibility that an exact deposit would naturally be created twice. Even so, geologists may be confronted with multiple examples of a given type of geological phenomenon (eg many examples of similar fault surfaces, or fossilised hydrothermal fluid systems). In such cases, the collective evidence available to geologists resembles a body of evidence akin to that available to an experimental scientist. In such instances, like experimentalists, geologists can formulate generalisations.

These differences between experimental and historical sciences are worth bearing in mind in the discussion that follows.

## POPPER AND FALSIFICATION

### The idea of falsification

Popper (1958, 1963) asserted that the fundamental attribute of scientific models is that they are falsifiable and argued that the essential mechanism for testing and refinement of scientific models is to attempt falsification. We agree that a key objective for mine geologists wishing to claim scientific status for their work is to constructively 'criticise our models' by designing tests that maximise the likelihood of disproving our geological hypotheses. There is an absolutely key prior step, which is ensuring that the 'purpose' of the model is properly defined and understood. If a hypothesis withstands such attempts at falsification, it is considered to be more reliable, but not 'proved'. If it repeatedly withstands such tests, we can increase our confidence in the hypothesis. The usefulness of the model increases in the sense that it is a source of more and more reliable predictions that are consistent with new observations or experimental results.

Popper (1958) argued that scientific models *must* be based on hypotheses that are, at least in principle, falsifiable, and further argued that only hypotheses that *are* falsifiable are *valid*. It is not possible to advance knowledge if a theory cannot be tested. For example, the hypothesis that a supernatural being created the universe by laying an egg cannot be subjected to a falsification test and resides in the realm of theology, not science. The hypotheses that constitute astrology are also mostly untestable and cannot be compared to those in astrophysics.

The concept of falsifiability has practical implications for geologists. We must frame our hypotheses in terms that are, in principle, falsifiable. For example, it is possible to test the hypothesis that ore is present at a given location by conducting an experiment (drilling a hole at that location). We should design our tests (for example, drilling) to be attempts at falsifying a well constructed hypothesis. Such drilling will be most useful for improving and refining our model if a conscious strategy of designing holes to challenge or invalidate our model is adopted. The same also is true if specific holes are sited to test areas of disagreement between two (or more) plausible models. The challenge lies in identifying those elements of difference between competing models that would result in material differences in economic outcome. To do this requires consideration of the full space of plausible hypotheses, and as well some subjective assessment of the likelihood of occurrence of those hypotheses.

In a hierarchy of importance for the activities of a mine geologist, we would place interpretation (ie geological modelling) at the highest level. Data acquisition is essentially mechanical (although still governed by theory). Spatial model construction (grade or other rock attributes) is largely concerned with the science of uncertainty. It is *interpretation* that is the uniquely 'geological' scientific activity for mine geologists – and it is here that the philosophical framework strays furthest from a strict 'Popperian' view of the world

### A more sophisticated idea of falsification

Many people, Popper included, realised that problems are encountered when attempting to move from the logic of falsifiability to the real-world practice of testing; that is attempted falsification (Champion, 2011). A strict adherence to the necessity for falsification has been referred to as 'falsificationism' (Feyerabend, 1993). The real value of falsification to the practice of science is that the next stage of progress is embedded in the last because a solution includes testing. Falsification is especially useful in mine geology, when applied to constructing and testing geometric models (usually as wireframes of interpreted lithology, alteration, weathering, structures, etc) used for resource evaluation. In our opinion interpretations can be efficiently improved using a falsification framework and we therefore regard it as a pragmatic and necessary tool for mine geologists.

Popper's strict approach to falsification can be taken to extremes (see for example, Feyerabend (1993), with a summary in Meynell (1998). A valuable feature of falsification is that it compliments other modes of scientific investigation. Feyerabend (1993) argued that investigation of a single hypothesis in falsification mode is weak compared to using a multiplicity of inconsistent theories. In fact such an approach of using multiple working hypotheses was originally proposed by the geologist T C Chamberlin in 1897, and we return to this shortly.

According to Feyerband (1993) there is an ambiguity associated with falsification because in every instance a negative result may be attributed to *either* a flawed hypothesis *or* an error or inaccuracy in the experiment, observation or underlying assumptions. The latter must always be considered. Cleland (2001) notes that students are usually acutely aware that procedural errors are a possible cause of a failed experiment. This is because repetition of classical experiments during laboratory exercises does not necessarily replicate a well established result, for example due to malfunctions or contamination of equipment. It is similarly possible that we get false signals in geological investigations. In summary: failing a test does not necessarily disprove the hypothesis and careful consideration of the context of the experiment is always important.

Chalmers (1999) gives a classic example of the use of incorrect theoretical assumptions. The Copernican hypothesis for a heliocentric solar system, in which the Earth orbits the Sun, was widely rejected by his contemporaries. One falsifying experiment proposed to test the hypothesis posited that the stars should exhibit measureable parallax error when the earth was at opposite sides of the sun. Measurements were duly made and no such parallax could be observed. The assumptive flaw was, of course, that the stars were close enough for such a difference to be measureable – which they are not.

Thomas Kuhn's book, first published in 1970 (Kuhn, 1996), was a significant critique of Popper. Kuhn pointed out that scientists, almost never practice strict 'falsification'. In fact, if a prediction fails a test, scientists often, quite rationally, engage in a search for conditions that might be responsible other than those proposed by the hypothesis. This amounts to a relaxation or adjustment or re-framing of the hypothesis. Cleland (2001) describes this as exercising the option of 'salvaging a hypothesis' by rejecting some component assumption. In other words, falsification may involve *modification of the hypothesis by rejecting some auxiliary assumption* rather than the total rejection implicit by strict adherence to falsification.

In the case of mine geology, this raises the important issue that the test of the hypothesis is always conceived within a conceptual framework which could itself be flawed; and with experimental approaches that can be erroneous. We consider that a modified falsification approach is usually advisable in geological investigations with the following characteristics:

- the hypotheses must be stated so that they are falsifiable; and
- after failure of a test other possibilities should be investigated before rejection of the hypothesis, which may include, in particular:
  - the possibility of error in the test (as previously discussed), and
  - the hypothesis itself should be evaluated for assumptions that may be erroneous or flawed.

For novel ideas, the geologists often must go looking for *confirmatory* evidence to convince themselves that the concept being investigated is fertile. Cleland (2002) describes this as '…the search for a smoking gun [which] is a search for *supporting* evidence for a hypothesis' (Cleland, 2002, p 483). Alvarez and Vann (1978) and Alvarez *et al* (1980) provide the classic geological example of the 'search for a smoking gun': they proposed the hypothesis that an asteroid impact was responsible for the extinction of the dinosaurs and many other taxa at the Cretaceous-Tertiary (KT) boundary. Cleland (2001, 2002) notes that in this instance, the primary hypothesis, that there was a major impact at the KT boundary, suggested looking for other evidence of impact, such as elevated Iridium and Osmium levels and presence of shocked quartz in the KT boundary sedimentary rocks. This evidence was sought and found, lending more weight to the hypothesis.

Geologists can and must look for confirmatory evidence for novel hypotheses. This approach requires that they need to intelligently visualise what types of evidence must be sought. This mode of investigative working is similar to that conducted at a crime scene. Such evidence is often well hidden in the complex, messy, partially preserved and incompletely exposed geological record. If found, the 'smoking gun' does not 'prove' our hypothesis but such evidence will usually significantly strengthen our confidence in a hypothesis, especially if we specify the type of evidence expected *before* we go looking for it. If we predict such evidence and *cannot* find it, our hypothesis is unconfirmed. Over time repeated failure to find confirmatory evidence will erode our confidence in the hypothesis, which in the framework Chamberlin's (1897) multiple working hypotheses theory, may lead us to prefer alternative hypotheses.

## KUHN AND PARADIGM SHIFT

Thomas Kuhn (1996) challenged Popper's falsification philosophy of science. He concluded, after a study of the history of various scientific developments, that the history of science has proceeded not by sequential falsification, but via successive 'paradigms', shattered by revolutions in which these paradigms were overturned. Kuhn also introduced the important role that sociology plays in the beliefs and behaviours of scientific communities. The interested reader is encouraged to read Kuhn (1996) along with the excellent summary and critical analysis of Kuhn's contributions by Chalmers (1999). We will now summarise this philosophy using examples drawn from geology.

Kuhn (1996) argued that a mature science is always characterised by a single 'paradigm'. According to Chalmers (1999) a paradigm is made up of the general theoretical assumptions and laws, along with techniques for their application, that the members of a particular scientific community adopt. In Kuhn's terminology, scientists working within a given paradigm practice what he calls 'normal science'. If new observations arise that seriously challenge the existing paradigm, or if a new theoretical framework is proposed that seems to better explain certain observations (or is a better basis for prediction), a 'crisis state' occurs. Such a crisis is resolved by the emergence of a new paradigm, in a discontinuous change referred to by Kuhn (1996) as a scientific revolution.

A classical example of a paradigm shift is the change from Newtonian to quantum/relativity physics, but subdisciplines can also undergo such revolutions. One example used by Kuhn (1996) of a revolution in a subdiscipline is the emergence of modern ideas about electric currents. The plate tectonic revolution is a relevant example for geology.

### Geological examples

A comprehensive discussion of the history of ideas and people behind the plate tectonic revolution is given by Oreskes and Le Grande (2001). For a discussion of the philosophical implications and some interesting observations on the sociological and psychological dimensions of the plate tectonic revolution, see Solomon (1992). Here we present an abbreviated account and draw some general conclusions.

Prior to the emergence of new types of data, including detailed seafloor bathymetry and sea floor magnetic imagery, the prevailing geological paradigm was of an immobile earth in which the continents and oceans occupied essentially unchanging positions. Previous contentions about mobile continents from the time of Wegner (1924) and Holmes (1929) were initially rejected by the overwhelming majority. There was consensus that the continents did not drift, collide or break apart. It is sometimes argued that this rejection of drift was based on lack of mechanisms for such large scale crustal mobility, but this is not correct. Oreskes (2001) points out there was a wide debate on possible mechanisms for continental drift, and in fact Holmes (1929) laid out a remarkable account that foreshadowed plate tectonics, at least in a cursory form. The work of Holmes (1929) even discusses the hitherto unknown concept of subduction.

It was not until the 1960s that new observations by geophysicists started the shift towards acceptance of continental drift because of symmetric sea floor magnetic striping in particular (Oreskes and Le Grande, 2001; and references therein). Solomon (1992) details the considerable sociological impediments to acceptance of this idea. The shift was more rapid among some subcommunities in geology than others (for example, acceptance was much slower in North America). Once the paradigm shift was underway, the hypothesis of plate tectonics spawned a range of testable predictions, some entirely new, which had profound impacts in economic geology. A survey of ore deposit models will show that the plate tectonic context is now a critical component of such models and has had considerable predictive success.

In summary, overcoming the status quo is usually difficult, and does not proceeded by simple falsification pathways. In the case of plate tectonics, many of the essential ideas existed long before the paradigm shift, but were rejected by the mainstream (so-called 'normal science'). To Kuhn a paradigm is a theoretical framework or 'structure' that becomes the boundary conditions of thought and action within a given scientific field. Scientists find it hard to reason outside of such existent theoretical frameworks. Kuhn argued that scientists must, at least to some degree, be uncritical of the paradigm in order to be able to investigate detailed aspects of that paradigm. It is therefore often very difficult for scientists to let go of long held views *even when the evidence seems clear that a new framework explains things much better*.

In the case of geological interpretations and ideas at deposit level, we have seen numerous examples of deeply held beliefs around specific ideas by geologists, where conflicting evidence is resisted strongly. This is one reason why, as much as familiarity with a deposit is valuable, a well-reasoned and constructive challenge to the status quo may be more so. These challenges often come from those with less commitment to previous ideas (Solomon, 1992).

An important conclusion for mine geologists to draw from Kuhn (1996) is that there are no so called 'pure facts'. Theory, or more broadly, paradigm, is always a framework for any observations. While the framework enables hypothesis construction, it can also constrain our thinking as geologists. Once we are anchored within a given paradigm, identifying potentially contradictory or falsifying observations becomes harder. The possibility that we fail to see or we misinterpret evidence because of this anchoring is heightened for a range of socio-cognitive reasons as explored by Solomon (1992) and summarised in Vann (2005).

A good example of a paradigm shift relating to a specific deposit model is the world class Olympic Dam Cu-U-Au deposit in South Australia. Original exploration models and early publications emphasised a syngenetic or syndiagenetic model for deposit formation in a sedimentary breccia (Roberts and Hudson, 1983). Continuing data collection as the deposit was further explored and then accessed by mine workings resulted in a major re-evaluation of the deposit origin (Selby, 1991), leading to the current hydrothermal breccia model.

## MULTIPLE WORKING HYPOTHESES

We have argued above that a key to development of robust models in mine geology is that they be examined critically in the spirit of falsification. Chamberlin (1897) preceded Feyerabend (1993) in proposing that it is advisable for scientists to keep more than one competing hypothesis alive. Such models should be mutually contradictory, whilst agreeing with the available data. This idea has great power in mine geology, where even relatively minor changes in interpretation may have serious economic implications, and major differences may have economically disastrous consequences. This is true for both the geological interpretation (for instance in interpreting shear continuity between *these* two logged shear intersections, rather than *those* two), and for the translation of these interpretations into 3D wireframes (for example, connecting two contact points in adjacent holes with a straight line versus inserting additional control points if the contact is interpreted to be curved).

There are clearly major benefits in being able to assess the impact of alternative hypotheses, and thus justify the expenditure necessary to test (or attempt to falsify) these hypotheses. In the first instance it is necessary to identify the key assumptions, then envisage plausible alternatives and test these by directed data acquisition (eg drill holes in strategic locations where conflicting hypotheses predict different geometry). Until recently, it has been difficult (or impractical) to generate and evaluate even limited numbers of alternative geological models (or more correctly the 3D computer representations of these). Increasingly, though, the generation of multiple, divergent, digital 3D models is practically achievable because of faster computers and improved automated or semi-automated 3D modelling tools. A straightforward and insightful example of the use of multiple geological interpretations in resource risk analysis is given by Jackson *et al* (2003).

If models are to be evaluated rigorously then having external critical review as well as a robust internal critical review culture, is essential. We believe that the establishment of multiple interpretive teams for major capital projects is a prudent and practical risk reduction (and thus value creating) mechanism, although this process must be well managed. It is difficult for an individual team (and more so an individual geologist) to develop a positive and genuinely critical environment for the generation of geological models. The interpretive process requires that we invest effort in imagining ideas, and it is human nature that once we have invested that energy we become the champion of those ideas. Geologists are not exempt. T C Chamberlin, a 19th century geologist, described this eloquently:

> *The moment one has offered an original explanation for a phenomenon which seems satisfactory, that moment affection for his intellectual child springs into existence; and as the explanation grows into a definite theory, his parental affections cluster about his offspring and it grows more dear to him. While he persuades himself that he holds it still as tentative, it is none the less lovingly tentative and not impartially and intemperately tentative* (Chamberlin, 1897, p 358).

It is interesting that the ideas of Chamberlin, whilst known by a small proportion of mine geologists, have wide currency and use in other fields (for example in biology, see Platt, 1964; Elliot and Brook, 2007).

## THE PROBLEM OF MODEL 'VALIDATION'

Oreskes, Shrader-Frechette and Belitz (1994) and Oreskes (1998) argued that verification or validation of scientific models of complex natural systems is *impossible*. This assertion has direct relevance for numerical models like resource estimates and conditional simulation models of spatial variables (Journel, 1974), which are increasingly used in mining applications.

In essence, agreement between models and new observations or predictions can only be taken as partial confirmation since acquisition of further data may yet invalidate the model. This is similar to the point made by Cleland (2001, 2002), that predicting and then confirming a 'smoking gun' increases the confidence we have in a model but that such confirmation is always interim and partial. The incomplete access we have to natural phenomena (we *never* have full knowledge of the orebody at every scale) means that models can thus only be evaluated and deemed to be fit for purpose and cannot be 'validated' in the strict sense. This is an important practical point because geologists must communicate the uncertainties in their models clearly in order to justify improvements. Giving geological models the status of 'truth' is always a mistake.

Oreskes, Shrader-Frechette and Belitz (1994) asserted that the primary value of models is 'heuristic'; ie to be used in a pragmatic way to guide decisions and further investigations. This aligns with the statement by George Box, quoted at the opening of this paper, that *the model is by definition wrong*, at least to some degree, *because it is a model not reality*. The real question is whether the model is useful – can it practically help guide better decisions?

The use of so called 'heuristic models' (experience based models, akin to 'rules of thumb') as a basis for decision making under uncertainty has been investigated in many fields (Tversky and Kahneman, 1974; Kahneman, Slovic and Tversky, 1982). The influence of psychology in making decisions in the face of the inherent uncertainty in geology is an important area for current and future research, for example there is ongoing research into this area of 'behavioural geoscience' at the Centre for Exploration Targeting (University of Western Australia and Curtin University of Technology).

### Rejoinder – the 'truth' of climate models

As an aside, those discussing climate science and climate models (which are numerical models of complex natural systems) would do well to heed the issues raised above relating to the *status* of models.

Statements that numerical climate models (or predictions based on them) have the status of 'truth'; and that, consequently, debate about the validity of predictions from these models is 'finished', are highly misleading. It is a fundamental attribute of predictive models of complex natural systems (like mineral deposits or climate systems) that they cannot be *verified*; such models have intrinsically interim status. Successful comparison of predictions generated by such models against new observations can be confirmatory and thus increase confidence in the hypotheses encapsulated in the model. Such agreements do not lend the model the status of truth; however, or 'end the scientific debate'.

It is true that some hypotheses, especially those that can be subjected to repeated controlled experiment, have been subjected to such repetitive scrutiny that it is very hard to imagine them being overturned. The basic laws of motion in the physics of the macroscopic universe fall firmly into this category.

The idea of 'retrodiction' in geology, meaning that a hypothesis (or hypotheses) can be framed and then the past record repeatedly interrogated to look for confirmatory traces, has been proposed (Kitts, 1978). The idea of the biological evolution of taxa over geological time is an example of a hypothesis that has been confirmed by repeated evidence in this retrodictive mode (Dawkins, 2009). In legal parlance it is beyond reasonable doubt.

Most *predictive* models do not have this status in geology (or climate science) and remain interim in nature, even if we steadily acquire more confidence in them as we fail to falsify them, or gather more confirmatory evidence. It serves well for mine geologists to remember this, and communicate it in a business context.

## CONCLUSIONS

Constructing testable models to explain reality is the definitive aspect of any activity claiming to be 'scientific'. In mine geology our primary job is to build models which accurately predict reality to an acceptable degree, be they geological models, grade control models or resource models. What's more, in mine geology we are often in the excellent position of having hypothesis-testing options, such as additional samples or new mine openings that can provide relatively rapid feedback on how good our predictive models are.

In mine geology, the idea of falsification is very practical and useful but needs to be considered in a sophisticated way. In particular, the collection of data and design of experiments must ensure that the hypotheses we frame are falsifiable, at least in principle. If not, our work cannot be defended as being 'scientific'. Note that some theories may be falsifiable in principle but not in practice *using current technology and methods* (Einstein's famous though experiments regarding quantum mechanics spring to mind).

Another important framework for considering the work of mine geologists is the idea of seeking confirmatory evidence (especially in the case of novel ideas). If we can find evidentiary traces that were predicted from a hypothesis *prior* to examination of the geological record, we increase the confidence we have in a model significantly. Testing of models with more than a single explanatory hypotheses is particularly powerful in the case of retrodictive modes of science, where we are trying to explain a set of evidence present today (in the geological record for example) that could arise from multiple possible mechanisms in the past. This drives the usefulness of multiple working hypotheses as a mode of thinking in geology.

Finally, the framework of assumptions that geologists use is often unchallenged. The idea of working within an unchallenged paradigm is not necessarily negative, and may well be required to generate useful results. However; it is important to be attentive to (and on the lookout for) constructive challenges to the status quo, because from this come all really important new scientific breakthroughs.

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