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Safety and the Logic of Hazard

Health and safety culture as a research problem.

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Executive Summary.

Our research proceeds from the assumption that, despite the unique forms of hazard traditionally associated with mining in South Africa, South Africans in significant measure tend to take a neglectful attitude toward Hazard *in general*. Our approach to the issue is not psychological; we prefer, instead, to consider the matter from a logical point of view, with a view to encouraging inquiry that identifies and qualifies the *forms of practical reasoning* that make such negligence possible. The research draws on the methods of logical and pragmatic analysis to clarify the notion of a health and safety culture *as a research problem*. We base these methods on the broader logical doctrine of *pragmaticism* (Peirce, 1992; 1998).

We adapt elements of Peirce's (*CP* 7.139-157) *economy of research*, so as to infer some of the criteria upon which the potential for research projects into a culture of safety can be assessed when researchers apply for funding. The theory's main point is that there is in the field of research in general, a ratio between research expenditure and the increase in knowledge that can be graphed in a manner comparable to the supply and demand curves of classical economics (Peirce, *CP* 7.147).

Explicit discussion of a health and safety culture occupies a bit less than two pages in the first volume of the Commission's report, and opens up two possible lines of interpretation, each of which may conflict with the other. 1) One would want to know whether or not this approach therefore views organizations and institutions as having a *special* relation to health and safety, distinct from the relation of other social aggregations to health and safety. 2) Secondly, it is unclear from the Commission's conception of a culture of safety, whether this entails a system of "separate but equal" (or similarly discriminated) cultures of safety. Thus one might suspect that health and safety in mines is a "culture" with characteristics distinct from that in, say, maritime transport, or passenger airlines.

We raise the issues because they have some bearing on the problem of defining 'culture'. Different collective responses to the idea will tend to draw on the ways representatives to the tripartite policy, regulation, and research advisory bodies customarily used the concept of culture in their historical dealings. Management's conception of *culture* might range over questions of taste in relation to themselves, but have strong ethnological overtones when considered in relation to South African mines' largely migrant workforce. Worker representatives or advisers, on the other hand, may see *culture* in somewhat more sociological or political terms.

We suggest such inquiry can proceed on the basis of the logical distinction between the concepts of *risk* and *hazard*. The kinds of acts we consider to be *risky* have an element of betting or gaming about them:

Definition: Logically, a hazard is essentially that of which it can be truly predicated of the Quality of a situation ***X***, such that in our attending to the matter we can assert that "***X*** is hazardous". For any situation to be predicated as Hazardous, therefore, entails a sustained series of encounters from which can be deduced a frequency with which a distinctly recognizable range of deleterious outcomes would be

experienced. If we are ignorant of such sustained sequences, we may predicate no more of such encounters than that they are *dangerous* or *risky*.

The initial research object of any managed health and safety programme, it follows, is twofold:

1) to study the record of relevant encounters in order to establish an initial estimate of the frequencies of various deleterious outcomes; and

2) to inquire into the mechanisms of such outcomes so as to establish means and procedures for reducing as far as possible their occurrence.

The subsequent object of research is to reduce progressively the "thumb-suck" factor involved in the estimation of hazard. An accident is what occurs under two possible sets of circumstances:

1) we reason correctly about the performance of a tool or equipment that malfunctions in the performance of the act; or

2) an apparently reasonable habit turns out to be unsuited to the context in which ordinarily functional procedures, tools, or equipment are being used.

If different classes of practical reasoning are then related to the *logical* constitution of the relevant hazards, the disjunctions between intentions and actions may be more clearly discerned and appropriate strategies devised to mitigate this.

The rationale behind the following recommendations is that taking on specificiable elements of the reality of hazards that affect safety and health, makes it possible to assess the feasibility of proposed research in terms of the Economy of Research.

We recommend that consideration be given to supporting the establishment of a dedicated electronic archiving service at a university with an established record of mining-related research.

We **recommend** further that the servers and administration be located at one of the universities, like Wits or Pretoria, with a long history of mine-related research. This should not exclude from consideration a centre like UKZN's Interdisciplinary Accident Research Centre.

We **recommend** that the Leon Commission's call for the Inspectorate to incorporate a legal division be taken to *include a research function*.

Finally, we **recommend** that the Mine Health and Safety Council, in conjunction with one or more research institution(s) carrying out established research projects, arrange an international conference on the Culture of Health and Safety in Mining, to be held at least 24-36 months *after* the pre-publication archive service has been commissioned.

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1. Introduction.

We present this report in response to the Mine Health and Safety Council's "Culture of Health and Safety" research thrust. Our project is based on PhD research into the logical ground of the concept of hazard, and draws on some eighteen years' employment in mining (1967-1985), in a variety of technical occupations. On the basis of this experience, we believe it is quite sensible that the Mines Health and Safety Act should recommend that steps be taken to encourage "a pervasive occupational health and safety culture in the mining industry and the establishment of the relationships between risk-taking behaviour and lifestyles outside the industry and risk-taking behaviour and conformance in the workplace" (MHSC, 2003: 22). The public prominence of the Leon Commission of Inquiry into Safety and Health in Mining certainly gave, and continues to give, great authority to its findings. Indeed, it is common cause that South Africa's deep-level hard-rock gold and platinum operations are highly hazardous working environments, requiring concerted measures to maintain the safety and health of all who work at these great depths. We therefore base our analysis of the health and safety culture as a research problem on the discussion of this matter in the Report of the Leon Commission (Leon et al, 1995).

We proceed on the basis that the Commission's discussion of the culture and safety in mining, and the Mine Health and Safety Council's linkage of the latter to a broader social perspective, demands research conducted at a somewhat higher level of abstraction in its initial stages. We have, consequently, summarized the relevant aspects of the concept of *Culture*, both logically and genetically, that would impact on the manner in which research proposals might be compiled under the rubric of the SIMRAC Health and Safety Research Thrust. We have taken the position that, initially, such research will be prosecuted under a general sociological or ethnographical umbrella, and that as a result projects will be of a somewhat different nature to those that have been completed (or are underway) as `engineering solutions'.

The layout of the report is as follows:

Section 2 offers a brief overview of the socio-cultural nature of South Africa's post-apartheid mining environment. We note that although the safety record is hardly admirable, there are areas where the safety record is comparable. We will argue that this provides a context for comparative inquiry, with wider applicability in the global mining arena.

Section 3 introduces the broad methodological umbrella of *pragmaticism*, serving to introduce the more detailed discussion that follows.

Section 4 discusses pragmaticism and the approach to the economy of research developed under its doctrine.

In *Section 5* we return to the Leon Commission Report, and discuss some pertinent issues that arise from the discussion therein on a health and safety culture in mining. We note that certain ambiguities are possible in the ways this issue can be interpreted, despite the Commissioners' clear understanding of the complex nature of the matter.

Section 6 comprises a brief summary of the genealogy of the modern concept of *Culture*. We note that there are cognate concepts, especially *Tradition* and *Custom*, that provide the prospective researcher with somewhat less vague topics of inquiry.

Section 7 outlines four broad areas of contemporary discourse in which the term *culture* is commonly used. We briefly discuss the ways these meanings might affect deliberations in a tri-partite decision-making process.

Section 8 draws on considerations in the economy of research to discuss formally some of the complexities arising when the various meanings of *culture* previously outlined, are treated as preference rankings. We use the problem of the *Voter Paradox* to illustrate the possibility that consensus in committee may well turn out not to present the representative view of the different parties' constituencies.

Section 9 contrasts this research problem with the projects undertaken to develop new equipment and procedures that will improve the material conditions for safety and health in mining. We draw on the principles of the economy of research to show

that it is better to maintain the current the level of support for technical development research, since this will continue to yield more immediately useful results.

In *Section 10* we outline an alternative approach to the sociological and ethnographic context of safety research. This uses the logical aspect of the pragmatic doctrine to distinguish between *Risk* and *Hazard*, marking the distinction between these concepts in terms of *practical* and *formal* reasoning, respectively. We outline some of the directions in which the relevant research might be pursued in future.

In *Section 11* we develop this distinction logically. We clarify the empirical basis for a sensible initial research programme that will provide a way of classifying the habitual preferences people display in acting in hazardous situations that lead to accidents.

In *Sections 12 and 13* we conclude by offering recommendations as to what our analysis indicates would be the most effective immediate actions to kick-start the general inquiry into what we understand to be the topic of the idiomatic expression "health and safety culture in mining."

However, the Commission's implicit conclusion that the South African mining industry constitutes a special case (1995: 37-38), requiring unique attention from the research community, could ultimately lead to findings that conflict with safety and health issues in other sectors or in the social realm at large. Our research therefore proceeds from the assumption that, despite the unique forms of hazard traditionally associated with mining in South Africa, South Africans in significant measure tend to take a neglectful attitude toward Hazard *in general*. This we presume simply on the basis of commonly reported facts: road accident statistics that indicate a higher than average rate of death and injury; the commonly reported very high rate of HIV infection among South Africans; and the Leon Commission's own findings that the mining accident rate, both fatal and non-fatal, is never the less unacceptably high by international standards. Having acknowledged this, it is important to stress that our approach to the issue is not psychological; we prefer, instead, to consider the matter from a logical point of view, with a view to encouraging inquiry that identifies and qualifies the *forms of practical reasoning* that make such negligence possible (and,

perhaps, even *acceptable* in some quarters).

2. Some Contextual Reservations on the Population to be sampled.

We do qualify these assumptions, on the other hand, by recognizing that people who come to work in the mines today find themselves in a unique social and political position. We consequently concur with the Leon Commission's (1995, Vol 1: 69) observation that

> The South African mining industry has two human problems that are not encountered by industries in more developed countries. Due to the unfortunate historical background of the country, the under privileged section of the population has received a spotty and, in many instances, very inadequate education. These effects are compounded by the fact that the labour force on the mines is recruited from many national and cultural backgrounds. As a result the employees do not have a common language of communication, but speak several African and European languages.

We also note that by far the greatest number of actual and potential mineworkers come from a population that is, as it were, in the midst of the transition from an erstwhile authoritarian form of life towards a dispensation grounded in widely recognized democratic norms. However, it is worth noting that the safety record of South African mines, although hardy admirable, is not the worst globally. The Leon Commission (1995, Vol 1: 15-16) noted that International Labour Organization (ILO) records for 1989 and 1991 had indicated that the South African record was better than those of Turkey, Yugoslavia, Papua New Guinea and Pakistan. Recently there have been frequent media reports of major accidents in the Chinese coal mining industry, involving tens and even hundreds of fatalities. The Leon Commission's comment on South Africa's labour force raises an echo in respect of the mining and industrial situation in China:

> Township and village enterprises (TVEs) … are making a significant economic contribution, and their national industrial output exceeds the output of state-owned enterprises. They are also a major source of employment, creating more than 120 million jobs. However, their adverse impact on the environment has become a major concern both for the Government and the public. The protection of workers' safety and health also lags far behind the economic performance of TVEs. Causes include the general lack of awareness, low level of education of managers and workers, outdated technology, and non-availability of technical and financial support (Machida *et al*, 1998).

It is arguable that there are political parallels between South Africa and China, on the basis of which one might think their health and safety environments are comparable. However, we do not believe that this ought to be pushed too far. The corporate industrial sector in South Africa is incomparably better organized historically, and, for all the impact that apartheid may have had in protecting this sector, those who work there (or may come to work there) can take recourse to a record of safety research and regulation of long provenance. We do not disagree with the Leon Commission's finding that South African mining (at least, up to 1995) had not succeeded in converting this record into the substance of a safe working environment.

This condition will not last forever, however, and we therefore accept that any regulations enacted on the basis of the findings of present-day research may themselves be of a transitional nature, given that many different kinds of relations in the wider social, political and economic environments are subject to the stresses of their own transitional status. This possibility, we will argue, does not exclude those managing the present transitional situation from recognizing that, in very general terms, safety and health are the consequences of forms of conduct based in clearly logical reasoning, applied to identifiably quantifiable conditions encountered in people's everyday going-on. Put differently, the fluidity of a given historical and political context does not affect the nature of the hazards associated with health and safety; such a situation merely provides an opportunity to change people's *relation to hazard* for the better. The present report analyses some of the paths along which health and safety culture research could be thus pursued. It is beyond our competence to predict how such opportunities would be exploited.

Yet the comparison with accident rates in developed nations like Sweden and Canada should not be dismissed on the grounds of development differentials. Although it is beyond argument that much of South Africa is underdeveloped, primarily as the economic result of previous racial separation policies, the mining industry here by no means takes the general Third-World form of isolated pockets of development in the midst of *broader* neglect. Indeed, most of South Africa's historical mining areas are located at the nodes of a nationwide network of transport, mining, industrial, agricultural, and commercial development that divides pockets of

more or less underdeveloped countryside from each other. While the country remains a major mineral exporter, a significant proportion of the mineral output is used domestically for metal production, electricity generation, and other industrial and commercial purposes. We understand that the main goal of the present transitional process is, in fact, to extend to all citizens access to employment in, and enjoyment of, the general system that exploits the products of South Africa's existing mining, industrial, agricultural, and commercial strength.

Given, then, that South African mining has developed into a sector that does rather more than "get materials out of the ground for their immediate use-value", and as such, in a developed environment, connects with the institutions of many other forms of economic and social activity, changes in one sector usually have consequences in the ways other sectors carry on their business. The development of mining *as we know it today* is coterminous with the rise of industrial society, its transportation technology, and the inherited financial and commercial customs, developed over generations, associated with this form of society. As the reach of the relations of industrial production, distribution and consumption have expanded into more spaces of social life, so the products of the beneficiation of mining commodities have expanded to fill these spaces. Conceptually, however, the inherent historical limit of any one industrial realm still tends to be the boundary of the national state. This is more than merely a linguistic and "cultural" limit, but one defined by considerations of *continuity in the affairs of everyday life*. Mining people and institutions can carry on precisely because they can assert (or display) some measure of confidence in quite non-mining matters such as the constancy of national currencies, railway gauge measurements, the rule of the road, the historical issues that both unite and divide national political formations, and many others. Despite the much-cited relegation of the idea of national identity to “imagined community” (Anderson 1983), the development *over several generations* of such structural and political factors results in a set of relatively coherent *inherited habits* within which citizens feel more or less at home (Tomaselli and Shepperson 1997: 212, 215). Thus the issue of a health and safety culture in mining must be examined in so far as it would be located within a technical, social, political and historical environment within which the community of citizens has *inherited* the technical practices, social relations, monetary forms,

political issues and historical narratives that they accept (or challenge) as *theirs*. We may be able to speak generally of health and safety in all kinds of contexts; but in relation to mining as we understand that business here, they are strictly understandable only within the ambit of the modern social realm.

To end this section, we must add that responsibility for the eradication of underdevelopment should not be made to rest primarily with the mining industry, irrespective of the historical economic advantages the sector enjoyed because of the presence of cheap migrant labour. The mining houses, and organized labour in the industry, can (and ought to) make significant contributions to this process, but the broader process has to include equal or more significant inputs from other business sectors (commerce, manufacturing, transport, agriculture, services, health, and so on), government, and the financial and investment sector. We stress this purely because the present-day success of these other sectors is very much a function of the historical success of South African mining, whatever the role of the latter's labour exploitation in the historical achievement of this success.

3. Methodology: on the predication "*is hazardous*".

There is a likelihood that some disagreement may arise over the precise manner in which one may define the term *culture*, and to avoid this the present research draws on the methods of logical and pragmatic analysis to clarify the notion of a health and safety culture *as a research problem*. We base these methods on the broader logical doctrine of *pragmaticism* (Peirce, 1992; 1998), the most frequently cited maxim of which asserts:

> Consider what effects, that might conceivably have practical bearings, we conceive the object of our conception to have. Then, our conception of these effects is the whole of our conception of the object (Peirce, *EP* 1: 132; *CP* 5.402).

Our analysis indicates that there are four possibly conflicting classes of *practical bearings* inherent in the different ways people define culture today, and we therefore apply this maxim to two related issues regarding safety, on the understanding that our results will have a bearing on a comparable analysis of health in the workplace. Because our methods rely primarily on the logical development of concepts, we first

provide a broad historical review of the relevant literature on the concept of culture. For brevity, we have taken the discussion on health and safety culture directly from Section 5.6 of the Leon Commission Report, Part 1, as the most readily comprehensible starting point from which to conceive the most immediately attainable research programmes.

Our focus in the present report is principally on safety, leaving aside the question of health for others to investigate; however, we anticipate that there will be considerable overlap between our discussion of safety hazard and the role of hazard in health. Later in the report we will define logically the concept of safety as the mitigation of *hazard*, for the present taking the term to refer to that quality in situations which, independent of our beliefs or purposes, has a quantifiable probability for causing harmful consequences when *not attended to rationally*. This approach enables us to consider safety issues from the point of view of institutions' readiness and capacity actively to address hazards associated with their endeavours. Never the less, there is no reason to believe that a similarly logical analysis of health as a hazard-related issue, is possible within broadly the same methodology. The approach further has the advantage of providing an analytical context within which formal reasoning about hazard can be addressed comparatively with the practical reasoning required for the day-to-day operations of institutions. As the basis for comparison, we employ the distinction between formal and practical reasoning, the Scholastic doctrinal distinction between *logica docens* and *logica utens*, as occasionally do logicians in the modern tradition; specifically, our method relies on Charles Sanders Peirce's association between formal logic and the discipline required for practising the sciences (Peirce MS 655; *EP* 2: 201; 340), and the *moral* basis of practical logic in the conducting of everyday affairs. Safety relies on the management of hazards that are *known*, and our conception is quite distinct from the currently fashionable (academically and politically) *precautionary principle*. The latter, when pursued strictly, *proscribes action* on the grounds of possible harm, with no further need to demonstrate the probability of harm than the fear that something *might* go wrong (Holm and Harris, 1999; Kurland, 2002). Similar considerations are applicable in respect of other quantifiable matters like productivity, efficiency, sales, distribution, and so on.

Finally, we adapt elements of Peirce's (*CP* 7.139-157) pragmatically conceived *economy of research*, with a view to unifying the preceding methods so as to infer some of the criteria upon which the potential for research projects into a culture of safety can be assessed when researchers apply for funding. Written between 1876 and 1877, and included in the 1876 US Coast and Geodetic Survey Report (published in 1879), Peirce's theory is a radical development of Stanley Jevons's (1871/1957) mathematical political economy and Cournot's (1838/1929) theory of the firm. The paper is highly technical, with about one-third of its length taken up by Peirce's mathematical derivations of his bi-directional funding allocation function, which we explain in more detail below. We will focus more on application of this function, and leave study of the mathematics to those better qualified than we. We provide a copy of the *Note* as Appendix B. The intellectual background to the *Note* is quite relevant to the present situation, and we will summarize this briefly.

4. Pragmaticism and the Economy of Research.

Between 1867 and 1870, Peirce (1839-1914) published a series of highly technical mathematical papers that, in various ways, have had a lasting impact on the subsequent development of symbolic logic. Among the applied research he carried out using elements of this mathematical work, he developed a sophisticated version of the concept of *probable error* (a precursor in 19th century statistical theory that anticipates the modern *confidence interval*; Peirce, *W*3: 114-160; see Wible 1998). Of less immediate importance in detail, but central to the entire subsequent history of the philosophical pragmatism, the first of these mathematical papers offers a very philosophical argument for a three-way system of universal predicative categories that relates each successive category *ordinally* and *presuppositionally* to the others. This categorical principle tends to come across as rather complicated when first encountered. We suspect that this is the result of people commonly reasoning quantitatively in terms of cardinal abstractions, whereas Peirce abstracted his categories ordinally. This is merely a preliminary hypothesis, and not important for the present report. Since the principle is central to our method of analysing Hazard, and has implications affecting the distinction we employ between formal and practical reason, we offer the following brief introductory outline of Peirce's thinking.

Over a period beginning in the mid-1860s, until a very few weeks before his death in 1914, Peirce (pronounced ‘purse') produced a prodigious amount of manuscript material covering a wide variety of scientific, logical, and philosophical topics. Widely respected in the early and middle years of his life, Peirce was effectively barred from further full-time research or teaching after a (somewhat manufactured) marital scandal in 1884-5 (see Brent, 1989). Never the less, he continued to pursue inquiries of extraordinary width and depth, even under the especially trying conditions following the economic depression of 1893. His posthumous papers, donated to the Harvard University Library, required two large cargo sleighs when transported from his home in Milford, Pennsylvania, USA. This very brief biographical note is offered to give readers an indication of the time Peirce devoted to his inquiry, the consistent logical and mathematical qualities of which are still being discovered some nine decades after his death. His output includes major writings on mathematics, metaphysics, geodesy, astronomy, economics, phenomenology, and logic; he also produced lesser but still-relevant work in physics, metrology, engineering, cartography, and lexicography, amongst others. His exclusion from the mainstream of American academic history may be one of the great scandals of the twentieth century; however, there was always a cadre of scholars who ensured that the work of preparing a definitive edition of his writings could finally be undertaken.

References to Peirce's writing are subject to some difficulty, principally because of the haphazard manner in which his papers were first edited by topic without reference to the date of writing. Because this original edition, *The Collected Papers of Charles Sanders Peirce*, published in eight volumes between 1936 and 1958, remained the standard reference for some decades, we adopt the convention agreed upon for this edition by citing by volume number, followed by paragraph number. A reference to Peirce, *CP* 7.138, therefore, indicates the source of our citation or material at Paragraph 138 of Volume 7 of the *Collected Papers*. Subsequently a project is underway at the University of Indiana in the USA, to produce a properly edited chronologically ordered collection of Peirce's writings. We refer to the new edition, *The Writings of Charles Sanders Peirce* (the final edition is projected to fill some twenty-five to thirty volumes) by volume and page number; for example *W2*:

221-5 refers to material found between pages 221 and 225 of Volume 2 of the *Writings*. In anticipation of the completed edition, the Chronological Edition editors have produced a two-volume chronological selection, *The Essential Peirce: Selected philosophical writings*, Volume 1 in 1992, and Volume 2 in 1998. These we reference as *EP*, followed by volume number and page(s). Other special editions, like Peirce's Cambridge Conference Lectures of 1898, we reference by the modern edition's publication details.

In brief, Peirce's overall *systematic philosophy* begins with the abstract deduction of three universal categories: Quality (of feeling); Reaction (of actualities); and Representation (of meaning and truth). These categories are ranked presuppositionally, in that it is possible to cognize a Quality without reference to anything else; however, to cognize an actuality presupposes an already cognizable Quality; finally, to Represent anything, or (more correctly) for anything to *represent itself*, presupposes already cognizable Actualities that in turn are presupposed by Qualities. Representation, that is to say, is the final term of a three-level *Ordinal Ranking* of modes of reality. Peirce, by the time of his mature writings, may be classified as possibly the most radical philosophical Realist since Mediaeval times. Indeed, as early as 1871 Peirce had declared that scientific logic was not strictly conceivable without accepting the thirteenth century doctrine of John Duns Scotus that general terms operate really in the universe, independently of the logician's opinion (*EP* 1: 83-105). This demand can be a bit hard to swallow for most people who were raised in modernist and postmodernist environments: both latter doctrines descend from the fourteenth century doctrine of William of Ockham, who (contrary to Scotus) tried to demonstrate that terms for general concepts are merely conveniences people apply to make sense of actualities that they experience as being alike in some manner relevant to their interests.

In analysing the various common definitions of the concept of *culture*, we follow Peirce's classification of modern philosophy, presented at a special lecture in 1903, on the basis of how the appeal of each different kind of system depends on its postulating that one or more of the three universal categories are really operative in the universe. Each category has its own peculiar *mode of reality*. In the case of

Qualities, it is the mode of possibility, that something *could be* the case, no matter how vaguely this may be possible. Indeed, Peirce speaks of qualities as "Real vagues". In the category of Actuality, the mode of reality is that of *existence*, that which is predicable of the brute or blind clash of experience. In Representation, the proper mode of reality is that of what *would be* under the circumstances proper to its eventuation. In reverse order, Representation entails the reality of continuity in the Universe; Actuality that of materiality; and Quality that of Chance.

The two paradigms of research that must be accommodated in trying to establish a culture of safety, that of Humanities for culture and those of Engineering and Medicine for safety and health, appear to employ incompatible methodologies because of their distinct approaches to the reality of the categories. To clarify the logical ground for this apparent incommensurability, we turn to Peirce's assertion (*EP* 2: 160-195) that metaphysics at the beginning of the twentieth century could be broadly but accurately classified as to how any doctrine accepted individual categories or combinations thereof. Given his system of three categories, then, Peirce (*EP* 2: 180) identified and described seven broad types of metaphysical systems as follows (we have changed the order slightly for clarity, and added examples of our own):

a) Category of Quality only: Nihilism and Idealistic Sensualism. These are doctrines that sometimes are mistakenly attributed to Friedrich Nietzsche, but may be detected in some of the arguments of Herder and Rousseau.

b) Category of Actuality only: this is more properly attributable to Nietzsche, especially in his somewhat contestable later writings on the Will to Power. A recent version is to be found in Jean-Paul Sartre's early philosophy, especially *Being and Nothingness* (1989).

c) Category of Representation only: Hegelianism of all shades. Largely self-explanatory, referring to the totality of reality in the development of the Idea, as both Quality and Actuality are "sublimated" (*aufgehoben*).

d) Categories of Quality and Actuality combined: Ordinary Nominalism. This is the doctrine of the great bulk of the academic Humanities in the present, and is the evolution of the doctrine of Ockham in the early fourteenth century. The defining modern philosopher is Thomas Hobbes (1651/1968).

e) Categories of Quality and Representation combined: Berkeley's doctrine, in which Actualities are only possible when they are perceived.

f) Categories of Actuality and Representation combined: the dualism of Rene

Descartes, as also taken up by Leibniz and Spinoza. Many physicists, chemists, engineers, and other natural scientists hold to this system, even if tacitly; the epistemology of the Philosophy of Science approach in the Analytic tradition.

g) All categories combined: This is more like a class of doctrines, which can be divided across Peirce's system, Kant's transcendental metaphysics, the Scottish Common Sense school of Reid and Stewart, and Aristotle's special developments of Plato's philosophy. We argue for the adoption of this approach in safety and health inquiry.

One can readily note how Humanities and engineering/medicine subscribe (however implicitly) to different modes of predicating the reality of the subject-matter. The former follow tradition d), the latter belong to tradition f). The Humanities position, in the main, is that law, regularity, and representation are inoperative in the Universe, being essentially constructed as an arbitrary convenience in a given historical or cultural or linguistic context. The Engineering and Hard Sciences' historical paradigm, on the other hand, treats the reality of feeling or qualities as 'mere' accidents, and like classical physicists, chemists and geologists they treat these as capricious, individual and 'subjective', contributing little or nothing to truth. On the other hand, these disciplines' capacity for productive innovation is inconceivable without treating Representation as an independent reality embodied in scientific law revealed through research.

Peirce wrote the 1879 paper on the economy of research during the period that he was beginning to work through the implications of Scotist realism for his system, and in it he considers the different ordinal relations between utility, cost, and effort as grounds for judging how research funds should be divided between projects. Despite the highly technical nature of the paper, its conclusion is very relevant to our concerns:

> It is to be remarked that the theory here given rests on the supposition that the object of the investigation is the ascertainment of truth. When an investigation is made for the purpose of attaining personal distinction, the economics of the problem are entirely different. But that seems to be well enough understood by those engaged in that sort of investigation (Peirce, *CP* 7.157).

What this conclusion suggests is that we may do well to distinguish between two broad areas of research in respect of mine safety in general. On the one hand, there is what we consider to be the more immediate research and development work necessary for creating the technology, systems, and procedures that can be

implemented within a definable time limit. Already, much of the research into, say, low-noise drilling equipment, effective mobile hanging-wall support systems, or safer and ergonomically suitable underground transport, can be understood as directed towards the implementation of new practices, or new and safer ways of performing traditional work. As products developed in these projects enter service, the forms of conduct required for their everyday use will have some cultural impact, but only within a restricted sense that is infrequently used today. Other potential research, for example the cataloguing and preservation of remaining documents from accident inquests, and earlier Commissions of Inquiry, can contribute to a broadening of the base of a tradition in safety. This way it is possible to develop an enhanced safety tradition, so that safety training can be improved in the medium term, at not too great an initial outlay.

On the other hand, we see a parallel need for blue-sky research, in the sense that it is carried out with *no deadlines for implementation* in view. This need is the basis for the single substantive disagreement we can raise against the Leon Commission's recommendations. The broader logic of social development suggests that the mitigation of social inequalities seems to be accompanied by increased safety in society at large. We do not really have any specific data to suggest that this correlation is well understood as a causal process. In this context we suggest that research be instituted at a more abstract level, and the means for managing this using the economy of research involves a form of cost-benefit analysis that the Commission viewed as inapplicable. We provide the Commission's comments on Research Management as Appendix A to the present report. Given the nature of the divisions between the developed and underdeveloped parts of South Africa, however, and the rather obvious link this has with the migrant labour system, there is considerable room for research that reviews the available material in this field. This would form the basis, or part of the basis, of conceptual inquiry aimed at elaborating a synthesis that looks into long-term outcomes across several areas of safety, all from a more general point of view. We return to the distinction between applied and abstract research later; for the present, we must note that when a multi-party decision process is implemented, there is a clear opportunity for researchers more concerned with advocacy than truth to propose projects designed to support the

conceptual positions of one party involved in the process, or to undermine the positions of other parties. In the context of the necessary blue-sky inquiry, we consider it quite essential that the benefits of advocacy research be studied very closely as to their long-term costs. At the applied level, on the other hand, this consideration is well managed through ordinary technical judgements, and the Commission's recommendation that suitable expert consultants be employed should, for the most part, obviate egregious clashes of interest (see Appendix A).

The more relevant point in indicating this distinction, however, is that bodies like SIMRAC, MOHAC, and MHSC, amongst others, must ultimately "get the most bang for the buck" out of the budgets allocated to them for disbursement to research projects. Peirce's Theory of the Economy of Research proposes a concise and quantifiable basis for deciding on the allocation of funds. More importantly, in its later form it also considers the distinction between the discovery of knowledge, that is to say, scientific inquiry, and the distribution of knowledge. In the terminology of political economy, Peirce considers the economy of research as the ratio of research cost to research utility. Cost, on the one hand, is measured in the number of repeated experiments and/or observations necessary before a result is obtained. Utility, on the other hand, is measured as the reduction of probable error. The latter, in effect, corresponds with the improvement of precision in knowledge.

The theory's main point, however, is that there is in the field of research in general, a ratio between research expenditure and the increase in knowledge that can be graphed in a manner comparable to the supply and demand curves of classical economics (Peirce, *CP* 7.147). Thus the law of diminishing returns is just as applicable in research as it is in, say, manufacturing or mining. Using a variation on the principle of transitivity, and abstracting from the cardinal quantities of cost to the ordinal relation of preference, Peirce derived the mathematical basis for a bi-directional marginal utility curve that would facilitate analysis of the cost-benefit ratios of multiple projects. We now provide a brief description, and verbal outline of the development, reproducing Peirce's two-function graphic for a two-project cost analysis (Figure 1).

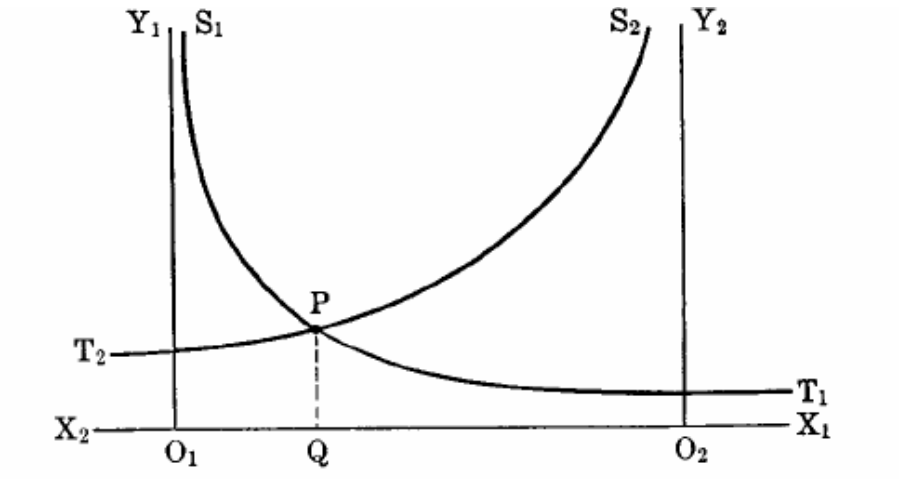

Figure 1. Peirce's bi-directional graphic representation of the cost to marginal utility functions for two research projects competing for allocations from the same funds (Peirce, *CP* 7.147. Reproduced with permission).

Let the horizontal line X_1-X_2 represent the funds available for the projects. The vertical lines Y_1-O_1 and Y_2-O_2 represent the *ratio* of marginal utility to marginal cost for each of the two projects. The cardinal value O_1-O_2 represents the total funding to be shared between the projects. The graph is essentially a development of Jevons's (1957/1871) *cardinal* representation of Diminishing Marginal utility for two competing commodities in the marketplace. Let the curves S_1-T_1 and S_2-T_2 represent the marginal utility to marginal cost ratios of each research project, each curve showing the *rate* at which marginal utility decreases over time for a given cost-effort input. Let us suppose, then, that the curve S_1-T_1 represents the rate at which a proposed research programme will yield immediate positive results in a relatively unexplored area of knowledge; S_2-T_2 is the line for a competing project that proposes to add knowledge to an already well-explored field. It follows from Peirce's derivations, reading off the abscissa from where the two curves intersect, that Project S_1-T_1 should receive the smaller proportion of the overall research funding allocation.

We suggest that the overall tenor of Peirce's theory permits us to approach the question of research into a culture of safety from a more economically manageable direction. Funding for safety and health research in South African mines tends to be allocated to the distribution agencies, like SIMRAC and MOHAC, on the basis of the collection levies, Fiscal allocations, and more or less specifically dedicated amounts from other research donor agencies. Once allocated, these funds tend to be

disbursed according to the relative urgency perceived for the different areas in which research can mitigate or eliminate certain hazards to the average worker's life and limb. Before employing this concept on the present research problem, especially in relation to the possibly partisan ways that parties to the research management process may demand that *culture* be understood, we review the Leon Commission's discussion (Section 6.5 of Volume I of the Report) of a health and safety culture. We then raise some questions about its possible interpretation in practice.

5. Health and Safety Culture in the Leon Report.

Explicit discussion of a health and safety culture occupies a bit less than two pages in the first volume of the Commission's report (Leon et al 1995: 66-67). This follows five detailed sections in which the Commission discusses and analyses the full range of submissions made to it during its sittings. The tone of this section clearly reflects the Commission's view (and, we suspect, exasperation) that the sum total of the evidence presented warranted some formal comment (more than an *obiter dictum*) on the general inattention toward health and safety in mines, and an equally formal statement about how to address this lack. We reproduce Section 6.5 of the Report in its entirety (edited slightly to enhance reading continuity), followed briefly by our initial assessment of how these considerations affect the research and legislation aspects a decade after the Commission sat:

> **HEALTH AND SAFETY CULTURE AND THE HIERARCHY OF MANAGEMENT**
>
> An acceptable health and safety culture at a mine, or in a mining company, can only be achieved by the management, from the top down committing itself to managing health and safety actively. This aspect of management must come before the other management functions, such as production or marketing. It represents an understanding by management that the people who work for the organisation are its most important asset. The concept of a health and safety culture is increasingly accepted in organisations that give a high priority to the health and safety of the persons they employ. The first requirement of such a culture is that it must start with a deep and visible commitment from the very top of the company - the Chairman and Managing Director of the organisation, down through all the ranks of management. Health and safety should be managed like all the other functions of the organisation, such as production or marketing, and receive priority attention. The most successful firms commercially are often the ones with the strongest health and safety culture. The concentration is on people as the route to excellence, both in the commercial and the health and safety field. The actions or omissions of people account for over 90 per cent of accidents and

> ill health, so a strong health and safety culture concentrates on people and their actions. Policy statements on health and safety form part of this culture. When such policy statements are a statutory requirement they often become voluminous and full of legal jargon and have little impact on the organisation. Simple statements of the commitment of chairpersons or managers to the health and safety of the work force are far more effective in conveying a genuine commitment to the welfare of the people employed by an organisation. The organisation of health and safety must be the same structure that runs all the other functions, such as production. The health and safety committee within the normal management hierarchy must establish sound policies for improving health and safety, provide manpower, resources, materials and motivation for the task, and review progress at frequent intervals. Good information is essential for this culture to be established. Health and safety issues can not be delegated to safety specialists and Government Inspectors. A Company safety organisation, headed by the most senior official must demand accountability, and provide resources to implement its policies. Health and safety accountability can not be compartmentalised into Safety Departments, or left to safety specialists. Health and safety is line management's responsibility. It can be managed in the same way as the other functions of an organisation. Health and safety is an integral part of every supervisor's and every manager's job. Every person who is responsible for the activity of people must accept responsibility for their health and safety. Each member of the management team should have well defined health and safety responsibilities, and each member of management should find time to be involved in and support the health and safety effort of his subordinates. Each level of management should continually show its commitment to health and safety, and hold more junior levels accountable, or praise them for a good performance. Salary increases and promotion should take into account health and safety performance.
>
> Objectives should be set for health and safety achievements as they are for production, and safety performance in terms of accident reduction, and health performance in terms of work days lost through ill health and other suitable criteria should be continually monitored. The Commission heard suggestions that an annual improvement in fatality rates of 3,4 per cent over a thirty year period would have provided a satisfactory situation in South African mining today … This objective would be considered very modest by many companies today. In the much neglected situation now existing in South Africa substantial improvements should be looked for in the coming years. The Commission noted evidence that showed that the value of a strong health and safety culture was well understood in some quarters … , but there was also evidence that in some major mining companies the safety culture was still developing …

This discussion seems to take the view that there is a special authority vested in industrial organizations or institutions, under which health and safety culture falls. This opens up two possible lines of interpretation, each of which may conflict with the other. 1) One would want to know whether or not this approach therefore views organizations and institutions as having a *special* relation to health and safety,

distinct from the relation of other social aggregations to health and safety. Such an exceptionalist conception creates the possibility that organizations or institutions are either *especially burdened* with health and safety duties common to society at large, on the one hand, or burdened on the other with *special duties* in respect of health and safety distinct from those of society at large. Either way, an industrial sector like mining, which covers a range of institutions and organizations, becomes subject to the public reasoning that it has some special dispensation associated with it, so that working in that sector either a) places especially onerous requirements on its members or functionaries, thus encouraging the inference that working there is not a desirable option; or b) demands exceptional training in health and safety, placing members or functionaries in a socially elevated sphere compared with their compatriots in other sectors or institutions. A comparable example of this kind of ambivalence may be seen in relation to the skills and discipline associated with a career in the Military (we are not arbitrarily constituting mining as the *equivalent* of a military career; we simply choose the military as an institution organized on lines sufficiently distinct from 'civvy street' so as to require a greater or lesser degree of adjustment in the recruits' ways of life on enrolment). On the one hand, a soldier's life may be perceived as subject to high levels of discipline and obedience, and thus at odds with a general perception that people should have the critical freedom to make life choices with greater latitude. Inside the military, on the other hand, trained and experienced personnel may reasonably conclude that the duties and skills of their calling confer on them a special status that elevates them above what they might come to perceive as the 'undisciplined masses' or 'self-interested Mob' it is their duty to defend. These are not so much mutually exclusive positions, as they may be taken as the termini of a continuum of possible relations between special institutions and society at large. Thus any individual citizen or soldier whatsoever may draw either inference, but their individual acts based on such a premise do not necessarily define the character of society at large. Reasoning of this kind does, however, come to assert a *stereotypical* quality; this may be of supreme importance in so far as later generations uncritically base their conduct on these representations. On the other hand, there is no *necessary* drawback to this. We will discuss this very thorny issue later in the report.

2) Secondly, it is unclear from the Commission's conception of a culture of safety, whether this entails a system of "separate but equal" (or similarly discriminated) cultures of safety. Thus one might suspect that health and safety in mines is a "culture" with characteristics distinct from that in, say, maritime transport, or passenger airlines. It is possible to construe the Commission recommendation that the Inspectorate of Mines be split into two distinct services, as based on reasoning of this kind. The Commission suggested that the larger and better-resourced of the two inspectorates would serve underground mines, while the much smaller quarries division would employ inspectors of lower grade. Two reasons were offered. First, that quarries employ a small fraction of the mining workforce, and by their opencast nature are less hazardous than deep or ultra-deep hard-rock mines. Secondly, and we feel that this is possibly the only viable justification for the division, the quarries inspectorate would serve as a transitional springboard from which those previously disadvantaged under apartheid could be introduced to, and gain the qualifications for, the technical skills and competencies of the envisioned health and safety inspection regime.

Neither of these points arises from our special understanding of mining. Rather, we raise the issues because they have some bearing on the problem of defining ‘culture'. Different collective responses to the idea will tend to draw on the ways representatives to the tripartite policy, regulation, and research advisory bodies customarily used the concept of culture in their historical dealings. Put differently, management's conception of *culture* might range over questions of taste in relation to themselves, but have strong ethnological overtones when considered in relation to South African mines' largely migrant workforce. Worker representatives or advisers, on the other hand, may see *culture* in somewhat more sociological or political terms. In respect of the "Bosses" they may represent culture in the language of (for example) class hegemony; in reference to their own constituency, they speak of culture as their birthright from the struggle for human rights. These are merely observations based on long experience with the debates of the late-apartheid period, and by no means should be taken as *predictions* for post-apartheid debates. We merely wish to indicate how it may be possible for the Commission's undoubtedly *affirmative* general intention in making this recommendation to become submerged

in extended debates on, or disagreements over, how to define *culture* for legislative, regulatory, or research funding purposes, so as to represent participants' respective constituencies' interests. We do not take the position that the Leon Commission was particularly negligent in its choice of terminology; the text of the report makes it clear that the brief discussion of a culture of safety in Section 6.5, was not hastily thrown in as a result of special-interest lobbying, or as an afterthought. However, the history of the concept of culture, which we proceed to discuss below, has tended as often to divide as to unite; any infelicities arising from the *interpretation* of the recommendation, therefore, need to be sought in how different parties infer that it should be *implemented*, and not in the recommendation itself.

6. The Modern Concept of ‘Culture': A brief genealogy.

The term ‘culture' originates from the Latin term *cultum est*, which is the past perfect participle of the indicative verb *colo, colere*. Modern cognate terms of the latter verb include *bucolic*, *colony*, *colonize*, and *colonial*, suggesting the possibility for a rather infelicitous conceptual relationship with some contemporary notions of *culture*. In particular, we note this relationship in the light of the frequent advocacy of *culture* as the preferred conceptual ground for creating engaged, democratic, and empathetic policies for global social development. Etymologically, however, it appears that the root syllable *kol-* referred in antiquity to verbs that had anything to do with farming or husbandry. In abstraction, therefore, the concept should be seen as covering the broader business of *bringing something from its germination or birth, to maturity*. Thus one of the classical Latin words for a farm was *colonia*, which one might translate as a "place of growing or raising" at which crops, sheep or cattle, or orchards were tended or brought to productive fullness. Indeed, the original idea for what we understand as a "colony" was for the Greeks of classical antiquity merely the establishment of farming communities in areas with better soil than that of Attica or Peloponnesus. A major source of both income and political power among the Republican Romans was an individual's demonstrable competence at managing *coloniae* in the territory of Sicily, where soil conditions guaranteed better wheat supplies to the millers and bakers of urban Rome. Later Romans similarly ‘colonized' the North African coast (modern Egypt, Libya, and parts of Tunisia) to *cultivate* food for the growing population of the post-Augustan Empire's urban

centres. Toward the end of the Republican era the eminent orator, lawyer and philosopher Marcus Tullius Cicero left an early record of the first abstraction of the business of the *colonia*. In a letter to his brother in about 55-50 BC, Cicero warns the latter that Romans' *cultura nostra* ("our culture") places moral limits on the lengths to which an aspiring politician may go in assuring enough votes to get himself elected.

However, the modern concept as implied in the Leon Commission Report, originated in the political and philosophical debates regarding social and political problems brought on by the outcomes of the Enlightenment (lasting roughly from 1650 to 1790). Conceptually, early theorists would seem to have adopted the term with full cognizance of its etymology. In brief, our readings indicate that the concept was developed to provide a conceptual ground for providing an alternative intellectual environment to the sometimes rigid and highly authoritarian 'rationalist' Enlightenment states of continental Europe (Schiller, 1793/1910, Letter VI). The monarchies of Enlightenment Europe, France being prominent in this regard, had translated rationalist philosophy (we identify some of the key sources below) into a political system that tended to enforce rationality in selected spheres where aristocratic authority most readily lent itself to this end. Critics noted that this inevitably occurred at the expense of the emotional and practical stresses such enforcement visited on civilization's supposed beneficiaries (for example, Herder, 1770/1969). The popular literature of the period, from opera and theatre to various polemical tracts both religious and secular, all indicate an ongoing concern with the disjunction between the rationalism of strategic reasoning and the (occasionally brutal) opportunism of everyday life.

Succinctly put, as a concept culture seemed to offer critics a solution to the problems of the Enlightenment applications of the concept of civilization. In general, this criticism veered between a demand for the abandonment or replacement of contemporary civilization (Rousseau, 1973), to calls for moderation of its breakneck growth (Coleridge, 1829/1976). Historically, then, the modern use of the terms *culture* and *civilization* complement each other under the broader problem of managing the development of human personality and society, respectively, from a

less-organized condition to one more organized (see Coleridge 1976; see also extracts collected in Rundell and Mennell, 1998). It is worth remarking in this regard that an early recorded English meaning of *civilization* was the "assimilation of the Common Law into the Civil Law" (1812; see *Shorter Oxford English Dictionary*). Immanuel Kant (1784/1998: 44), realising the importance of the related concepts of *freedom* and the *primacy of individual conscience*, held that people would need to be *civilized* "in all sorts of social grace and decorum" merely in as much as they were members of states; in a perfectly free world, they would be in "large degree …, through art and science, *cultured*." We will return below to some of the unexpected outcomes of Kant's prescriptions.

Gottfried Herder's (1770/1929) original German formulation adopted the term ‘kultur’ to explain conceptually the common qualities that linguistically homogeneous peoples shared. Responses to this noted that *kultur* tended (a) to submerge what was common to all humanity in favour of what were in fact wholly local and contingent circumstances (Kant, 1794/1998); and (b) to restrict human reason to the realm of expressive action at the expense of the species' well documented history of intellectual and scientific accomplishment (Schiller, 1793/1910). In English, Samuel Taylor Coleridge (1829) translated the German ‘kultur’ into ‘cultivation’ (basing his discussion on both Kant's and Schiller's responses to Herder). The more or less direct transliteration ‘culture’ soon became commonplace in English. Because at the time there were many German-speaking people, fragmented politically along mostly either religious lines, or geographically among the supra-national empires of Austria, Russia, and the nominal Holy Roman Empire, we read Herder as arguing in justification for the formation of a politically distinct German national state. The German language had become codified in the aftermath of Martin Luther's adoption of a vernacular translation of Christian Scripture, developing into a distinct oral and literary form that persisted from one generation to the next in the same manner *but in a different social and political environment* to those of the English or French nations.

The problem for the German speaking peoples of the broader European region, therefore, was to explain an apparent *unity* (in artistic creation, scientific and philosophical thought, and in religious matters) across this wide area. All these

indicators should, the basic reasoning proposed, effectively *entitle* German-speaking peoples to a national body equivalent to that of the Dutch, French and English-speaking worlds. Culture, it was argued, was that quality of identity (or feeling, or authenticity, as we will elaborate below) *carried across* generations through the languages unique to every society that had developed to the level of *nationhood*. In short, the logic of German nationhood was based on that people's possession of *culture*, in contrast with other nations' possession of *civilization* (Elias, 1989/1998: 226-7).

As the concept became absorbed into common discourse, it served to make rational the common *feelings* that being a member of a coherent society conferred on its members. Both Herder's and Schiller's arguments were, therefore, based on a criticism of rationalist philosophies current at the time, most of which held that the category of Quality had no independent reality, and that reason could only comprehend the categories of Actuality and Representation. These philosophies, derived pre-eminently from Leibniz, Descartes, and Spinoza, place a strong emphasis on the universality of the material and rational realms. In reaction, Herder and his contemporaries among the so-called German Romantics argued that the mode of reality ascribable to common feelings, proper to the category of Quality, was primary. In a further contrast, they held that the mode of reality of the universal, the category of Representation, was something ascribed merely on the basis of how societies judged relations between their emotional and material realities, on the one hand, and on the other the clash between societies when their respective ascriptions of Universal reality contradicted each other. The concept of *tradition* thus becomes comprehensible in the context of how such judgements are transferred across generations.

Strictly speaking, tradition is the body of knowledge passed over from one generation to the next in the maintenance of *culture* (from the Latin *trans* – across, and *dare* – to give). In many instances tradition is also taken to include written texts, but a very influential understanding of the concept is that it means *orally transmitted* knowledge (Ong, 1982; Sienaert and Bells, 1988). The sometimes common-sense identification of culture with tradition could thus obstruct inquiry into safety culture,

unless it is specified early whether a specific project is to include or exclude written knowledge in the form of records. In this report we will always consider tradition as both oral and written in content, and indicate any deviations from this usage. Generally, the idiomatic identification of culture and tradition stems from the role, whether positive or negative, that traditional knowledge plays in the broader business of culture (as the latter is idiomatically used). However, traditions operate whether a society is civilizing or not, whereas culture, as noted, is more properly conceived as a moderating factor in societies undergoing the disruption of rapid political, social, technological-economic, and scientific changes. The modern and late-modern (often mistakenly self-ascribed as 'post-modern') theoretical milieu has come to consider tradition as a concept that accommodates the principle, taken from structural linguistics, that specific habits of modern intellectual reasoning can only be taken as assigning Universal Characters (category of Representation) on the basis that the latter represent arbitrary conventions adopted because such characters work for those who use them (see, for example, Macintyre, 1988).

Finally, many claims to cultural identity are made in relation to *forms of conduct* that are better understood as *custom*. The latter is properly applied to actual forms of conduct that present generations take up as a form of *inheritance* from earlier generations, and which may be valued on these grounds alone, irrespective of any real contemporary utility. As forms of conduct, the actions that we might conceive as customary are essentially routine performances specified in the course of community members' growing up into their social environments. As such, individuals who may question custom will have the contested routine or action explained in terms of tradition. Thus an explanation for as thoroughly modern a custom of personal hygiene as washing one's hands before eating, may take the form of a reason couched in traditional idiom. In general, though, the relation between tradition and custom is one of rationalization: tradition offers reasons for the pragmatic necessity or moral goodness of the performance of customary actions. Thus where early conceptualisations of Culture stressed the category of Quality, the concepts of Tradition and Custom may be seen to stress in practical idiom the categories of Representation and Actuality, respectively.

From the viewpoint of a culture of safety, it should be noted that *culture*, *tradition* and *custom* all refer to qualities of education, knowledge, and conduct that are *already* established; each may be understood as a facet or dimension of *past reality* interpreted in relation to *present conditions*. For the purpose of facilitating consideration of the cultural aspect of proposed research topics, we will outline in the following section four principal groups of meanings into which the concept of culture has developed since the early nineteenth century, and which could be used in debate on a health and safety culture. It is important to ascertain, for any proposed research under the culture of safety rubric, firstly how far two or more of these four meanings of the concept may be present; and, secondly, to note the extent to which ascriptions of 'culture' may be more readily interpreted in terms of tradition or custom. This is because in any of these cases the recourse to one or other term (whether explicitly or under the general rubric of "culture") will always indicate that *in logic* the concept is a reference to past realities. As such, therefore, culture, tradition and custom generally assert some *determinate character* about some or other state of affairs in the present based on causal (or quasi-causal) influence of the past.

7. Contemporary Cultural Discourse: "A vague and baggy monster".

Critical theorist Raymond Williams (1989: 158) first used the appellation "vague and baggy monster" to describe *cultural studies*, many practitioners of which accept Williams as one of the 'founding fathers' of their field. Williams (1988: 87) had described the word *culture* as "one of the two or three most difficult words in the English language", and subsequently noted how the academic field of Cultural Studies had fragmented very quickly into numerous squabbling factions. We have borrowed Williams's epithet as a heading, because a relatively quick study of the historical and disciplinary ways that people have spoken of 'culture' shows that the ascription is also very apt in respect of that concept itself. Naturally enough, from the research point of view, the general project of a health and safety culture in mining could depend significantly on how precisely or loosely the concept of *culture* is defined.

The literature offers definitions that appear to refer to several apparently quite

distinct concepts. For general readers trying to improve their grasp of the notion, this may lead to some confusion as to how to decide to act. We provide the following broad breakdown of the most common classes into which various definitions may fall:

1. Culture treated as the customary logical, institutionalized, and material practices associated with a definable group of people in the environment of a distinct tradition of tool-using behaviour.

2. Culture applied as a collective noun for a distinct grouping of people, defined by national, ethnic, or religious category.

3. Culture as referring to the *symbolic identity markers* adopted by minority populations in theologically, ethnically, or linguistically pluralistic national societies.

4. Culture divided into high, low, or popular forms, especially in respect of literary or entertainment forms.

To clarify some possible consequences of these distinct classes of definitions, we proceed to examine how each represents the cognate concepts of civilization, tradition, and custom, under Peirce's system of categories. Needless to say, it would be essentially self-defeating to end the analysis at that point; the following is therefore merely an all-too-brief prolegomena to our more specific treatment of the relation between the culture of safety as a research project, on the one hand, and the general economy of research, on the other.

The *first definition* is no longer common in contemporary cultural discourse, but there are several sources in the literature that support the view that it was influential up to about half a century ago. In line with much nineteenth century research practice, this approach accepts the reality of Actuality (in the form of customary practices) and of Representation (in the form of tradition). The meaning developed from American anthropologist Edward Tylor's (1871) definition of culture in terms of the artefacts, tools and weapons that a community or people produced and used in the pursuit of their everyday lives. Possibly the first anthropologist to incorporate the concept of *culture* into the discipline, Tylor conceived of it as the expression of peoples tradition and custom, in the form of tools like "hand shuttles, crossbows, blowguns, drills, screws, water wheels, and other instruments, tools and weapons" found in different

societies (Harris, 1979: 279). In so far as these artefacts may be construed as applications of a form of *scientific* reasoning, Tylor may be interpreted as having drawn on the Kantian postulate that people are cultured by science. The corollary of this, naturally, is that the folklore and traditional tales of the studied communities constitute the philosophical impulse in the culture of the people. A prominent recent development of Tylor's thought is to be found in the revised Marxist research methodology of Marvin Harris (1979). On a more abstract philosophical level, Charles S Peirce has inspired Karl Popper (1972) to propose that people think not merely *in* words or pictures, but also *with* the tools or tool-like properties of the various kinds of equipment and artefacts that are available in their worlds (see also Skagestad, 1993, 1996; Sowa, 1984, 2000).

In general, this first group of meanings relates to what can be seen as the accomplishment of civilization predicable of a society, a people, or a community. In the nineteenth century, of course, there was much research (both serious and ideological) into matters like the criteria of or for civilized behaviour, the demarcation of the civilized from the barbaric, and the like. In the latter half of that century, many tracts using more or less ideologically slanted readings of the concept of Evolution appeared (Newcomb, 1886; Spencer, 1958; Eliot, 1856; Pearson, 1900) to bolster assertions about the evolutionary necessity for (nineteenth century) industrial society. This entailed a system of *ranking* civilization forms, with principles of comparison based on "survival of the fittest" indices that marked societies as "more evolved" or "less evolved" as the case might be.[1] Under the circumstances, given the comparison between, for example, Northern and Western Europe, on the one hand, and Russia or the Ottoman Empire on the other, it may have seemed sensible in some quarters to assign as an index of civilization the degree of theological authority present in relation to the level of industrial progress. One consequence of this is that colonial administrators often had to implement policy based on an appreciation that customs based on, for example, ancestral authority through animistic traditions, represented a principal obstacle to efficient control. Among the historical solutions attempted in Africa and South America was the importation of labour from non-animist traditions, especially China and India. Naturally enough, local populations already alienated as a result of their religious and moral concepts'

[1] The "survival of the fittest" conception is Spencer's, although often mistakenly attributed to Darwin.

rejection on the basis of animism, found this development a rich source of further grievances as a consequence.

Culture's *second* class of meanings emerged as improved transport and communication made relations between the corners of the globe more regular and reliable, making such discrimination appear less and less ethically tenable. Tylor's student, Franz Boas (1940), responded to these developments by incorporating Herder's concept into anthropology, developing field-work methods to establish how much people's cultures are essentially autonomous and sovereign, analogous with the *political* sovereignty of nations. As such, this meaning accepts the reality of the categories of Quality and Actuality, but holds that Representation is situational and historical in nature. One of the most illustrious later exponents of this trend was Margaret Mead (1928). In Africa, this meaning has entered the philosophical and political discourses of liberation movements, by way of the debates triggered when the missionary Placide Tempels first published his book *Bantu Philosophy* (Tempels, 1945/1969). Tempels argued, on the basis of having learned and spoken the languages of the communities of the then-Belgian Congo in which he had practised, that the Bantu Group of languages had a series of common conceptual words and sounds that warranted the inference that people who spoke such languages naturally, experienced a different form of reality to that encoded in European languages. Tempels's work led to much comparable ontological analysis, breaking new ground in the study of African religious thought (see especially Mbiti, 1969, 1970, 1971, 1975). Subsequent work has shown many shortcomings in the ethnological basis Tempels had assumed in making his claims (Eboussi-Boulaga, 1968; Hountondji, 1983; Mudimbe, 1989; Masolo, 1994). Herder's postulate that spoken language and conceptual structures correlate with people's cultures and customs was clearly an influence, however indirectly some one-hundred and fifty years after the idea gained currency, and continues to crop up in popular discourse. Since 1990, some South African academics have taken up this discourse as part of the transformation process (for example, Shutte, 1993), but it has yet to gain broad currency. The principal objection remains that of Paulin Hountondji (1970), for whom the broader "ethnophilosophical" exercise set in train by Tempels remains addressed to colonial administrators and not to Africans trying to confront their worldly condition.

A very common recent use of this conception of culture has been to extend it to 'hyphenated' minority community identities. The sheer weight of US media may suggest this logic is common only in the United States, but it is becoming increasingly common elsewhere. The identity claims of African-Americans, Hispanic-Americans, Asian-Americans, Caribbean-Americans, and so on, apply an ethnographic marker to a social grouping that is itself not ethnographic but political and sociological in its general meaning. In South Africa, the approach also influences some local cultural debates, with people using racial-ethnographic markers to ascribe identity (whether by the self or by outsiders) to the country's historical 'groups'. In general, this concept makes much of the distinctness and authenticity of tradition, with a strong emphasis on the veracity of indigenous forms of storytelling and moral fables (reasoning that derives largely from the work of a follower of Herder, the linguistic anthropologist Jakob Grimm). A people may thus be said to follow their culture in so far as they revere particular mores and folkways, and the degree to which they ensure that such behaviour is transmitted more or less untainted by outside influences to succeeding generations. It follows, therefore, that if the *culture* so defined comes under critical scrutiny in terms of a non-indigenous moral or other framework (for example, the Declaration of Human Rights), then this conception of culture validates a defence of the relevant practices based on a notion of cultural sovereignty; the sometimes explicit justification may be offered that such scrutiny is a form of interference in an internal affair. The influence of this version of the concept on contemporary political and sociological theorists cannot be overestimated (Taylor, 1989: 368-392).

The *third* form of meaning for 'culture', as a *symbolic identity marker* for minority populations, evolved from geo-political trends evident in the latter half of the previous century. As such, it is the outcome of some four decades of *critical cultural theory* originating in criticism of the first two concepts, being essentially a radical development of Boas's model (see for example Winch 1958, 1970; contributors to Hymes 1972). In a reaction to the events that accompanied the withdrawal of the Imperial powers from the colonial world after 1945, anthropologists sought critically to account for the apparent failure of their profession to foresee and explain the ways that post-colonial peoples managed (or, tragically, failed to manage) their transition

to national sovereignty. In brief, the consensus was that the concepts of development enshrined in the policy documents of the multilateral institutions (the World Bank, UNESCO, the International Human Rights Declaration, and so on) overlooked or, worse, ignored the 'cultural context' within which formerly subjugated peoples had defined their collective identities (Melkote and Steeves, 2001; Servaes, 1999; Said, 1978). With the subsequent migration of people from the developing to the developed world, immigrant communities adopt various customary practices as *expressing* their separate community status in their new abodes.

The general intellectual response to these criticisms has been to remodel the methodological basis of anthropology, with a concomitant shift in the ways that development strategies in the non-industrialised world should be formulated (Melkote and Steeves, 2001; Servaes, 1999). The latter trend was much influenced by the primacy of Structuralist Linguistics (Saussure, 1916/1974; Levi-Strauss, 1968) in the middle decades of the twentieth century. One of this paradigm's central postulates is that the semantic dimension of language consists of nothing more than the arbitrary ascription of a verbal symbol to Actualities. In effect, this development radicalised the nominalist doctrine that reality inheres only in the categories of Quality and Actuality; it became common practice to treat the *arbitrary* relation between sign (word, gesture, and so on) and object as one that could only be enforced by an Authority of some kind (Foucault; Bourdieu; Laclau). In abstraction, this Authority has often been ascribed to 'Culture', and this ascription marks the main distinction between this meaning and that derived from Boas.

Marvin Harris (1979: 279-82) traces an additional factor in the origin of this approach, noting the formal academic separation of anthropology from sociology implemented following a suggestion by anthropologist Alfred Kroeber and sociologist Talcott Parsons (1958). Because early American anthropology largely followed the first form of the concept, some doubt arose as to just what in fact separated the two disciplines. After considerable debate, Parsons and Kroeber (1958: 582-3) concluded that it would be "... useful to define the concept of culture for most usages more narrowly than has been the case in the American anthropological tradition, restricting its reference to transmitted and created content and patterns of values,

ideas, and other symbolic-meaningful systems as factors in the shaping of human behavior and the artifacts produced through behavior." The leading early practitioner of this approach was Clifford Geertz (1973), who interpreted the "transmitted and created content and patterns of values, ideas, and other symbolic-meaningful systems" as constituting cultures as "texts to be interpreted" semiotically (Geertz, 1973). Many subsequent researchers have elaborated on Geertz's dictum, often drawing on the High Theory of late-20th century styles of literary criticism (see Hardt, 1993; Comaroff and Comaroff, 1992; Starfield and Gardiner, 2000).

One outcome of these developments has been for policy-makers to proceed on the assumption that every community that marks itself as distinct from others, must possess a culture that is wholly unique in the authority it exerts on members' experience of Actualities. Recent British and European Union (EU) enactments in education, health and social welfare policies, and even in relation to civil and common law, have begun to treat minority or immigrant communities as culturally sovereign. By making the concept applicable to more and more local groups, the critical paradigm of culture comes more and more to emphasize *customs* as analytically equivalent to linguistic identity. This development would suggest that the South African mining industry may need to consider not merely a single or perhaps coherent ‘culture of safety', but the management of a greater or lesser plurality of distinct autonomously developed cultures of safety.

Finally, the division of *culture* into forms of ‘high culture', ‘low culture', or ‘popular culture' remains influential. Because of the influence of this interpretation in British and colonial education policy, it is occasionally mistaken among post-colonial writers as a basic postulate in the logic of culture (see, for example, Hayman, 1982; Steadman, 1982; Vaughan, 1984). The original classification of culture into ‘high' and ‘low' is frequently ascribed to the thinking of 19th century commentators like Matthew Arnold (1935), John Ruskin, and William Morris (see Williams 1958), and is a development of Schiller's (1793/1910: Letters XII-XV) classification of human reasoning into instinctive drives for Play, Action, and Reflection. However, these writers largely followed on the work of Samuel Taylor Coleridge (1976), who held that the *business* of culture was to be entrusted to a secular "Clerisy" or special class

of intellectual, independently of State or Church. Although the actual terminology of classification varies somewhat from one theoretical school to the next, the distinction between ‘high' (elite, aristocratic, hegemonic, and so on) and ‘low' (working-class, popular, mass, majority, and so on) remains highly influential in South Africa nearly a century after Union in 1910. Most commonly, the classification is applied to the forms of communication and entertainment that appeal to the different parts of a given society.

Criticism of these classifications arises principally from the view that they are based on a hierarchical system of social, economic, and political authority that mirrors both the English class system *and* the relation between colonial administrators and the indigenous populations of the Empire. The Marxist tradition often finds this approach quite compelling, given the association of the classifications with the industrial division of labour. It is important to note, however, that the approach was also highly influential in South Africa's nationalist liberation movement, where ‘popular culture’ became a forum for anti-apartheid activism (Klitgaard, 1993; Wright and Mazel, 1991; Pfordresher, 1986; Vaughan, 1984). Many former activists, presently in Government and civil society, continue to employ this concept in various ways. In many respects, contemporary versions of this approach tend to assign one of the three other classical accounts of culture to the different levels. The criteria for this assignment may, of course, be couched in terms of any of the classical approaches: thus the authenticity of a particular set of customs common to a certain class of people in a country may be treated as an ‘authentic' culture of the masses, or the ‘popular' culture of the nation untainted by commercial ("MacDonaldization" or "Disneyfication") or elitist factors. Overall, this approach tends to divide populations more or less distinctively into these cultural attachments on the basis that High Culture treats formally of the Category of Representation; Middle Culture with Actuality; and Low Culture with Quality alone, usually assigning to the ‘masses' a primacy of Feeling over Thought.

8. Culture as Research Problem: the paradox of choice.

To summarise the foregoing, we have noted that it is possible to consider the most common meanings of culture as actually reflecting an idiomatic approach to one of

the cognate concepts of tradition or custom. Thus any research to be conceived in relation to the promotion, establishment, or analysis of a culture of safety may actually gain more precision if conceived in terms of the methods generally applicable to *culture*'s historical conceptual cognates. The problem with settling on a policy definition of culture is simply that to choose from four kinds of definition, ignoring for the purposes of clarity certain preferred definitions that combine two or more of these classes, yields a bewildering range of possible *preference rankings*. Idiomatically stated, reaching consensus over what *culture* means in a tripartite research advisory process is likely to open a large can of worms.

This is quite easily demonstrated for a context of research assessment based on tripartite consensus decisions, by using a model that exploits the logical and mathematical property of transitivity. The principle may be stated as follows (Arrow, 1963: 13):

> The property … of consistency in the preferences between different pairs of alternatives may be stated more precisely as follows: If *x* is preferred to or indifferent to *y*, then *x* must be either preferred or indifferent to *z*. In symbols,
>
> Axiom: For *x, y,* and *z, xRy* and *yRz* imply *xRz*..
>
> A relation satisfying [this] Axiom is said to be transitive.

These rank-orderings are the basis of an economic *theory of the consumer*. The present problem is not one of consumption utility, but of research utility. However, there is little *logical* difference between a committee deciding on research funding allocations among competing proposals, and the classical model of the consumer having to choose which brand of toothpaste to buy (we don't for a second want to suggest, however, that this logical identity trumps, or *ought* to trump, educational, political, and other preferences). In the former situation, the principle can be less formally stated as the so-called *voting paradox*:

> Let A, B, and C be … three alternatives, and 1, 2, and 3 individuals. Suppose individual 1 prefers A to B and B to c (and therefore A to C), individual 2 prefers B to C and C to A (and therefore B to A), and individual 3 prefers C to A and A to B (and therefore C to B). Then a majority prefer A to B and a majority prefer B to C.

> If the community is to be regarded as behaving rationally, we are forced to say that A is preferred to C. But in fact a majority of the community prefer C to A (Arrow 1963: 3).

We have already noted how Peirce's theory of the Economy of Research actually yields a form of preference ranking for research project fund allocation. Let us therefore state this principle more formally in terms of the present topic:

Let $\boldsymbol{C}_1$, $\boldsymbol{C}_2$, $\boldsymbol{C}_3$, and $\boldsymbol{C}_4$ represent the practical effects of the four conceptual variations of culture; we treat each symbol as a preference on the part of a contributor to the debate over the relevance of a research proposal.

A preference order will therefore be represented as

$$\boldsymbol{C}_1 > \boldsymbol{C}_2$$

And

$$\boldsymbol{C}_2 > \boldsymbol{C}_3$$

And

$$\boldsymbol{C}_3 > \boldsymbol{C}_4$$

If, therefore, $\boldsymbol{C}_1 > \boldsymbol{C}_2$, and $\boldsymbol{C}_2 > \boldsymbol{C}_3$, then

$$\boldsymbol{C}_1 > \boldsymbol{C}_3$$

Similarly:

If $\boldsymbol{C}_2 > \boldsymbol{C}_3$, and $\boldsymbol{C}_3 > \boldsymbol{C}_4$, then

$$\boldsymbol{C}_2 > \boldsymbol{C}_4$$

And

$$\boldsymbol{C}_1 > \boldsymbol{C}_4.$$

If every preference is to be treated equally in the disbursement of research funds, then each must be valued as a cardinal quantity, that is to say, as being worth a certain amount of cash money to be spent on reducing some perceived lack of knowledge, or, in 19th century jargon, reducing the probable error. What is the

probable error (or, in recent terminology, the spread of the confidence interval) in the case of research into *culture*? This is not ascertainable in cardinal terms, because if *all* preferences are equally distributed across a national population then it is quite possible that the total number of stated preferences, when viewed as a simple choice in favour of one of four options, could exceed the total population. The mechanism of this can be readily demonstrated, analysing the preferences for four possible ways of conceiving culture in practice.

Let some sub-collection in a national population give priority to any cultural concept $\boldsymbol{C}_1$; and, further, assume that our four conceptual cultural conceptions are sensibly present for this sub-collection. As such, the non-priority conceptions $\boldsymbol{C}_2$, $\boldsymbol{C}_3$, and $\boldsymbol{C}_4$ will actually be distributed across this sub-population as follows:

$\boldsymbol{C}_1 > [\boldsymbol{C}_2; \boldsymbol{C}_3; \boldsymbol{C}_4]$

Where $[\boldsymbol{C}_2; \boldsymbol{C}_3; \boldsymbol{C}_4]$ represents the six possible combinations (3! = 6) in which these three options can be ranked. It is thus apparent that a stated priority asserted for a practical implementation for one out of four concepts of culture, could in fact represent six different rankings of these four concepts. The set $[\boldsymbol{C}_2; \boldsymbol{C}_3; \boldsymbol{C}_4]$ decomposes into the following possible rankings:

$\boldsymbol{C}_2 > \boldsymbol{C}_3 > \boldsymbol{C}_4$

$\boldsymbol{C}_2 > \boldsymbol{C}_4 > \boldsymbol{C}_3$

$\boldsymbol{C}_3 > \boldsymbol{C}_2 > \boldsymbol{C}_4$

$\boldsymbol{C}_3 > \boldsymbol{C}_4 > \boldsymbol{C}_2$

$\boldsymbol{C}_4 > \boldsymbol{C}_3 > \boldsymbol{C}_2$

$\boldsymbol{C}_4 > \boldsymbol{C}_2 > \boldsymbol{C}_3$

The same distribution of preferences may be assigned to sub-populations for which concepts $\boldsymbol{C}_2$, $\boldsymbol{C}_3$, or $\boldsymbol{C}_4$ have priority. Now it is possible that research can establish with more or less accuracy how such priorities are distributed across a national population. Such a question might, for example, be added to the national population census form. The problem, however, is that to establish with accuracy the *complete*

preference set for every possible ordinal ranking of these four concepts, will yield no practically significant result. The distribution of preferences is subject to the Voter Paradox, precisely because preferences of this sort are in reality never equally distributed across a population. How does this apply to cultural preference?

Let the ranking of preferred conceptual appreciations be distributed among a population such that (A) a small majority gives priority to $\boldsymbol{C}_1$ over $\boldsymbol{C}_2$, $\boldsymbol{C}_3$, or $\boldsymbol{C}_4$; and (B) those who give priority to $\boldsymbol{C}_2$ form a minority that never the less is cardinally greater than the sum of those who give priority concepts $\boldsymbol{C}_3$ and $\boldsymbol{C}_4$. That is:

(A): $\boldsymbol{C}_1 > [\boldsymbol{C}_2; \boldsymbol{C}_3; \boldsymbol{C}_4]$; and

(B): $\boldsymbol{C}_2 > [\boldsymbol{C}_3: \boldsymbol{C}_4]$.

The overall ordinal ranking of *priorities* remains $\boldsymbol{C}_1 > \boldsymbol{C}_2 > \boldsymbol{C}_3 > \boldsymbol{C}_4$. However, the ways that each preference has priority over the others, all being sensibly present to the whole population and distributed as outlined above, offer six possible ranking orders within the groups asserting each given priority. In other words, for an enumerable group giving priority to $\boldsymbol{C}_1$, there are six possible ways they might rank their second and third preferences. By the same token, the same must hold for those sub-populations whose stated priorities are for $\boldsymbol{C}_2$, $\boldsymbol{C}_3$, and $\boldsymbol{C}_4$ respectively.

In practice, this means that there are in fact always fewer *actual* preference rankings than possible ones. Now in the minimal majority scenario, the distribution of preferences translates into a minimum breakdown of percentages as follows:

> For (A), at least 51% of the sample population must assert the preference $\boldsymbol{C}_1 > [\boldsymbol{C}_2; \boldsymbol{C}_3; \boldsymbol{C}_4]$; and
>
> for (B), fractionally less than 25% of the sample population must assert the preference $\boldsymbol{C}_2 > [\boldsymbol{C}_1; \boldsymbol{C}_3; \boldsymbol{C}_4]$.

Following the distribution breakdown of non-priority preferences, it is therefore quite conceivable that the majority that prefers $\boldsymbol{C}_1$ could itself have a majority of members for whom either $\boldsymbol{C}_2$ or $\boldsymbol{C}_3$ might form the next-best preference were circumstances to change sufficiently to warrant such a change of priority. Add this small internal majority to the existing overall minority that prefers $\boldsymbol{C}_2$, and the overall majority shifts

dramatically; in the case of C_3, when added to the minority that prefers C_3 the preference for C_2 may even become the majority. This indicates that if the research priorities into safety and health culture concentrate resources on minority preferences, small outlays *could* accomplish relatively large results. The main problem, however, remains the accurate assessment of how the priority and secondary preferences are distributed in society at large; as preferences, these choices may be subject to arbitrary changes as a result of the very broad acceptance among contemporary intellectuals of consensus-based decision methods. Consequently, research needs to proceed from the best available conceptions based on findings that settle doubt independently of whatever might be the reigning opinion (irrespective of how this came to achieve this status) in a committee-style funding allocation process.

9. Cultures and the Economy of Research: Two paradigms.

The foregoing has demonstrated that the idea of *culture* opens up a rather broad and indeterminate set of possible public preferences, with many possible points over which disputes or contradictions may arise. We also suggested that greater conceptual clarity becomes possible if we shift attention away from *culture* onto its historical cognate concepts of *tradition* and *custom* as areas proper to research into a culture of safety in general. We have no doubt that the same holds in respect of industrial health, but we do not pretend to any particular expertise in the field. In the present section we return to the Economy of Research (Peirce, 1879/*CP* 7:139-157), as a preliminary to subsequent discussion about the choices facing the funders of safety culture research.

The theory provides a quantified description of the measure of precision possible in the inductive testing of hypotheses; further, this quantification also offers the means to assess the diminishing return on research investment. The latter occurs when the cost of a small reduction in probable error (continuing with Peirce's own terminology) exceeds the benefits of that reduction:

> … when an investigation is commenced, after the initial expenses are once paid, at little cost we improve our knowledge, and improvement then is especially valuable; but as the investigation goes on, additions to our knowledge cost more and more, and, at the same time, are of less and less worth. Thus, when

> chemistry sprang into being, Dr. Wollaston, with a few test tubes and phials on a tea-tray, was able to make new discoveries of the greatest moment. In our day, a thousand chemists, with the most elaborate appliances, are not able to reach results which are comparable in interest with those early ones. All the sciences exhibit the same phenomenon, and so does the course of life. At first we learn very easily, and the interest of experience is very great; but it becomes harder and harder, and less and less worth while, until we are glad to sleep in death (*CP* 7.144).

As is customary in much of Peirce's published work, it is well worth attending to his insertion of considerations like that of the last quoted sentence. This may be because Peirce, himself an accomplished practising scientist in several fields, treated all science not as the production of knowledge, but as the

> … total principal industry of a social group, whose whole lives, or many years of them, are consecrated to inquiries to which they are so devoted as to be drawn to every person who is pursuing similar inquiries, and these inquiries conducted according to the best methods so far found out, their different inquiries being so nearly of the same nature that they thoroughly understood one another's difficulties and merits, and could after a brief preparation have generally each one have taken up and carried on the other's work, although probably not with quite his success (Peirce, 1910: MS 655; cited with permission).

From the point of view of the economy of research, therefore, this conception of science permits us to consider as generally *vague* a topic as a culture of safety on two distinct levels. In the first paradigm, we consider the mode of inquiry into *culture* as most relevantly pursued at the most general level for the present. Because *culture* can be ascribed so many characteristics, it is likely that small inputs from widely dispersed researchers in a number of disciplines concentrating on tradition and custom, may be able to provide the greatest reduction in vagueness for the least outlay of funds. This entails a research programme that is collaborative in the broad sense, by which we mean that instead of seeking to fund a single mega-project that may be falsified by a single contrary finding of sufficient scientific integrity; the project should set to *connect* research in a variety of locations and across several fields. In effect the more general the level of inquiry, the greater the level of communication there needs to be between different projects. We should add that we are not replicating nineteenth-century conceptions of the "international language of Science", but stressing the distinction between scientific and technological inquiry. Findings in general inquiry are considerably more falsifiable than in technological Research and

Development projects, in which the research entails a far higher level of capital and human resource expenditure in order to improve already-established equipments and procedures. In other words, this form of research draws on already-established general conclusions to confront questions arising from the actual customs present in mining. This does, however, have an impact in relation to the overall culture of safety project, because new kinds of equipment and the associated changes in practice and procedure change the overall customary conduct of mining. The introduction of continuous mining machines in collieries, for example, changed the whole system of mine design, from bord and pillar to longwall, with associated changes in mineral transport from tramming to conveyors, and ventilation accordingly as face temperatures rose because of the heat generated at the cutting face. Unlike inquiry into logically vague but never the less practically influential problems like *culture*, these developmental research projects involve a certain degree of protection for intellectual property; thus developments are subject to different economic imperatives, and we see no reason to comment further in this regard. In any case, the progress reports of individual projects available on the SIMRAC database, seem to us to show a great deal of productive energy, and will no doubt continue for some time to do so.

We do not therefore reject or disparage the ongoing research into improving the safety of procedures and equipment; such highly specific "engineering solutions" (as some critics would have it) clearly meet relatively limited but highly specifiable needs in a broader general quest for an overall improvement in mine health and safety. In general, such research is funded on the basis of immediate utility, specified in advance, and therefore treats Truth as a concern equal to utility, or even subordinate to the latter. This dilution is generally not of great concern, however, simply because it is possible to select the researchers available for such work because they are already demonstrably well-versed in the relevant methods and technical expertise. Put into Peirce's terminology, such researchers can proceed on the basis of a clearly specifiable probable error associated with well-defined Actualities. In general, we would venture to assert that the collective global experience of engineering and geological hazard estimation and reduction is sufficient to meet the needs of even the most complex Actualities of deep-level hard-rock mines in South Africa. By the

same token, global experience with the chemical and combustion hazards of coal mining can be (and is) sufficient for safety in South African collieries. In summary, South African research practitioners in the "engineering solution paradigm" can (and, we believe, *do*) dispose of high levels of competence, both in themselves and through their capacity to collaborate with their peers in other countries. The principal problem, to restate much of what has gone before, is that the presence of actively hazardous conditions seems not to be readily apparent to the people who, in all good faith we might add, come to gain employment in South Africa's mines.

On the other hand, developments over the past six decades or so of cultural inquiry have culminated in a tradition of topic-specific sociological or ethnographic research subjects, often defined as 'transgressing' the traditional boundaries of science because of their 'multidisciplinary', 'cross-disciplinary', 'interdisciplinary', or 'trans-disciplinary' nature. This "new paradigm" has undoubtedly transformed the academic Humanities. By selecting as objects of research such specific issues like class, gender and sexuality, media and literacy, performance art, and the broad range of 'popular' literary forms, some two generations of theorists and researchers have developed a *non-specific disciplinary* approach to inquiry. However, there are weighty historical reasons to suspect that the apparent democratization of inquiry claimed for this style is not attainable. Having said this, however, we do not make the further claim that this unattainability constitutes the inter-, multi-, cross-, and non-disciplinary tendency as being without value. Although there have been some highly acerbic, and sometimes quite comic, criticisms of the trend (Haack, 1997; Sokal 1996a, 1996b; Sokal and Bricomont, 1998; Windschuttle, 1997; Sandall, 1980) we consider that from the point of view of the economy of research, it was best that such methods to establish Truth *be shown to be of limited value in practice*. Knowing that institutions cannot dispense with disciplines simply means that traditional disciplines can continue their development, albeit with a broader awareness than previously of their limits, and of their dependence on other disciplines. Put differently, knowing what cannot be attained without the traditions of the disciplines has also improved our knowledge of the limits of what can be attained within them.

The present project started life as an 'interdisciplinary' inquiry, using the field of

Cultural Studies as its methodological origin. We have, as already noted, come to the conclusion that simply trying to clarify what constitutes the 'cultural' in Cultural Studies is possibly a thankless drudgery. There are too many ways for inquiry into specific issues to founder on long-term incompatibilities between the many definitions of *culture*. However, we have also noted that a critically aware reading of the history of the ways various theorists have appropriated the concept over time (some two-hundred and thirty years) reveals that the concept has developed in response to various *other topics* in specific established disciplinary areas. By approaching the problem of a culture of safety methodologically from the established methods and knowledge of these disciplines, we suggest, the rather more concrete issue of improving South Africa's mine safety record *in the longer term* can be addressed economically and productively in the short term.

10. Linking Culture and Safety: a Third Paradigm.

Research in general does not constitute a homogeneous activity, simply because of the range of methods needed to cover the enormous plurality of possible objects of inquiry. In the present instance, we might distinguish two rather obvious broad areas of research effort: the focus on the *cultural* aspect of a culture of safety, and the focus on the *safety* aspect of the problem. This would, on the face of it, appear to disaggregate the problem into parts, neither of which adequately captures the qualities of the whole. However, the presence of four distinct ways of conceiving culture leads to at least four potentially conflicting areas of debate. From the economy of research point of view, we have already shown briefly and without the test of proof, how complex a decision becomes when having to navigate the total number of possible preference rankings the parties involved may express. Too many different kinds of methods need to be taken into account. It is on this basis that the present project argues that research into *culture* may proceed more economically if it is carried out in terms of cognate proxy concepts that are more amenable to already-understood methods.

On the other hand, *safety* can be viewed as much more than merely a statistical expression, like ratios of injury to employment numbers or economic activity (for example, fatalities per million shifts worked). Quite simply, safety involves a

specifiable kind of *reasoning about reality*. This is distinct from trying to establish *attitudes*, in our opinion a psychological endeavour hardly less precise than trying to pin down a definition of culture. Whatever may be said further about culture, and what meaning individuals or groups might ascribe to it, it is a concept that itself is a form of reasoning about reality. We suggest such inquiry can proceed on the basis of the logical distinction between the concepts of *risk* and *hazard*. These involve distinct forms of abstraction, in that the former is applicable to *individual actions or endeavours*, whereas the latter applies properly to *conditions that are independent of the actor*. Risk, that is to say, abstracts on the relational process from action to consequence; hazard abstracts on that from antecedent to action. It is clear that the Leon Commission considered both risk and hazard in its report, although the final recommendations covered the need for risk management in considerably more detail than issues of hazard. On the other hand, it does not seem unreasonable to interpret the Commission and the subsequent statutory call for a "health and safety culture" in terms of the latter's relevance to the general business of mitigating hazard. After all, a mine that suitably incorporates hazard-reducing features into its general design and operating procedures, is one both easier to manage and to work in it; a *tradition* of well-designed mines is conducive to the development of practical working *customs* that minimize workers' exposure to hazard. Already, therefore, proper attention to the design of mining installations and operations would pre-emptively address two of the cognate elements that we believe are constitutive of a culture of health and safety. To arrive at the cultural issue along this route, then, we need to find the link between the logical structures of risk and hazard, on the one hand, and the kinds of practical reasoning managers and workers use in their everyday conduct in a reasonably well-designed mine. To begin this, we develop the broad logical definition of Hazard.

In their revised submission to the Leon Commission (1995, Vol 2: 31), the Chamber of Mines provided the following definitions of "Hazard", "Risk", and "Safe":

(i) **"Hazard"** means a source or an exposure to danger (*sic*);

(ii) **"Risk"** means the probability that injury or damage will occur;

(iii) **"Safe"** means free from any hazard.

In effect, our earlier distinction actually reverses the conceptual meanings of Risk and Hazard, in that we draw on the development of these concepts from their root words. We do not thereby seek to belittle either the Chamber or the Commission for having more or less accepted these definitions as a working basis for the Commission's continued work; neither are we merely stating a contrarian position simply to make a point of some sort. Instead, our aim is to clarify any distinct markers that will enable research into safety to proceed without overlaps or duplications because of semantic confusions arising from conceptual imprecision.

To dispose first with the meaning of *risk*, then, we note that the English word derives from the Italian verb *risico*: to run towards or into danger. Naturally enough, the senses in which *danger* may be conceived are manifold; in idiomatic terms, we are familiar with the term "risk capital", which quite precisely defines the sense of "danger that the venture so capitalised will go bust." Dangers can be quite metaphorical or fanciful in everyday speech. One doesn't tug on Superman's cape; or spit into the wind; and so on. In general, however, these admonitions and examples refer, as already noted, to *acts* and their *possible outcomes*. The reason such acts are risky, therefore, is simply because that toward which the *action* is directed has qualities that may backfire on the actor in defiance of her or his intentions. Superman may swat at you without heeding his strength; the wind may leave you humiliated as it blows it all back in your face; and so on. The point is, simply, that we don't speak of Superman or the wind as risks; but we do ascribe this to what we dare or venture to do.

In a sense, the kinds of acts we consider to be *risky* have an element of betting or gaming about them. This is the clue to our understanding of *Hazard*, because the word (often spelt *hasard*, *hasarde*, or *hazzard* in early texts) first enters the English language in the Mediaeval period, as the name for *a game of dice played on a board*. It is believed that the actual name was taken from the name of the Crusader's castle in which the game was first designed. The throwing of dice, of course, is probably the commonest form of practical applied statistics; although, ordinarily, people playing hazard's modern-day equivalents like Monopoly or Snakes-and-Ladders would hardly ever consider their game as such. The real purport of

this, however, is that any sustained long-term sequence of throwing two fair dies yields relatively stable frequencies with which given totals will be thrown. What does this mean when taken into the logic of safety research?

Definition: Logically, a hazard is essentially that of which it can be truly predicated of the Quality of a situation ***X***, such that in our attending to the matter we can assert with confidence that "***X*** is hazardous". For any situation to be predicated as Hazardous, therefore, entails a sustained series of encounters from which can be deduced a frequency with which a distinctly recognizable range of deleterious outcomes would be experienced. If we are ignorant of such sustained sequences, we may predicate no more of such encounters than that they are *dangerous* or *risky*.

The initial research object of any managed health and safety programme, it follows, is twofold:

1) to study the record of relevant encounters in order to establish an initial estimate of the frequencies of various deleterious outcomes; and

2) to inquire into the mechanisms of such outcomes so as to establish means and procedures for reducing as far as possible their occurrence.

The first objective is, categorically speaking, to examine the Actuality of accidents; the second is to explore what Qualities these occurrences have in common such that inquiry can hold them up as Representation for analysis in general terms. Clearly, this sort of research is unlikely to proceed so easily in practice. In researching the present project, we found (as did the Leon Commission) that the state of the mining industry's accident records was, to put it charitably, unsatisfactory. The Commission also noted that the manner of recording accident investigations was equally as unsatisfactory. On the other hand, the very long record of mining globally shows that mining hazards are common to practically every form of excavation; it would not be too much to state that *falls* of ground (as opposed to *bursts*) constitute gravity as possibly the most common hazard in *every* form of mine. Of course, to state the matter this way is to over-simplify safety to the same degree that one over-complicates the game of Snakes-and-Ladders by calling it practical applied statistics. Thus the subsequent object of research is to reduce progressively the "thumb-suck"

factor involved in the estimation of hazard.

11. Hazard and Practical Reasoning.

In the everyday run of things, people habitually employ their modes of practical reasoning to get on. It would be no overstatement to assert that as long as things don't go egregiously wrong when we assay some habitual action, then our practical reasoning is suited to the context of its everyday application. Now the nature of all practical reason is that it is essentially a well-tested system of hypothetical inference: if ***p*** then ***q***; since ***p*** is the case, so it follows that ***q*** would be the case *for the most part* when ***p*** obtains in practice. Practically, this holds whether one analyses single acts and consequences, or chains of inference. Whenever I enter my home and I flip the switch, the light would come on; whenever I start a barbeque and I strike a match, it would burst into flame so I can apply the flame to the kindling which would catch alight and start the fire; and so on. Being *practical*, such reasoning is essentially under the control of context. We know, for example, that a match would ignite when struck, but this doesn't warrant our striking matches at odd hours just to test the theory that matches ignite when struck. The *habit* that matches have of igniting when struck determines the practical meaning of this here match *as* a match, and the *utility* of such a reliable habit of ignition makes the possession of matches a quite practical habit to develop in people's everyday goings-on. Somebody's having matches becomes part of everyday reasoning, and this enables much other practical reasoning to proceed, to most intents and purposes, perfectly seamlessly.

We need to acknowledge that our practical reasoning habits are just that: *habits*. We don't go through the whole chain of reasoning about matches or light switches every time we use these; part of our reasoning is simply the reassurance that things work, most of the time, and the more frequently the better. We don't need to do this simply because things like matches and light switches have progressed far from their early status as scientific curiosities, and have become practical *technology*. When they don't work, we have a set of equally tried-and-tested techniques for fixing them. On the other hand, all *scientific* inquiry, which as Peirce observed in his *Note on the Economy of Research* is necessarily guided by the norm of discovering Truth, takes the form of the inductive testing of hypotheses about what *would be* in respect of

some subject-matter *under the relevant circumstances, were they to occur*. Peirce distinguishes three distinct forms of logical induction, based on the mathematical distinctions between kinds of sets. Since all inductive methods rely on sampling, the set-theoretical nature of the collections from which samples are drawn limits both the kinds of inductive inferences that can be drawn and the scope of the methods developed for pursuing inquiry into such collections.

First, there are methods based on induction from strictly enumerable samples, meaning that because the total population from which the sample is drawn is known, the sample can be genuinely *random*. This permits the researcher to "judge what approximate proportion of the members of a collection have a predesignate character by a sample" drawn randomly from that population (Peirce *EP* 2: 98), thereby establishing a frequency ratio for some event or occurrence in the long run. By 'long run', Peirce (*EP* 2: 100) means that "if the occasion referred to upon which the event might happen were to recur indefinitely, and if tallies were to be kept of the occurrences and the non-occurrences, then the ratio of the one number to the other, as the occasions went on, would indefinitely converge toward a definite limit." Because of the nature of the population, it should be noted that this 'indefinite convergence' does not enable an inquirer to fix any ratio of occurrences in advance: ratios will be established in the course of the inquiry, tending never the less in the long run to a definite figure (Peirce *EP* 2: 101).

There are populations from which genuinely random sampling is not possible, because they have no definite upper limit. A national population established by census is essentially an enumerable set from which a conceivably random sample may be drawn; the Set "all of humanity since the Peace of Westphalia in 1648", on the other hand, includes a wholly indefinite number of humans into the future. This population is *identifiable* (after all, any reader of this paper belongs to it) but not *enumerable*. However, it is quite possible to assign to this population an ordering, for example, in generations since 1648, and assign to its members an ordinal value (the thirteenth generation after Westphalia, for example). However, Peirce asserts that because there is a sensible order that can be assigned to the population, a researcher can propose to draw a sample "under the guidance of a precept under

which we can enlarge any sample drawn indefinitely, and can also draw an indefinite number of samples" (Peirce, *EP* 2: 101). Supposing, then, that one can propose a correlation between the predesignated quality to be established (say, "died between ages 25 and 40 while employed as a mineworker"), the precept of sampling, the population sampled, and what the future may hold; then "we can infer inductively the proportional frequency of that character in future experiences of members of the same collection; and the induction must approximate indefinitely, though irregularly, to the true proportion" (*ibid.*). Peirce likens the 'precept' of sampling to the procedure for throwing a die when testing the frequency with which that die comes up with a specific value. In general, this method of sampling relies on the *repetitive application over an indefinite period of a method* to a collection, with the results tending toward some proportional value characteristic of that collection.

Research based on this method of sampling is applicable in the present context when seeking to establish trends that would continue to obtain when action is *not* taken to address the condition in question (here, of course, the condition being health and safety in mining). However, the object, to paraphrase Karl Marx's *Eleventh Thesis on Feuerbach*, is not just to understand the present situation but to change it. Already, considerable work based on a further form of inductive reasoning is, in our opinion, delivering results. These are the engineering solutions in which researchers, having analysed the regularities associated with specific conditions, "simply conclude that future experience will be like the past" (Peirce *EP*2: 103) and design equipment or procedures that obviate the future frequency with which this experience would occur. Other projects, not yet in the prototype stage but in the process of accomplishing this, have proceeded by developing and applying a method of observation or analysis, recording where and why the method has not succeeded in explaining the regularity in question, and then gone on to refine the method to account for earlier failures. Persistence in refining the method thus yields actionable findings that may provide the basis for designing suitable new equipment or procedures (Peirce, *EP* 2: 103).

But the broader rubric of inquiry into the conceptually contestable subject of a *culture of health and safety* goes beyond merely single regularities, and is applicable, as we

have presumed, across a much broader population. In essence, it is only possible to predicate of a population that it enjoys, is the beneficiary of, or partakes in the virtues of a culture of health and safety, if that population exists in an indefinite *future* because we cannot with confidence predicate of the *present* population that it so relates to health and safety. There are errors and terrors lurking in this assertion, because of the *inherited rank ordering of society* that remains close to the habitual going-on of a large subset of the present population. Several generations were previously consigned to social membership limited to employment in fields where the habits proper to the *logica utens* of traditional communities only qualified individuals for temporary migrant sojourn as manual labourers in modern communities. Inquiry may show what influence this discriminatory division between tradition and modernity may have had; certainly, we suspect that there is a suppressed or implicit reference to this in the revised submission of the Chemical Workers Industrial Union to the Leon Commission (Leon 1995a: 26):

> With regard to training workers, they are unable to recognise danger when they see it or properly to evaluate the nature and extent of the danger or how to react to it. This was due to inadequate training, insufficient communication and insufficient information.

The first sentence of this statement is best read as an assertion of fact. In effect, the problem lies in the second sentence's ascription of "insufficient information" as the cause for this. Over a period of decades, managers at all levels probably sent out a greater volume of "information" in the form of memoranda, instructions, procedural orders, and other such missives, than they would like to guess. To workers habitually able to infer from the presentation of Hazard to the formulation of action to mitigate it, such memoranda etc would have been a nuisance, perhaps, but they would have acted more or less (perhaps very often *less*) effectively in implementing whatever management had instructed. To workers for whom *any communication* from a higher level of management is habitually an *order*, a corresponding habit of obedience *or disobedience* would be the preferred option in a habitual ranking of preferences. For others who habitually defer to superiors on the basis of the latter's greater experience or qualifications, perhaps, the outcome would have been not question why, but to do whatever the shiftboss or foreman instructed them to do.

In this limited selection of brief examples we have deliberately employed the device of the *stereotype*. We noted earlier that stereotypes often evolve out of the different possible relations that people could infer about the institutionalisation of a culture of safety. By the same token, different definitions of the concept of *culture* readily give preference to the stereotyping of in-group and out-group qualities, usually in the form of what kinds of reactions people display under particular circumstances. But although the notion of a stereotype has received a bad press in the social sciences, we distinguish between a stereotyped inference, and a *bigoted* inference. Bigotry is never harmless; on the other hand, we consider it to be *impossible* for people to develop practical reasoning in the absence of stereotypes. Bigotry entails automatic rejection of the validity of *anything* done a member of an out-group; indeed, the bigot necessarily acts on the basis of an out-group member's status as a *pariah*: the preferred inference from the encounter with a pariah is rejection. Stereotyping, however, is merely the habitual inference learned in respect of those we meet in the course of learning who is close community, and who is distant but not necessarily threatening. Logically, practical reasoning uses stereotypes instead of explicitly expressing the universal and existential quantifiers that might be applicable for formal reasoning about the same subject-matter. What concerns us in the present report, however, is not the stereotypes people use in inferring their responses to other people, but the stereotyping of *contexts*. Put differently, we consider it a worthwhile topic of research to identify, classify, and conceptualise the habitual ways that people stereotype hazardous situations in the process of *inferring what to do about the situation*.

This has an element of ethnographic methodology in it, with the associated difficulties we noted about ethnography and anthropology in our discussion of the different meanings of *culture*. An additional problem arises when taking on the task of finding a method for fixing the practical purport of a general health and safety culture that, by all reliable accounts, is not present in the broader population from which mines draw their workforce. We need first, therefore, to formulate an hypothesis as to just what would constitute the foundations for such a *culture*, with the additional necessity for characterising the population from which the sampling is to be carried out. Because we consider this project to be focussed on the most

general level of inquiry aimed at conditions in an indefinite future, then the numerical nature of the population from which we must sample becomes something other than what we have already outlined. This is largely because, under any system of open and predominantly voluntary social formations (a bit of longhand for "free democratic society") persons are definable not merely as "atomic individuals" or in terms of a single community of origin or limited pattern of customary behaviour, but in terms of their actual and possible *relations with other persons*. That is to say, that the relations that make up any one group are defined *both* internally by the rules of membership of the group, *and* externally by the possible relations such groups may have with each other *in an indefinite range of possible encounters between individuals and groups*. Historical and local factors qualify this assertion sufficiently for us to recognize that such openness is a *normative goal* and by no means an operative system.

Among the culturally important factors, taking the etymological origin of the concept in the business of raising something and bringing it to maturity, is the simple social fact that generations born into new sets of institutional relations have a different way of conducting themselves under those relations than did the generation who worked to put these institutions in place. New generations, in other words, become "posted" in respect of their relationship with institutions that either changed, or were introduced for the first time, as the result of actions taken by an earlier generation that is still for the most part alive in their communities (Shepperson, 1995: 90-91). A changed statutory environment for mine health and safety, for example, has a very different meaning for the surviving generation of the activists, union-members, tripartite negotiating teams, and mine ownership and management classes of the transition (1991-99), than it *would have* for the new generations born after the new establishment. A "post-apartheid" generation, that is to say, reasons practically with institutions that are *already there*, having the reality of existing Actualities and not the reality of Possibilities that they were for their elders. In this emerging situation, a very much more complicated set of relations between institutions, communities, and persons evolves, making the task of sampling these relations increasingly dubious from a statistical point of view. Naturally enough, there will still be specific enumerable populations from which statistical data can be gathered with confidence;

this, however, is a matter of administrative research guided by the establishment of facts, and not of general inquiry guided by the long-term norm of Truth.

For the purpose of sampling populations that “cannot be considered as a collection, since it does not consist of units capable of being either counted or measured, however roughly, and where probability therefore cannot enter, but where we can draw the distinction of much and little, so that we can conceive of measurement being established”, Peirce (*EP* 2: 105) offers a third approach to inductive reasoning. This takes the form of *sampling of possible consequences of hypotheses*, and applies in cases where the aggregate from which the sample is drawn does not permit even pseudo-random sampling. Unlike the other forms of induction, this is not inherently self-correcting, but none the less can bring errors to light if the inquirer persists with the method. However, because the testing of a prediction is not sufficiently definable to be treated as a unit, the researcher must begin to look for facts relating to the hypothesis, in so far as these can be reconciled with the hypothesis even if unexpected. Peirce's point is that we "cannot ordinarily hope that our hypothesis will pass though the fire of induction, absolutely unmodified."

> Consequently we ought not to conclude that it is absolutely correct, but only that it very much resembles the truth. In so far as further induction will modify it, as it must be expected that it will do, if it is not to meet with downright refutation, it can hardly fail that the modification will come about gradually. We shall first find facts, reconcilable yet unexpected. These will be discovered in greater volume, until they show that a modification of the theory is necessary (Peirce *EP* 2: 105).

This particular approach borders on the realm of hypothesis formation (what Peirce called *abductive* inference), anticipating Ludwig Wittgenstein’s remark that knowledge does not come upon us in a blinding flash, but instead the “light dawns over the whole” (1974: §141). On the other hand, this form of induction is proper to developing populations that exhibit the kind of continuity that underpins Peirce’s ultimate evolutionary metaphysics (which we will not elaborate any further here).

An accident, then, is what occurs when the consequence of some everyday or routine act goes deleteriously wrong in respect of the habitual reasoning under which the act was performed as part of the expected continuity of everyday practice. In the present research context, this will occur under two possible sets of circumstances:

1) we reason correctly about the performance of a tool or equipment that malfunctions in the performance of the act; or

2) an apparently reasonable habit turns out to be unsuited to the context in which ordinarily functional procedures, tools, or equipment are being used.

Type 1 accidents or health hazards we address with engineering solutions, assessing the design of the tool or equipment to minimize the breakdown experienced; or we critically assess maintenance procedures so that the condition that caused the breakdown can be prevented from developing. Type 2 accidents we address by inquiring into who followed what reasoning in the decision to act in *that* particular manner under the given circumstances. In any given event, elements of both types of accident may be present: the explosion at Chernobyl generating reactor in 1986, for example, involved the combination of an unsuitable reactor design with maintenance personnel taking shortcuts during routine testing. Inquests, therefore, often need to assess which of the forms takes priority in preventing further occurrences of the same accident. It is important to observe at this stage that there can be a considerable time delay between an act and a subsequent accident, sometimes with quite disastrous effects because of the diminution of apparent risk with time. An example that springs to mind is the Merriespruit slimes dam collapse of February 1994. Here, inattention to the civil engineering aspects of slimes dam design while the dam was being constructed and during the years it was in use, came to a head long after the mine had stopped production. Arguably, a similar situation may be said to have prevailed at Kinross Mine Number 2 Shaft, where polyurethane pipe insulation caught fire and gassed more than a hundred workers on the ventilation exhaust side of the blaze. In the latter instance, management had been given solid information about this hazard, but had vacillated about what needed to be done to mitigate it.

Mining personnel familiar with the *Reef* Magazine of years past, will recall the repeated injunction, concluding incidents chronicled in the monthly "Accidents That Could Have Been Avoided" column, for improved training of workers following fatalities resulting from Type 2 incidents. However, improved training can take many forms, and the economics of worker training in the context of a still largely migrant mine labour force, often ends up introducing only minor procedural changes. In any

dispensation, newly-recruited workers bring more or less unpredictable histories of practical reasoning into the workplace, the object of vocational training being to instil additional habits that are more relevant to the workplace. Unpalatable though it may be, many South African recruits may still tend to act habitually on reasoning proper to the late policies of the unlamented "Own Affairs" models of public education. Despite the many statutory imperatives enacted and implemented since 1994, especially in respect of labour and education, the persistently high level of accidents and epidemic infection in South Africa suggest that the practical reasoning people *bring to these hazards* may fall short in the actions inferred from Actuality to Representation. On the other hand, it may be the case that these reasoning habits are not necessarily different from those found in industries to which migrant labour travels from persistently underdeveloped communities in which much value is still ascribed to traditional and customary practices. On this basis alone, it would be funds well spent to review the sociological status of migrant labour (if any) in the mines of Turkey, Papua New Guinea, the republics of the former Yugoslavia, Pakistan, and other nations with comparable mining accident rates. The relation in question would be between forms of practical reasoning in the labourers' home communities and the reasoning recorded in accident inquiries. There is no guarantee that this record is going to be available: we are of the opinion that *all* investigations try to establish what inferences were made, from encountering a hazard to the action that triggered it. Thus it may be of great utility to include experienced Legal researchers in a project of this sort, mainly because of their experience in teasing out chains of inference in the testimonies of witnesses to accidents. We certainly consider that the Leon Commission's recommendation that the Inspectorate of Mines establish a legal division should be taken to include a research mandate in order to participate in the broader health and safety culture inquiry.

The issue, then, would be a compilation of *arguments* proffered as reasons for accidents, classified not by the contents of the beliefs in question, but by *kinds of inference* employed in the situation. If different classes of practical reasoning are then related to the *logical* constitution of the relevant hazards, the disjunctions between intentions and actions may be more clearly discerned and appropriate

strategies devised to mitigate this. In short, the qualitative disjunction between (a) the reasoning employed in deciding to act and (b) the outcomes as determined in terms of the logic of natural hazard, itself constitutes a hazard that can be analysed quantitatively. This is because if with an increasingly determinate frequency such a specific disjunction is recorded, then this indicates that there is an underlying *habitual tendency* (having law-like characters but not itself of the nature of a law) that further inquiry may well uncover. Because a sustained disjunction between practical and logical reasoning across a population or sample must lead to a relatively specifiable number of occasions upon which actions decided on the basis of this reasoning must fail, it follows that explanations offered to accident inquiries from those involved (as victims, witnesses, supervisory personnel, management, and so on) are also to some extent descriptions of *reasoning about the acts and/or omissions that led to the incident*.

12. Conclusions.

It is of the essence in employing methods derived from Peirce's philosophy, that inquiry be designed so that natural laws and tendencies *reveal themselves* in so far as the limited perspectives of particular projects permits. This discovery is the proper mandate of formal reasoning, in that the latter abstracts from Qualities and Actualities in the ongoing business of inquiry into how reality Represents *itself*. We have offered a preliminary formal logical definition of Hazard, and analysed some elements of the nature of practical reasoning with a focus on the part that practical reasoning plays in accidents. We have therefore argued for the advantages of dealing with the culture of health and safety by approaching the matter by way of research that tries to establish the practical reasoning that management and workers habitually use when confronted with Hazard. We believe that this will skirt the possibility of controversy arising over the correct definition of culture during the early phases of the health and safety culture research programme.

As distinct from formal reasoning and its relation to research and development methodology, we have also discussed the possibility that greater attention must be given to quantifying the ways that workers' and management's *practical reasoning* stands in relation to the logical structure of hazards. The continually growing

possibility for new relations between groups and individuals in industrial society as an historical phenomenon may suggest that we should rule out any form of *general* inquiry that relies on the regular sampling techniques of administrative statistical analysis. Over the indefinite course of human social developments, from the unrecorded but still recognizably human past found in paleontological remains to the conceivable but still unrealized futures open for humanity, there is effectively no fixed limit to the possible social arrangements that could be discovered in the aggregate of possible humans who will ever have lived. Of course, there *are* specific populations the numerability of which (in their specific contexts of inquiry) will permit research based on regular statistical sampling methods; this does not permit one, given Peirce's stress on Possibility as a quality of continuity, to conclude that one can draw necessary conclusions about the human future. We must, that is to say, continually test our hypotheses against experience, correcting as we learn from the errors that this experience reveals. Thus when looking at a culture of health and safety in the broader research environment of industrial society, this background of potentiality demands that the community of inquiry must itself evolve as a special relationship with other communities in the business of inquiry at large.

The key to accomplishing the goal of establishing greater precision in the safety-related definitions of *culture* does not, however, lie solely in the number of projects undertaken. Instead, we consider that this kind of inquiry works best when researchers, as members of a social group, enjoy the greatest possible degree of freedom of communication of findings between *and beyond* projects. As results get communicated outside the immediate circle of funded inquiry, researchers in these other fields may find that problems encountered by those doing the culture of safety research, are amenable to some measure of resolution when subjected to other methods or forms of analysis. Thus, we conclude that a core support function for establishing a broader culture of safety research programme would be the provision of an online communications facility through which researchers could circulate pre-publication versions of their findings.

A very effective model for this service is the "ArXive" physics and mathematics archive at Cornell University in the US, taken over from the Federal nuclear research

facility at the Los Alamos National Laboratory. Interested researchers register on the ArXive system database and select from a list of keywords the topics about they wish to receive notification. Those actively doing research in the field submit pre-publication electronic scripts to the archive, providing a list of relevant keywords and an abstract. The archiving programme catalogues the submission according to the keywords provided, and notifies (or automatically forwards the abstract to) subscribing researchers whose areas of interest more or less coincide with those signalled by the submitting author (based on subscribers' selections from the list of keywords). Subscribers who find such submissions to be of sufficient interest to their own projects, can then readily download a copy of the pre-publication paper for closer study. As with all such work-in-progress communication, the rules of non-citation without permission continue, of course, to apply. In this manner, findings are subject to something like a peer-review system, without the costs associated with running an academic journal. Equally important, information about projects is as current as the latest submissions; proposed projects can be compared with material in the archive, thereby drastically reducing the likelihood of isolated teams "re-inventing the wheel" when the effort could be better expended on a complementary aspect of the same broad project. By the same token, work-in-progress findings announced on the archive can be subjected to falsification testing in a variety of contexts, in so far as subscribers seek to test research claims in their own specific fields of research. There is a problem with the copyright status of archived pre-publication papers, however, because not all academic journal publishers are willing to allow published authors to leave pre-publication material available free of charge for downloading off the internet. However, this is not a general problem, and some publishers are happy to accommodate such an arrangement. The Sparc Open Access Network is an online advocacy group that follows developments in this area, and could be brought on board in a partnership should such a culture of safety archive be considered.

In areas of everyday conduct, in relation to which many accidents occur because of what we sometimes too readily classify as "non-compliance", the economy of research suggests that whereas commonly-used psychological methods may reveal people's *motivations* in acting, these findings do not enable criticism of the chains of

reasoning from motive to act (however implicit these might be until elicited under the conditions of accident inquiry). Generally, it is the function of forensic inquiry to establish some measure of accountability for accidents; but the fact that this process also assigns some measure of *guilt* to those so found responsible, means that *evidence* of participants' reasoning is made subject to procedures of *testimony* elicitation. In more idiomatic terms: the more expensive the lawyer you can afford, the less accountable you are likely to be found. Potentially valuable accounts of how people reason about their relationships to the workplace, their colleagues, their occupations, may be effaced from witnesses' testimony under some conditions; however, research of this nature does not fall under the disciplinary rubric of *either* statistical *or* logical methods. There is no reason why experienced legal researchers should not join the project, therefore, using their expertise jointly with ethnographers, logicians and statisticians, in teasing out the patterns of reasoning in the legal record. Further such reasoning may be sought in, amongst others, the record of accident insurance claims and the arguments for redress in civil claims resulting from accident-related injuries or fatalities.

The communicative nature of the class of projects we have outlined in our report must be maintained, and besides encouraging communication with the establishment of a pre-publication archive this needs to be supplemented with more traditional intellectual/academic procedures. Once the archive has been established (and this may require funding in the form of travel to various established research units overseas for study purposes), an international conference on the culture of health and safety in mines should be organized. Such a conference need not (and, perhaps, *should* not) be arranged too soon. We believe that a suitable time would be, say, 24-36 months after the archive has begun attracting submissions and subscriptions, and once the legal research team has forged working relationships with existing accident research and other projects. In this manner, institutions and researchers who have become aware of SIMRAC's broader research thrust through the archive and networking with existing researchers, can bring to the broader project the necessary "critical mass" (to use the MBA-type jargon) that will hopefully encourage other funding sources to begin contributing to related projects.

In concluding, we do not believe that it would serve any purpose to fund the establishment of a new academic journal devoted specifically to the cultural aspect of occupational health and safety. It is precisely because there are such a huge number of journals, hosted in dozens of institutions globally, that we consider this to be an unpromising line of research support. Instead, we suggest that during the period that the archive is being publicised, SIMRAC identify existing journals with a suitable profile and negotiate ways to develop (as far as possible) strong research links between existing projects and the journals' host institutions. Aside from helping to avoid new projects re-inventing the wheel, as also would be served with the archive, this would encourage local researchers to "think outside the box" because such linkages tend to define the "box" much more precisely. This is because each such project has a history, and these histories in aggregate include lines of inquiry that, for whatever reason, may have led to blind alleys in their original context but which may actually provide clues for innovative projects in other contexts. In this kind of research relationship, Peirce's third mode of induction defines the difference between the blue-sky inquiry we have suggested, in contrast with the more focussed technological approach used in engineering solutions.

13. Recommendations.

The rationale behind the following recommendations is that taking on specificiable elements of the reality of hazards that affect safety and health, makes it possible to assess the feasibility of proposed research in terms of the Economy of Research. Principally, we have tried to identify lines of inquiry that avoid the pitfalls of hermeneutic and literary-critical methods, especially as these tend to apply Nominalist doctrines to topics that involve the *epistemological* structure of beliefs or reasoning habits in sample populations drawn from migrant labour forces. A sound knowledge and experience in Nominalist methods will, of course, still be important in other projects, especially of an ethnographic nature, but our research indicates that the present stage of the culture of safety research thrust would be more profitably focussed on logical and not epistemic issues.

Specifically, we suggest that data of this kind should enable researchers to isolate and classify the kinds of inferences people employ in hazardous conditions where

tight production deadlines apply, and relate these rationalizations to other forms of applicable reasoning (for example, the logic of production scheduling; the logic of regulatory statutes; and, decisively, the inferences people employ in non-occupational hazardous environments, like driver and pedestrian use of roads). Once it is possible to establish broad tendencies in the ways people who enter industry reason about their preferences for acting in one way rather than another, then the problem of culture, the general business of raising and tending to maturity, can be addressed across the subject-matter to which it properly applies: the indefinite future population-at-large who will benefit from safer working environments *in general*.

We recommend that consideration be given to supporting the establishment of a dedicated electronic archiving service at a university with an established record of mining-related research.

We **recommend** further that the servers and administration be located at one of the universities, like Wits or Pretoria, with a long history of mine-related research. This should not exclude from consideration a centre like UKZN's Interdisciplinary Accident Research Centre, which, although specialising in road accident research, may have much to offer in respect of the broader methodology of Hazard analysis.

We **recommend** that the Leon Commission's call for the Inspectorate to incorporate a legal division be taken to *include a research function*, one element of which we have suggested should be testing accident inquiry records for any classifiable patters of disjunctions between practical reasoning and the logic of hazards.

Finally, we **recommend** that the Mine Health and Safety Council, in conjunction with one or more research institution(s) carrying out established research projects, arrange an international conference on the Culture of Health and Safety in Mining, to be held at least 24-36 months *after* the pre-publication archive service has been commissioned.

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APPENDICES.

Appendix A: Section 7.3 of the Report of the Leon Commission into Health and Safety in South African Mines. Included to substantiate the present report's argument that the initial research into the problem of a health and safety culture *must* proceed on the basis of a form of cost-benefit analysis.

Appendix B: "Note on the Economy of Research." Copied with permission from *The Collected Papers of Charles Sanders Peirce*, Volume VII, 7.139-157. Included for the convenience of referees who wish to check Peirce's mathematics.

APPENDIX A

7.3 MANAGEMENT OF RESEARCH

It is in the interests of the employees, the employers and the State to maintain acceptable levels of health and safety in one of the most important industries of the country. The tripartite principle of joint control of certain issues affecting all concerned in the industry has been generally accepted by all parties giving evidence before the Commission. Clearly, research aimed at improving health and safety falls into the category of issues in which the tripartite principle should apply.

The effective management of research involved decision making at two levels. First, it is important to formulate a clear policy with regard to the research objectives, targets and priorities. The decisions in this regard must be made on the basis of full understanding of the health and safety conditions in the industry and should involve risk assessment and the analysis of the importance of making improvements in certain areas (the aim is not to do cost-benefit analysis which is not a realistic undertaking in this type of research.) This level of decision making should be the responsibility of SIMRAC. This central Committee would also decide such issues as the method of initiating research, the system for finalizing the research programme, the process of reconciling available funds with the requirements of proposed research projects, contractual arrangements with research bodies and so on. The second level of decision making is the technical level. Once the objectives, targets and priorities are approved, it is the technical committees responsibility, inter alia, to formulate the announcements calling for project proposals, the evaluation of the proposals received and the assessment of research progress.

It should be realised that the responsibilities of the technical committees include highly specialised tasks. No one questions that the management of a mine requires specialist knowledge and considerable experience. Surprisingly, it is often overlooked that the management of research is an equally specialised occupation and demands considerable experience. The membership of the current

subcommittees appears to consist mostly of mining and other types of engineers with some experience in the management of mines or plants. It is unfair to expect such groups to control and guide to the best advantage research conducted by outside bodies whose interests do not entirely coincide with those of the three parties involved in mining. To overcome this problem the subcommittees need to be reinforced by independent technical or expert advisers (see Ch. 7.5).

The SIMRAC should remain the main co-ordinating body which advises the DMEA on the conduct of mine health and safety research. The composition of the Committee should, however, be changed drastically so as to reflect the tripartite basis of all mining operations. The DMEA should have three members on the Committee, with the GME as Chairperson, employers and employees should each nominate three representatives. One representative should be nominated by the mine officials. They should be appointed by the Minister for a period of four years, and the Minster should provide SIMRAC with its terms of reference. Since many mining companies are not members of the COM, there is no single body that can claim to represent employers, and it should be incumbent on the Chamber to agree a mechanism for selecting representatives from the whole industry. Similarly the NUM is the largest trade union, but there are others and these organisations between themselves should come to an agreement on a list of representatives. It is recognised that this system will not ensure direct representation for those who are not members of any trade union or ‘association.

In the event of disagreement between the parties on names to be put forward, the DMEA should request each sector representing 5% of the employers or employees to nominate not more than three persons to SIMRAC. In the case of employers the size could be measured by revenue generated, and in the case of employees by the size of membership. The final selection of representatives should be left to the Minister, acting on the advice of the GME. In terms of the above arrangements SIMRAC should have ten voting members. As the Committee would not necessarily involve only technical or medical persons, it would be essential to have in attendance one or two experts. These experts would advise SIMRAC on the technical and medical aspects of research. This responsibility would require that these experts

have a thorough knowledge and understanding of research needs, monitoring the progress of research projects and operation of the relevant technical sub-committees. Probably the most appropriate way of ensuring that such expertise is available to SIMRAC is to appoint one or two research advisers for health and safety. These advisers would have a significant role in ensuring the effective operation of the system. The research advisers would either be serving in a full time capacity on the GME's staff or would be recruited on a part time basis from elsewhere.

The existing sub-committee system of SIMRAC could be retained with some modifications and an addition. These sub-committees would be technical bodies, where membership would not depend so much on representativeness but on technical background and experience. Consequently, persons not employed in the industry could also be nominated for membership. Nominations to each sub-committee would be presented to SIMRAC by each party represented on the Committee. SIMRAC would then select and appoint the membership and chairmen of its sub-committees. The chairmen of the sub-committees should attend SIMRAC meetings in a non-voting capacity.

In view of the earlier recommendation that SIMRAC's research programme should be enlarged to include certain health issues the formation of a health sub-committee is suggested. As the prevention of many occupational diseases is wholly or partly in the domain of mining and other engineers, the formation of an independent health subcommittee may not appear to be the appropriate solution to some. It is therefore important to ensure that there is cross-fertilisation between committees in this regard. This can be achieved partly by overlapping membership and partly by the use of independent advisers.

The COMMISSION RECOMMENDS that:

-the existing established system of a main committee, SIMRAC, and its sub-committees be retained. SIMRAC should be responsible for advising on research policy and the sub-committees would be technical bodies which, in general would look after sectional problems;

-SIMRAC should consist of ten members, reflecting the tripartite nature of the mining

industry. Each of the three main parties should be represented by three persons, and the mine officials by one person with the GME as chairperson ex officio. The details of the nomination and appointment procedures should be as described above;

-SIMRAC's sphere of responsibility should be enlarged to include relevant health research matters;

-the current list of sub-committees should be extended by adding a further sub-committee to look after health research issues;

-the sub-committee should be technical bodies, where membership reflects technical background and experience rather than sectional interests. SIMRAC should appoint its sub-committees, based on nominations from the interested parties;

-one or more research advisers should be appointed to assist in the administration of the programme and advise SIMRAC and its sub-committees on research issues; and

-the research advisers and chairmen of sub-committees should attend SIMRAC meetings as nonvoting members.