FEYERABEND REVISITED: EPISTEMOLOGICAL ANARCHY AND DISCIPLINED ECLECTICISM IN EDUCATIONAL RESEARCH

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ABSTRACT *Discussion between the adherents of various orientations within educational research has often generated more heat than light. A pervasive analogy drawn in these discussions has been between the philosophy of science and educational research. In this paper I explore the value of several influential perspectives within twentieth century philosophy of science as means of understanding what researchers in education do, and why. I suggest that Paul Feyerabend's 'anything goes' epistemological perspective has much to offer in supporting rich educational research. If positivist standards of validity and reliability are no longer considered appropriate for some forms of educational research, however, new standards for justification and representation, explicitly stated within the research, will be necessary.*

> The following essay is written in the conviction that anarchism, while perhaps not the most attractive *political* philosophy, is certainly excellent medicine for *epistemology*, and for the *philosophy of science*. (Feyerabend, 1975, p. 17, italics in original)

Introduction

The technological and social successes of the natural sciences - particularly the physical sciences, biology and geology - during the 19th and 20th centuries has led to an intriguing consequence, the expansion of rules, modes of discourse and philosophical perspectives from the natural sciences into other discourses and forms of life. Academic psychology's long love affair with the modernist program and with highly generalised rules of cause and effect modelled on the physical sciences (Polkinghorne, 1992) is perhaps the strongest example of this in the 'human sciences', but perspectives from the philosophy of science have also been strong and persistent referents in the literature of educational research.

Lee Shulman's (1986) chapter 'paradigms and research programs in the study of teaching' used the perspectives of the philosophers of science Thomas Kuhn (1970) and Imre Lakatos (1970) to explore the field of research on teaching. In attempting to confront 'counsels of despair for the behavioural sciences', Nathaniel Gage (1996) persistently defaulted to Karl Popper's (1968) 'falsificationist' perspective within the philosophy of science as a framework for the justification of knowledge claims. In this paper I suggest that Paul Feyerabend's (1975) 'epistemological anarchy' may provide a more useful, powerful and productive referent for research in education than do the perspectives of Popper, Kuhn and Lakatos. Each of these philosophical schemes, however, is of value in allowing educational researchers to create rich alternative descriptions of the enterprise in which they are engaged.

It is important to acknowledge that this suggestion is by no means a new one within the social sciences. The work in the philosophy of science on which the discussion draws was published in the late 1960s and early 1970s, and Shulman's (1986) use of ideas from Kuhn (1970) and Lakatos (1970) fifteen years ago is only one example of the influence of these ideas in the field of educational research. I do believe, however, that these ideas have sufficient power and potential, as yet only partly realised, that there is value in revisiting them as a means of opening up discussion around the issues of appropriate approaches to, and standards for, conducting and representing research in education.

I do not mean to suggest that models from the philosophy of science, intended principally to describe the natural sciences rather than the human and social sciences, are the only, or even the best, referents for research in education. I am also not advocating a simple shift of allegiance from one philosopher of science - say Popper - to another, Feyerabend. Instead I am using these perspectives from the philosophy of science as *analogies* and *metaphors*.

The intention of this article is to suggest that there are better ways of understanding what it is that educational researchers do, and the theoretical underpinnings of those practices. One good way of introducing and describing those approaches is to extend the analogy between research in education and research in the natural sciences, and to extend the appeal to the philosophers of science for rich descriptions of the things scientists do. But it should be remembered that this *is* an analogy.

Having made the claim that the blend of approaches I am advocating is 'better' than some other popular approaches, I must also turn my critical focus onto

myself, and answer the questions 'What counts as better?', 'Better on what grounds?' and 'Better under what epistemological assumptions?' It is precisely this critical focus, rather than an uncritical acceptance of any prescriptions from philosophers, that I am advocating: an approach that makes explicit - first to ourselves and then to others - the philosophical perspectives that make the practices of educational researchers meaningful. This becomes, in the final analysis, a question of what values underpin those practices.

Justification and falsification

Nathaniel Gage (1996, p.8) employs Sir Karl Popper's (1965, 1968) 'falsificationist' theory of the nature of scientific knowledge in his critique of Kenneth Gergen (1994), and what Gage calls 'counsels of despair for the behavioural sciences'. He argues that since Gergen's position is *unfalsifiable* it is therefore *unscientific*. Alan Chalmers characterises falsificationist perspectives on the nature of science as follows:

> Theories are construed as speculative and tentative conjectures or guesses freely created by the human intellect in an attempt to overcome problems encountered with previous theories and to give an adequate account of the behaviour of some aspects of the world or universe. Once proposed, speculative theories are to be rigorously and ruthlessly tested by observation and experiment. Theories that fail to stand up to observational and experimental tests must be eliminated and replaced by further speculative conjectures. Science progresses by trial and error, by conjectures and refutations. Only the fittest of theories survive. While it can never be legitimately said of a theory that it is true, it can hopefully be said that it is the best available, that it is better than anything that has come before (1982, p. 38).

Popper's perspective is itself a step away from logical positivism: it does not assert that scientific theories are true, simply that the best available theories are those that (a) explain observations better than competing theories and (b) have yet to be falsified.

Under this view, the criterion for a statement or hypothesis being scientific, rather than some other kind of statement, is that it is falsifiable, that is, that some form of empirical test is capable of being performed which could prove the hypothesis wrong. A common example used by falsificationists to illustrate falsifiability is the statement 'All swans are white'. A single sighting of a black swan is sufficient

to falsify this statement. (In my own Western Australian context, of course, all swans are black. Does this say something interesting about the context dependence of supposedly universal generalisations?).

What kind of evidence would be considered sufficiently strong to falsify hypotheses within the behavioural sciences? The problems of the theory-dependence of observation and experiment (Chalmers, 1982) have made pure falsificationist perspectives difficult to sustain even in the physical sciences, let alone in the almost infinitely more complex and context dependent human world. Popper's approach appeals to experiment and observation to test and falsify theories, yet 'theory of some kind precedes all observation statements' (Chalmers, 1982, p. 32). That is to say, falsificationism - at least of a naive kind - is inherently circular: it appeals to observation to test theory, yet theory exists prior to and implicit in all observations.

Is it possible to make generalisations within the human sciences which are just as falsifiable as those in the natural sciences (especially the physical sciences), yet have not already been falsified? While defending the possibility of making useful generalisations and theories based on empirical research in these fields, Gage does not appear to think that such theories can be held as strongly as chemical and physical theories. He retreats to a perspective which he calls 'affirmativism', described as 'an attitude that affirms the value of the generalisations and theory thus far achieved and the value of the search for more' (1996, pp. 14-15). I'm not sure Gage would agree, but 'affirmativism' seems to me to have more in common with the 'epistemological anarchy' advocated in this paper than with Popper's perspective.

Paradigms and research programs

One significant challenge to falsificationist accounts of the nature of science is that of Thomas Kuhn (1970). Kuhn explained the historical accounts of scientific revolutions using the idea of a 'paradigm'. Kuhn uses the term somewhat confusingly, sometimes considering a paradigm to be a particular exemplary scientific achievement that sets standards for practice and canons for evidence within a particular field. This is the kind of paradigm characterised by David Stenhouse (1986) as a 'Public Demonstration Paradigm' (PDP). The more powerful and pervasive image of a paradigm, however, is that of a complex, inter-related structure of theories and ideas which both explains current observations and suggests interesting questions and directions for further

research. Stenhouse (1986) describes this sense of the term as a 'Common Assumptions Paradigm' (CAP).

An individual scientific 'fact' therefore, is not an entity unto itself, but is given meaning and reality through its place within the complex web of meanings which forms the paradigm. A statement such as 'hydrogen is an odourless flammable gas' presupposes a class of things which are described as gases, conventions of odour and a definition of what 'flammable' means – that is, 'combines with available oxygen from Earth's atmosphere, releasing energy, when ignited by a source of heat'. Of course, this definition of inflammability itself requires definitions of atmosphere, heat, and ignition: and so on, almost *ad infinitum*.

Kuhn described scientific revolutions as 'paradigm shifts' - the defeat of an existing weaker paradigm by a more powerful, useful one. He saw this as a dramatic, revolutionary process, and drew attention to such examples from the history of science as the Copernican revolution. Before such a revolution can occur, however, a dominant paradigm must exist. Kuhn describes the time during which this is the case as 'normal science'. He describes fields of endeavour where there exists no single dominant paradigm, but a variety of competing paradigms, as 'prescientific', and notes that in such a situation scientific writing becomes much more cumbersome, since there exists no common vocabulary of key concepts which can be 'taken as read'. Each new scientist must derive every concept required in the argument from first principles, and must even postulate such principles.

While Imre Lakatos' (1970) perspective differs in a number of important ways from that of Kuhn, Lakatos' 'research programs' are similar enough to Kuhn's paradigms that to some extent they can be treated together for the purposes of the current discussion. Both are complex structures of internally consistent theories which may be superseded (in different ways) by better such structures. The key difference is that Lakatos saw the history of science as more evolutionary than revolutionary. Rather than the complete overthrow of the older paradigm in a brief and violent revolution, he described research programs as growing together in similar soil, but competing with one another for adherents and resources. Research programs which cannot compete strongly begin to wither away, as scientists and research funding desert them. These are described as 'degenerating programs'. At the same time other research programs seem to be becoming more useful and explaining the world better. Resources flow toward these, and they become 'progressive' research programs. It is possible, through a change in the

'climate' within which the research is conducted, for a degenerating program to become progressive again, and vice versa.

Each of these perspectives has value for thinking about the practices of research in education at a number of levels. In terms of the 'paradigm wars' (Gage, 1989, Paulston, 1990), I believe Lakatos' scheme of degenerating and progressive research programs is more powerful than Kuhn's more revolutionary approach for describing theory change. Clearly both the qualitative and quantitative research programs (broadly defined) in education are growing and thriving, and it seems unlikely that there will be a complete 'scientific revolution' in which allegiance switches completely to one paradigm or the other - and, in my opinion, neither should there be. Instead methodologies are chosen for particular purposes, based in particular sets of values and epistemological commitments, and used in particular contexts.

Perhaps the most powerful description from these perspectives in the philosophy of science is Kuhn's notion of a 'prescientific' community, one in which there exists no single over-arching paradigm (of Stenhouse's [1986] CAP type) but a variety of competing perspectives and approaches. I would argue that this describes very well the field of educational research as it currently exists, and that Kuhn's description of how science is conducted in such an environment is a powerful referent for the conduct of research in education. In representing educational research in written reports to our colleagues and constituencies, there is no Common Assumptions Paradigm to which educational researchers can appeal, and which would simplify the task of writing. It is necessary to state explicitly within the writing both the assumptions under which the work is conducted and justified, and the appropriate standards under which its quality should be judged. The issue of standards for research in education is discussed in more detail below.

Epistemological anarchy

Paul Feyerabend (1974, 1975, 1978, 1987, 1999a, 1999b) advocates the idea that fixed paradigms, research programs and sets of standards for the justification of knowledge are incapable of producing scientific revolutions and of producing genuinely new knowledge. In *Against Method* (1975), he suggests that several of the 'paradigm cases' (i.e. the Public Demonstration Paradigms described by Stenhouse [1986]), such as the Galilean revolution in astronomy *could not have occurred* had the strictest rules of Popper, Kuhn and Lakatos' schemes been enforced. It is when such standards are contravened, he claims, and 'heretical'

research proceeds - through rhetoric, persuasion, rule-breaking and sometimes outright trickery - that knowledge is advanced.

> Successful research does not obey general standards; it relies now on one trick, now on another; the moves that advance it and the standards that define what counts as an advance are not always known to the movers. ... A theory of science that devises standards and structural elements for *all* scientific activities ... is much too crude an instrument for the people on the spot, that is, for scientists facing some concrete research problem. (Feyerabend, 1988, p. 1, italics in original)

Further, he argues that:

> A scientist who wishes to maximise the empirical content of the views he holds and who wants to understand them as clearly as he possibly can must therefore introduce other views; that is, he must adopt a pluralistic methodology. ... Knowledge so conceived is not a series of self-consistent theories that converges towards an ideal view; it is not a gradual approach to the truth. It is rather an ever increasing ocean of mutually incompatible (and perhaps even incommensurable) alternatives, each single theory, each fairy tale, each myth that is part of the collection forcing the others into greater articulation and all of them contributing, via this process of competition, to the development of our consciousness. (Feyerabend, 1975, p. 30)

These perspectives suggest that, far from a drive toward the homogenisation of educational research into a single dominant paradigm, however rich and complex, it is more valuable, richer and more defensible to imagine and elaborate as many competing and incommensurable perspectives as possible, in order that our description of our field of interest become as rich and complex and multi-faceted as it has the potential to be. Rather than defining ourselves in Kuhn's terms as a prescientific culture in search of an over-arching paradigm, it is both more freeing and more productive to celebrate our 'epistemological anarchy' (Feyerabend, 1975).

When I use the term 'our' in this discussion, it is intended to connote the educational research community as a whole. I recognise, however, that this sense of the term is in some ways too broad for convenient use in the arguments I am making, because many members of the community already conduct their research activities in ways similar to those I am advocating. On the other hand, others are

deeply committed to quantitative paradigms, and do not accept that the issues these more eclectic approaches to research in education are intended to address are real issues. In the final analysis, the 'we', 'us' and 'our' I am addressing consists of everyone who aspires to richly, flexibly represent and understand what happens in classrooms, in the service of changing students' learning experiences and teachers' professional lives for the better.

Research in education can perhaps be understood as contributing to our understanding of the life in classrooms, not through adding a new piece to a predetermined, rule-bound jigsaw puzzle, but through adding a new wash of colour to a watercolour of an ever-changing scene. Each study, through its different hues and highlights, provides more detail, but the picture itself is never finished, because the seasons keep changing and the sky is never the same colour. But the picture is worth all this energy, because it tells us things about the landscape - and about ourselves - that we never knew.

This image may seem too passionless and pretty. It comes in part out of my reaction to pointless and wasteful controversy in educational research. Epistemological anarchy should not, however, be confused with a bland relativism for there will continue to be controversy and passionate disagreement. Feyerabend is a great exponent of stinging polemical writing - the section of 'Science in a Free Society' (1978) that is devoted to responses to his critics is entitled 'Conversations with Illiterates'. A black wash on a watercolour may obscure earlier colours, and discussion in educational research will no doubt continue to be heated as long as educators care passionately about education.

The key difference between the kind of disciplined eclecticism I am advocating here and an absolute relativism in which any perspective is considered to be as good as any other lies in the public, social processes of discussion and contestation which continue to occur under the former. The challenge of assessing the value of different research approaches in the 'prescientific' milieu described above, however, is a significant one. Where no generally recognised, unitary set of standards exist, what standards are appropriate for evaluating research projects and reports? This issue is discussed in more detail below.

What I wish to suggest, however, is that controversy and dissension in which the epistemological, ontological and axiological perspectives of the participants remain *hidden* is essentially wasted energy and effort. No-one is convinced, no-one learns, people are unnecessarily hurt. On the other hand, discussions in which the participants have reflected critically upon their own assumptions and

values, and are able to make these explicit for others, have the potential to be far more productive.

Feyerabend values all perspectives and traditions for a number of reasons. He suggests that:

> A free society is a society in which all traditions have equal rights and equal access to the centres of power. ... A tradition receives these rights not because of the importance (the cash value, as it were) it has for outsiders but because it gives meaning to the lives of those who participate in it. But it can also be of interest for outsiders. For example, some forms of tribal medicine may have better ways of diagnosing and treating (mental and physical) illness than the scientific medicine of today and some primitive cosmologies may help us to see predominant views in perspective. To give traditions equality is therefore not only right but also most useful. (Feyerabend, 1978, p. 9)

That is to say, as well as political and ethical grounds for choosing not to snuff out the rich diversity of competing descriptions and traditions within society generally and educational research specifically, there are excellent pragmatic grounds: competing perspectives and descriptions throw light into corners left dark by their rivals, and force one another into clearer definition.

Relativism and valuing

To say that all perspectives and traditions have value is not at all the same thing as saying that all traditions have *equal* value *for a particular purpose*. As noted elsewhere in this discussion, what I am advocating is not a bloodless relativism but an involved and reflexive interaction between, on the one hand, an eclectic but purposefully selected set of theories and, on the other, educational research practice.

Feyerabend struggled with readers' misunderstanding of his own work, and devoted the first section of his 1987 book 'Farewell to Reason' to a detailed discussion of issues of relativism. He defines relativism as the belief that:

> ...customs, beliefs, cosmologies are not simply holy, or right, or true; they are useful, valid, true *for* some societies, useless, even dangerous, not valid, untrue *for* others (Feyerabend, 1987, p. 7, italics in original)

If this fundamental relativism about theories and descriptions is accepted, how, and on what grounds, is it possible to select those theories that are 'useful, valid, true', for *our* society, *our* purposes? Donald Polkinghorne (1992) makes a distinction between the modernist epistemology of academic psychology and the postmodern epistemology of practising psychologists. He describes how, through their actual interactions with clients, practitioners develop rich, powerful sets of schemes and descriptions which they then bring to the case of a new client or new approach.

> The psychology of practice ...has come to understand that the human realm is fragmented and disparate and that knowledge of this realm is a human construction without a sure foundation. Yet this understanding has not led to a retreat into a disparaging scepticism; rather, it has led to an openness to diverse approaches for serving people in distress. The psychology of practices body of knowledge consists of the aggregate of the professional community's experiences of what has been beneficial to clients. The criterion for acceptability of a knowledge claim is the fruitfulness of its implementation. The critical terminology of the epistemology of practice has shifted from metaphors of correctness to those of utility. (Polkinghorne, 1992, p. 162)

This describes nicely the approach to research in education that I wish to advocate. If educational researchers are not to become as irrelevant to educational practice as Polkinghorne claims academic psychologists have to psychological practice, means must be found for research in education to move beyond its present largely modernist referents (and I do recognise that there have been significant changes in this area in the past decade, particularly in relation to the work of Guba and Lincoln [1989] and Denzin and Lincoln [1994]) to more powerful and flexible postmodern epistemologies and axiologies. Research which, through selecting from a rich blend of methodological possibilities, is able to access 'the professional community's experiences of what has been beneficial to [learners]', and which accepts that the 'criterion for acceptability of a knowledge claim is the fruitfulness of its implementation' (Polkinghorne, 1992, p. 162) is far less likely to become irrelevant to what happens in real classrooms.

The 'professional community' in this instance, however, consists not just of classroom teachers, but of the overlapping spheres of knowledge, practice, theory and influence of both teachers and educational researchers. The discourses and practices of classrooms and universities are brought into dialectical tension and

used to support and challenge one another. 'What works' – what has been shown to be 'beneficial to learners' (Polkinghorne, 1992, p. 162) – is itself not simple and unitary, but dependent on contextual factors including the age, sex and social class of the students, the type of school, and the regimes of assessment, evaluation and control implemented by governments and school councils.

Such approaches to research force educational researchers back to the explicit consideration of their own personal, social and educational values. The grounds for selecting particular goals for both education and educational research can no longer be referred to a unitary and unproblematic set of objective standards, but must be chosen and developed through consideration of what educational researchers value about these activities.

They also require much more engagement with those who are involved in the activities and practices of teaching and learning. It is immoral if only the values, theories, culture and needs of the researcher are considered in making decisions about what combinations of methods and descriptions will be used in educational research:

> ...my concern is neither rationality, nor science, nor freedom - abstractions such as these have done more harm than good - but the quality of the lives of individuals. This quality must be known by personal experience before any suggestions for change can be made. In other words: suggestions for change should come from friends, not from distant 'thinkers'. It is time to stop ratiocinating about the lives of people one has never seen, it is time to give up the belief that humanity ... can be saved by groups of people shooting the breeze in well-heated offices, it is time to become modest and to approach those who are supposed to profit from one's ideas as an ignoramus in need of instruction... (Feyerabend, 1987, p. 17)

This commitment is similar to the 'immersed' action research approaches of Paolo Friere (1970, 1982) and Orlando Fals Borda (1979) in South America, and resonates with Dick Corbett and Bruce Wilson's (1995) plea that researchers 'make a difference with, not for, students'.

Interstudy and intrastudy eclecticism

Both Nathaniel Gage (1963), in the first edition of the Handbook of Research on Teaching, and Lee Shulman (1986), in the third edition, acknowledge - and indeed celebrate - the possibility of different *communities* within the greater

educational research community pursuing different paradigms or research programs. They accept that each such paradigm is to be valued for its role in throwing open questions and highlighting issues which in some way complement the issues raised by the others.

The approach I wish to advocate, however, goes a little beyond this type of 'epistemological tolerance' (Feyerabend, 1974). While I would certainly applaud an eclecticism that encourages tolerance on the part of each research orientation for the perspectives of others, I would also argue that some of the most powerful and valuable research occurs when competing paradigms and perspectives meet *within* a particular study.

When reading or reviewing quantitative studies I am often frustrated: a clear and statistically significant correlation has been established, but what does it *mean*? The addition of an interpretive component - some student and teacher interviews, a brief narrative about life in these classrooms - would make the information presented so much richer and more meaningful. Quantitative research measures something - often something like achievement on standardised tests. Researchers need to explain *why* they value this kind of outcome, rather than simply assume its value.

Similarly, when reading interpretive studies, frustration at their overly contextualised nature can occur: a little quantitative information would be useful in telling me about the similarities and differences between the researcher's context and my own. Such information can increase the connectedness of interpretive research across educational contexts.

Again, I do not mean to suggest that such combinations of different techniques and approaches will necessarily be smooth and harmonious. When combining research approaches which have different epistemological bases, the approach is *dialectical* rather than synthetic: synthesis would remove the very richness that the dialectical interaction of incommensurable perspectives is intended to foster.

One example of the power of a dialectical approach to understanding is to think about the contributions to education of the disciplines of psychology and sociology. It is of little value to decide that psychology's emphasis on the cognition of an individual student is 'wrong', and that sociology's focus on the social relations within the classroom is 'right', or vice versa. Neither is it particularly valuable to try to subsume both perspectives into a single one. Richness and complexity that may be crucial to a productive framing of the

problem would be lost. Instead, by first looking at a particular educational problem through the 'lens' (to use an almost cliched metaphor) provided by psychology - the effects and influences and perspectives of the individual - and then looking at the same problem (although it cannot be exactly the same problem) through the 'lens' of sociology, a richer blend of descriptions is available than through either discipline alone.

To think dialectically in a research situation, then, is to metaphorically put on the 'spectacles' provided by one theoretical perspective, learn what can be learned of the situations, contexts and events which are of interest, then remove those spectacles and replace them with a different pair (in some ways, the more different the better), and see how the view changes. What is visible now that was hidden before, and vice versa? Since all observations are of necessity theory laden, the most powerful approach available for increasing the usefulness of observations is to change the theories with which they are laden: there is no God's eye, unbespectacled view available. Possibly three sets of theoretical lenses will be more powerful than two, and maybe even four than three. However presumably it is not practical to go on adding detail forever, or the picture becomes so rich as to be chaotic.

This image is, of course, over-simple: it assumes that the 'object' of study will sit still under different theoretical perspectives. But the object itself changes with the theoretical perspective, as different facets of school life are selected and neglected. Unlike a naive conception of 'triangulation' (Denzin, 1988) of theories or methods, the use of a variety of epistemological perspectives, theories and methodologies within a study is intended, not to provide a more accurate portrayal of a static 'object', but to create richer, 'thicker' (Geertz, 1973) descriptions of the complex life in classrooms. Egon Guba and Yvonna Lincoln (1988) have noted that 'triangulation ...carries too positivist an implication, to wit, that there exist unchanging phenomena so that triangulation can logically be a check' (p. 240). (This paragraph is, of course, itself a statement made from 'within' a particular theoretical perspective: in other views, perhaps the object of study *is* invariant under different theoretical descriptions!)

Justification and representation

Given a commitment to plurality of methods, and a commitment to the value of exploring research programs from 'within', using standards chosen by the researcher-participants themselves, as well as from the perspectives of competing paradigms, how is it possible to ensure that the research which is conducted is of

value to the educational and the broader community? How can decisions about the allocation of money, energy and other resources be made, if there exists no monolithic, unitary set of standards (kept in a glass case in Paris, perhaps?) against which all educational research can be measured?

Egon Guba and Yvonna Lincoln (1989, Chapter 8) have suggested two sets of standards for interpretive research, (Technically, their focus in this book is on program evaluation rather than directly on research. However these points remain highly relevant). They say:

> If we accept the definition of disciplined inquiry as set forth by Cronbach and Suppes (1969), it seems clear that standards for judging the quality of such inquiry are essential. The Cronbach and Suppes definition (1969, pp. 15-16) suggests that disciplined inquiry "has a texture that displays the raw materials entering into the arguments and the local processes by which they were compressed and rearranged to make the conclusions credible." Thus a disciplined inquiry process must be publicly acceptable and open to judgements about the "compression and rearrangement" processes involved. (1989, p. 228)

Guba and Lincoln's 'trustworthiness' criteria - credibility, transferability, dependability and confirmability - parallel the traditional quantitative criteria of internal and external validity, reliability and objectivity respectively. The 'authenticity' criteria - (a) fairness, and (b) educative, (c) ontological, (d) catalytic and (e) tactical authenticity - are unique to this type of research. They go beyond attention to the *methods* used in disciplined inquiry to draw attention to the ethical and relationship issues that are also deeply important in inquiring into the lives of teachers and learners. Fairness deals with the extent to which alternative constructions of the research process and results, arising from the participants in the educational situation rather than from the researchers, are an explicit part of the reporting of the research. Educative authenticity 'represents the extent to which individual respondents' understanding of and appreciation for the constructions of *others* outside their stakeholding group are enhanced' (p. 248). Ontological, catalytic and tactical authenticity relate respectively to respondents' ability to (a) know and understand their own situation more deeply, (b) be stimulated to *act* in order to improve their situation and (c) be *empowered* to act.

While the criteria proposed by Guba and Lincoln provide valuable guidance to researchers within a number of perspectives, there exist other criteria, perhaps

more controversial which can be used to evaluate research in education. John Van Maanen (1988) notes that if narrative forms of inquiry - specifically the 'impressionist tales' he advocates - are to be taken seriously, aesthetic and literary criteria become important:

> Literary standards are of more interest to the impressionist than scientific ones... In telling a tale, narrative rationality is of more concern than an argumentative kind. The audience cannot be concerned with the story's correctness, since they were not there and cannot know if it is correct. The standards are largely those of interest (does it attract?), coherence (does it hang together?), and fidelity (does it seem true?). Finally, since the standards are not disciplinary but literary ones, the main obligation of the impressionist is to keep the audience alert and interested. Unusual phrasings, fresh allusions, rich language, cognitive and emotional stimulation, puns and quick jolts to the imagination are all characteristic of the good tale. (Van Maanen, 1988, pp. 105-106)

In describing impressionist tales, particularly in the above excerpt, Van Maanen probably over-states the reliance on literary standards rather than disciplinary ones, at least for the case of research in education. While it is *necessary* for narrative representations of research that the tales presented must engage readers and hold their attention, for the purposes of research it is not *sufficient*. Research accounts must demonstrate (partly through the tales-in-themselves and partly through the meta-text with which we surround them) (a) that they are grounded in the researchers' personal experiences within schools, (b) that the things they say are in some sense 'about' those experiences and about the school, and (c) that the assertions and conjectures developed are significant and valuable within the field of education. In other words, both the *verisimilitude* and the *utility* of the research for teachers and students become important criteria for its value.

Where such a plethora of alternative descriptions and standards exists for both the conduct of the research and its representation in the research report, I would suggest that similar approaches to those Thomas Kuhn (1970) describes as occurring in 'prescientific' cultures are essential. Since the boundaries of the many and varied research programs and perspectives within education are constantly moving, and since the best studies will contravene and challenge existing boundaries, it is incumbent upon researchers, in presenting their research for evaluation (whether by dissertation committees, journal editors or funding bodies), to make explicit within the research report the epistemological, ontological and axiological commitments embodied in the research, and the standards by which they themselves have evaluated it. In this way, the research

report does not fall into a situation where it is evaluated 'with the tools or under the perspective of a competing theory' (Bauersfeld, 1988, p. 41). Instead, the standards themselves are judged for their ethical, philosophical and educational adequacy and relevance, and the research is then evaluated on appropriate terms.

Conclusion

The field of educational research is rich in alternative descriptions, competing orientations, incommensurable perspectives. When used pluralistically and held in dialectical tension, these have the potential to expand the understanding and practical wisdom of members of the educational research community in unprecedented ways. Feyerabend's 'anything goes' epistemology, combined with a consciousness of the necessity for explicitly stating the standards of justification and representation which researchers have applied to their own research, and with a commitment to real and personal engagement with those whom the research is intended to serve, provides a philosophical base for such a pluralistic/anarchistic approach.

I would like to think that the energy of educational researchers and philosophers - rather than being expended in attacking alternative orientations and defending preferred perspectives - might be channelled into the development of an ever richer blend of approaches to understanding the rich, complex life that occurs wherever people are learning together.

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