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# Sceptical or clerical? Theory as a barrier to the combination of research methods

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## Abstract

There is considerable and growing interest among social scientists and funders in the potential of research that mixes the approaches usually termed 'qualitative' and 'quantitative'. One of the obstacles to further exploration of that potential is the frequently cited notion that these approaches represent more than methods – they are incommensurable 'paradigms'. This paper discusses some of the reasons why these purported paradigms are not as much of an obstacle to mixed methods work as is sometimes portrayed. In doing so it touches on the nature of social scientific progress, and the role of different kinds of theory in that progress. The paper concludes that, as currently conceived, much theory writing is worse than useless. It is, like the notion of qualitative and quantitative paradigms, an obstacle to the development of theoretically appropriate mixed methods work.

## Why not mix methods?

Commentators such as Teddlie and Tashakkori (2003) and Cresswell (2003) have recently advocated greater use of a mixture of methods in social science research. This has become something of a general phenomenon in the last decade (Brannen 1995), in economics (Lawson 2003), psychology (Debats et al. 1995), and sociology (Rogers and Nicolaas 1998). In the UK ESRC Research Methods Programme there are, at the time of writing, two funded projects solely and explicitly about developing models for combining methods. In education, this trend has been linked to concerns about improving both the skills base of educational researchers and the quality of educational research (NERF 2001), as well as a recognition that the methods chosen to conduct research should fit the research question being posed (National Research Council 2002). In general, the debate has, for the most part, moved away from whether data from different sources should be combined, to a much greater focus on how this is done (National Institute of Health 1999).

However, attempts to build the capacity of research communities to conduct such work immediately hit a considerable problem (among others, see Gorard

2004a). This is the existence and reproduction of single method identities among new researchers, based on the idea that adopting a method automatically means also adopting an entire 'paradigm'. Work conducted in the two paradigms of 'qualitative' and 'quantitative' allegedly cannot be mixed because it will be of such different types, having incompatible meanings and values (and so on). I will not labour the point, which will be familiar to most readers, but give one example of such an argument: 'Because the two paradigms do not study the same phenomena, quantitative and qualitative methods *cannot* be combined' (Sale et al. 2002, p.43).

This paper starts with a discussion of this idea of paradigms, largely from the point of view of sociology and education where the majority of researchers use only qualitative methods. The paper, therefore, devotes much of its space to a critique of the way in which avowedly 'qualitative' researchers apparently use the notions of theory and paradigm to protect themselves from having to deal with a larger range of evidence (e.g. Hodkinson and Smith 2004). This focus is also necessary because they, more than any other group, are the ones suggesting that the combination of data from different 'paradigms' is impossible. However, it should be noted that equivalent issues arise in other fields, such as economics, where the majority of researchers use only *quantitative* methods, and there are extensive critiques of this situation elsewhere (e.g. Gorard 2003a, 2003b, 2004b).

## Is there a role for theory?

Intriguingly, the first question to address is whether there is any place for theory at all. For example, within education, despite the relative maturity of the field, there are so many important and basic practical questions to which we do not know the answer that our key starting point is often not much of a theory at all but genuine ignorance. These are often fairly straightforward situations where we are ignorant of the consequences of important decisions we are forced to make in practice (Tymms and Fitz-Gibbon 2002). They include the cost/benefits of single-sex teaching, decreased class sizes, homework, group work, rote learning and many others. Now, of course, we *could* argue that class size is a social construct or that a sex dichotomy does not do justice to the continuous nature of gendered experience, but the people who fund public research in education - the taxpayers and charity-givers and their political representatives - know perfectly well what they mean by these concepts. If they want to know whether having more or fewer pupils in a class makes a difference to specific examination outcomes, for example, then the role of theory is severely limited in the ensuing research. As researchers we could simply vary class sizes in controlled ways over a large relevant sample, and monitor and measure the pre-defined outcome. If properly conducted, the research leads to an answer which is then advertised, and policy can be made on the basis of the answer (while it is not the fault of the researcher if this does not ensue). Public policy amelioration would then be more nearly based on proven models tested against their alternatives in experimental comparisons (Slavin 2002). We *may* start the research with our own ideas and hunches about what will happen but a fully-fledged prior theory, which may be no more than a belief or superstition, can actually be less than helpful. The need to make new explanations consistent with some already established body of theory tends to stifle progress (see also Feyerabend 1993). When setting out to test a

practical question such as those above, imagining the eventual argument structure on which a knowledge claim will be based helps ascertain the form of the data it would require, and so helps the planning of research. But theory, as it is usually envisaged, is largely irrelevant to such practical arguments (Toulmin 1958).

A similar point is made from a very different perspective by Thomas (2002) who claims that “educators' irrepressible interest in theory leads qualitative inquiry [in particular] into sterile terrain” (p.419). A lot of their writing is not about theory at all, but is simply 'theory talk', with the associated epistemological cachet and prestige that this creates for a certain type of peer-reviewer. A theory can be a set of statements about the social world that can be tested empirically (clearly not its use in a lot of UK work), or a kind of tool for thinking often referred to as a ‘lens’. Thomas (2002) claims that qualitative researchers presumably want the tool kind of theory, but that in practice they also want to use the knowledge-claiming properties of the first. They are trying to have their cake and eat it too, and their over-commitment to pre-ordained theory is leading to mistakes of interpretation in what are otherwise relatively simple observation data. Theory use of this kind is, therefore, a hindrance to progress. It is too often confused with speculation or subjectivity (To 2000). However, as the paper tries to show in successive sections, while there are dangers to social scientific progress arising from the over-use of theory ideas, scientific theories are actually a fundamental component of research.

## The problem with theory as paradigm

In the sociology of science the notion of a 'paradigm', as a description of the sets of socially accepted assumptions that tend to appear in 'normal science' (Kuhn 1970), has some considerable descriptive value. A paradigm is a set of accepted rules within any field for solving one or more puzzles – where a puzzle is defined as a scientific question that it is possible to find a solution to in the near future, to distinguish it from the many important and interesting questions that do not have an answer at any particular stage of progress (Davis 1994). 'Normal science' in Kuhnian terms is held together, rightly or wrongly, by the norms of reviewing and acceptance that work in that taken-for-granted theoretical framework. A paradigm shift occurs when that framework changes, perhaps through the accumulation of evidence, perhaps due to a genuinely new idea, but partly through a change in general acceptance. Often a new paradigm emerges because a procedure or set of rules has been created for converting a more general query *into* a puzzle. But, what Kuhn saw as normal science could also be simply passive and uncritical rather than genuinely cumulative in nature. It could be based on practices that differ from those stated, because of deceit, either of the self or the audience (Lakatos 1978, p.44), and because practitioners conceal their actual methodological divergence in practice (Gephart 1988).

However, the term now tends to be used very loosely. Instead of using 'paradigm' to refer to a topic or field of research (such as traditional physics) which might undergo a radical shift on the basis of evidence (to quantum physics, for example), some commentators now use it to refer to a whole approach to research including philosophy, values and method (Perlesz and Lindsay 2003). These

commentators tend to use the term conservatively, to defend *themselves* against the need to change, or against contradictory evidence of a different nature to their own. The idea of normal science as a collection of individuals all working towards the solution of a closely defined problem has all but disappeared. The concept of paradigm has, thus, become a cultural cliché with many meanings (and some of this was inherent even in the original formulation). It is now almost meaningless.

The most unhelpful of the supposed paradigms in social science are the methodological ones of 'qualitative' and 'quantitative' approaches. Unfortunately, some novice research students can quickly become imprisoned within one of these purported 'paradigms'. They learn, because they are taught, that if they use any numbers in their research then they must be positivist or realist in philosophy, and they must be hypothetico-deductive or traditional in style (see, for example, such claims by Clarke 1999). If, on the other hand, students disavow the use of numbers in research then they must be interpretivist, holistic, and alternative, believing in multiple perspectives rather than truth, and so on. Sale et al. (2002), for example, claim that 'The quantitative paradigm is based on positivism. Science is characterized by empirical research' (p.44). Whereas, 'In contrast, the qualitative paradigm is based on… multiple realities. [There is] no external referent by which to compare claims of truth' (p.45). Such researchers 'evidently believe that the choice of a research method represents commitment to a certain kind of truth and the concomitant rejection of other kinds of truth' (Snow 2001, p.3). They consider that the value of their methods can be judged completely separately from the questions they are used to answer.

What is ironic about this use of the term 'paradigm' to refer to a methods- and value-based system in social research is that it has never been intended to be generally taken-for-granted, in the way that 'normal science' is. Rather, it splits the field into two non-communicating parts. Therefore, a paradigm of this kind cannot be shifted by evidence, ideas, or the fact that others reject it. It becomes divisive and conservative in nature, leading to 'an exaggeration of the differences between the two traditions' (Gray and Densten, p.419) and an impoverishment of the range of methods deployed to try and solve important social problems.

When we act pragmatically in our non-research lives we do not usually invoke a paradigm as our starting point. In preparing a large formal party, for example, we would use documents, conversations, and numeric accounts in an unproblematic way. We would not, presumably, reject the advice of the caterer simply because it was expressed verbally, nor would we refuse to calculate the amount of food or drink required simply because that would involve numbers. If we did, the organisation of the party would suffer. 'We may consider ourselves utterly devoted to qualitative research methods. Yet when we think about investigations carried out in the normal course of our daily lives, how often do measuring and counting turn out to be essential to our purposes' (Crotty 1998, p.15). Why then do we behave any differently in our research, which is, presumably, just as important? Methods should be used 'as a tool serving the questions pursued, rather than allowing them to constrict the range of inquiry' (Beyer 1992, p.62).

The distinction between 'quantitative' and 'qualitative' work, on which this notion of paradigms is based, is anyway exaggerated. Most methods of analysis use some form of number, such as ‘tend’, ‘most’, ‘some’, ‘all’, ‘none’, ‘few’ and so on. Whenever one talks of things being ‘rare’, ‘typical’, ‘great’ or ‘related’ this is a statistical claim, and can only be so substantiated, whether expressed verbally or in figures (Meehl 1998). Similarly, quantification does not consist of simply assigning numbers to things, but of relating empirical relations to numeric relations (Nash 2002). The numbers themselves are only valuable insofar as their behaviour is an isomorph of the qualities they are summarising. Statistical analysis is misunderstood by observers if they do not consider also the social settings in which it takes place, and the role of 'qualitative' factors in reaching a conclusion (MacKenzie 1999). Normal statistical textbooks describe ideal procedures to follow, but several studies of actual behaviour have observed different common practices among researchers. 'Producing a statistic is a social enterprise' (Gephart 1988 p.15), and the stages of selecting variables, making observations, and coding the results, take place in everyday settings where subjective influences arise (Gorard 2004b).

Even such an apparently basic operation as the measurement of a length involves acceptance of a series of theories about the nature of length and the isomorphic behaviour of numbers (Berka 1984). It is important that we even revisit such theories on occasion (Prandy 2002), and their reconsideration could be part of a genuine paradigm shift (see above), but the ideas within them will have been tested, and found useful in making predictions about the future behaviour of the world. As with ‘number’ and ‘length’, so also with many of our basic concepts and classifications for use in social science – ‘sex’, ‘time’, ‘place’, ‘family’, ‘class’ or ‘ethnicity’ (Gorard 2003a). Such ideas are tremendously powerful and useful, but they remain theories and so should be susceptible to change. Theory, in this sense, is part of our ordering of all experience (Dubin 1978).

It is, therefore, somewhat impractical to sustain an argument that all parts of all methods, including data collection, carry epistemological or ontological commitments (Frazer 1995, Bryman 2001). Rather, researchers tend to confuse the issues, shuttling from technical to philosophical differences, and exaggerating them into a paradigm (Bryman 1988). No research design implies either qualitative or quantitative data even though reviewers commonly make the mistake of assuming that they do – that experiments can only collect numeric data, observation must be non-numeric, and so on. Observation of how work is conducted shows that qualitative and quantitative work are *not* conducted in differing research paradigms, in practice. The alleged differences between research paradigms (in this sense) prevail in spite of good evidence, not because of it (Quack theories 2002).

World views, anyway, do not logically entail or privilege the use of specific methods (Guba 1990), but may only be thought to be so due to a common confusion between the logic of designing a study and the method of collecting data (according to de Vaus 2001, and Geurts and Roosendaal 2001). 'The researcher's fidelity to principles of inquiry is more important than allegiance to procedural mechanics... Research should be judged by the quality and soundness of its conception, implementation and description, not by the genre within which it is conducted' (Paul

and Marfo 2001, pp. 543-545). In real-life, methods *can* be separated from the epistemology from which they emerge, so that qualitative work is not tied to a constructivist paradigm, and so on (Teddlie and Tashakkori 2003).

Perlesz and Lindsay (2003), who are not constructivists, believe that positivists (their term for anyone using numbers in research) hold reality to be independent of and external to any observer. Constructivists, on the other hand, often hold there is no external reality, and that findings are 'created' solely through the research process (and triangulation between methods is therefore impossible since there cannot be different views of the same reality, because the reality *is* the different views). However, the key point about positivism, and the reason it is so seldom advocated today, is exactly the opposite. Positivism denotes a belief that entities do *not* exist independently of their measurement, and is therefore more similar to relativism than either logical empiricism or realism. This view has long been discredited (Cook and Payne 2002). Rather than worrying about rather petty distinctions between constructivism and social constructs, given that no one is suggesting that we have direct experience of an objective reality, we should be more concerned with finding better ways of describing what we *do* experience (Rorty 1999). For the present we could do worse than adopt a position of being ontologically largely realist (there must be something for us to research), epistemologically somewhat relativist (trying to make sense of and unify different perspectives), and methodologically fairly pragmatic (using whatever methods it takes to get the job done).

## The problem of non-scientific theory

Holmwood and Stewart (1983, 1991) have written about a distinction between 'productive' and 'non-productive' sociology. The former tradition of work uses theories that try to simplify and unify explanations of the social world, permitting a greater level of complexity only when those simpler attempts fail. It does not adhere to firm *a priori* categories or classifications, looking rather for straightforward and parsimonious explanations of social processes which are consistent with the evidence. This generally leads to new theories in the form of transferable and testable resources for researchers to help them both explain and predict empirical phenomena. The latter tradition of non-productive work, on the other hand, starts from a premise that social experience is confounding, contradictory and paradoxical, because that is how it often appears to new researchers. This tradition, therefore, preserves its prior theories for much longer because for its researchers a theory does not fail when it is contradicted by experience (that is merely further confirmation for their premise). Where such theory develops it tends to do so in an *ad hoc* fashion that is not clearly related to the findings of subsequent research.

Of course, the distinction between these traditions is not sharply delineated. Any theory can be affected by processes and phenomena unrelated to the empirical evidence, and the difference between the groups would therefore be one of degree. And any theory totally at odds with the evidence from the social phenomena it seeks to explain is likely to be overthrown eventually, even in the non-productive tradition. But this does not mean that the two extremes do not exist.

The use of theory, in education research for example, often involves the 'adulation of great thinkers' such as Lyotard, Vygotsky or Foucault according to Tooley and Darby (1998, p.56). As they describe it, this is not a scientific approach to explanation through the use of theory, and does not involve testing or specifying criteria for failure of the theory. Rather, it appears to stem from a literary criticism background, which rewards ingenuity in applying literary ideas from one writer to the writing of another. It is common for 'researchers' in this tradition to try and explain some new phenomenon using the thinker's framework, but they do so by only arguing *for* it (using it as a ‘lens’, in their own terminology). Tooley and Darby (1998) give examples of journal articles where the theory palpably does not fit the data but remains apparently unhurt by the experience, at least in the eyes of the article's author. The use of vague definitions of unfamiliar terms, the easy creation of new terms, and the perception of contradictions as attractive rather than anathema, all mean that the theory can not be easily tested by new evidence. There is also commonly a lack of consideration of any alternative explanations (Gorard 2002a).

Perhaps this is why the process of research does not seem either to alter these theories through a consideration of new data or help our understanding of the data. The template for such work is rather - here is the evidence, here is the explanation, and here is its similarity to the writing of the great thinker. In the same way that evidence does not seem to affect theories, sociological theory has had little impact on any of the important findings of empirical study. ‘This does not mean that general theorists are not cited by sociologists who do empirical research; but these citations usually appear at the beginning of the article as a ceremonial citation and have little influence on the actual conduct of the work’ (Cole 1994, p.140).

Referring to the theories of grand thinkers like Freud or Marx, Hollis (1994) comments that 'these theories were awash with confirming evidence but for the unsatisfactory reason that their adherents could square them with whatever happened' (p.72). Rather than specifying in advance the conditions under which a theory would be deemed to be false (however, unlikely that might appear in prospect), adherents of theories often defend their position in advance by arguing against logic itself. For example, MacLure (2003) treats poststructuralism as just such a theory. She says, “by 'theory' I have in mind that loose collection of continentally influenced approaches with literary and/or psychoanalytic leanings that often go under the names of poststructuralism, postmodernism, deconstruction and discourse analysis” (p.134). She defines it as follows - 'Perhaps the nearest one could get to a common characteristic of poststructuralism would be a radical suspicion of reason, order and certainty' (p.180). Therefore, this is a theory that can be defended against contrary evidence because it rejects the very notion of logic on which contradiction is based by conflating reasonable doubts about certainty in social science with doubts about reason itself.

‘Reasoning is a way of testing and sifting ideas critically. It is concerned with how people share their ideas and thoughts in situations that raise the question of whether those ideas are worth sharing' (Toulmin et al. 1979, p.10). The line of argument must be exposed, and stand up, to criticism. If theories make a difference compared to their alternatives (i.e. they lead to different, however subtly, accounts of

the world) then the evidence for or against each theory can be presented, and all commentators can then argue from common ground (i.e. if theory T implies that event E will happen, then if E does not happen it affects the truth value of T). If theories lead to different accounts than their alternatives, then these differences can be sought in evidence and the persuasiveness of the theory can be reasoned. On the other hand, where they do not make a difference, then they are not testable. They become merely articles of faith (or 'religions'), a voice for their users' own 'rage against reason' (Hacking 1999, p.67), and not part of the research process at all.

Such an approach to the rejection of reason and the celebration of contradiction is used by Usher and Edwards (1994) with the work of another grand theorist - 'to make sense of Lyotard demands that we avoid totalisation and thus the argument that there are inconsistencies in his position' (p.171). Whatever 'totalisation' means, it is clear that Usher and Edwards will not treat logical contradiction within a theory as any kind of hindrance to its use. According to common definitions of the term - such as that of the American Heritage Dictionary of the English Language (2000) that theory is 'a set of statements or principles devised to explain a group of facts or phenomena, especially one that has been repeatedly tested or is widely accepted and can be used to make predictions about natural phenomena' – what MacLure and the others are talking about is not really a theory at all but a pre-existing stance for conducting their research.

Typical of the grand theorists used frequently by subsequent researchers is Bourdieu (1990) who asserted that because the social world is complex (clearly plausible) then any theory about it must also be complex. The latter is not so plausible. It would appear that any theory will be a simplification of the social world it claims to help explain (otherwise it would simply repeat that world in its entirety). In fact, one the main reasons that the physical world appears simpler than the social world to some observers is precisely because we currently have simpler theories to explain the physical world (Turner 2002). This is because the science of the physical world is more advanced than that of the social world, rather than because it is intrinsically simpler. If theories have to be simpler than the worlds they explain, then we have to decide how much simpler these can be. In scientific approaches the answer has tended to be that a theory should be as simple as possible while remaining faithful to the observed evidence (Morgan 1903). Bourdieu does not agree, but does not explain why. According to him, if critics do not agree with him it is because they do not understand. But, 'in my view, the real purpose served by the obscurity of Bourdieu's prose is to protect his own work from refutation' (Sullivan 2002, p.26).

Leaving aside the actual value of Bourdieu's theories in explaining his *own* findings, it is harder to see the justification for using these ideas to help interpret work by others in different fields. Yet this happens frequently (Howe 1995). Such theory is used by (mainly) qualitative researchers to help their results to try and influence practice or policy, but why it is necessary to do so is not usually explained. The problem, for qualitative work conducted in isolation, is that its findings are not usually generalisable – by its nature it is not used to make general claims, and in practice its conclusion are rarely presented in warranted fashion. So mono-methodic

qualitative researchers tend to eschew generalisation, but then they have to face the issue of why they are doing the work, and why anyone else should be interested in their results. There is a real danger then that the results from predominantly small-scale qualitative studies are largely and unfairly ignored (Swann and Pratt 1999). Howe (1995) believes that researchers use theory to suggest that their work provides them with something more epistemologically secure than mere ungeneralised observations.

Bourdieu's theory of habitus is ideal for such uses because it is vague. For example, 'Habitus can be viewed as a complex internalised core from which everyday experiences emanate' (Reay 1995, p.357). What does that mean? Bourdieu gives habitus many meanings in his own work, and it is often used by others despite being very difficult to operationalise. 'It is unclear what the concept of habitus adds to such work' (Sullivan 2002, p.16). Yet it may be this very slipperiness that is a key attraction. 'The conceptual *looseness* of habitus... makes possible adaptation rather than the more constricting straightforward adoption of the concept within empirical work' (Reay 1995, p.357, my emphasis). This is not theory as any scientist would understand it - it is, rather, an inkblot test used as a stimulus for the imagination.

Mills (1959) asks of this kind of theory use, ‘after all the impediments to meaning are removed… what then is being said?’ (p.27, although it should be recalled that he also warned of the dangers of empiricism abstracted entirely from theory). He translated examples of grand theory into simpler styles, and shows how anodyne their ideas often are, and his purpose in doing so is to help theorists from ‘their useless heights’ (p.33). He claims that most commentators who appreciate grand theory do not, actually, understand it but find it fascinating precisely because of its unintelligibility. Many users of grand theories have generally abdicated their responsibility to their audience for describing social processes plainly (and when converted into plain English the ideas often become either trite or simply nonsensical). Theory becomes, for them and us, an endless and pedantic elaboration of points and distractions. The advancement of any grand theory usually ceases with its founder anyway (To 2000). In future studies, 'rather than proceeding forward toward the testing and formulation of this theory, the data are made to fit it' (p.8-9). The theory has, therefore, become a system of belief. It may be easier to sustain such a belief in a social science rather than a natural science because the data are more fleeting. But while the general validity of social theory may therefore be limited by its specific geographic, social and temporal context, ironically the assumption of its extended applicability is 'usually held by followers and users who romanticise the function and power of the theory, [which] virtually forecloses the possibility for the advancement of the original inquiry' (p.9).

## Sceptical or clerical?

The standard approach to research is logical empiricism (see above, and Paul and Marfo 2001), which has gained its strength from it commitment to learning based on experience and experimentation. At heart it is sceptical rather than dogmatic. For science, the results have been astounding in comparison to fields such as law or politics that did not adopt the same approach (Kroto 2003). Logical empiricism was

the basis for the Enlightenment, and from the outset it appealed to the dispossessed and ordinary citizens. Empiricism was deemed a radical approach offering resistance to authoritarian epistemologies, especially those of clericalism which promoted the importance of doctrine *over* evidence. It was adopted by radicals as a means of reforming education, and promoting social class harmony. Writing was couched in simple language wherever possible, to ensure that its arguments were robust, to allow wider consideration and dissemination, and to keep it grounded and of practical use. In practical field such as education, housing, health, crime and so on, we are primarily concerned with substantial arguments and should therefore, according to Toulmin (1958), ground our claims in the practical context of each situation, rather than in the abstract principles that earlier philosophers and religious leaders wished to impose on us. But one of the main reasons that practitioners and policy-makers do not use the findings of academic research today is that it tends to be written in a form of language that they cannot comprehend, for which the use of theory must take a substantial part of the blame (Hemsley-Brown et al. 2002). It might be no exaggeration to say, in the twenty-first century, that the growth of research is still being retarded as it was in previous centuries by a kind of reactionary clericalism (Steele 2002).

Although post-modernism, for example, has been presented by some as the end of theory, for others it clearly *is* a theory (MacLure 2003). It casts doubt upon 'the self-evidence of the idea of knowledge as emancipatory' (Ramakers 2002, p.636). As an intellectual approach to research (rather than in art and architecture whence it sprang) it is, therefore the antithesis of the liberating anti-clerical tradition. Instead it resurrects the privileged position of clericalism, and emasculates people as activists (Gonzalez 2002). The idea that postmodernism (or poststructuralism) can be a force for social change or improving justice is an illusion, because ‘those who describe themselves as postmodern… are not as radical, original, or relevant to moral and political deliberation as they sometimes think (Rorty 1999, p. 4). In this 'post'-age, formal religion is faltering in developed countries, yet that same mindset of believing in grand accounts of the world in the absence of, or even despite, evidence is still powerful among an academic aristocracy.

It is not clear that the terms post-modern or post-industrial, describe the world in any meaningful or useful way. Their 'central ideas are that the world has fundamentally changed and that people are only interested in consumption and their individual lives. The evidence is overwhelmingly against them here' (Cole 1994, p.31). However, these are not theories that exist to be tested, or subjected to evidence. Like solipsism, they cannot be tested in any practical way. Knowledge for them is, anyway, only meaning. 'Realities are discursive; that is, there is no direct access to a reality 'outside' discourse' (MacLure 2003, p.180). Research is here merely the deconstruction of meaning rather than a search for the truth (or preference) or practicality (what works). And so, '...Post-modern theorizing alternates between banality and absurdity, between triviality posing as revelation and bizarre claims that conflict with common sense' (Bailey 1999, p.162). As Gomm (2004) points out, by denying the possibility that there is any means of judging knowledge claims to be more or less true, postmodernism makes research a completely pointless activity. Theories such as these have become religions,

contributing to a waste of research time and effort that could be devoted to useful reform and improving popular justice in the real world inhabited by the people who actually fund the research (Howe 1995).

'Being sceptical in some way or other can be considered to be the driving force behind human intellectual endeavour' (Ramakers 2002, p.632). Scepticism can be converted from being simply a challenge to certainty to being a component of good scientific research, and theoretical development. It is something we need to foster (Shavelson et al. 2003), largely through the development of our ability to create and consider substantive (i.e. not solely methodological) alternatives to any of our research conclusions (Weiss 2002). However, the scepticism associated with postmodernism is very different to that traditional idea of *close* scrutiny. Rather, it is generally relativism 'of the worst kind' (Ramakers 2002, p.631). The problem with this relativism is that it is inherently contradictory, being itself based upon a universal claim. It is standard practice for relativists to claim to know something which is not possible if their theory is correct (Winch and Gingell 1999). For example, if different social groups have different notions of truth that cannot be understood by outsiders then who is the observer who can see from outside? If middle-class teachers cannot understand working-class kids then how is it possible for middle-class education researchers to realise this? The observation that everyone has a different viewpoint is useful but commonplace, but it leads us to *relativity*, which is an injunction to researchers to express their findings in ways that would be true for all observers (Turner 2002).

There is a very real danger that theory as a system of thought prescribes the conduct of the research by influencing the subject of which it is intended to be an investigation (To 2000). We need instead a healthy scepticism about all theory (Rossman and Wilson 1991). ‘In many cases general theoretical/methodological stances are just stances: slogans, hopes, aspirations, not guidelines with clear implications that are followed in practice’ (Platt 1996, p.275), and the ‘worship of theory [is] inhibiting cumulation’ (Davis 1994, p.196). By 1994, the National Opinion Research Center General Social Survey had received over 100 suggestions for extra questions to be added to the annual survey, but not one of these was based on the justification that they would test a social theory. Apparently the desire to test our cherished ideas is absent. One problem is that the widespread use of theories in social research without recourse to testing leads to the adoption and rejection of alternative theories on an *ad hoc* basis. Theory becomes an intellectual fashion item which, through association with the research, creates a poor public image for all academic research.

Pre-judged stances for research, such as feminism or anti-racism, may, like autobiography or even serendipity, help determine the nature of the research questions to be asked (Agozino 1999). They help us decide what is important to study, for while commitment to a cause is not the same as objectivity, neither is it, necessarily, in opposition. But such commitment or prior perspective/theory must *not* be allowed to pre-determine the answer. The kinds of evidence generated by the different starting points should be able to be set against each other - and in an ideal research world the evidence uncovered by any researcher would be very similar

whatever their starting point. The first commitment of the researcher is to the quality of the research – for poor research, with findings driven by the desires of the researcher however worthy, is demonstrably unethical (Gorard 2002b). The chief criterion that identifies research as an enterprise *sui generis* has to be the capacity for surprise. If it is not possible for the research to bring us up against evidence that our pre-conceived ideas (our prior bets) are wrong, then we are not doing research (even if it is labelled as such).

Again, a comparison of progress in natural science can help us see how commitment to improve society or public life is totally compatible with genuine curiosity as a researcher. Of course, all scientists want good results, but if Watson and Crick had produced a different model of DNA that was just as useful then they would have had no reason to stick with a counter theory (Cole 1994). Compare this with someone who argues that social researchers should take sides before collecting data (Griffiths 1998), such as a feminist who may not propose a theory that sex discrimination is infrequent, for personal reasons, even when it fits the data better. Or a school effectiveness researcher who uses more and more complex statistical analysis to find the effect 'that must be there'. Their goal here is political rather than cognitive. This is one of the key barriers to progress in social science, and just about the only one that *can* be changed (we cannot change the complexity of our research sites or the mutability of our data, for example). Progress could be seen as an evolutionary process with no specific 'life-form' destined to emerge from its pressures. Similarly, no specific theory has to be so, and we might have made equivalent or even better progress with other theories. So our ideas are not inevitable, even when they are based on 'reality'.

## Theory and science

Research findings, and the models based on them, represent a simplified description of a system that assists us with calculations and predictions. They do not represent complete truth, and are good and useful only in so far as they enable us to make good decisions or improve performance (West and Harrison 1997). There are certain shared assumptions underlying all research, whatever methods are used, and there are no pure ontological or epistemological divisions in practice (Denscombe 2002, Gorard 2002c). In the same way, a policy-maker who believes that human rights are an inalienable part of the soul and one who thinks that human rights are simply an admirable invention may both suggest the same policies. Similarly, in natural science the actual philosophy adopted by practitioners makes no difference to how they proceed (Rorty 1999). Scientists who support Kuhn's views are not going to do much different in their research to those who support Weinberg, for example.

Research requires rigorous creative reasoning supported by an appropriate mixture of methods, and findings leading to testable models or theories. A specific design or method does not make a study scientific, for 'the question drives the methods, not the other way around' (Feuer et al. 2002, p.8). At core, the nature of scientific inquiry is the same in all fields (National Research Council 2002). In fact, a consideration of how social science research is actually done, rather than how methodologists often claim it should be done, suggests that nearly all studies

proceed in the same way – contextualised, value-attuned and ‘post-positivist’ (Eisenhart and Towne 2003).

Recognising the existence of genuine multiple perspectives does not mean the end of truth as an ideal. We could, for example, view one research site in terms of its efficiency, economy, heating, and lighting etc. Each account so generated may be true, but they are also, quite properly, orthogonal to all of the others. We cannot, because of this, seriously assert that anything must be true. But 'interpretivist methods and analyses are sometimes abused to *justify* a lack of rigour' (Denscombe 2002, p.22). Such commentators are confusing the perfectly proper realisation that one phenomenon can be viewed from many perspectives, with the idea that anything can be a perspective. They are confusing the widely accepted notion that knowledge is socially constructed with the invalid inference that any social construct can be deemed knowledge. Truth, according to Howe (1988), is a normative concept, like ‘good’. It is what works in practice, for that is how we recognise its truth. Where research has been testable, and has practical consequences, a kind of evolutionary natural selection has led, over time, to the universality of logical empiricism. For, while there may be plausible arguments put forward to believe that pain is good, or that two plus two equals five, research based on such premises would be non-viable and actions based on that research could be dangerous (Tooley 1999). Researchers with such idiosyncratic views are, eventually, eliminated, just as someone who *genuinely* did not believe in an external reality might soon be knocked down by a bus. This slow evolutionary pressure means that we do not have to go as far as Kroto (2003) suggests by denying the benefits and results of the scientific approach (medicine, transport etc.) to those who decry scientific endeavours.

In fact, the researcher who claims not to be scientific may merely be insufficiently aware of the basis of their own approach - there are many examples of social scientists who claim to be extreme relativists, for example, while behaving with respect to the ideas of others as nothing of the sort. 'So unconscious is the average social scientist... of the gnoseological presuppositions of his [sic] study that he finds it only too easy to avow allegiance to doctrines wholly at variance with the philosophical pre-requisites of his own researches... intellectual fashions are made up of avowed philosophies and not assumed ones' (Postan 1971, p.ix, see also Bricmont and Sokal 2001). In the same way, many readers will have observed commentators decrying the ability of science to control or predict future events (with some justification), but then advocating astrology, for example.

The call for better more responsible publicly-funded social science is largely for empirical evidence and reasoned argument, as opposed to prejudice and untestable opinion, and it is in response to those people who seriously propose unscientific research that allows fictional drama, for example, to be treated as evidence rather than simply a way of disseminating evidence (in Mayer 2001). ‘We have to put up with an appalling amount of bunk… simply because we cannot draw a firm line between what is legitimate academic sociology and what is not’ (Davis 1994, p.188). However, mixing methods does not necessarily involve a commitment to social research as a natural or even a social science. Design science, for example, includes creativity, artisanship, craft principles, inspiration and fuzzy science.

However, at the end of the day any artefact we create has to work for the design to be deemed successful. There is little room for relativism here (Bailey 2001). Either the artefact works as the design intended, or it does not. The key point is that a specific design or method does not make a study scientific (but only if it allows *direct* investigation of the question being asked). Dewey (1916) warned against 'our predilection for premature acceptance and assertion… Even in the case of failure, we are inclined to put the blame not on the inadequacy and incorrectness of our data and thoughts… Science represents the safeguard of the race against these natural propensities and the evils that flow from them'.

## Discussion

Theory helps us to decide what and how we research. It helps us to measure and explain. It can be crucial in the transfer of research findings to new settings, and an important end-product of research. Above all, theories are cheap. They allow us to consider alternative positions simultaneously, to make progress in parallel, and to accept failure without great cost. Theory and research are usually inseparable complements, since a piece of research tests ideas stemming from a theoretical model, leading to modification of the theory (Dubin 1978). Theories lead via logical deduction to hypotheses, via operationalisation to observation, via scaling and measurement to empirical generalisations, and so to further theories.

But there must be a balance in our use of theory. A theory is a tentative explanation, used for as long as it usefully explains or predicts real-world events, not an end in itself. As soon as theory itself becomes an obstacle to what or how research is conducted then it becomes worse than useless. Above all, theories must be subject to testing and then discarded whenever they consistently do not match up to empirical observation. Theories will always be under-determined by the evidence on which they rest, since any finite number of observations can be explained by a potentially infinite number of theories. By itself, theory rarely leads us to new inventions or the creation of useful artefacts.

All potential aviators in the 1900s followed the same Newtonian theories of physics, for example, but some of their ensuing planes flew and some did not - it was only the testing that sorted the one from the other. The theory alone is not *sufficient*. Nor is the theory *necessary,* for if, on the other hand, someone had managed to fly in some way that contradicted the prevailing theory then it would be the theory that would have to change. The mere act of flying would be far more powerful than the most plausible theory could ever be. Science and technology, therefore, often advance through instrumentation rather than theory, with the theoreticians playing 'catch-up' (Davis 1994). But in social science, methods are often not as highly regarded (as opposed to being merely talked about). We have good methods for studying individuals in social settings, but many people study instead large groups and huge global or temporal trends using somewhat naïve methods. Academic research funding tends to be given to big questions that we are likely to be unable to answer, and so on.

It is important for practical progress in our field that we begin to be more careful in our use of theory, and cease criticising a piece of work simply for not having a grand theoretical framework. At one level theory seems to be a framework, such as socio-culturalism or constructivism, that is used as a tool to help generate ideas and questions for research. But these frameworks are protected by their advocates from any testing scrutiny or the consideration of counterfactuals. They are, in effect, articles of faith. This does not make them any worse, or indeed any better, for generating ideas than any other approach. What they must not be allowed to do is help determine the *results* of research. At its most abstract level, theory is about how and what we can know about the social world of our research. Theory at this level is often turned into paradigms. This is not a useful form of theory for most researchers, for two related reasons. First, while it is perfectly possible for a philosopher to have many views of the nature of knowledge that are logically consistent with our everyday observations - such as solipsism, or even a belief that everything is random – ethically, it is not possible to hold any such views while conducting applied and publicly-funded research. Second, if a 'paradigm' privileges particular forms of evidence then this leads to a kind of knowledge relativism in which the different parties cannot even argue coherently with each other, since each can legitimately reject the very nature of the argument used by the other party (note that this is a very different situation to querying the quality of the argument). If all research were to lead only to the finding that the results depend on the prior perspective of the researcher, then it would cease to be funded and cease to be listened to by outsiders with any respect at all.

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