Two concepts of ‘paradigm’

Writers of nursing research textbooks continue to invest a great deal of significance in paradigms. For some authors, the terms ‘qualitative’ and ‘quantitative’ themselves identify a paradigmatic difference (Lusardi 2004, Munhall 2007, Houser 2008, Streubert Speziale & Carpenter 2007). Others, however, identify a series of paradigms which cut across the qualitative/quantitative boundary. In the latter case, there are numerous typologies, each of which represents a different permutation of –isms: positivism, post-positivism, interpretivism, constructivism, and the naturalistic paradigm (Ellis & Crookes 2004, Parahoo 2006, Polit & Beck 2008, LoBiondo-Wood & Haber 2010, Rehm 2010, Slevin 2010). All of these are ultimately traceable to a *locus classicus* source, *Naturalistic Inquiry*. Lincoln & Guba (1985) modified their taxonomy over the years, but we can currently add critical theory and the participatory paradigm to the list (Lincoln & Guba 2003). A third group of authors, aware of the doubts occasionally expressed about paradigm discourse (Atkinson 1995, Thorne *et al.* 1999, Morgan 2007), or recognising the ‘end of the paradigm wars’ (Oakley 2000), or hedging their bets in view of the popularity of mixed methods (Creswell & Plano Clark 2006, Andrew & Halcomb 2009), are more non-committal, even if ‘paradigm’ remains an obligatory point of reference (Watson *et al.* 2008, Holloway & Wheeler 2010).

The ubiquity of the term ‘paradigm’ appears to imply widespread commitment to a concept which was introduced nearly fifty years ago (Kuhn 1962), even though there are sceptics. Kuhn himself suggested dispensing with the word as early as 1974, and clearly regretted his use of it (Kuhn 1974). He has been echoed intermittently ever since, most recently by a philosopher who takes a strong interest in nursing, and who regards the term as a piece of ‘philosophical jargon that should be dropped from our discourse’ (Risjord 2009, p. 202). While I sympathise with Risjord’s proposal, I think it needs modification. What should be ‘dropped’ is *one sense* of the word ‘paradigm’; and it is not the sense associated with Kuhn.

I will argue for two main claims. First, the sense of ‘paradigm’ as it is employed in research methods textbooks has virtually nothing to do with Kuhn’s original sense (although Kuhn’s use of the term is not unambiguous). It is an independent invention whose origins are obscure. Second, on closer inspection, the research textbook version of ‘paradigm’ does not make sense. It represents research as essentially capricious, contingent on the whims and preferences of individual researchers; and, although it is often projected on to the historical record, the ‘history’ written to fit the paradigm contrast tables is a travesty. The demise of the textbook paradigm idea clearly has significant implications, one of which is briefly sketched at the end, although a more detailed exposition must await a further paper.

K-paradigms and m-paradigms

I will begin by reviewing the differences between what contemporary methodological writers mean by ‘paradigm’ (or, as I shall refer to it, m-paradigm) and what Kuhn intended (k-paradigm). I’m aware that several different senses of the word were identified in *The Structure of Scientific Revolutions* (Shapere 1964, Masterman 1970); but later work (Kuhn 1970a, 1970b, 1974) does a great deal to clarify what he had in mind (Hacking 1983, Bird 2008), and I propose to draw substantially on those essays here.

Substantive / metaphysical.

For Kuhn, the main constituents of a k-paradigm – or 'disciplinary matrix', to use the term he preferred subsequently – are symbolic generalizations, models and exemplars (Kuhn 1974). These are substantive and specific theoretical commitments shared by the members of a particular community of scientists. Symbolic generalizations are expressions, such as 'f=ma', which can be cast in mathematical or logical form. They identify precise relationships between the variables delineated in a given theory. Models are analogical representations; for example, the idea that gases are collections of microscopic billiard balls in constant, random motion. Exemplars are 'concrete problem solutions', historically specific points of reference which constitute theoretical or experimental landmarks in the discipline. All of this contrasts with the m-paradigms described in the research methods texts. The constituents of an m-paradigm are ontological, epistemological and axiological items, referring to such abstract, ahistorical concepts as the nature of 'reality', the meaning of 'knowledge', and the basis of values. This shift from substantive to metaphysical elements is perhaps the most obvious difference between m-paradigms and k-paradigms. Whereas the latter are organised round real-world, concrete, how-does-this-work questions, along with techniques which have proved successful in answering questions of that type, the former are organised round philosophical abstractions.

Puzzles / underpinnings

Normal science, the main form of intellectual activity once a k-paradigm has been established, consists largely of puzzle solving. Any k-paradigm will have an indeterminate number of loose ends, situations in which it is not immediately obvious how the k-paradigmatic theory can be applied; and the day-to-day business of normal scientific enquiry is to resolve these situations, and show how the theory does in fact fit recalcitrant aspects of the world. For example, it took over 60 years to work out how the moon's trajectory could be derived from Newton's laws of motion (Kuhn 1962, p. 39). There is no equivalent to puzzle solving in the case of an m-paradigm, since its metaphysical constituents are construed as a set of philosophical assumptions (Parahoo 2006) or underpinnings (Munhall 2007). These do not give rise to fitting-the-world problems needing solution, because the m-paradigm – lacking models and symbolic generalisations – makes no substantive, this-is-how-the-world-works claims. Instead, the 'philosophical underpinnings' prescribe a certain type of research method (experiment, in depth interview), and direct the researcher's attention exclusively to questions deemed amenable to that methodological approach (causal questions, experience questions). Where a k-paradigm includes concrete puzzles, together with landmark examples of how previous puzzles were solved, an m-paradigm requires the use of particular methods on abstract philosophical grounds, and restricts attention to the questions deemed amenable to methods of precisely that kind.

Resources / beliefs

K-paradigms consist of problem-solving resources; m-paradigms consist of beliefs. Normal science, according to Kuhn, is a matter of solving puzzles through the use of resources, particularly exemplars, provided by the k-paradigm. A crucial part of science education is learning to solve textbook problems by employing these resources imaginatively, thereby coming to understand in a 'hands-on' way exactly what the resources are and what sorts of problem they can be used to solve. It is through this process that students 'acquire' the k-paradigm; and further progress in the discipline concerned will depend on the continued, undisputed success of the k-paradigm's resources in tackling new problems as they arise. In contrast, m-paradigms are comprised of second order beliefs, not first order puzzles and resources; and they do not have to be acquired through creative puzzle-solving activity. If a student already believes that '"real reality" exists "out there", driven by natural laws' (LoBiondo-Wood & Haber 2010, p. 134), then she has an antecedent affinity with positivism or postpositivism. If, on the other hand, she believes that 'multiple realities exist, influenced by culture and environment', then her affinity is clearly with constructivism. These m-paradigmatic beliefs are not themselves resources, and students do not

expend any intellectual effort in understanding how they can be used to solve theoretical problems. At best, they learn a simple set of directions: 'if you believe that truth is objective reality, then you will select a quantitative/deductive approach; however, if you believe that truth is the subjective expression of reality, you will adopt a qualitative/inductive approach' (LoBiondo-Wood & Haber 2010, p. 136).

Shared examples / axioms

One of the surprising features of Lincoln & Guba's (1985) account of m-paradigms is the fact that they are presented as axiomatic systems, an idea which is taken explicitly from Euclid's *Elements*. In their example, naturalism is defined by a series of five axioms, specifying the basic assumptions from which the characteristics of naturalistic enquiry, in operational practice, can be logically derived. There is an ontological axiom ('there are multiple constructed realities'), an epistemological axiom ('knower and known are inseparable'), an axiological axiom ('inquiry is value-bound'), and so on (pp. 37-8). The 14 'characteristics of enquiry' which immediately follow are claimed to be 'logically dependent' on these axioms, although the corresponding rules of inference are not identified, and proofs are not provided. This appears to be more than an analogy. The axioms define the m-paradigm, and the methodological 'characteristics' are deduced from them in the same way that, in Euclid, theorems are deduced from the initial five postulates. In marked contrast, a k-paradigm is clearly not an axiomatic system. There are no axioms, no theorems, no logical derivations. Kuhn (1974) is quite explicit about this, identifying shared examples as the core concept, and completely rejecting the idea of 'rules': 'Shared examples can serve cognitive functions commonly attributed to shared rules. When they do, knowledge develops differently from the way it does when governed by rules' (p. 319). So, where m-paradigms are axiomatic systems, k-paradigms are sets of concrete examples representing successful scientific practice, and 'shared' in the sense that they are universally acknowledged by the relevant scientific community, and provide common benchmarks for the continuation of normal science.

Functional / arbitrary

The constituents of a k-paradigm have specific functions. The k-paradigm acquires its status because it is more successful than its predecessors 'in solving a few problems that the group of practitioners has come to recognise as acute' (Kuhn 1962, p. 23), although it can never explain all the facts with which it is confronted. So, for example, symbolic generalisations are the formal components of the disciplinary matrix; models mediate between the theory and the world, making it possible for the generalisations to be applied to particular problems; and the exemplars represent instances of successful problem-solving which are already to the k-paradigm's credit. For every constituent of the k-paradigm, a substantive and unequivocal reason can be provided for its inclusion. With m-paradigms, things are very different. The m-paradigm's 'axioms (basic beliefs) are arbitrary and may be assumed for any reason' (Lincoln & Guba 1985, p. 36). They 'must be accepted at face value', and 'cannot be tested for truthfulness against some external norm'. The m-paradigm represents 'a distillation of what we *think* about the world (but cannot prove)' (pp. 14-15; italics in original). Unlike the constituents of a k-paradigm, the axioms of the m-paradigm are not accountable to anything. They have no function except as arbitrary postulates, and they can be included in the axiom set for any reason, or no particular reason. This contrast between the functionality of k-paradigm components and the arbitrariness of m-paradigm basic beliefs could hardly be more striking.

Cognitive habits / conscious opinions

As a consequence of learning the k-paradigm, a scientist acquires a collection of cognitive habits, some of which she may not be fully aware of, since her education involves unarticulated assumptions as well as explicit problem-solving strategies. Often, these habits of thought will not be apparent until there has been a scientific revolution, and it becomes possible to see – with the benefit of hindsight – that a habit of mind inculcated by the old k-paradigm was preventing the recognition of something which the new k-paradigm makes visible. In this sense, a k-paradigm's resources often turn out to be, in retrospect,

barriers (Margolis 1993). Instead of hard-to-identify cognitive habits, m-paradigm discourse favours cognitive verbs which imply conscious awareness: 'believe in', 'believe that', 'recognize', 'emphasize', 'take the view that', 'value', 'appreciate', 'choose', 'select' and 'adopt' (Parahoo 2006, Munhall 2007). It refers to individual worldviews and 'personal taste' (Polit & Beck 2008, p. 18). The word 'assume' is used occasionally, but mainly in the context of positivism or postpositivism, where it standardly implies 'believe wrongly'. The image is, not of cognitive activity which is inculcated, habitual, non-transparent, and conceivably difficult to eradicate, but of creeds, faiths, perspectives, tastes and preferences, all of which are conscious, considered, and open to inspection or critique.

Community / individual

A k-paradigm is, by definition, something which the members of a scientific community share (Kuhn 1974). Scientific investigation is organised around the symbolic generalizations, models and exemplars accepted by a determinate group of people, each of whom recognises the same disciplinary landmarks. A k-paradigm intrinsically *belongs*, as it were, to the community concerned. However, the community has independent existence; it has its own networks, culture, social structure, and strategies for education and apprenticeship. Individuals are socialised into the community, and learn the paradigm as a result of this process. This is not true of m-paradigms, which can be ascribed to individuals independently of their membership of any community. Indeed, m-paradigms are often equated with an individual's 'worldview' (Kikuchi 2003), and the choice of an appropriate method is said to depend 'on researchers' personal taste and philosophy', the selected m-paradigm being the one 'that corresponds most closely to your view of the world' (Polit & Beck 2008, p. 18). Students are accordingly advised to choose the approach 'that flows from the "how" you think, your worldview, your propensities' (Munhall 2007, p. 25). Underlying this advice is the assumption that the individual has an antecedent belief system, on the basis of which she will adopt a matching m-paradigm from those available in the academic market place, together with the methods it prescribes. This marks a significant shift from a disciplinary concept of k-paradigm to an individualised concept of m-paradigm – or, in more sociological terms, a transition from communitarian to consumerist models (Etzioni 2003).

In series / in parallel

According to Kuhn, a k-paradigm is intrinsically singular for any given discipline. It incorporates the symbolic generalisations, models and exemplars which the members of the scientific community in question universally accept. But one k-paradigm can sometimes be superseded by another, and this occurs when certain problems – Kuhn calls them anomalies – addressed by the established k-paradigm prove resistant to the technical puzzle-solving resources which that k-paradigm provides. At this point, a scientific revolution takes place and, when the dust finally settles, the old k-paradigm will have been replaced by a new one. K-paradigms, then, appear in series. They occur successively, one after the other. This is rather different from m-paradigms, which are always presented in parallel. Although it is often claimed that positivism is discredited, and so out of the running, the remaining m-paradigms are all currently available, all simultaneously on offer. An echo of the singularity of k-paradigms survives in the idea of a 'dominant' m-paradigm, usually said to be positivism or postpositivism. But this has nothing to do with Kuhn's concept; it is an ideological description signalling opposition to what authors who use the term regard as inequitable funding arrangements. Meanwhile, nothing in the discourse of m-paradigms has the function Kuhn attributes to anomalies. The arguments offered against positivism, for example, do not refer to intractable empirical problems which terminally resist its characteristic problem-solving techniques. Rather, they are purely philosophical, in that its 'notions' are 'questioned', its 'ideas' are 'thought to be naïve' (Parahoo 2006), and its ability to 'answer complex questions about the nature of the human condition' (Holloway & Wheeler 2010, p. 24) is doubted.

In summary, a k-paradigm is the disciplinary matrix of symbolic generalisations, substantive models, and exemplars which – at any one time, for a particular scientific community – define a field of enquiry and provide the resources used to solve puzzles concerning the fit between theory and world. Learning a k-paradigm requires the study of these functional resources, and results in the acquisition of cognitive habits which will facilitate puzzle solving, but which can subsequently make it difficult to recognise the merits of a new k-paradigm. In contrast, an m-paradigm is a set of (abstract and arbitrary) metaphysical beliefs – ontological, epistemological, axiological – which form the postulates of an axiomatic system, and ordain a particular research methodology. The m-paradigm is selected from a range concurrently available in the market place, and is selected by an individual on the basis of its affinity with her antecedent opinions. The m-paradigm embodies neither puzzles nor puzzle-solving resources, and inculcates in the individual no hard-to-identify cognitive habits other than those she already possesses.

The origin of m-paradigms

It is clear that we are dealing with two distinct concepts. They are biochemically different, their DNA unrelated. So it is not unreasonable to wonder about the origin of m-paradigms, given that k-paradigms are incontestably attributed to Kuhn. In this respect, the key text is Lincoln & Guba (1985). That book may not be the first to adopt the m-paradigm concept – a number of sources, including Ford (1975), Patton (1978) and Reese (1980), are cited in its first chapter – but it is undoubtedly the most influential, and is ultimately responsible for the understanding of 'paradigm' to be found in the methodological textbooks I have referred to.

Lincoln & Guba (1985) begin by introducing the idea of a metaphysical truth, which 'cannot be tested for truthfulness against some external norm such as correspondence with nature' (p. 14). 'Metaphysical beliefs,' they suggest, 'must be accepted at face value', and 'represent the ultimate benchmark against which *everything else* is tested'. A set of metaphysical beliefs may be constituted into a system of ideas; and 'we shall call this systematic set of beliefs, together with their accompanying methods, a *paradigm*' (p. 15). So paradigms 'represent a distillation of what we *think* about the world (but cannot prove)'. In a footnote, they add that this 'definition of *paradigm*' is 'consistent with the use of that term by Kuhn' (p. 45; all italics in original).

That the latter claim is misconceived will be apparent from the analysis I have already presented. The definition has all the hallmarks of a m-paradigm, and bears no resemblance at all to Kuhn's concept. A paradigm, according to Lincoln & Guba, is a collection of metaphysical beliefs, with 'accompanying methods'. It represents what 'we' think about the world; and, being unanswerable to nature, can neither be proved nor tested. There is not even a faint hint here of substantive theories being fitted to the world; of puzzles being solved by the imaginative use of models and exemplars; of the gradual acquisition of a paradigm through intense study and practice. Rather, a paradigm is something 'we think' which must be accepted 'at face value'.

There is an interesting story to be told about the conditions which made it possible for Lincoln & Guba to adopt the m-paradigm concept, believing it to be Kuhn's. The authors they cite are participants in a historical process, a gradual intellectual transition during which *paradigm* underwent a transformation: from the defining attribute of a scientific community to a property of individuals, and from a matrix of substantive puzzles and resources to a collection of metaphysical beliefs. Ford (1975), for example, presents a hybrid picture. She recognises that paradigms consist of the symbolic generalisations and the 'well-tried methods' of science (p. 10); but she still refers to them as 'paradigms of thought', 'thoughts wrapped up in thoughts about thoughts' (p. 2), and introduces the idea that a paradigm incorporates a set of 'basic beliefs which shape our own common-sense world' (p. 16). She, too – coincidentally, in a

footnote – suggests that her own interpretation is confirmed by Kuhn's own writings (p. 186). Lincoln & Guba take *basic beliefs* and *our* from this account, and translate them into the unprovable, but also unchallengeable, 'what we think'. They are equally selective with Reese (1980). They ignore the entry on Kuhn – which refers to paradigms as scientific 'models or examples of central importance', and which takes as an example the solar system model of the atom (p. 287) – and quote instead the section on metaphysics (p. 353). One could trace further influences and connections, but limitations of space do not permit a detailed study of the transition from k-paradigms to m-paradigms here.

The exclusivity of paradigms

Let me now turn to a characteristic which k-paradigms and m-paradigms appear to share. Although, as I have already noted, k-paradigms occur in series while m-paradigms occur in parallel, both concepts imply a certain kind of exclusivity. In each case, it is impossible for an individual to adopt, or inhabit, two different paradigms simultaneously.

This bald statement must be qualified. A measure of uncertainty has crept into methodological writing during recent years concerning the extent to which m-paradigms are incompatible. Lincoln & Guba (2003), for example, now believe that it is possible to 'blend elements of one paradigm into another', especially if the m-paradigms concerned 'share axiomatic elements that are similar, or that resonate strongly between them'. In this respect, 'elements of interpretivist/postmodern critical theory, constructivist and participative inquiry fit comfortably together' (p. 267). However, there is still one uncrossable m-paradigm boundary. This is the divide between 'positivist and interpretivist models', whose axioms remain mutually contradictory. 'Just as in the past, one must choose sides; one still cannot embrace both positivism and non-positivist perspectives' (Donmoyer 2006, p. 16). In nursing research texts, this relaxing of the ban on m-paradigm transgressions is equally apparent; but so too is the one non-negotiable boundary between positivism and interpretivism. These, it is generally agreed, 'have their roots in different assumptions about social reality' (Holloway & Wheeler 2010, p. 28), and constructivist ideas 'sit uncomfortably with those who want to uncover the "truth"' (Parahoo 2006, p. 43). (Some writers do claim to combine positivism and constructivism. I will consider them later.)

Subject to this qualification, both k-paradigms and m-paradigms are mutually exclusive. However, the reasons for this exclusivity are different in each case. According to Kuhn, a k-paradigm shift involves not merely a change in theory but an across-the-board change in other k-paradigmatic constituents: the entities that can reasonably be looked for, the puzzles to which the new theory immediately gives rise, what might count as possible solutions to those problems, and the universally recognised landmarks of k-paradigmatic success. For example, the discovery in 1781 of Uranus, which had been observed earlier but which had been assumed to be a star, changed the constituents of the solar system and motivated a search for similar objects, resulting in the discovery of asteroids after 1801. At the same time, it created new puzzles about Uranus's orbital elements, and in particular the discrepancies between its predicted and observed orbit, for which the existence of another new planet was anticipated to be the most likely solution (Neptune was discovered in 1846). The elements of a k-paradigm, then, are organised around a series of question-and-answer relationships. Theoretical claims prompt empirical questions, which in turn prompt a search for answers, which will eventually generate further questions (Kuhn 1977, Mayo 1996, Nola & Sankey 2007). The aim of the scientific community is to produce a comprehensive and coherent account of the fit between theory and world, and this involves finding solutions to puzzles, devising models, adapting and extending exemplars, locating new objects whose existence is implied by the theory, and so on. It is not possible for an individual to adopt two different k-paradigms at the same time because, during a period of normal science, there is only one k-paradigm, one set of questions, one set of puzzles, open for business. To adopt a different k-paradigm would be to remove oneself from the scientific community altogether.

The reason for the incompatibility of m-paradigms – or at least the incompatibility of positivism and the interpretivist m-paradigms – is very different. It is a consequence of the fact that their 'axioms are contradictory and mutually exclusive' (Lincoln & Guba 2003, p. 267). For example, it is not possible to believe that 'reality is single, tangible, and fragmentable' and, *at the same time*, believe that 'realities are multiple, constructed and holistic' (Lincoln & Guba 1985, p. 37). Equally, if 'knower and known are independent, a dualism', they cannot also be 'interactive, inseparable'. If one believes that reality is 'a construction of the individuals participating in the research', then it is presumably not possible to believe that 'objective reality exists independent of human observation' (Polit & Beck 2008, pp. 14-15). This is a logical difficulty, confronted by individual researchers. The question concerning participation in a scientific community, whose members work on a common set of puzzles with a common set of resources, does not arise. The fundamental problem is not being able to hold two mutually contradictory beliefs simultaneously.

In each of the examples just cited, the two incompatible claims belong to the same category. They are both ontological, or both epistemological. In the terms suggested by the now familiar paradigm contrast tables, where rows represent metaphysical categories and columns represent different m-paradigms (for example, Lincoln & Guba 2003), they are (as it were) 'horizontal' inconsistencies. In practice, however, the incompatibility of m-paradigms applies to diagonal relationships as well. This might seem a bit odd: in view of the fact that, for any given m-paradigm, each 'basic belief' is construed as an independent 'axiom', one might imagine that it would be possible to combine beliefs diagonally, an ontologically positivist claim (for example) with an epistemologically interpretivist one. But this possibility is never optioned. Indeed, it is strenuously resisted, because if it were taken seriously, the whole framework of m-paradigms would simply fall apart. How could there be separately identifiable m-paradigms at all if a pick 'n' mix approach were allowed? Instead, as the paradigm tables suggest, each vertical set of claims (that is, each m-paradigm) is regarded as a package deal. Naive realist ontology is attached to a dualist epistemology; both are attached to an 'extrinsic' ethic, 'tilting towards deception'; and all three of them are attached to an 'experimental /manipulative methodology', 'verification of hypotheses', and 'chiefly quantitative methods' (Lincoln & Guba 2003, pp. 256-7). These metaphysical associations (or closely related ones) are ubiquitous, indeed universal. The m-paradigm offers beliefs about reality, knowledge, values and methods on an all-or-nothing, take-it-or-leave-it basis. It is a vertical tramline from which there is apparently no escape. A positivist view about the nature of reality commits you to a positivist view about the nature of knowledge , and both are inextricably glued to a positivist view about method. According to the methodological textbooks, m-paradigms are hierarchical strangleholds, forbidding any kind of lateral movement.

The viability of m-paradigms

If there are two concepts here, not so much distinct as wholly antithetical, we might ask whether there is any reason to prefer one to the other. Are they both viable, in different domains? Do they both have something significant to say about the nature of qualitative research? Or are there grounds for thinking that they are not equally robust, coherent or illuminating? In the rest of this paper, I will argue that the m-paradigm concept is effectively broken (and should be dispensed with), but that k-paradigms, subject to one important qualification, still represent a valuable resource. I will begin by offering four reasons why the m-paradigm idea does not work.

The arbitrariness of m-paradigms

On what grounds does a novice researcher come to identify with a particular m-paradigm? The rhetoric, as I noted earlier, favours conscious 'selection' on the basis of antecedent beliefs, perspectives, tastes and preferences. The selection having been made, the m-paradigm 'guides' subsequent methodological decisions. For example, if someone antecedently believes that 'multiple constructed realities exist', then the natural choice is constructivism; and, once this m-paradigm has been selected, the researcher will be 'guided' towards 'hermeneutical/dialectical' methods. There is something slightly peculiar about this. It is unlikely that, before encountering a methods textbook, most people give much thought to the singular or multiple, constructed or unconstructed, nature of reality. But even if some do, it is not clear what the basis would be for any conclusions they might have reached. A study of philosophy? A reflection on personal experience? A particular liking for science fiction? Recall that, according to Lincoln & Guba (1985, pp. 14/36), 'basic beliefs are arbitrary'; they can be 'assumed for any reason', and 'cannot be tested for truthfulness against some external norm'. So an antecedent study of philosophy, aside from being unlikely, is also unnecessary. It seems to follow that the selection of an m-paradigm is entirely a matter of caprice. Methodological decisions, according to Lincoln & Guba, depend on the whims and preferences of the individual researcher. There is no logical, philosophical, or empirical constraint on the selection of an m-paradigm, no mental discipline that can resist the purest metaphysical fancy; for basic beliefs 'must be accepted at face value', and they 'cannot be tested for truthfulness against some external norm'. On this view, it would be possible to invent one's own eccentric paradigm, and conduct research accordingly, however ridiculous it might seem to others. Given the logic of m-paradigms, the enterprise of empirical inquiry is always, and entirely, frivolous. This is surely not a philosophy that can be taken seriously.

Reality and method

Setting aside the whimsical nature of m-paradigm selection, there is a tricky question about the logic of the inference it implies. The standard account of m-paradigms (certainly Lincoln & Guba's) suggests that an understanding of the world precedes a method-determining choice. First, a description of reality, then an inference about method. This, too, seems a little odd. If method is a *consequence* of ontological conclusions, on what grounds are those conclusions reached? The m-paradigm represents 'a distillation of what we *think* about the world (but cannot prove)'. Fine. But on what grounds do we *think* it? How does one attain an understanding of the structure of reality without some form of investigation? And how can an investigation be carried out without some sort of method? Or perhaps knowledge of this kind – that reality is singular or plural, constructed or unconstructed – is somehow *a priori*? Intuitive? Innate? The subject of revelation? I do not mean to be flippant, but the claim that beliefs about reality anticipate method seems to put the cart rather firmly in front of the horse.

If not flippant, then possibly naive. An ontological-knowledge-of-reality, it might be argued, is not the same as an empirical-knowledge-of-reality. It does not require empirical data, and therefore does not require an empirical method. But an argument trading on negatives (ontological knowledge is *not* this, and does *not* require that) is not particularly convincing; and it fails to answer the question posed in the previous paragraph: where does such knowledge come from? As it happens, ontological discussions are replete with empirical data. *Naturalistic Inquiry* certainly is. Like many other authors, Lincoln & Guba attempt to recruit scientific data, generated by positivist methods, in order to vindicate constructivism, hermeneutics, trustworthiness, and authenticity. In philosophy, this might be termed a self-stultifying argument; but the main point is that they use empirical data (and an unexplained, undiscussed method) to arrive at ontological conclusions.

Axiomatic systems

The idea that m-paradigms constitute axiomatic systems is an interesting one, even though Lincoln & Guba do not use it consistently, and it appears to have been quietly dropped from the repertoire. In the opening chapter of *Naturalistic Inquiry*, there is a brief 'digression' on Euclidean and non-Euclidean (specifically, Lobachevskian) geometries, which concludes by observing that 'different axiom systems have different utilities depending on the phenomena to which they are applied' (Lincoln & Guba 1985, p. 36). Euclidean geometry, for example, is useful on the terrestrial scale, where distances are small; but on the cosmic scale, where distances run to light years, Lobachevskian geometry is the preferred option.

Interestingly, having introduced this idea, Lincoln & Guba unaccountably fail to make use of it. If the analogy with geometry is to be taken seriously, then different m-paradigms will 'have different utilities depending on the phenomena to which they are applied'. For example, constructivism would be useful in certain circumstances, positivism in others. Admittedly, this requires a convincing account of exactly which circumstances are conducive to which m-paradigm; but assume, for the sake of argument, that an account of this kind can be provided. The disappointment is that Lincoln & Guba do not take this line. Instead, they insist on the incompatibility of the positivist and constructivist m-paradigms, because their 'axioms are contradictory and mutually exclusive' (Lincoln & Guba 2003, p. 267). True enough. But if the analogy with geometry holds, these axioms are merely constituents of two different formal systems. They should not be construed as alternative 'descriptions of reality'. Rather, the m-paradigms should be conceived as separate tools, each useful for a different kind of job.

However, it is the 'descriptions of reality' interpretation which Lincoln & Guba embraced, and which has since predominated. Unfortunately, this is also interpretation that has created all the trouble. 'There are multiple constructed realities' has one kind of sense understood as a postulate in a formal axiomatic system. It has a different kind of sense understood as a substantive claim about the world.

Package deals

For the fourth reason, let me return to the idea that m-paradigms are package deals, with permutations of 'basic beliefs' being strictly limited, and the boundaries between different m-paradigms – or at least between positivism/postpositivism and interpretivism – being sharply defined and strictly patrolled. The problem with this framework is that it is implausibly rigid and historically inaccurate (Paley 2005). I suggested earlier that the paradigm contrast tables forbid diagonal movement, or any other kind of pick 'n' mix, hybrid arrangement. Historically, however, hybrids have been the norm. Logical empiricism, for example, bears little resemblance to the 'positivist' column in the paradigm tables; and, if it were plotted into the standard grid, it would trace a zigzag pattern across it.

Even a statement like this must be qualified because the logical positivists were a heterogeneous group – they did all not hold the same views – and they tended to drop ideas that did not work. But, in general, they were not (as the paradigm contrast tables portray them) naive realists, foundationalists, or dualists (Friedman 1999, Richardson & Uebel 2007). They abandoned verificationism, were sceptical about causation, and were politically committed leftist democrats (Cartwright *et al.* 1996, Giere & Richardson 1996). They preserved a concept of objectivity (Carnap 2000), but their theory of truth 'developed step by step from a correspondence-theory into a restrained coherence-theory' (Hempel 1935, p. 49). None of this corresponds to the infrangible matrix of the paradigm tables, which ordain that realism, dualism, foundationalism, causation, correspondence, objectivity, quantification, reductionism, and an 'extrinsic ethic tilting towards deception' are inextricably bound together under the heading 'positivism'.

Of course, the 'hybrid' description fits only if the m-paradigm matrix is presupposed. Dismantle that, and it becomes evident that, during the past century, conceptualisations of science (including the logical empiricist versions) have been complex, subtle, nuanced... and unrestrained by artificial boundaries. At this point, the very idea of an m-paradigm collapses. It is, to borrow Lincoln & Guba's (2003) account of objectivity, 'a chimera: a mythological creature that never existed' (p. 279). Without the inflexible m-paradigm superstructure, methodological discourse is released from its philosophical ball and chain, while a number of familiar arguments and rhetorical moves are undermined. I will return to this line of thought in the closing section. First, however, I will comment briefly on k-paradigms.

Kuhn and k-paradigms

Kuhn did not make a fetish of the ideas for which *The Structure of Scientific Revolutions* (SSR) is best known: paradigm, normal science, revolution, incommensurability. He regarded them as a heuristic for the analysis of historical episodes (Sharrock & Read 2003), and he made no use of the terminology in his subsequent study of the black-body problem (Kuhn 1979). After several decades of inconclusive debate about the epistemological implications of Kuhn's work (Bird 2001, von Dietze 2001), there has been a shift towards interpreting it as an account of scientific practice rather than a theory of scientific knowledge (Rouse 2003), and a corresponding shift towards construing it in historical, evolutionary, cognitive, and computational terms (Margolis 1993, Sharrock & Read 2002, Nickles 2003). Indeed, Kuhn precipitated the current interest in naturalised epistemology (Kitcher 1993, Goldman 1999), and inspired philosophical interest in '*real science* rather than trivial logical surrogates for real science... real scientists possessing real cognitive abilities operating in real scientific communities developing and experimentally testing models of the real world in real historical time' (Giere 1997, p. 497). This, as I suggested earlier, is the most significant difference between k-paradigms and m-paradigms. According to Kuhn, 'paradigm innovation is rooted in knowledge of the phenomena and not in any philosophical understanding of truth' (Sharrock & Read 2002, p. 204).

This is the qualification I referred to before. Provided epistemological issues are finessed (or ignored), the Kuhnian concept of a paradigm is alive and well. Let me briefly allude to three examples of current philosophical/historical research motivated (and, indeed, anticipated) by Kuhn's work. First, the idea that there is an experimental tradition which is relatively autonomous from the theoretical tradition in the history of science. This idea is not especially prominent in SSR, but is discussed at length in a later essay (Kuhn 1976), which describes the development of the classical sciences, closely associated with mathematics (and reaching back to antiquity), in contrast to the Baconian experimental sciences, which emerged in for the first time in the seventeenth century. These two traditions were almost completely independent of each other – methodologically, economically and institutionally – and they did not begin to converge until the nineteenth century. This theme can still be pursued in contemporary studies. As the 'new experimentalists' (Franklin 1986, Galison & Stump 1996, Mayo 1996, Mayo & Spanos 2010) have shown, 'experiment has a life of its own' (Hacking 1983). Many aspects of experimental practice, including innovations in design, method and instrumentation, survive even radical theory change. A casual acquaintance with SSR might lead one to think that this is an anti-Kuhnian theme; but greater familiarity with his writing confirms that it is not.

Second, Kuhn's specification of symbolic generalisations, models and exemplars as the constituents of k-paradigms has led to a much greater interest in the relation between them. In particular, there is now a burgeoning literature on models, and the sense in which they, too, are independent of theory (Morgan & Morrison 1999, Giere 2006, Bailer-Jones 2009). Instead of being illustrative or ancillary, models carry most of the burden of representation, and 'do so in a way that makes them autonomous agents in the production of scientific knowledge' (Morrison 1999, p. 39). Still, the autonomy is not total. The interest

in models centres on the way in which they are constructed from bits of theories, bits of empirical data, perhaps some mathematical formalism, and the odd metaphor or two (Boumans 1999). There have been philosophical studies of this construction process in the context of physics, chemistry, economics and, embryonically, sociology (Cartwright 1999, van den Bogaard 1999, Phan & Amblard 2007). A general and provisional conclusion echoes Hacking's observation about experiment: 'models can have a life of their own' (Morrison & Morgan 1999, p.18).

As a final example, consider the cognitive studies of scientific practice (Carruthers *et al.* 2002, Rupert 2009), including the 'cognitive-historical' methods adopted by Nersessian (2010) for her enquiries into model-based reasoning, and more specifically a study of the cognitive dynamics of paradigm shifts by Margolis (1993). The latter is particularly interesting because it provides a convincing alternative to the strategy of taking the exclusivity of k-paradigms to be predicated on logical incompatibility. Margolis, by contrast, takes it to be consequence of the cognitive habits inculcated by a specific k-paradigm. Such habits, as I have already noted, are primarily resources; but, by the same token they function as barriers by making it difficult to recognise something which the succeeding k-paradigm makes visible. Each of the case studies examined by Margolis provides evidence for the existence of a highly specific habit of thought, associated with the old k-paradigm, which made it virtually impossible to accept that a new k-paradigm was necessary if the (acknowledged) anomalies were to be resolved. For example, the reason why Priestley (and others) found it difficult to abandon the idea of phlogiston was the entrenched habit of thought, a fundamental intuition, which saw a flame as clear and undisputable evidence of something 'escaping' from the substance concerned. This was so 'obvious' that it was simply not possible for the alternative theory – that the substance was absorbing something, namely oxygen – to be correct. In general, Margolis' work shows how analytically useful the k-paradigm concept remains, provided it is interpreted in cognitive, rather than epistemological, terms.

Concluding thought

If the m-paradigm is a chimera, the world changes subtly but substantially. Perhaps the most significant consequence is that a familiar argument becomes unavailable. 'Applying the criteria of one m-paradigm to studies conducted in another begs the question. It is illegitimate to evaluate constructivist research by appealing to positivist axioms, because canons of evidence are m-paradigm-relative; so constructivist studies cannot be evaluated on the basis of positivist criteria, and must not be accountable to positivist protocols. Specifically, the positivist requirements of validity, reliability and generalizability cannot be applied to research carried out in the constructivist m-paradigm.' Once the m-paradigm concept has gone, this argument collapses; and with the demise of the argument, the questions it was used to evade become pertinent again. Questions such as these: what procedures should qualitative researchers use to minimise the risk of error? How can they distinguish truth from falsehood? How can they determine degrees of probability? What is qualitative research's equivalent of the error-statistical canon? What is its equivalent of protocols designed to avoid cognitive bias? What effective procedures can it substitute for the mythological acts of fusing horizons, reflexivity, transferability, and bracketing?

There was a time when these questions could be written off as positivist obsessions. But the collapse of m-paradigms prevents such cheap-and-cheerful, quick-and-dirty, root-and-branch dismissal. Positivism is not (and never was) an m-paradigm, but a diverse, fluid and sophisticated philosophy of science. The difference between getting-it-right and getting-it-wrong is not an outmoded 'objectivist' concern. It is a distinction is to which every qualitative researcher should be committed.

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