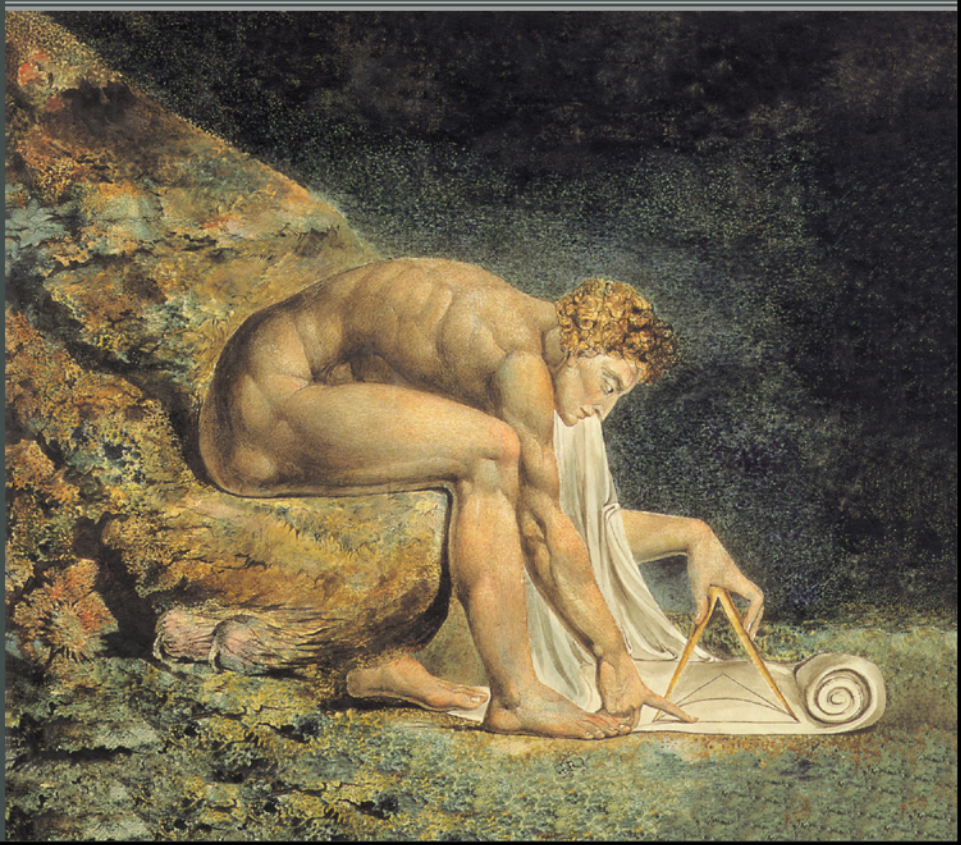
Philosophy and the Sciences of Exercise, Health and Sport

Critical perspectives on research methods

Edited by
Mike McNamee



Philosophy and the Sciences of Exercise, Health and Sport

Philosophy and the Sciences of Exercise, Health and Sport answers these questions and more in a unique interdisciplinary study that calls on researchers working in sport, exercise and health to reflect critically on the nature and aims of scientific enquiry in these disciplines. The book addresses the underlying assumptions and development of both the very idea of science itself and what shape scientific enquiries ought to take in the fields of exercise, health and sport.

Written by a range of internationally respected philosophers, scientists and social scientists, each chapter addresses a key issue in research methodology. Questions asked by the authors include:

- Do natural and social scientists need to understand philosophy of science?
- Are statistics misused in sport and exercise science research?
- Is sport science research gender-biased?
- How do external and commercial interests skew professional guidelines in health and sport research?
- Should scientists focus their attention on confirmation of theories, or on attempts to falsify them?

Philosophy and the Sciences of Exercise, Health and Sport serves notice to sport and health researchers to think more philosophically about their subject and its scientific bases. It is essential reading for postgraduate researchers seeking to establish a sound theoretical foundation for their work.

Mike McNamee is Senior Lecturer in Philosophy at the Centre for Philosophy, Humanities and Law in Health Care at the University of Wales at Swansea, UK. He is also co-editor of the Routledge series, Ethics and Sport.

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For Cheryl, Megan and Ffion with love

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Preface and acknowledgements

In these days of research selectivity, the reader will probably be aware that edited collections do not add much to one's professional CV. Writing chapters for such books then might appear to have even less prestige. The only currency in the market (it is said) is articles in internationally respected peer-reviewed journals. These are of course key indices of quality research output. They are not, however, the exclusive homes for 'real' scholarship that some believe. Something of importance must have driven the authors here to accept my invitation to contribute to this collection. Can they all be mistaken? I think not (well, I would say that, wouldn't I?!). Collections such as these, I believe, are highly important in this regard: if people are to teach courses in new areas, or new ones in old areas, they need resources. It is my hope that this book augments the research methods texts that proliferate in exercise, health and sports sciences and that it at least begins to open a space for more considered discussion of issues pertaining to the nature and methods of scientific enquiries, and the professional socialization of would-be scientists in these fields.

I thank the authors wholeheartedly for the wisdom they have shared in these pages and not least of all for their tolerance of my editorial interferences. Finally, my thanks go to Andrew Bloodworth for his diligent proof reading, to Simon Whitmore (formerly of Routledge) for his early encouragement of the text, and to Samantha Grant of Routledge (Taylor and Francis) for her patience and support.

1
Positivism, Popper and Paradigms: an introductory essay in the philosophy of science

Mike McNamee

Philosophical questions in natural and social scientific research

That we need science to understand matters of disease, exercise, fitness, health, illness and so on is undisputed. Whether any empirical or scientific enterprise could properly proceed without philosophical reflection is not universally agreed. A simple thought, however, should arrest any potential dispute. How, we might well ask, could scientists investigate exercise, measure fitness, or evaluate health and illness without first clarifying the very concepts that they sought to research? Are exercise benefits objective or subjective? What type of fitness do we wish to measure? Shall we use broad or narrow conceptions of health? What are the logical relations between disease and illness? All these simple questions are essential to scientists and other professionals in the sphere of exercise, health and sport. And they are, of course, all philosophical ones pertaining to the concepts we employ, whether as students, or lecturers, or researchers, in our professional lives.

What is less obvious, perhaps, is the array of questions that are assumed in the very nature of the methods, reasonings or theories that underlie the activities of scientists. Why ought we to consider philosophical aspects of the production of knowledge in science? For researchers, and well-published ones at that, the kinds of reflections on fundamental questions seem a mere annoyance: Are there are any absolute truths? Is relativism the only alternative to absolute truth? Can science be interest-driven and objective? Are theories incommensurable? Is there a unity of method in science? Is and ought scientific enquiry to be viewed as amoral? These are questions that certainly get in the way for some. To what extent are they an obstacle to be overcome in the production of knowledge? Are they merely of antiquarian interest to the modern researcher? In short, is there a need for a Philosophy of Science in exercise, health and sport research?

It is my contention that philosophical reflections on the natures and methods of sciences is simultaneously critical *and,* sadly, marginalized. In the course of 20 years of lecturing I have found, in the various universities where I have taught, conditions favourable and unfavourable to philosophical reflection. Every year, in research methods courses, I have been called upon to perform two apparently valued tasks. I am sure my experience is not an isolated one. First, typically in Lecture 1, the resident philosopher, if one's department is still lucky enough to employ one, will need to romp through the entire history of the philosophy of science. In this vein, I well remember a colleague

complaining in a course planning meeting for a postgraduate research methods module, that they could not possibly fit into 3 hours an introduction to a certain software package. Is it not remarkable then, that one should be required to traverse—at speeds that the term 'breakneck' hardly begins to describe—the entire terrain of philosophy of science to students equally in/experienced in both science and philosophy. My colleague was impervious to the irony. Second, and increasingly these days, the philosopher is wheeled in to speak about research ethics in the conduct of scientific enquiries and to interrogate issues such as anonymity, confidentiality, privacy, the ab/uses of 'gatekeepers', voluntary informed consent and so on. These two functions are critical to the *education* of researchers and not merely their training. The cultivation of broader concerns to inform their knowledge and understanding of scientific research is critical to their becoming reflective practitioners as opposed to mere scientific technicians. Nevertheless, these two contributions aside, the remainder of such courses, typically, is a mixture of methods and techniques of data gathering and analysing, dissecting and disseminating. (And all shall worship at the wonders of scientific method and its techniques.) Yet, is this not proper? Ought one really to expect anything else in courses typically called 'Research Methods'?

Over the time of teaching such material, I have come increasingly to believe that most of the students, from doctoral to undergraduate programmes, could not give a coherent account of the distinctions between research methods and research methodologies. And their dissertations and theses often bear testimony to this assertion. Perhaps it is the fault of the supervisors, who may well stand in similar ignorance. Yet the ability to articulate one's methods is one thing; to justify them is another. To show how this problem may or may not have been conducted otherwise, and to show that the manner in which it was conducted was appropriate or even optimal, to show how observation is theory-laden, to appreciate how data can be a hostage to method, is crucial and all too often ignored. Even where it is not overlooked it is not taken seriously. This is a strong claim. Let me say a little more then about how I think this happens.

The processes whereby scientists typically become technicians is a complex one and I am not fit to tell the story especially well. I will therefore restrict myself to a few observations that will yield at least part of the context for the justification of this text as well as the provision more widely of philosophical reflection across the range of scientific domains.

The idea of the lonely scholar conducting experiments may find its romantic home in Galileo's tower, but it scarcely comes near the modern reality of scientific research in exercise, health and sports as elsewhere. Researchers typically hunt in packs. At the postgraduate level generally, but especially at doctoral level, research teams in and out of laboratories focus on specific issues and techniques. Those teams and laboratories become reputed for certain types of research: department X is brilliant with certain biochemical assays, department Y is more focused on epidemiological work; research unit Z is excellent on survey work; team F focus on high performance, department A on individualized qualitative work (or post-structuralist feminist critique, or figurational analysis, and so on). Their funding is generated by key publications which then secure private or governmental monies in order to produce more research, and so the cycle goes on. The mix can be either methodological and/or theoretical. And 'paradigmatically', as I shall note below, this all operates at a level below conscious consideration or reflection.

Scientific labour has necessarily become specialized. Teams divide their labour from the mundane blood collection techniques, the assays, or the questionnaires or interviews, the drafting of data tables and so on. The statistical analyses will typically be done by a specialist. Other critical tasks, whether writing the funding proposal, the review papers (state of the art [science] summaries), final versions of international journal articles, or the keynote lectures, will be assigned. That fragmentation of the process is now essential to much modern science. And, lest it be thought that I am biased, while this has been the norm in natural sciences for a long time now, it is increasingly becoming the norm in the social sciences too. Departments are rated more highly where their research themes are tightly focused and where their colleagues collaborate in shared ends and agreed methods. The benefits of such managed research are too obvious to recount, the drawbacks more subtle. I have often met PhD students who have already completed one or more experiments but performed no literature review. When asked how they decided upon the methodological approach they simply said—without a whiff of disquiet or even unease—that all that had been set out by the lab director or principal researcher or that the method was so obvious that no serious reflection was required on it. Equally, I have met funded PhD students in the social sciences who had failed to appreciate that their funding was predicated upon particular theoretical approaches that, mid-way through, they had come to challenge with great discomfort. Perhaps most pernicious of all, and increasingly prevalent in the days of 'publish or perish', is the attribution of authorship: whose name gets on to the published research—and often in what order—reveals hierarchies of power that seem ineradicable in much modern science. Yet the inputs to the research are often so varied in quality and quantity that there are real and pressing questions to be asked about the researcher's names on papers no less than the appearance of signatures on the originality clause at the front of every thesis. We can also ask questions of scientific integrity and the im/proper socialization processes of future generations into a particularly cynical conception of science. Such are the prices of modern, managed research and the efficient production of knowledge.

These sketchy scenarios raise questions about the relations between values, theory, method and data, about research funding, and about editorial biases in certain forms of research or historically privileged conceptions of scientific questions and solutions. They are every bit as important for researchers as the selection of case study or survey, or of invasive versus non-invasive techniques for the measurement of aerobic and anaerobic metabolism. These are the types of questions that I have asked the authors of this text to address. In the process of the book's history, I set out to find authors who had genuine authority in their fields and to offer them a question that was of some scientific and professional moment. I did not want a chapter written merely on a fascinating philosophical puzzle within a scientific context. I wanted to display the urgency and privilege of philosophical reflection *in* answering scientific questions. Very often, in the process of drafting and redrafting, that question became revised and refined; some authors fell by the wayside, others joined in. Equally it was my contention that the type of reflection we called 'philosophical' in scientific matters was not the exclusive province of philosophers. Every good scientist in their activities needs to address conceptual questions just as much as they must address epistemological ones. Ought not every scientist to consider the alternative conceptions of the phenomenon they are researching before they operationalize their definitions of the subject they propose to investigate?

Ought not every scientist to reflect on the relations of theory, method and data? My contention that they should is carried into the choice of authors for the text. Only three are professional philosophers. Yet each of the natural or social scientists that have contributed to the text has, as is demonstrated here, thought deeply and philosophically about the nature and methods of their enquiries. I also confess to a deeper, political, motive. Were a philosophy of science text to be written merely by a philosopher or philosophers, I sceptically assume that it would not be received as well by the multiple audiences whom I have targeted. Indeed, it may not even be read by them nor reviewed in the natural and social scientific journals of exercise, health and sport. External criticism can often be dismissed as impractical and/or irrelevant, outdated, uninformed, and these are the most polite of the disavowals. Criticism from within the quarters of scientific domains, from authoritative voices, cannot be dismissed *ad hominem* with a clear conscience: whether the proposition a person propounds is true or false, it will not be so merely *because* it is the view of this or that particular person. Were mere philosophers to propound some of the views set out here, my intuition is that swift rejection might well follow.

The text is not an original one in the 'philosophy' of science. Nor is it even a typical one. And so for those who seek detailed discussions on the nature of causality, or of explanation, or of inference to the best explanation, realism and anti-realism in science, will be disappointed with the range of the material covered here. First, it is not intended that the authors especially challenge or add to the parent discipline. Rather the more modest aim is that they illustrate a range of philosophical questions that have grown in the fields of our professional endeavours. Second, the cut between natural and social scientific research makes these areas ripe for enquiry. In this way, the term 'fields' of enquiry takes on a more literal meaning. Typically, exercise, health and sport do not form single disciplinary contexts. They are properly to be understood within a matrix of disciplines from anatomy, biochemistry, biomechanics, philosophy, physiology, psychology, sociology and beyond. The book, I hope, instantiates the need for, and the benefits to be had from, a spirit of tolerance of the multidisciplinarity of contributions to our fields. What I shall do in the remainder is to further sketch out in a superficial way a selected portion of the philosophical terrain that provides little more than background notes to the chapters herein.

Two cheers for positivism and *the* scientific method

'What are the objects of scientific enquiry?' we might ask. Recognizing that exercise, health and sports research have offered fruitful fields of scientific labour, could there be a science of anything or indeed everything? Well, of course, the idea that anything might be scientifically understood is a con testable claim. Not that long ago, however, it would have been clear that what designated a scientific enquiry was the method adopted. It is worth considering some historical aspects of this idea.

It is widely held that, until the seventeenth century, the term philosophy was used to refer to any systematic enquiry of any subject after which certain methods of enquiry, certain ways of arriving at knowledge, come to be privileged. A particular picture of rationality replaces the ancient tests of reasonableness (Toulmin: 2003). In the wake of

the Copernican revolution, which dislodged the earth from the centre of the known universe while replacing it with the sun, came Galileo's use of a mathematical vocabulary to help to describe the physical world. Crucially, we witness the rise of the experiment to support careful observation and develop generalizations, hypotheses and theories for scientific explanations. Whether we are to label Bacon an inductivist[1] or not, there are clearly the seeds here of the patient accumulation of facts that are tested against experience in a controlled manner so as to become more certain of the order of the natural world. It is in the seeds of these loosely collected ideas that the term 'positivism' is typically situated.

It is something of a surprise then, that the term 'positivism' is not a hostage to the history of natural science itself. The term 'positivist philosophy' was first coined by the French sociologist Auguste Comte in the early nineteenth century. In the wake of the success of experimental methods, scholars typically cite the earlier empiricist influence of David Hume in his *An enquiry concerning human understanding* (1739) who rejects the reasoning from 'first' principles. He writes:

> When we run over libraries, persuaded of these principles, what havoc must we make. If we take in our hand any volume; of divine or school metaphysics, for instance, let us ask: *Does it contain any abstract reasoning concerning quantity or number?* No. *Does it contain any experimental reasoning concerning matter of fact and existence?* No. Commit it then to the flames: for it can contain nothing but sophistry and illusion. (Cited in Hacking 1983:44.)

Among the things that Hacking notes from this quote is the positivistic penchant for slogans. That spirit survives today in those who assert blindly that unless problems have some quantificationist or experimental basis, they cannot claim scientific status. That which is not wrought from *the* scientific method must therefore surrender all pretence to science (thereby to proper objectivity). Of course a whole host of unscientific biases are in operation here (see Parry, Chapter 2, on the ideological elements of positivistic thinking). What we can retain here is the positivist's strong sense of antipathy to metaphysics, on which I shall comment below.

Of the term 'positivism' specifically, Halfpenny (1982:15) notes not one but three senses or conceptions of the term in Comte's writing. First, positivism refers to a theory of historical development in which the growth of knowledge contributes to the development of progress and social stability. This conception of positivistic philosophy sounds very much a product of its age, while the second and third conceptions have a more modern ring.[2] Second, positivism refers to a claim that only a certain kind of knowledge counts as scientific and that it must be based upon observation of publicly available entities. Finally, positivism entails the claim that all science proper can be integrated into a unified system.

Even if academics were faithful to Comte's original work, confusion might arise in the applications of a term that slid between the three different senses. Yet modern natural and social scientific research methods talk in exercise, health and sport research is sometimes so loose that the term itself falls into disrepute. Nowhere is this more the case than with the all-pervasive term 'paradigm' (discussed below), which is typically cited without any

precise meaning in mind. Likewise, calling a researcher or research design 'positivistic' often indicates little more than mild and unspecific abuse. When content seems to attach to the ascription, it might mean little more than a predilection for statistics, or a privileging of experimental method, or a dependence on hypothesis testing as a *sine qua non* of a proper researcher.

Comte's positivist philosophy owed a debt to Condorcet's *Essay on the development of the human mind* (Hacking 1983). In this development, which was in sympathy with Hume's empiricism, there were three phrases: (i) the theological stage; (ii) the metaphysical stage in which divinities were replaced by metaphysical entities; and (iii) the final stage of positive science. For Comte, positive science rested on the ability to determine the truth and/or falsity of propositions. Hacking (1983:45) writes 'Propositions cannot have "positivity"—be candidates for truth-or-falsehood—unless there is some style of reasoning which bears on their truth value and can at least in principle determine the truth value'.[3]

Despite the heterogeneity of scientists and commitments that are often grouped (or merely thrown) together, Hacking (1983:42–3) discerns six positivistic ideas which I summarize here:

1 an emphasis upon *verification* (or some variant such as *falsificationism*) to settle truth claims;
2 a commitment to *observation* as the content or foundation of all our non-mathematical knowledge;
3 a *rejection of innate causes* and instead an acceptance of the constancy with which events of one kind are followed by others;
4 a *downplaying of explanations* which should be used to organize phenomena but do not provide deeper answers to the 'why?' questions over and above the noting of their regular occurrence;
5 a restriction of reality to the observable and a *disavowal of theoretical entities;*
6 a summation of ideas 1–5 in the phrase *against metaphysics*.

Hacking concludes thus: 'Untestable propositions, unobservable entities, causes, deep explanation—these, say the positivists, are the stuff of metaphysics and must be put behind us' (1983:42). A further point might be added to this list. Typically, in the first half of the twentieth century the philosophical branch of positivism (logical positivism as it came to be known) held specifically that our interrogation of language allowed us to set up discrete categories such as those between fact and value, and propositions that could be known to be true analytically (by definition of the words as in the closed concepts of mathematics or logic) or synthetically (by experience—for which substitute here: experiment). I shall refer specifically to the problems of the fact-value distinction in the final section below.

Despite the fact, then, that the term 'positivist' has fallen into disrepute, it still shares many ideas that natural and social scientists feel at home with, however much they might baulk at the term. Most scientists are still anti-metaphysical; many consider verification appropriate in certain circumstances. Even those committed to falsificationism, after Popper's radical ideas (see Spurway and Noakes, Chapters 3 and 4, for physiological applications of his ideas) there is still a positivistic element in the idea of a single criterion to demarcate science from non-science and a commitment to the unity of

scientific method. So, if positivism is dead (I shall avoid a temptation to remark, after Twain, that reports to that effect might be a little precipitous), at least some of the spirit of positivism remains in logical empiricism, to which I shall briefly turn.

(Logical) positivism, empiricism and Popper

Those who will not confess (in public anyway) to being positivists, or positivistic, might well own up to being fully paid up empiricists. The two are often slid together casually in research methods discussions. Clarifying their relations may be helpful if only to make sense of some of the important reactions to them in the work of philosophers such as Popper, Lakatos and Kuhn.[4]

Salmon (2001) claims that the fundamental tenet of logical empiricism is that empirical evidence in conjunction with logic as well as mathematics and formal logic underwrites all scientific knowledge. Importantly he notes that the form of reasoning that the logic takes may include either induction or confirmation. He goes on to issue a warning against the too casual use of labelling communities of scientists under specific commitments:

> Contemporary logical empiricists disagree, however, about such basic issues as the nature of empirical evidence, the status and structure of confirmation or inductive inference, the nature of scientific explanation, and the character of scientific theories, to name but a few examples. (Salmon 2001:233.)

In the early part of the twentieth century the logical positivists held very much to the view that meaningful (scientific) propositions had to be verified. Rudolf Carnap, one of its chief architects, stood continuous with the tradition that was committed to a bottom-up picture of science. Careful observation, systematic recording and controlled experimentation gave us data that accumulated to describe, predict and prosecute the regularity of the world.

Despite the reputation Karl Popper now enjoys, he was, during his early academic life, something of an outsider. The intellectual dominance of the 'Vienna Circle', which drove the logical positivist movement, was a group to which he neither belonged nor identified with.[5] Yet, as I have already remarked, he shares many of the commitments of the positivists such as the distinction between observation and theory, the movement toward the one true theory of the universe, the structure of reasoning and the unity of science (Hacking 1983); this is why Hacking still refers to him as a positivist. Among Popper's great contributions to the philosophy of science, however, is his effective reversal of the bottom-up procedure. Instead of making the spirit of confirmation drive scientists, he insists that it is falsification, not verification, that scientists ought properly to aim at in order to better understand the world (see Parry, Spurway and Noakes, Chapters 2–4 respectively[6]). What seems now obvious is a great leap forward in our understanding of science. Inductivism is based upon an inference from a great number of observations of the relations of phenomena (i.e. the sun has come up dutifully every morning—to use the crudest of examples) to a general conclusion—ideally a law-like formulation—that the

sun will always rise in the morning. But, as Hume argued long before, this does not guarantee that the event will happen the next time. No proof is established, *viz.* the truth of the claim that the sun will come up tomorrow *because* it has with unfailing regularity come up in the past. By contrast, one observation to the contrary will falsify the generalization that the sun always comes up in the morning. As Magee puts it: 'The entire conception of science that had prevailed for getting on for three hundred years cannot be right. The rug is pulled out from under what had been the very basis of Western thought for centuries' (1997:50). Perhaps, the nub of Popper's claim here is that scientific laws always go beyond experimental data and experience. Having challenged successfully the traditional method he proposed his own model of scientific method as a form of problem solving where one sought to reject weaker theories for stronger theories but always with the idea that the best knowledge we have is always provisional, never finally provable. Science at its best was an interplay of conjecture and refutation: a dialectic between opposing scientific theories and speculations that worked off the friction each gave the other in the processes of opposition. This idea, coupled with the belief in the unity of scientific method, drove him to demarcate science from what he called pseudo-sciences, such as Freudian psychology or Marxist sociology, both of which claimed to be scientific in the traditional sense. He found in Hume's original here the idea that if one could not in *principle* falsify propositions within a purported science—as was true of both Freudian and Marxist theory—then the claim to scientific status was bogus. The aftermath of this rejection, along with the dominance of an alternative paradigm in psychology, is still felt by Freudian scholars (see McFee for a critique of the often misguided rejection based frequently on misconceptions of his work in the context of sports psychology in Chapter 5).

Whatever revolution Popper sparked, perhaps the most notable aspect of continuity between his thinking and positivists' is the idea of the unity of science. However different verification and falsification are, they are both an attempt to provide a criterion of demarcation between science and non-science and as such they presuppose the idea of the unity of scientific method. In both cases, the positivistic conception of the science predicated on observation, hypothesis and experimental affirmation came under increasing attack.

In one clear way, the humility that attends Popperian science and the faith that we place in its spectacularly successful findings is supported or supplanted (depending on the statisticians involved) by ideas of probability. Given the impossibly heavy burden of truth, scientists of all persuasions typically trade in probabilities. Gower exemplifies this move in Bertrand Russell: 'In the induction chapter of *The Problems of Philosophy,* Russell makes it clear that the aim of inductive arguments is, given the truth of their premises, to make their conclusion *probably* true' (1997:189, emphasis added.).

Russell's principle of induction runs something like this: given that we have a sufficient number of positive observations and no negative ones, then we can be nearly certain that a given law is true. As we have seen, this confidence is later shattered by Popper. But this does not render impotent the uses of probability. And the confidence of our probabilities can be put to good practical use—a point not lost in the public appreciation of science. Put at its most simple level, scientists and everyday folk want to assign a numerical quality to the relations between events, and they express these as ratios. This gives tremendous power to the idea that science can predict—with greater or

lesser confidence—the likelihood of given occurrences. What ratios cannot do—but what many social and natural science undergraduates naïvely believe they do in fact do—is establish anything with absolute certainty: they prove nothing. Indeed it is argued by Reichenbach that an appreciation of the ramifications of this point rent asunder the positivists from the empiricists:

> An analysis of meaning [according to positivists] which any proposition of science contains nothing but a repetition of "report propositions." Since every report consists of statements about the *immediate present,* science states nothing but relations existing between present phenomena. This conclusion, however, is in sharp contrast to the actual practice of science, for scientific propositions make assertions about the *future*. Indeed, there is no scientific law which does not involve a prediction about the occurrence of future events; for it is the very essence of a scientific law to assure us that under given conditions, certain phenomena will occur. (Cited in Salmon 2001:235.)

Examples of precisely how statistical techniques are used in science are many and various and this is not the place to list them. But it is worth noting that one early view in empiricist thought was that they might be used not merely in the experiments themselves, but actually in preferring certain theories above others. Moreover, the force of tradition in statistics is not without its problems. Just as a community of scientists tends to approach problems and agree upon solutions in similar ways, so certain techniques and models come to dominate thinking in statistics (see Cooper and Nevill, Chapter 6, for a particular malaise in exercise and sport sciences) without critical reflection and in biomechanics, which itself cannot be undertaken without the support of statistical modelling (see Yeadon, Chapter 7).

The weight of a whole range of criticisms from the middle of the twentieth century onwards, from philosophers, historians and social scientists alike, culminates in an increasing attack on the scientific method. Bogen captures the reality of the scientific mindset as opposed to the naïve conception of science and scientific progress:

> People once believed a fabulous engine called the Scientific Method harvests empirical evidence through observation and experimentation, discards subjective, error ridden chaff, and delivers objective, veridical residues from which to spin threads of knowledge. Unfortunately, the engine is literally fabulous. Lacking a single method whose proper application always yields epistemically decisive results, real-world scientists make do with messy, quirky techniques and devices for producing and interpreting empirical data which proliferates as investigators improvise fixes for practical and theoretical problems which bedevil their research. (2001:128.)

He goes on to observe that after the demise of positivism:

> Decades would pass before philosophers of science began to appreciate how much the epistemic value of empirical data as evidence for or against a scientific claim depends upon the way it was produced, and the degree to which some features of scientific practice can be illuminated by considering facts about data production instead of logical relations between theoretical claims and descriptions of empirical results. (2001:132.)

Some of those features relate to the effects of technology and laboratory equipment, the salience of patterns of socialization for scientists and other cultural factors that affect observation and the perception of significance (see McFee, Chapter 5, Noakes, Chapter 8, Brackenridge *et al.,* Chapter 9, and Williams and Williams, Chapter 13, for a variety of instances of these problems).

There were of course other key contributions to the philosophical debate. Hansen's notion of the theory-ladenness of observation has long been well taken in the social sciences. Here the impossibility of theory neutrality is acknowledged by all and for a long time (the theory selection is at times bewildering: functionalism, structural-functionalism, Marxism, neo-Marxism, critical theory, figurationalism, the many forms of feminism, and so on are taught from the very beginning as the lenses through which we observe the social world). Yet in natural science, the shared backgrounds of researchers are often so tight that theoretical disagreement arises with much less frequency or is itself acknowledged with much less damaging implications. Equally, Lakatos' critique of Popper's oversimplified account of scientific progress and rejection (see Parry, Chapter 2) gave further reason for philosophers of science to sharpen their teeth on more realistic descriptions of the actual workings of natural scientists. The literature that developed further amplified the climate of scepticism towards the scientific project traditionally conceived. Yet it was Kuhn's historicized account of scientific methods and theory that contributed to what has been called the death of empiricism. Indeed, so strong was the tide of criticism launched by the book, that one author was moved to title an article 'Did Kuhn kill logical empiricism?' (Reisch 1991). I shall therefore consider a key feature of Kuhn's thinking, the paradigmatic nature of science, which is commonly passed over in the non-philosophical literature on research methods and methodologies.

The unbearable slipperiness of paradigms

Kuhn's contribution to our critical understanding of science must be situated in the context of a growing disenchantment with positivistic philosophy of science. How ironic it is then, as many commentators have observed, that Kuhn's famous text *The Structure of Scientific Revolutions* was produced in a series entitled The Encyclopedia of Unified Science'. Unsurprisingly, perhaps, it became the last in the series. Effectively, it ended the myth. Kuhn much later on remarked:

> I aim to deny all meaning to claims that successive scientific beliefs become more and more probable or better approximations to the truth and simultaneously to suggest that the subject of truth claims cannot be a relation between beliefs and a putatively mind-independent or 'external' world (1993:330).

But the reach of Kuhn's work and its complex nature are not charted here. Critical commentaries are legion (e.g. Horwich 1993). My concerns here are limited to his use, and the widespread subsequent use, of his novel idea: paradigms.

One of the problems that has bedevilled methodological discussion in theses and research papers has been the all too casual use of the term 'paradigm'. Indeed so proliferous and so careless is its use, that even though it has seen to become a *sine qua non* in methods discussions, it has, at the same time, been rendered almost meaningless because of a lack of precision in its use. The problem is twofold. In the first instance one wonders just how many of the authors who casually cite "paradigm" (Kuhn 1962) have even read the book. Before guilt is apportioned, expiation is in order. The fault lies partly with Kuhn himself, since in that first edition, as he later confesses:

> By and large I take great satisfaction from the interest it *[The Structure of Scientific Revolutions]* has aroused, including much of the criticism. One aspect of the response, however, does dismay me. Monitoring conversations, particularly among the book's enthusiasts, I have sometimes found it hard to believe that all parties to the discussion have been engaged with the same volume. Part of the reason for its success is, I regretfully conclude, that it can be nearly all things to all people.
>
> For that excessive plasticity, no aspect of the book is so much responsible as its introduction of the term 'paradigm,' a word that figures more often than any other, excepting the grammatical particles, in its pages (1977:293–4.)

And even more starkly: '*Paradigm* was a perfectly good word until I messed it up' (Kuhn 2000:298).

All this is more remarkable when set against the fact that the term does not appear in the index of the original 1962 edition of *Structure of Scientific Revolutions*. He then goes on to observe that were he now to insert the reference it would read 'paradigm' p. 172 *passim*. Masterman (1972:61–5) went so far as to chart 21 different uses of the term. Given the tendency to refer without specification to the concept it is worth listing these senses here:[7]

1 a universally recognized scientific achievement (p. x);
2 a myth (p. 2);
3 a philosophy or constellation of questions (pp. 4–5);
4 a textbook, or classic work (p. 10);
5 a whole tradition, and in some sense, a model (pp. 10–11);
6 a scientific achievement (p. 11);
7 an analogy (p. 14);

8 a successful metaphysical speculation (pp. 17–18);
9 an accepted device in common law (p. 23);
10 a source of tools (p. 37);
11 a standard illustration (p. 43);
12 a device, or type of instrumentation (pp. 59–60);
13 an anomalous pack of cards (pp. 62–3);
14 a machine tool factory (p. 76);
15 a Gestalt figure which can be seen two ways (p. 85);
16 a political institution (p. 92);
17 a standard applied to quasi-metaphysics (p. 102);
18 an organizing principle which can govern perception itself (p. 112);
19 a general epistemological viewpoint (p. 120);
20 a new way of seeing (p. 121);
21 something which defines a broad sweep of reality (p. 128).

When, then, authors cite 'paradigm' and refer to Kuhn, one is left wondering which sense precisely they are adopting. Of course the items on the list are not entirely independent. Masterman classifies them into three broad categories which themselves are neither hermetically sealed nor exhaustive: (i) *metaphysical* or *metaparadigms* (senses 2, 3, 8, 17, 19, 21 and a potentially further sense: map (p. 108)); (ii) *sociological paradigms* (senses 1, 6, 9); and (iii) *artefact paradigms* or *construct paradigms* (senses 4, 9, 10, 12, 13, 15). Partly responding to Masterman, partly to a legion of other critics, Kuhn (1972, 1977) later responded that there were two general senses of paradigm:

> Whatever their number, the usages of "paradigm" in the book divide into two sets which require both different names and separate discussion. Our sense of "paradigm" is global, embracing all the shared commitments of a scientific group; the other isolates a particularly important sort of commitment and is thus a subset of the first. (1977:294.)

A few observations are worth making here. First, working within paradigms in both senses allows scientists to get on with the business-as-normal of everyday scientific activities. As is well known, under this description of settled (if silent) agreement, scientists are operating in 'normal science'. Their activities are building upon received and—at that time, at least—unchallenged wisdom. What is less often observed is that the examples Kuhn persists with, and which inform and are informed by his famous analysis, are characteristic of natural science. In sharing the paradigm, therefore, these scientists have 'assimilated a time-tested and group-licensed way of seeing' (1970:189). This is why, for them, questions regarding scientific method are not pressing. Second, it is far from clear then, how 'normal' science can pertain in the social sciences where the very idea of 'normal' science in his sense does not obtain. It might be argued that during the early periods of sociology, positivistic thought briefly held, but in modern times the situation was never so stable; agreement in theory and method was always elusive. And the prospects in postmodernity are certainly no better. His remarks bear this out directly: 'the practice of astronomy, physics, chemistry, or biology normally fails to evoke the controversies over fundamentals that today often seem endemic among, say, psychologists or sociologists' (1977:viii).