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RETHINKING SPORT AND EXERCISE PSYCHOLOGY RESEARCH

Past, Present and Future

Rethinking Sport and Exercise Psychology Research

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Why Rethink?

Introduction

The title of this book is ambitious; radical, even. It implies some fundamental change is needed in the way we do research in sport and exercise psychology; and further, that we can diagnose the problems of the *past* and *present* and prescribe the solutions for the *future*. It is important, therefore, that we first qualify these bold claims—of past errors and for better ways forward—and add the necessary caveats that all good fallibilists must make. Foremost among these is the caveat that, since we have 'a stake in the game', we aim to describe (Bourdieu, 1975: p. 40), we must first acknowledge our own strategies and assumptions and the lenses through which we will view the strategies of others. Since this is a book partly about how sport and exercise psychologists *do* research, some of our lenses are psychological and sociological (or historical) in nature. Yet where we venture into questions of how research *should* be conducted (and disseminated), theories from psychology and sociology have only limited value. It is in these cases, then, that we draw more explicitly on normative philosophical theory. In making our theoretical stance more explicit, we hope to promote more open and transparent debate,

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making it easier for our inevitable critics to engage with the arguments we intend to make.

An early example of the type of work we aim to undertake in this book is Rainer Martens' paper from the first issue of *The Sport Psychologist* characterising two sport psychologies: academic sport psychology and practicing sport psychology (Martens, 1987). Of these two approaches to research, the orthodox 'academic' approach tended to dominate the field up to the late 1980s, argues Martens, and continues to be highly influential (Keegan, 2015; Vealey, 2006). In this orthodox academic approach, research is conducted in laboratory settings by objective scientists, conducting controlled experiments, while seeking answers to questions that lack practical relevance. Martens criticises this approach to research, pointing out (rightly) that: (a) researchers are not and cannot be objective; (b) that psychological theories are insufficiently developed to allow for controlled lab-based research; and (c) that the findings of such studies lack relevance to practitioners. Martens' alternative 'practicing' sport psychology, based on a 'heuristic' view of science, is more appropriate, he argues, due to a close connection with practitioners and their real-world problems. The heuristic view involves a more realistic and flexible approach to research where prior knowledge and bias is acknowledged (and used), and where a range of alternative methodologies (e.g. more qualitative and idiographic approaches) are applied in problem solving situations.

Martens' paper raised some important questions for researchers in the late 1980s that remain relevant today. What kinds of research are people doing in sport and exercise psychology? What are the implications of taking different approaches? What can we claim (and not claim) for research findings derived from different 'paradigms'? What constitutes progress in sport and exercise psychology? Unfortunately, Martens' analysis was based on a personal and partial evaluation of research conduct of the time and his theoretical understanding of 'the scientific method' was narrow and occasionally misinformed. His ultimate conclusion—that the 'heuristic paradigm' is better and deserves much more attention to enrich the existing body of knowledge—is therefore difficult to accept. It is these two flaws—the lack of evidence of the existence of the two positions and the over-simplified conceptualisation of science—that we aim to address and correct in this book. By conducting more systematic surveys of research conduct in sport and exercise psychology on the one hand, and interpreting this conduct against more explicit psychological, sociological and philosophical theory on the other, we hope to bring up to date and develop the important discussion that Martens initiated back in the late 1980s.

Since Martens' analysis began with the flawed assumption that there is such a thing as an orthodox scientific method—a strange and incoherent hybrid of Baconian and Popperian ideas—it seems sensible and necessary to begin the book with an extended exploration of the different views on this subject, thereby allowing critics to examine our 'philosophical baggage' before it is taken on board, enabling a more constructive debate (Dennett, 1995: p. 21). Although this is a book about research in sport and exercise psychology, we argue throughout that in order to gain the necessary critical distance from the field, it is important to draw on ideas and theories from outside of the discipline. Hence, in this first chapter, we focus primarily on introducing the main sociological and philosophical ideas that underpin much of the analysis are arguments that follow in the more substantive chapters.

Philosophical Baggage

With the exponential growth of science in the last century, professional philosophers developed a parallel concern for explaining this progress. Starting with the so-called Vienna Circle in the 1930s, through the intellectual high-point of the 1960s and 1970s, and concluding with the so-called postmodern turn and the 'science wars' of the 1980s and 1990s, philosophy of science emerged as a fertile field of study in the twentieth century (Fuller, 2006). Of the many well-known names associated with the many and varied debates, we have chosen to focus on just four: Popper, Kuhn, Lakatos and Feyerabend. Aside from being perhaps the best-known philosophers of science of the last century (Agassi, 2014; Stove, 1982), our four protagonists also represent a broad range of contrasting positions, therefore enabling the widest possible debate. We begin this chapter proper with an outline of each position and the main points of agreement and disagreement between them. The order of presentation is chronological not hierarchical (i.e. Feyerabend does not necessarily represent an improvement on Popper), and it should be noted that, while we have tried to present a nuanced and critical narrative in each case, space demands a somewhat caricatured account.

Karl Popper and 'Critical Rationalism'

As the clock struck 7.30 p.m. on October 13, 1958, the audience looked on in anticipation as two men approached the lectern at the annual meeting of the Aristotelian Society in Bedford Square, London. The first man was a solid and self-confident figure; the second man was small and unimpressive with no presence at all. Yet to the surprise of the audience, still drying themselves after the downpour outside, it was the second man who was to give the presidential address. Over the following hour, the speaker proceeded to demolish hundreds of years of philosophising on scientific method, including the ideas of many members of the distinguished audience. According to one eyewitness report,[1] the ideas were too radical to be fully appreciated at the time, and the following debate focussed on particular historical interpretations of certain pre-Socratic philosophers. It would be another year before the president's landmark text, *The Logic of Scientific Discovery*, was published in English, and a further six years before 'Sir' Karl Popper would be considered the world authority on scientific method.

The summer of 1919 was instrumental in Popper's intellectual development (Popper, 1969). The ideas he would unleash on the unsuspecting audience at the Aristotelian Society almost 40 years later were forged in interwar Vienna. Many of Popper's core ideas were developed, characteristically, through a process of criticism, the most well known of which are probably *falsification* (or the *demarcation criterion*) and the *hypothetico-deductive* method. These related ideas first occurred to Popper as a 17-year-old as he noticed important differences in the popular scientific theories of the time. Specifically, he noticed that his socialist friends

[1] This 'creative non-fiction' is derived from Bryan Magee's account of his first face-to-face encounter with Popper (Magee, 1998). Magee's earlier book on Popper (Magee, 1973) is an excellent (and mercifully brief) introductory text.

and psychoanalysts were impressed by the seemingly infinite explanatory power of the theories of Marx and Freud, respectively. Everything could be explained by these theories, yet they ruled nothing out. Critics who raised contradictory evidence were summarily dismissed: the critics of Marx were under the spell of 'false consciousness'; those who denied Freud were suffering from un-analysed 'repressions' (Popper, 1969: pp. 46–47). In stark contrast, Einstein's theory, which had been tested that year by Eddington's observations, was:

> ...utterly different from the dogmatic attitude of Marx, Freud and Adler… Einstein was looking for crucial experiments whose agreement with his predictions would by no means establish his theory; while disagreement, as he would be the first to stress, would show his theory to be untenable. This, I felt, was the true scientific attitude… Thus I arrived… at the conclusion that the scientific attitude was the critical attitude, which did not look for verifications, but for crucial tests; tests which could refute the theory tested, though they could never establish it. (Popper, 1978: p. 38)

Popper began to see that these different types of theories were associated with very different methods. The theories of Freud and Marx were considered scientific because they had been arrived at through systematic and 'objective' observations; that is, they had an empirical basis. Einstein, by contrast, had proposed a bold and exciting conjecture and defined the conditions under which it should be tested. Later, in his *Logik der Forschung* (1934), Popper developed formal logical arguments against the theory of induction—the dominant explanation of scientific method since Bacon's *Novum Organum* inspired the Royal Society—and of the logical positivism of the Vienna Circle: the theories of science he felt had granted undeserved credibility to Freud and Marx.

With respect to induction, Popper argued that valid knowledge could not be the product of repeated observations for two main reasons: (1) all observation is preceded by theory; we cannot observe without a point of view—we are not 'white paper' as Locke supposed—so induction is mistaken; (2) since there might always be a falsifying instance, or 'black swan', around the corner, we have no reason (logically) to expect the future to follow the past. Induction, Popper argued, is neither logically or psychologically necessary as an explanation of the growth of knowledge. It is entirely dispensable; an optical illusion (Magee, 1973: p. 31). The only reasonable way to proceed, then, is to create bold and imaginative theories to solve problems and submit them to criticism by searching for falsifying instances. Theories that are formulated in such a way as to be easily testable, or *falsifiable*, are scientific (as was the case with Einstein); theories that avoid criticism, like those of Marx and Freud, are *pseudoscientific* and dogmatic (Popper, 1969). In this way, Popper developed his famous *demarcation criterion* between science and pseudoscience.

Popper's central argument here was to point out the logical asymmetry between verification and falsification: no amount of evidence can prove you right; yet any amount of evidence can prove you wrong. Much academic labour can be (and has been) wasted, warned Popper, searching for verifications of, or supporting evidence for, a theory. Building up huge piles of evidence in support of pet theories is, in Popper's view, anti-scientific; and the theories developed in this way *pseudoscientific*. So, if scientific theories are testable, in spite of being unprovable (Magee, 1973), we are left with knowledge that is *fallible*, but which can be improved (or made more 'truth-like') through rigorous theory testing and the elimination or errors. Popper was therefore both a realist and a fallibilist.

Truth, for Popper, was an important regulative concept. Progress in science 'involves increase in truth content' (Popper, 1974: p. 1102) which means that theories have to explain known facts (i.e. they have to be as good as rival theories in this respect) *and* predict new facts. Theories with greater empirical content have greater testability since they specify the conditions under which they would fail. Having been subjected to and survived a series of tests, a theory has a greater 'degree of corroboration' which is 'synonymous with the degree of severity of the tests it has passed' (Popper, 1959: p. 392). Scientists should therefore 'hold on, for the time being, to the most improbable of the surviving theories, or more precisely, to the one that can be most severely tested (i.e. that has greatest explanatory power, content, simplicity and is least *ad hoc*)' (Popper, 1959: p. 419). Saving a theory from criticism by inventing ad hoc hypotheses is a cardinal sin for the Popperian scientist, whose attitude is described succinctly by Magee (1973: p. 23):

> Popper proposes, as an article of method [rather than logic], that we do not systematically evade refutation, whether by introducing *ad hoc* hypotheses, or *ad hoc* definitions, or by always refusing to accept the reliability of inconvenient experimental results, or by any other such device; and that we formulate our theories as unambiguously as we can, so as to expose them as clearly as possible to refutation.

Here we see, for the first time, another important element of Popperian thought: the distinction between logic and method. In much of his work, Popper used logical analyses as the basis for methodological *prescriptions*. He was prepared to accept, if only reluctantly, that scientists may act in illogical and irrational ways (e.g. by saving theories from criticism with ad hoc hypotheses), but remained optimistic in developing his normative theories for scientific method. Popper's so-called *hypothetico-deductive* method, therefore, is a prescription for developing theories to solve problems; deducing solutions (or making predictions); and then testing the predictions against experience. In later work, Popper came to express this method in a brief four-stage schema, represented below (Magee, 1973: p. 65):

$$\mathrm{P}^1 \rightarrow TS \rightarrow EE \rightarrow \mathrm{P}^2$$

P¹ stands for the initial problem, or *problem situation*, since all problems have a history, including previous unsuccessful attempted solutions. This problem must be formulated as clearly as possible by the researcher to enable others to understand, criticise and help solve the problem. **TS** is the *tentative solution* offered by the researcher, which is often the product of intuition or creative insight. Again, as we have seen, tentative theories must be formulated clearly; they must explain known facts and also predict new facts. As one of Popper's famous students put it: 'a theory must be made to stick its neck out' (Lakatos, 1970: p. 111). There then follows the all important **EE**, or *error elimination*, stage (sometimes written as CD for critical discussion). Here, the task is to design and execute the most severe test of the theory imaginable. The harsher the test, the greater the degree of corroboration of a theory. Again, this is a side of scientific activity which demands creativity, a quality that some Popperians have tried hard to promote (e.g. Medawar, 1969). By eliminating errors in theories, or discarding them altogether, we move towards more truth-like (or better corroborated) accounts of the phenomena under study. A by-product of this critical process is *new problems* (**P²**), fundamentally different to the initial problem, which would have been temporarily solved or changed in light of the investigation.

To illustrate by example, consider how a Popperian researcher might engage with the popular psychological phenomenon of 'flow'. Csikszentmihalyi (2002) defines nine dimensions of flow, five of which describe broad characteristics of the experience (sense of control, action-awareness merging, loss of self-consciousness, time transformation, autotelic experience), and four of which suggest conditions (challenge-skills balance, clear goals, unambiguous feedback, concentration). It is also argued that flow precedes optimal experience and, by association, optimal performance in sport (Jackson & Roberts, 1992). The Popperian may begin, therefore, by determining a problem to which flow presents a tentative solution (e.g. how can an athlete get into the optimal psychological state to perform at their best?). They would then proceed to articulate the theory in its simplest and strongest possible form and determine the conditions under which the theory would fail (i.e. they would need to specify what kinds of severe tests they could conduct).

This second step is problematic, as recent research has suggested that flow is undertheorised (Swann, Keegan, Piggott, Crust, & Smith, 2012). Specifically, the particular combination or sequence of conditions that cause flow are poorly understood. Moreover, there is not even agreement about how many of the 'dimensions' need to be present before a flow state can be classified (Cf. Jackson, 1996). In short, flow has a low level of corroboration because: (a) it has not been formulated in a testable form (though see Swann, Crust, Keegan, Piggott, & Hemmings, 2015) and (b) has therefore not been subjected to any serious criticism. Very few papers challenge Csikszentmihalyi's nine dimensions with much of the contemporary research employing psychometric instruments that continue to verify the theory (Jackson, Martin, & Eklund, 2008). One may argue that, from a Popperian perspective, flow researchers have been 'playing tennis with the net down' (Khalil, 1987: p. 123), and given that flow was first theorised 40 years ago, with applications to sport since 1992, this reflects very poor progress.

Popper was a perfectionist polymath genius and workaholic. He would frequently work through the night, all days of the week, for most of the year, only taking brief walking holidays in the Alps for recovery. He therefore made important advances in a range of fields, including logic, probability theory, epistemology, political science and metaphysics. We have only touched on a narrow segment of his work here—the philosophy of science that he came to call *critical rationalism*—yet the full force of Popper's thought is hard to appreciate without an understanding of the relationships between his ideas across these fields (Magee, 1973: p. 17; Fuller, 2006: p. 26). For the scientists, politicians and historians who have invested in this endeavour, Popper's 'philosophy of action' has had a 'highly practical effect' (Magee, 1973: p. 10). Nobel Prize winners in biology (Sir Peter Medawar, Jacques Monod), physiology (Sir John Eccles) and physics (Sir Hermann Bondi) and well-known economists (e.g. Taleb, 2007) have all expressed an explicit debt to Popper's very practical influence on their approach to science. However, Popper has had his fair share of criticism, too. The best known of his critics was the American historian, Thomas Kuhn, who opened up a critical debate where Popper's former students, much to his chagrin, would come to play a central role.

Thomas Kuhn and 'Normal Science'

At the age of 27, with a freshly minted PhD and a Junior Fellowship at Harvard, Thomas Kuhn strode confidently into the first of the 1950 William James Lectures, expecting a show. He was not disappointed. Having already decided to dedicate himself to the study of science, Kuhn was enraptured by the lecturer—one Karl Popper—and his narrative of bold and inventive scientists, liberally criticising one another's theories through crucial experiments. This story was very different from the prevailing positivist historical account, which characterised science as a plodding, objective and cumulative enterprise. Yet despite his attraction to Popper's revolutionary philosophy, Kuhn left the lecture theatre with a niggling sense of doubt about its accuracy as a historical account.[2] Fifteen years later, Kuhn and Popper would meet again, but on that occasion Kuhn, as author of the wildly successful text, *The Structure of Scientific Revolutions*, would have star billing.

The meeting in question took place on July 13, 1965, at the International Colloquium in the Philosophy of Science at Bedford College, London. Although Popper chaired the session that afternoon, Kuhn's work was very much the focus of attention in the subsequent publication (Lakatos & Musgrave, 1970). In their exchange, Kuhn initially took a highly deferential stance, possibly due to Popper's recent knighthood, pointing out all the points where he and 'Sir Karl' agreed. Like Popper, Kuhn argued that we approach everything in light of a preconceived theory and that science moves forward in leaps when scientists engage in criticism of theories (Rowbottom, 2011). Kuhn, however, fundamentally disagreed about the frequency with which such criticism might occur. Where Popper imagined criticism to be *the* crucial characteristic of science, Kuhn—through his historical studies of post-enlightenment astronomy, physics and chemistry—felt that this attitude occurred only very rarely, under special social conditions (Kuhn, 1970). In short, Kuhn felt that Popper had overemphasised the 'revolutionary' side of science, ignoring almost completely the actual practice of scientists, or what he called 'normal science'. Since Kuhn's book has become so popular, with over 80,000 academic citations to date, and thus subject to mass misinterpretation and widespread misunderstanding (not least by Martens, 1987), it is worth tracing his ideas closely, as they appear in *The Structure of Scientific Revolutions*, before engaging in a critical review.

In outline, Kuhn's (1962/1996) narrative has three main parts: (a) he begins with a description of the characteristics of 'paradigms', or social formations wherein scientists engage in 'normal science'; (b) he goes on to explain how 'scientific revolutions' occur, following the build-up of 'anomalies' which eventually lead to a 'crisis' in the community;

[2] This sketch is constructed from numerous sources, including Kuhn (1974: p. 817), Preston (2008: pp. 5–4) and a brief primary account from *The Harvard Crimson* (anonymous author, Feb 17 1950).

(c) finally, Kuhn describes 'conversion processes' and the ways in which young scientists are socialised into the new paradigm, before considering, in conclusion, how progress in science can be understood. We look at each of these three stages in more detail below.

In developing his concept of a paradigm, Kuhn drew explicitly on the work of Polanyi (tacit knowledge) and Wittgenstein (language games) (Kuhn, 1962/1996: pp. 44–45). Paradigms contain theories, exemplars and methods that are accepted without question by the community, binding members together through a common set of assumptions. He argues that scientific communities cohere around a paradigm, which, though often not understood explicitly, enables scientists to determine significant facts, suggest problems or puzzles to solve and offers exemplars for solving them (ibid: pp. 25–34). Paradigms are formed through a process of debate, where groups of scientists come to adopt similar views, eventually forming distinct 'schools' of thought. One school will eventually come to be perceived to be more successful—that is, its paradigm will be most effective in suggesting and solving new puzzles—at which point *all* scientists in the field will 'convert' to the dominant paradigm and engage in 'normal science' (see Fig. 1.1). To use Fuller's (2006: p. 37) colourful terms, the 'paradigm succeeds in monopolising the means of intellectual reproduction' much in the same way as did the Ministry of Truth in George Orwell's *Nineteen Eighty-Four*.

The paradigm, then, provides both the 'rules of the game' and the nature of the intended outcome in science (Rowbottom, 2011: p. 122). Kuhn was clear that, unlike Popper's notion of theory testing, activity in a paradigm was self-referential and inherently dogmatic. No scientist challenges the theory (or theories) at its centre, partly because of the way they have been trained, and partly because the paradigm has a real perceptual effect of closing down the scientist's awareness of alternative ways of looking at things. As Kuhn points out: 'work in the paradigm can be conducted in no other way, and to desert the paradigm is to cease practicing the science it defines' (Kuhn, 1962/1996: p. 34). This means that normal scientists develop a 'monomaniacal concern with a single point of view' (Feyerabend, 1970: p. 201), becoming 'intolerant of theories invented by others', with a literal 'inability to see phenomena that do not fit the paradigm' (Kuhn, 1962/1996: p. 24). Kuhn therefore characterised normal

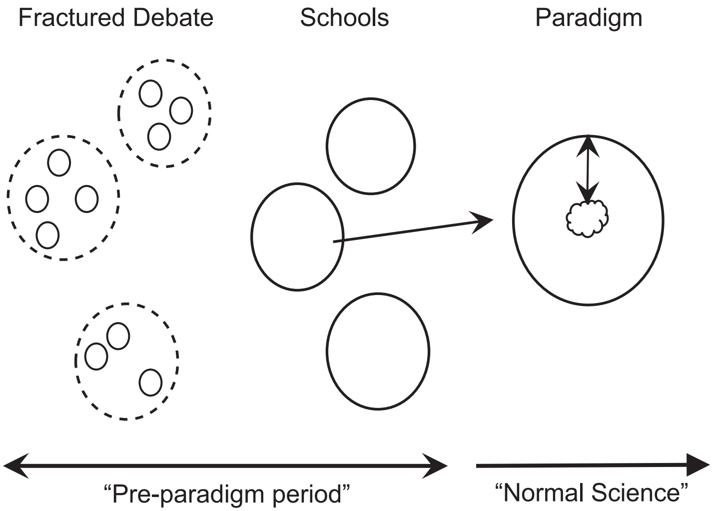

Fig. 1.1 The process of paradigm formation (Adapted from Kuhn, 1962/1996: p. 48)

science as a 'mopping-up operation' or puzzle-solving activity where the aim is to 'solve problems with known solutions (supplied by the paradigm) that test the skill of the scientist' (ibid: pp. 36–37).

Kuhn marshalled his historical examples—mainly from chemistry, astronomy and physics—to show that the paradigm (and associated professionalisation) has a twofold effect on science: first, it leads to 'immense restriction of the scientist's vision and to a considerable resistance to paradigm change' and, second, to an increase in the detail and precision of observation and information (e.g. the development of precision instruments and apparatus) (ibid: p. 65). For Kuhn, the first effect explains why periods of normal science may last for generations, while the second effect explains the great pace of 'progress' in science:

> So long as the tools a paradigm supplies continue to prove capable of solving the problems it defines, science moves fastest and penetrates most deeply through confident employment of these tools… retooling is an extravagance to be reserved for the occasion that demands it. (ibid: p. 76)

Kuhn drew on Bruner and Postman's psychological experiments on perception incongruity (ibid: pp. 62–64) to explain how these rare 'retooling' occasions, or revolutions, might occur. In short, he argued that the awareness of anomalies in the paradigm opens up a sort of window in which conceptual categories become adjusted. Since this is one of the most controversial parts of Kuhn's explanation (Cf. Fuller, 2006; Rowbottom, 2011), it is worth a closer look.

Under normal science, any scientist who discovers evidence that runs counter to the paradigm (i.e. an anomaly), which is at any rate highly unlikely, would assume a mistake on their part, rather than declare the paradigm 'refuted'. Like the poor carpenter who blames his tools, it is a poor normal scientist who blames his paradigm (Kuhn, 1962/1996: p. 80). Yet some anomalies are deemed worthy of scrutiny, and are identified by the scientist who 'can apply the precision instruments to locate anticipated phenomena, *but also* recognise that something has gone wrong' (ibid: p. 65). Kuhn offered some characteristic examples but admitted that he had 'no fully general answer' to the question of how such crises arise (ibid: p. 82). He conjectured that some anomalies that begin as puzzles eventually turn into Popperian counter instances and are likely to be noticed by younger scientists whose indoctrination is not yet total.

As anomalies accumulate and crisis sets in, so begins a brief but stormy period of extraordinary science. Here Kuhn draws on an extended analogy with political revolutions to explain the process of paradigm change. Revolutions are initiated by a growing sense that the existing institutions cannot solve important problems. And because there are no supra-institutional (or supra-paradigmatic) authorities to adjudicate between polarised 'camps', there is a 'resort to techniques of mass persuasion' (ibid: p. 93). Debates about the choice between paradigms are therefore necessarily circular since each group uses the paradigm to argue in the paradigm's defence (ibid: p. 94). In short, a paradigm shift is irrational.

Revolutions, for Kuhn, involve a Gestalt switch on the part of scientists who adopt new instruments and look in new places: 'what were ducks in the scientist's world before the revolution are rabbits afterwards' (ibid: p. 111). The conversion process to a new period of normal science, however, is a long, piecemeal process (see Fig. 1.1). Full conversion may occur over a generation, with conversions happening 'a few at a time until, after the last holdouts have died, the whole profession is practicing under a single, but now different, paradigm' (ibid: p. 152). Again, Kuhn's response to deeper and more detailed sociological question of 'how conversion is induced and resisted' is evasive: 'our question is a new one', he claims, 'so we shall have to settle for a very partial and impressionistic survey' (ibid: p. 152). One part of this process on which Kuhn had much to say, however, was the training of the next generation, once a new period of normal science has been established.

The process of socialisation into science was, for Kuhn, highly authoritarian. He believed that 'science students accept theory on the authority of the teacher and the text, not because of the evidence' (ibid: p. 80). Popper was disgusted, of course, countering that the normal scientist 'has been badly taught… a victim of indoctrination' (Popper, 1970: p. 53). Yet under Kuhn's system textbooks were a source of great importance and interest, representing critical 'pedagogical vehicles for the perpetuation of normal science' (Kuhn, 1962/1996: p. 136). Textbooks, thought Kuhn, are written in very deliberate ways to depict a version of history and an interpretation of the facts that fit the dominant paradigm. Indeed, the highly selective examples that 'entangle theory with exemplary explanation' often found in textbooks 'suggests applications *are* the evidence of the theory' (ibid: p. 80). Textbooks also play an important 'bonding' role in scientific communities through: (a) communicating the vocabulary and syntax of the scientific language; (b) actively obfuscating the ways in which normal science was established; and (c) through selection and distortion, depicting science as a cumulative (as opposed to a punctuated) activity (ibid: pp. 136–141).

Having explained the nature of a paradigm, the normal science that it entails, and the processes by which paradigms change and new generations are indoctrinated, Kuhn concludes by turning to the thorny issue of *progress*. As we have seen, Popper was a realist who used 'truth' as a regulative concept: a yardstick against which to measure progress. Kuhn, by contrast, was very much a relativist (although he argued otherwise). Since there can be no supra-paradigmatic standards for judging between theories, 'progress lies simply in the eyes of the beholder' (ibid: p. 163). Kuhn equated progress, rather, as a function of the *rate* of puzzle-solving.

Because of the absence of competing schools and the associated absence of peer scrutiny—since peers in a paradigm agree on fundamentals—scientists can 'get on with puzzle-solving largely uninhibited' (ibid: p. 163). Again, in complete contrast to Popper, progress is greatest, for Kuhn, when science is at its most dogmatic. The vision of the scientist as professional technician now appears, as Kuhn claim that 'unlike engineers and doctors… the scientist need not choose problems because they urgently need solution' (ibid: p. 164). For Kuhn, this fact also explains the different rates of progress in the natural and social sciences: where the latter choose difficult problems of social importance, the former, through their insulation from wider society, can simply busy themselves with puzzle-solving. Which group, asks Kuhn rhetorically, 'would one expect to solve problems at a more rapid rate?' (ibid: p. 164).

So what of Kuhn's influence in sport and exercise psychology? We take an example from one of the most popular fields of research at present: motivation and self-determination (Chap. 4 includes a more detailed investigation). In the preface to their *Handbook of Self-Determination Research*, Deci and Ryan (2002) describe the origins of their text, reporting on a 1999 conference, where:

> people came with a shared vocabulary, a shared set of concepts, a shared system of thought and a shared familiarity with the extensive research literature. This allowed everyone to begin immediately discussing important and penetrating issues. (p. x)

There follows a series of papers where Self-Determination Theory (SDT) is applied and extended in a host of disparate fields. Then, in the final chapter, where 'future directions' are suggested, Kuhn's influence is once again evident, with exhortations to 'test, extend and refine the tenets of SDT… apply the concepts to new domains… [and] integrate research findings from a multitude of studies' all under the metatheory which gives the concepts their 'true meaning' (Deci & Ryan, 2002: p. 432). In their final rallying call, Deci and Ryan (2002: p. 433) remind their reader that 'several major theoretical problems remain to be solved, new areas of application await careful consideration, and countless refinements would make the theory more exhaustive and precise'. In our own studies of the application of SDT in sport and exercise psychology (see Chap. 4), we have noted how studies over the last 5 years have drawn heavily on correlation methodology. There is a tendency to treat SDT as a paradigm—not a theory to be tested against experience—and when anomalies appear, they are explained away with reference to methodological errors or ad hoc hypotheses. Normal science, it seems, is alive a well in some corners of the field.

Returning to the start of the section, the substance of the debate that occurred at Bedford College in 1965 should now be clear. While academic etiquette required that Popper and Kuhn concede ground to one another, their basic visions of science and scientists could hardly have been more different. While reluctantly admitting that normal science exists, Popper (1970) saw it as 'a great danger to science and, indeed, to our civilisation' (p. 53). Moreover, he regarded the turn to psychology and sociology for enlightenment concerning the aims and possible progress of science as 'surprising and disappointing' (p. 57). Where logic has little to learn from psychology, argued Popper, 'the latter has much to learn from the former' (p. 58). Kuhn protested, of course, claiming that, as a historian, he had 'examined closely the facts of scientific life' which had consistently showed that much scientific behaviour had 'persistently violated accepted methodological [i.e. Popperian] canons' (Kuhn, 1970: p. 236). Where Popper saw ducks, Kuhn saw rabbits. Yet it should come as no real surprise that Kuhn the historian saw the world of science quite differently to Popper the logician. Indeed, their arguments were of fundamentally different kinds: Popper was making a *prescriptive* case for science, for the attitude and methodology scientists *ought* to adopt (i.e. that were logically sound); Kuhn on the other hand was concerned with *describing* science, attempting to lay bare the socio-psychological forces and factors that shaped behaviour. In many respects, they were arguing past one another.

Aside from some of the issues already mentioned—for example, that Kuhn 'leaves so vague' the conditions under which revolutions occur (Rowbottom, 2011: p. 119)—Kuhn's consistent ambiguity on the *description* versus *prescription* issue became a main source of criticism against him. Feyerabend (1970) accused Kuhn of deliberately avoiding the issue in trying to appeal to both camps: philosophers and historians. In his critical comparison, Fuller (2006) also notes that Popper's 'normative horizons were always more expansive than Kuhn's' (p. 26), and further that Popper and his followers seized upon a glaring weakness in Kuhn's theory: its lack of constitutional safeguards (p. 46). Scientists should always be *trying* to falsify their theories, just as people in democracies should always be *invited* to find faults with governments and consider alternatives (Fuller, 2006: p. 46). By contrast, Kuhn's authoritarian and irrational vision of science, governed by elite peers, where normal scientists lurch from one crisis to the next in 'contagious panics', is hardly appealing (Fuller, 2006; Lakatos, 1970). For his critics, then, Kuhn's normal scientist, disconnected from society, cuts a pitiful figure; with his explanation of revolutions fundamentally incomplete.

Despite these criticisms, there is no denying that it is Kuhn's vision that has become the dominant 'paradigm' of the day (Fuller, 2006). This is particularly so in the social sciences, where his descriptive account (not least misunderstandings of his concept of 'incommensurability') has been mistaken for an excuse to *avoid* criticism (Cf. Denzin and Lincoln, 2005). Or, as Feyerabend put it: 'by accepting Kuhn's account as a clear new *fact*… they [social scientists] started a new and most deplorable trend of loquacious illiteracy' (Feyerabend, 1978: p. 66). Whatever the consequences of Kuhn's success—an explanation for which is beyond the scope of this text—one of his early and unlikely champions was Popper's student and direct successor at the London School of Economics, Imre Lakatos. While some have intimated devious intentions on Lakatos' part (cf. Agassi, 2008), there is no doubt that, in bringing Popper and Kuhn together, Lakatos intended to create intellectual space for his own 'middle way' philosophy of science (Motterlini, 1999).

Imre Lakatos and Scientific 'Research Programmes'

Late in the evening on Friday, July 16, 1965, Imre Lakatos sat at his desk reflecting on a long and momentous day. Earlier that week, after years of effort, he had finally managed to engineer the meeting of his mentor, Karl Popper and the famous American historian, Thomas Kuhn.

A contented smile crept across his face as he reached for a paper and pen and dashed off a boastful note to his friend, Paul Feyerabend, who had been kept at home due to one of his regular bouts of illness. Lakatos had heard about the meeting second hand, so didn't comment on details, but he noted to his friend how the time was now right for his to own middle way philosophy of science to take centre stage. It took Lakatos a further 5 years to publish the papers from the 'International Colloquium in the Philosophy of Science' which, when it appeared under the title *Criticism and the Growth of Knowledge*, granted a measly 6 pages to Popper, 74 pages to Kuhn and 105 pages (one third of the text) to himself!

As a Popperian, Lakatos stood firmly against the irrational 'mob psychology' science he saw in Kuhn: 'submission to the collective will and wisdom of the community', thought Lakatos, was a poor recipe for normal science (Lakatos, 1970: p. 178). Moreover, since Kuhn identified no rational causes or standards in revolutions, he leaves us with only weak psychological or social psychological explanations which are useless as methodological prescriptions (ibid: p. 179). However, Lakatos also regarded Popper's logical standards as sociologically naive and historically untenable. His subsequent innovations on (or clarifications of) Popper were small but important; and his alternative historiographical methodology was arguably more sophisticated than Kuhn's (Feyerabend, in Motterlini, 1999: p. 16). His designs on winning the debate he staged in 1965, then, were at least partly successful. We will consider his Popperian methodological modifications and his historical methodology in turn.

In his co-edited volume of the 1965 conference (Lakatos & Musgrave, 1970), Lakatos begins his 105-page essay by arguing that Kuhn attacked a form of 'naive falsification'—a Popperian straw man—and further, that by strengthening the Popperian position, one can present the history of science 'as constituting rational progress rather than as religious conversions' (ibid: p. 93). Lakatos' distinction between 'naive' and 'sophisticated' falsification—a version he attributes jointly to himself and Popper (ibid: p. 181)—is summarised in Table 1.1.

Lakatos strengthened Popper's notion of falsification by making two important qualifications. First, we do not appraise single theories in isolation, but rather a series of theories: it is not possible to falsify a theory without the presence of a better alternative. A sport psychologist trying

Table 1.1 Naive versus sophisticated falsificationism

	Demarcation	Falsification
Naive	Any theory which is experimentally falsifiable is scientific	A theory is falsified by an observation statement that conflicts with it
Sophisticated	A theory is scientific only if it has corroborated excess empirical content over its predecessor (or rival)	A theory is only falsified if another theory has been proposed which: 1. has excess empirical content (predicts novel facts) 2. explains the previous success of its rivals 3. has some excess content that is corroborated (has passed tests)

Adapted from Lakatos (1970: pp. 117–122)

to understand the effects of arousal on the performance of a team of athletes, for example, does not *test* 'inverted-U theory' (Landers & Arent, 2010) without also having 'catastrophe theory' (Hardy, 1996) and 'reversal theory' (Kerr, 1997) in mind as possible alternatives. Hence, only a series of theories—or what Lakatos called a 'research programme'—can be considered scientific (ibid: pp. 118–120). Second, there can be no such thing as a crucial experiment; at least not if they are meant to be experiments that can instantly overthrow a research programme. (ibid: p. 173). Following Kuhn, Lakatos argued that the *defence* of a research programme (leading to greater stability) was just as important as its *attack*. In Lakatos' conception:

> criticism does not – and must not – kill as fast as Popper imagined. *Purely negative criticism… does not eliminate a programme. Criticism of a programme is a long and often frustrating process and one must treat the budding programme leniently*… It is only constructive criticism which, with the help of rival research programmes, can achieve real successes. (original emphasis) (ibid: p. 179)

What emerges, then, are a series of new methodological prescriptions, essentially based on Popper, but qualified by Kuhn-like socio-psychological insights. Or, as Lakatos put it in his final lectures: 'from a logical point of view, it is quite possible to play the game of science according to Popper's rules… the only problem is that it has never happened in this way' (Lakatos, in Motterlini, 1999: p. 98).

Lakatos' resulting 'sophisticated' methodological prescriptions can be summarised as follows (this list is a composite drawn from Lakatos, 1970; Motterlini, 1999 and Zahar, 1982):

1. Treat budding research programmes leniently (i.e. persist even in the face of criticism)
2. Nevertheless, try to look at things from different points of view
3. Put forward theories which anticipate novel facts (make theories 'stick their necks out')
4. Compare programmes on the basis of:

   (a) Their heuristic power (the extent to which they suggest fruitful new solutions);
   (b) Their degree of corroboration (assessment of the severity of tests theories have passed).

Research programmes, in general, are characterised by a *hard core* of accepted theories (rather like a paradigm), surrounded by a *protective belt* of auxiliary hypotheses. For example, social facilitation theory, as formulated by Zajonc (1965), contains a basic law concerning the relationship between performance and the presence of others: well-learned skills remain robust under observation-induced stress, whereas poorly learned skills break down. This general 'drive' or 'activation' theory could be said to be the hard core of the programme, and the multiple hypotheses that have been added more recently (e.g. evaluation apprehension; alertness and monitoring hypotheses; challenge-threat hypothesis; distraction-conflict hypothesis; self-presentation hypothesis), the protective belt (cf. Strauss, 2002). What remains, then, under the Lakatosian scheme, is to evaluate the extent to which such programmes are *progressive* or *degenerating*.

Progressive programmes are those in which scientists act in accordance with principles of sophisticated falsification, or where theories have excess empirical content over rivals which has also been corroborated (Blaug, 1991: p. 172). Put simply, they predict new facts and survive harsher tests. We may ask, for example, which of the three arousal theories already mentioned makes the most novel predictions, and which on balance has stood up to criticism? Such questions would be important for those conducting literature reviews prior to undertaking new empirical research in this field. Rather than simply selecting the most fashionable 'paradigm' (*a la* Kuhn), Lakatos would argue that researchers should identify the most progressive programme according to these criteria then work on corroboration.

Degenerating programmes, by contrast, make no novel predictions; they simply explain what is already known and 'save' theories from criticism by adding increasingly ad hoc auxiliary hypotheses (Motterlini, 1999: p. 2; Khalil, 1987: p. 124). Perceptive readers will notice a more than passing resemblance between degenerating programmes and Kuhn's normal science. But where Kuhn saw normal and revolutionary *periods*, occurring in a sequence, Lakatos saw progressive and degenerative programmes existing in a state of simultaneous and perpetual *interaction*, whose fluctuations were worthy of historical study (Feyerabend, 1970: p. 212; Zahar, 1982: p. 407).

Lakatos' new demarcation criterion, therefore, aimed to distinguish not between science and pseudoscience, but between *good* science and *bad* science (Motterlini, 1999: p. 3). To this end, Lakatos went so far as to recommended that scientists should identify and work on progressive research programmes and, moreover, that economic and intellectual resources be distributed in the same direction (Motterlini, 1999: p. 7). Some critics characterised such rationalist recommendations as pure propaganda, however, arguing that degenerating programmes are sometimes 'revived' by scientists and become progressive (Feyerabend, 1970). In such cases, deserting the degenerating programme may, in fact, be damaging to progress. Lakatos conceded that there was no purely rational case for following progressive programmes, but argued that a historiographical programme of research using 'progressive' and 'degenerating' as *ideal types* may at least help us identify how (and how often) such cases occur. Such a methodology of scientific research programmes (MSRP) would involve the 'rational reconstruction'[3] of individual cases in order to understand the 'reasons and strategies which have produced new ideas' (Motterlini, 1999: p. 16).

At the time of his death, nobody had applied MSRP to a historical appraisal of the social sciences (Lakatos, in Motterlini, 1999: p. 106), though the task has since been undertaken with some vigour in economics (Cf. Blaug, 1991; Hands, 1985; Khalil, 1987). Such examples have demonstrated the potential value of a Lakatosian historiography to other social sciences: identifying the hard core of programmes; describing their long-term growth; identifying ad hoc developments and patching-up procedures; and also debating where genuinely progressive shifts occur. We would argue that MSRP may be fruitfully applied in sport and exercise psychology, in systematic reviews and in justifying the selection of particular theories in, say, theses and dissertations. Taking the earlier example, based on Strauss' (2002) review, the hard core of social facilitation theory (SFT) was established in the 1960s following many years of 'anarchy'. It is worth asking, therefore, to what extent the auxiliary hypotheses added over the last 50 years represent progressive or degenerative shifts? Has SFT merely been 'patched-up', or have some of these hypotheses constituted bold new predictions that have been corroborated through experimental results? Without the conceptual toolbox of Lakatos, Strauss (2002) implies a series of degenerating shifts over the last 40 years in SFT yet is unable to reformulate the theory in a progressive way. The application of MSRP might therefore offer a valuable analytical tool for studying psychological research programmes in future.

To summarise, Lakatos felt that he had improved on Popper's methodological prescriptions, maintaining the rationalism of 'progressive' science, while salvaging from Kuhn the idea that some degree of tenacity is necessary in defending a programme against criticism, instilling some stability. In the final analysis, he was unable to offer clear reasons or criteria for moving from degenerating to progressive programmes (or vice-versa), or for when (and how vociferously) scientists should adopt a defensive or

[3] By 'rational reconstruction', Lakatos has in mind an explicitly theory-informed historical study, using his concepts as a particular lens through which to view historical cases. Feyerabend, though critical of Lakatos' prescriptions for science, considered this theory 'vastly superior to Kuhn's', and one that would 'definitely lead to more detailed research' (Feyerabend, in Motterlini, 1999: p. 16).