Journal of the Philosophy of Sport, 2009, 36, 111-126

Beyond Consent? Paternalism and Pediatric Doping

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In this essay, I argue that the issue of pediatric/adolescent doping is one that merits serious philosophical attention. I consider whether an adolescent who is legally competent to consent to medical pharmacologies such as contraceptive pills ought to be allowed to consent to doping products. The former case, well known in medical ethics, is often referred to as "Gillick competence" following the court case brought by the mother of an adolescent (Mrs. Gillick) whose consent was first considered satisfactory by her medical doctor and then the Law Lords of Great Britain. I first discuss issues of vulnerability and exploitation of adolescent athletes that might underwrite a soft paternalistic response. I go on to argue that the harms attendant to doping, as opposed to the regulated use of the medical profession to prescribe oral contraceptives, are of a potentially greater magnitude to the successful adolescent patient/sportsperson themselves in contrast to the relatively well known risks of contraception. I also argue that the complexity of the weighing of potential harms and benefits are such that informed consent cannot be reached by adolescents. Moreover, given the public prominence of the WADA antidoping legislation, and the general public support for them, there will necessarily be a lack of transparency in the potential consent process, which undermines any audit for the accountability of the consent process. I conclude that Gillick competence ought not, therefore, to be viewed as a precedent for pediatric or adolescent consent to doping and that the "weak" or "soft" paternalistic prevention of doping is justified.

The issues that orbit around the use of performance enhancing drugs has been one of the most discussed issues in both descriptive and normative sports ethics.[2] As is well known, the worlds of elite sport are regulated by the World Anti Doping Agency (WADA)[3] whose code (WADC)[4] defines and proscribes certain processes and pharmacological products that comprise doping and which receive various bans from athletic competition as their sanction. The WADC, however, says nothing in detail or in particular about pediatric doping, nor does the international professional association for sports medicine (International Federation of Sport Medicine [FIMS]). There is then something of a lacuna here. With respect to adult doping I think it is fair to say that a significant number of philosophers (24; 43; 44; 52) are skeptical of the legitimacy of the bans on doping. A few notable scholars

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and scientists have written explicitly against public and political opinion and declared that the bans should simply be revoked if they are grounded purely on ethical considerations (24; 32; 49). Moreover, that skepticism has found its way into such august medical and scientific journals as *Nature* and *The Lancet*. Their liberalizing or libertarian approaches to doping are sometimes driven by a quasi-medical lobby who advocate the use of technology for all aspects of human enhancement[5] and who view sport as a key vehicle for their legitimization. There is always the danger, in supporting the banning of certain substances of practices that one is considered a Cassandra or a timid, narrow-minded, conservative.[6] Worse, for those who value their academic freedom, one might be seen as an apologist for global institutions such as the International Olympic Committee (IOC) or WADA.

In his excellent analysis of paternalism with respect to the doping issue, Brown (11) does not discuss the issue of pediatric or adolescent[7] doping in a sustained way[8]. He does write, however, of the potential vulnerability of young people to the harmful effects of doping (11:18) with risks "far outweighing any possible benefits of temporary superior athletic prowess." It will be thought by some that the wrongness of pediatric doping is self-evident. Perhaps this explains the lacuna in the pronouncement of official organs such as the FIMS, the IOC or WADA. To what extent is this presumption warranted?

What particular problems are generated when the populations under consideration are themselves legal minors? In this essay I shall raise a number of problems and consider one central difficulty within the issue: the lack of capacity to consent to doping in an informed manner by a child or adolescent. Now since doping ought, according to the skeptics, to be permissible for adults it is at least open to question whether the same opportunity ought to extend *ceteris paribus* to legally competent children or adolescents. Moreover, there appears to be a prima facie precedent in medical ethics law where respect for the autonomous desires of one's patients is thought to be of paramount importance whether they are adults or, for the purposes at hand, competent minors. Thus I consider whether the notion of well known legal test of "Gillick competence" to contraception[9] in the absence of parental consent, sets a precedent for the child or adolescent who wishes to dope in order to enhance their sporting performance. Both scenarios certainly share at least the similarity that the child or adolescent might be considered competent to frame their conception of what is in their best interests.

In exploration of these issues I distinguish, for the purposes of argument, two aspects of the problem. First, I argue that there are particular and specific good grounds for paternalism with respect to sports legislation against pediatric doping viz. the exploitation of what is effectively a vulnerable population. Secondly, I compare and contrast Gillick competence with the possibility that pediatric might competently authorize a physician to assist their doping. I conclude that the former should not be thought of as a legal or ethical precedent for the latter. In order to buttress this position I present a range of evidence that underwrites the precautionary disposition I argue for and, in particular, argue that any consent an adolescent could proffer would be invalid due to an inability to weigh (with sufficient informedness and comprehension) the potential benefits and harms arising.

Is Pediatric Doping Really a Problem and If So What Kind of Problem Is It?[10]

One might reasonably ask whether the question at hand is a hypothetical one. Do adolescents really engage in doping behaviors? A short answer suffices: indeed they do. There is certainly international evidence of a small but significant proportion of adolescents engaging in doping practices. The data available must, however, be treated with caution on a number of grounds. In the first instance, the dominance of research emanates from North America (5). It does not follow that the problem exists predominantly there. This may or may not be true. It merely indicates that more research is being carried out in the USA than anywhere else. Recent epidemiological surveys in the USA suggest that most children experiment with drugs such as alcohol, marijuana, and tobacco (26). Why should doping products be any different? Although mindful of the fact that they are USA-based, recent review articles suggest between 3 and 12 per cent of adolescent males have used anabolic androgenic steroids, (hereafter AAS) at some point (59) while others present figures of between 4–11 per cent (14) while an earlier study puts the rates at between 5 and 11 per cent (2).

A second methodological point refers to the construction of the research tool used to gather the data on pediatric doping. Kanayama et al. (30) suggest there may be methodological weaknesses in such studies where high prevalence rates are deduced from questionnaire-reliant methodologies. In particular they express concern regarding the over-reporting of steroid use in females, although they also consider prevalence rates for adolescent males to be over-estimated. Kanayama et al. (30) criticize questionnaires that refer generically to "steroids" without further qualification. They argue that this leaves open the possibility of conflation with the use of nonperformance enhancing steroids such as corticosteroids (commonly used painkillers), which may lead the respondent to think that steroids are freely contained within sports supplements. The authors also criticize questions that refer to steroid use "without a doctor's prescription." This is especially important since this may be taken to infer that doctors commonly prescribe steroids whereas AAS prescription for girls is almost unheard of. Also reference to the number of 'times' steroids have been taken is misleading (30) because AAS tend to be taken in courses. 'Cycles' of AAS can last between 6 and 12 weeks (2). It is suggested that as knowledge about these drugs has developed, through their wide reporting in the media, the likelihood of over reporting steroid use through response to such ambiguities has reduced (30). Summarizing their position, Kanayama et al. (30) maintain that earlier prevalence rates are unduly influenced by ambiguous research questions. Having established that there is such a phenomenon as pediatric doping (albeit one that appears more concentrated in the context of American high school athletics and one of whose prevalence we are not certain) we must also note that the practice is gendered. The use of AAS by females is reported to be lower, around 1–2 per cent admitting using steroids (59).

What kinds of substances are abused in the name of athletic success? Probably the greatest single family of substances ingested are AAS that are used to promote muscle growth. It is widely agreed that power-based sports are most vulnerable to doping and these are most likely to feature on a list of substances desired by pediatric athletes who are tempted to dope (6). But pediatric doping is

not confined to AAS use. Laure et al. (36) in a study of high school athletes in France, found of the four per cent of athletes who admitted doping at least once, 13 per cent specified peptide hormones, this group including the human growth hormone, as well as tamoxifen[11] and EPO. Wanjek et al. (56), also indicated use of growth hormone by German adolescents, 0.4 per cent of respondents compared with 0.7 per cent having used AAS. As Allen and Frost (1) report, however, there is a lack of data concerning the psychosocial benefits or harms from recipients of human growth hormone even in therapeutically indicated populations. This point in particular, regarding the uncertainty of known effects is an important component of the case for paternalistic measures with respect to pediatric doping.

Having established that the problem of doping is extant in adolescent sports, how ought we to frame our ethical responses to it? As I have said, perhaps most philosophers of sport, sport pedagogues, and parents shudder at the thought of the untrammelled pursuit of athletic success that would drive their children or charges to such measures and would embrace a paternalistic stance against the would-be pediatric doper. Justifying the intuitive wrongness of pediatric doping, and the need for some for paternalistic protection, is another matter. And a fuller consideration of the issues would need to situate the ethics of pediatric doping in the broader context of philosophical discussions relating to children's rights to self-determination, or the much discussed right to an open future (19; 22; 53).

Given, however, the prima facie presumption of harm prevention for children it would seem we would need to consider the adoption of some "weak" or "soft" (21; 23 respectively) paternalistic measure which refers to the intentional overriding of a person's preferences when that person is thought incompetent to form a sufficiently rational and autonomous conception of their own interests. By contrast "strong" or "hard" paternalism (21; 23 respectively) refers to the over-riding (or at least supplanting) of the desires of competent adult persons, a policy he thought ought only to apply in special cases. Our scenario, referring as it does to legal minors, thus only invokes the possibility of "weak" or "soft" paternalism. Defenders of the child's potential competence may argue that where a child has capacity to comprehend the action and its risks and benefits any paternalistic intervention will be a "hard" or "strong" one. I shall argue, however, that only "weak" or "soft" paternalism typically applies in the context of pediatric doping. In defending this position I explore the possibility of constructing a rationale for justified paternalism in two phases: the first is that the population under consideration are vulnerable and therefore in need of protection, since the sports system (or key actors therein) exploits their absolute desire for athletic success. Having set out the paternalistic case I move on to a more in-depth discussion of the powers of consent of successful pediatric athletes.

Are Pediatric Athletes Vulnerable?

What does it mean to say that pediatric athletes are vulnerable? To what or whom are they vulnerable? First, it is necessary to accede that something more than the intrinsic vulnerability of all humans needs to be established. Hobbes (27) long ago pointed out even the strongest are vulnerable to attack in their sleep. Moreover, most readers of this journal, beyond the first flushes of youth, can testify to their

being vulnerable to the ravages of aging and the reality of diminishing physical and psychological powers. What more can be said of pediatric athletes for us to consider them vulnerable to the extent of needing paternalistic protection?

Being youthful, let us agree without argument that the life plans of adolescents' norms and identity-constituting attachments are to a certain degree in flux. That is to say, their life plans are neither fully formed nor fully informed. This will render them (potentially at least) vulnerable to certain controlling influences that may undermine their rational and autonomous decision making. Three such forms of improper influence: *coercion, manipulation* and *persuasion* can readily be identified (8: pp. 94–98). The limit case for improper influence was witnessed in the state sponsored and medically supervised training of young East German athletes in the 1970s and 1980s (50). Less dramatically, what often renders elite pediatric athletes particularly vulnerable, and requiring protection, is the manipulative or even coercive character of their relationship in the athlete:coach:parent triad. The wealth of evidence illustrating excessive parental influence over their children's future sports careers should not be underestimated (19; 53).

In order to develop this first point further it is necessary to stipulate a context for my concerns. It may be difficult to conceive why recreational pediatric athletes would succumb to the temptations of banned performance enhancing processes or products.[12] For present purposes, however, I shall restrict my discussion of doping to already successful adolescent athletes. I have in mind contexts as diverse as senior high school baseball, basketball and football in a large US cities, or professional soccer academies for youths in Europe or South America, or even the practice of recruiting 5 and 6 year old jockeys to professional camel racing in the middle east (17). What these scenarios share, is the potential for exceptionally lucrative financial contracts and high social status even from a very young age.[13]

I suggest that what makes elite pediatric athletes vulnerable to harms is the presence, whether real or perceived, of these enormous financial incentives whether as contracts or potential contracts (17). The very presence of such inducements, and the international market in youth sport labor from which it has emanated, has the power sufficiently to corrupt clear thinking in relation to the adolescents' future interests.

Taking unreasonable risks is not something we generally promote to the young. I agree with Russell (48) that we ought not to inure children from all possible harms arising from sports and indeed that exposing them to a restricted measure of them may indeed promote virtuous dispositions and powers of practical reasoning of value to them at all stages in their lives. But where the temptation to pursue highly specialized and potentially risky careers, ones that increase the possibility of harms, and/or foreclose broader development and thereby limit vocational and other social opportunities for growth and well-being, it is not in a child's interests that they be allowed to pursue them[14]. I therefore consider these lucrative inducements to be undue influences on the adolescent athlete.

I am aware that it might be argued that these influences regularly affect competent adults who still favor risking their future health in elite or professional sport. Nevertheless, we typically allow their participation in sports out of a respect for their freedom to choose the activities they wish insofar as they do not harm others.[15]

Leaving aside coercion and manipulation, which are unambiguously unethical practices, I have argued that the mere presence (or perception) of very significant financial rewards along with the tangible and ongoing elevated status of peers renders the adolescent vulnerable to decisions that do not properly weigh potential rewards and risks with present and future best interests. I am mindful that the discussion so far elides a potentially important distinction between children and incompetent adults. Thus far the argument would apply to both categories[16]. In what follows, however, I shall restrict myself to pediatric contexts.[17]

I have thus far assumed that successful adolescent athletes are vulnerable to the extent that their life stage does not typically permit autonomous decision making on matters of potential magnitude as is the case in doping. This assumed incapacity is compounded by the presence of significant financial rewards that may be thought to unduly influence their decision making, especially in the context of pushy parents or coaches. This argument may not persuade those who think that children have greater powers of autonomy than I have assumed thus far. I shall therefore proceed to argue that soft paternalism is necessary to protect pediatric sports populations from exploitation before discussing the shortcomings of their capacity autonomously to consent.

Exploitation, Elite Sports and Pediatric Doping

Everyday usage of the words "exploit" or "exploitation" does not necessarily embody negative connotations. People speak unproblematically about exploiting marketing opportunities in ways that are not morally troublesome. To exploit a thing with moral standing however is wrongful by definition. But why ought we to think of the elite pediatric sportsperson as being exploited in his pursuit of doping products or practices? In developing this discussion I will draw upon Stephen Wilkinson's defense of the (fairly remunerated and consensual) trade in organ selling even though I am not sympathetic with his positive conclusions for that particular project.

With respect to ethical usage, to exploit something is to use it wrongfully. So exploitation becomes a species of instrumentalization. Wilkinson writes "A exploits B (in this sense) if A treats B merely as a tool for, or a means of, achieving A's goals." (58: p. 33). Underpinning what is morally wrong with this treatment of persons are two Kantian principles. First, to treat someone instrumentally is to offend the Categorical Imperative by treating another person merely as a means (or instrument) to our own ends. Secondly, to treat another as fungible is to conflate the concepts of price and dignity; the former applies to all objects and subsumes their "replaceability" but the latter does not, and to give a price to human persons is to offend their dignity.

Developing on these two foundations, two senses of exploitation can be discerned: "disparity of value" and "wrongful use." The two instances of exploitations differ in that what is wrong is the particular use in question. Wrongful use exploitation arises where one is used as a *mere* instrument for another's goals and thereby fails to treat the other as an end in themselves. Cases such as these abound in sports where surrogate glory or wealth is sought through the auspices of the adolescent or child. Perhaps the worst of recent cases arose in the Indian subcon-

tinent where a child of 4 was being trained to run marathon distances clearly, in part, for the financial well-being of the coach. Wilkinson reserves the label "wrongful use exploitation" with "instrumentalisation" (58: pp. 34–35) for cases such as these. It seems fairly clear that to force one's children to commit to work in elite sport for economic advantage (however mutual) is an instrumentalization of the child, but it is also the case that it offends something in the irreplaceable nature of childhood itself understood in a linear way: that the early stages of life are to be valued on their own terms and cannot be regained once past.[18] In such cases the adolescent, in the absence of parental and other's undue influences, instrumentalizes his or her youthful body.

Moreover, exploitation can occur also in cases where one is unfairly used (under-rewarded). What we know of elite sports is that the vast majority of athletes will *not* earn the huge salaries we read about in *Time* or *Sports Illustrated*. That is not only the preserve of a tiny minority but also it is restricted to a relatively small sample of sports that are caught up in the commodified media driven-elect. So it seems reasonable to say, on the basis of the tiny conversion rate between the "hopefuls" and the "successfuls", that putting one's health at risk in the case of doping is a dangerous long shot (even when it is the only possible shot an athlete has).

Regarding unfair use exploitation Wilkinson writes: "Everything depends on the context in which the use takes place, on the relationship between the user and the used, and (most importantly of all) the quality of the used person's consent." (58: p. 43). His thesis is that where an individual is fairly rewarded there is no exploitation so long as the agreement to the conditions that are potentially deleterious to their well-being are properly understood and subject to the agent's own informed desires. Thus far I have questioned whether the fair reward argument holds water since so few pediatric athletes ever succeed to the professional arenas of adult commercialized sports and that the potential rewards induce risks that otherwise would not be entertained. But for the purposes of argument let us assume that some children so gifted in the natural lottery, with the requisite character traits, and a lorry load of good luck, are indeed destined to make it to the big game. Ought they to be allowed to consent to doping? I shall explore the propriety of this state of affairs via an analogy with the Gillick competence of adolescents to consent to contraception.

Gillick Competence: A Sketch

Given that our present concerns are philosophical more than juridical, I shall offer no more than a sketch of the idea of "Gillick competence" and the subsequent "Fraser Guidelines"[19] that are used widely in medical world in the UK at least. The term[20] derives from the name of a parent, Mrs. Gillick, who brought a case against the National Health Service in the UK for allowing her 14 year old child to consent to contraception without her knowledge or authorization.

The case turns upon the principle of respect for autonomy to determine one's life plan and the actions therein. Its *locus classicus* is John Stuart Mill's arguments pertaining to noninterference and self-determination. Loosely put, persons should be able to do with their bodies and their selves as they wish so long as they are of

sound mind and do not harm others. Originally Mill held that 'idiots and infants' comprised the exception to his original formulation of the principle of noninterference/self-determination, but it has been extended in recent times to children who are possessed of the competence to understand the nature and consequences of the issue at hand. In medico-legal terms, the Gillick precedent allowed children who demonstrated the relevant competence to grasp the intervention(s) proposed, to accept or reject that treatment without parental authorization which had hitherto been normal practice. Might this precedent extend to a successful adolescent athlete, who wanted to dope?

Comparing and Contrasting Adolescent Competence to Consent to Contraceptive Treatment and Doping

Are there good reasons for thinking that children or adolescents could properly be thought of as "Gillick competent" to consent to dope? To begin with it will be useful to consider competence as a logically incomplete concept. It is analogous to the concept of fitness. One cannot properly ask the question whether X is fit, without specifying the purpose for which one is or is not. So, with the idea of competence to consent, we should not expect a universally applicable answer. The question does not permit it. If we wish to know whether someone is fit to give consent, we must ask whether they are competent in the relevant sense.

Culver and Gert (16) refer to this as task-oriented competence. For children or adolescents properly to give informed consent to the use of doping substances they must understand what they are being asked to do before they can competently give consent. Can we judge that children or adolescents are informable about the complex of benefits and harms that may attach to doping? We might have little confidence in the informability of pediatric athletes if we based our judgment upon what we know of present young elite athletes. Understanding the pros and cons of contraception seem straightforward by comparison. The young cyclists interviewed in Vest Christiansen's (54) study had, unsurprisingly, heard about the blood boosting drug EPO, but knew little about other potential doping agents such as growth hormone, steroids and amphetamines and their potential effects.[21] Though not informed as regards doping then, to what extent adolescents are informable and capable of comprehension of the task at hand?

Given the scientific controversy that surrounds the efficacy in addition to the health-threatening effects of various products that fall under WADA's banned list, it is difficult to comprehend how anyone without a degree in the relevant biomedical sciences could come to a reliable understanding of the scientific benefits and costs or risks of all the various doping substances and processes—especially given that it is widely thought athletes have used therapeutic drugs only tested not tested on healthy populations, or that they use off-label medications, experimental drugs, and that they are often required to "guesstimate" appropriate dosages and cycles. This lack of knowledge of side effects following usage may undermine both adult and adolescent populations—at least for many doping products and processes.

This picture of uncertainty regarding doping effects is not homogeneous. While side effects of AAS are relatively well known for adults, those for creatine

(a very widely used synthetic substance used for the production of explosive power and which is not banned) or recombinant human growth hormone (banned) and other products are not known in full either by scientists or physicians. As I have noted above doping use may be thought to be experimental without the protocols of (e.g.) randomized controlled trials or evidence based decision making. And this may be because of the experimental nature of the drug, or its unique application to nontherapeutic purposes, or merely that its efficacy in therapeutic cases renders uncertain its effects in patients not suffering from any medical condition. Moreover, a more general point about pediatric pharmacology is worth making. It is well known that very little of today's pediatric prescription drugs were ever tested on pediatric populations (15). Indeed it is widely thought unethical to experiment on populations without direct therapeutic need and benefit (20). And so the pharmaceutical industries statistically extrapolate dosages from adult populations. The lack of awareness of the effects of performance enhancing drugs on pediatric populations is expounded by the American Academy of Pediatrics Committee on Sports Medicine and Fitness (3: p. 1104):

> Virtually no experimental research on either the ergogenic effects or adverse effects of performance enhancing substances has been conducted in subjects younger than 18 years.

This gap in understanding threatens to radically undermine the possibility of informed consent by adolescents. It also brings into serious doubt even the possibility of adults or physicians advising adolescents as to what would be in their best interests so as they could come to an informed decision.

Nevertheless, most professional bodies in relation to sports medicine and sports governance assert the harmfulness of AAS, and potential of other such substances, and this is one of three criteria by which such products and processes are banned by WADA.[22] Baker et al. (7: p. 481) note that:

> The FDA in the United States has received more than 800 reports of adverse effects associated with use of products containing ephedrine alkaloid since 1994. These serious adverse effects include hypertension, palpitations, neuropathy, myopathy, psychosis, stroke, memory loss, heart rate irregularities, insomnia, nervousness, tremors, seizures, heart attacks, and death.

If we compare this Pandora's Box with the potential physical and psychological harms of contraception it seems that both the scope and the magnitude of potential harm is magnified in doping. While the ingestion of oral contraceptives is not entirely without risks it, is by comparison, regulated and the professional practitioners who prescribe it have extensive knowledge and have undergone professional training. Moreover, it is performed under conditions that emphasize the health and safety of the patient.[23] Given the public prominence of the WADA antidoping legislation, and the general public support for them, there is a resultant lack of transparency in the potential consent process. This lack of transparency renders all but impossible any audit for the accountability of the consent process. Compared, however, with the unregulated practice of 'ethnopharmacologists' (often entrepreuneurial graduates in biochemistry who have little or no concern for the health of those whom they supply drugs to (45)) it may be safe to assume

that the risks are rather more controlled—though clearly one could not be anywhere near as confident with "backstreet abortions", which may be more like-for-like in its comparison.

In certain communities the use of oral contraception may result in the excommunication of the potential mother. This clearly constitutes an exception rather than the rule. Whether merited or not, however, doping cheats at the elite level face the full sanction of the sports communities which partly constitute their emerging identity. WADA's rules indeed prevent those found guilty of doping even from training with their respective clubs or teams. Moreover, high profile doping cheats make good copy: there is little chance of escape from the media's scrutiny and subsequent *schadenfreude*. In terms of status, those convicted are required to hand back those medals won under unfair conditions. Moreover, in the UK at least, those guilty of doping violations are required to pay back state funding since they have broken their contract to compete "clean." Given the fleeting nature of athletic careers, many of which are not lucrative despite the publicity given to footballers' grossly inflated wages, being convicted of doping can mean serious financial hardship. It seems difficult to conceive of adolescents being able to consider so seriously the mid and long term consequences of getting caught which would be necessary for them to be considered competent to consent to doping. That much said the complex factors that attend abortive surgery are hardly less weighty. What may be said in their favor, perhaps, is the possibility of reversibility: except in tragic circumstances the potential mother may elect to become pregnant and give birth at a later date. The analogous option may not be realistic for the doping athlete who is caught since their period of excommunication is intended precisely to be an effective end to their athletic careers. With respect to adolescent contexts, it would be a strong minded individual indeed who continued to train at the highest levels for 1, 2, or 4 years while being unable to compete or receive financial assistance to allow them to focus on developing their athletic potential.

Another sociopsychological factor to be considered is whether Gillick consent to doping would be properly understood in relation to future addiction. We may cast drug abuse in sport more generally as a pediatric issue since the genesis of doping behavior can begin in adolescence.[24] A further important consideration then will be something like a slippery slope argument; if we permit doping in pediatric sports will this serve to legitimize, for example, wider drug abuse, or even genetic modifications? It is a serious concern then that doping may lead the adolescent on a path to addiction to a point where autonomous decision making is itself undermined:

> Youth with life choices and options that are perceived as being limited are more likely to engage in high risk behaviors, such as substance abuse and unprotected sexual activity. Youth may rationalize or perceive other social or personal "benefits" from substance abuse that override any identified health concerns. Many of them do not realize the negative consequences of drug use and abuse; some even believe that it is normal to use various drugs. The greatest risk for long-lasting dysfunctional patterns of substance abuse is the onset of use before age 15 (26: p. 394).

It might be argued that I am sliding together here issue of performance enhancing and social drug use. There is some truth in this claim but also some justification for it. One of the key issues regarding illicit doping is that of access. Athletes in some sports have potential access to doping products through the frequenting of, among other places, fitness gyms where off-label use of pharmaceutical is rife (45). So the elision, in terms of the possibility of gateway drugs, to increased access to other drugs is not unreasonable. A more substantial philosophical response to this position might be to say, well we must not conflate rational autonomy with prudence.[25] Rawls' (47) well known thought experiment might apply here: ignorant of personal individuating facts (such as age, intelligence, ethnicity, sexuality and so on), rational agents necessarily choose prudent laws because they were unaware that they would benefit or be harmed by their future application. But why not entertain risky lifestyles? Why ought "chronological parochialism" (12) necessarily to prevail?

In relation to adult doping, Tamburrini (52) has questioned whether we may justifiably link this phenomenon with widespread drug abuse. This position is undermined, however, by the millions of dollars commercial companies pay to sports icons to promote their product based, as it is, on a wealth of data concerning the appreciation of market brands even by very young children. As Greydanus and Patel (26) note that a career of substance abuse may pivot around adolescent years thus we are properly protective of this time slice. Moreover, a recent international study found that those athletes who use ergogenic nutritional supplement use were more likely to report doping too (46). Perhaps the most important issue at hand here, from a public health perspective at least, is whether AAS or other preferred doping substances are "gateway drugs" (31). These substances are so described for the reason that their use increases the likelihood of progression to other drugs. Moreover, given the motivational differences between doping substances it is not clear how we should think of the most common substances such as AAS, EPO, or hGH. A questionnaire based cross-sectional study in Sweden of more than 2000 16–19 year olds, led Kindlundh et al. (33) to suggest that the motivation to enhance performance and body appearance has commonalities with other forms of substance abuse. Of course this in itself does not lend authoritative support for the application of the gateway principle. One might think that if the adolescent has gone to the point of using doping substances that the athlete has crossed a line in *his*[26] attitudes[27] toward rules and the perceived value of the perfection of his capacities. Why not go further? The point is moot. Perhaps what is most interesting is not the nature of the drugs themselves but the cultures into which the choices and behaviors are generated or the networks into which adolescents must operate if they are to access such drugs (31). But the evidence does not exist to warrant anything other than the application of a precautionary and thus soft paternalistic approach.

In this section I have presented argument and evidence regarding the complexity of which renders it problematic to think that adolescents might typically comprehend the nature of doping and its attendant benefits, harms and the risks and probabilities that pertain to them. And I have contrasted it with the relatively well known exposure to such in the case of consent to contraception in Gillick-like cases. Despite the palpable fact that physicians have assisted in pediatric doping, I have challenged legitimacy of the physician who would advise an ado-

lescent that it would be in their best interests to dope on the grounds of potential harm and the incompleteness of medical understanding of doping products and processes with nontherapeutic adolescent populations of which our hypothetical successful athletes are one.

Conclusion

I have tried to show how the issue of pediatric/adolescent doping is one that merits serious attention. While the philosophical literature on doping rarely considers pediatric populations, it seems the case that the legitimacy of their pursuance of these products and processes might be thought to fall under the conceptual province of "Gillick competence" to consent to contraception. In this case, as sometimes happens elsewhere in the doping debate (24; 49), we would be transposing a norm—like harm prevention—from medical ethics into the domain or sports ethics. Unlike the Gillick case I have tried to articulate the background to the successful adolescent sports world, the vulnerability of athletes theirin and the tendency toward their exploitation. With specific respect to the capacity of adolescents to actively consent to doping I have adopted a precautionary approach. Although the gravity of both decisions is manifest, I have argued that the harms attendant to doping, as opposed to the regulated use of the medical profession to prescribe oral contraceptives or terminate pregnancies under conditions of nonparental consent, are of a greater magnitude to the successful adolescent patient/sportsperson themselves. I have also argued that the disputed scientific bases for the harms of pediatric doping are such that the informational requirements of informed consent render any would-be consent invalid and would undermine the legitimacy of a physician who advised an adolescent to dope. I conclude that Gillick competence ought not, therefore, to be viewed as a precedent for pediatric or adolescent consent to doping and that the "weak" or "soft" paternalistic prevention of doping is justified.[28]

Notes

1. This address was given as the Warren P. Fraleigh Distinguished Scholar Lecture for the International Association for the Philosophy of Sport, Tokyo, Japan, September 2008.

2. For historical overviews see Dimeo (18); Hoberman (28); Hoberman (29) and Waddington (55). For philosophical overviews see Brown (13); McNamee (38: pp. 177–193); Miah (43;44).

3. See http://www.wada-ama.org/en/. Accessed 17.05.09

4. Most recently revised in 2009: see http://www.wada-ama.org/en/dynamic.ch2?pageCategory.id=250. Accessed 18th March 2009.

5. For a critique of which see McNamee and Edwards (39).

6. See the caricature of the timidity of bioconservatives in Bostrom (9).

7. I shall use these terms interchangeably throughout the essay.

8. Though there has been descriptive ethical work in the area. For example, see for example Laure (34) and Laure and Binsinger (35).

9. I note that it has been suggested that the competence may even extend to adolescents who wish to have abortive surgery.

10. I gratefully acknowledge the assistance of Andrew Bloodworth with whom I compiled two reviews of literatures on doping ethics for UK Sport as part of social scientific studies on the values and norms of elite young athletes with respect to doping (40 unpublished; 41 unpublished). Parts of this section are reworked from those reports.

11. So as to avoid the appearance that this is a U.S. phenomenon, consider the research of Baker et al. (7) in the economically deprived Valleys of South Wales in the UK, who found 22 per cent of the respondents in a study of health club users used tamoxifen, a substance that is described as a 'nonsteroidal antiestrogen for women with ductal carcinoma in situ (DCIS) and for women at high risk of breast cancer.' (7: p. 481).

12. I do not mean by this to suggest that recreational doping is rare or unimportant. It clearly is a matter of serious concern in the West where many seek pharmacological enhancement of their physicality in order to achieve greater social capital that goes along with a muscle bound appearance (7; 45).

13. See David (17: pp. 160–178) for a critical review of the trade in youth sport labor.

14. In holding this position I take it that I am in agreement with Russell (43) who criticizes of DADs (danger averting devices) because of their attempt to eliminate all physical risks from childhood leaving them thus unprepared for the risks that will surely attend their adult lives. This criticism does not extend to pediatric doping.

15. There are exceptions to be noted. In Italy, for example, preparticipation heart screening is compulsory for those engaging in sports. Moreover, all professional boxers have mandatory brain scans.

16. I am also mindful that the incompetence argument was attempted in defense of Ben Johnson at the Dubin inquiry. It was argued, unsuccessfully, that his powers of autonomy were sufficiently etiolated (in addition to the fact that his relationship with his training entourage so manipulative) served to reinforce his heteronomy.

17. Though I grant that the age of 18 which is typically taken for legal majority is arbitrary as is the boundaries of the concept of childhood is (see 4).

18. It is of course true that no time slice can be replaced once past. Nevertheless, the time of life which is the object of this discussion is particularly valuable owing in part to maturational issues of a biological and psychological kind. In these years significant harms may attend drug use as has been witnessed in the East German state sponsored scenario noted above.

19. The Fraser guidelines are issued to UK doctors in scenarios such as pediatric consent to contraception in the absence of parental or proxy consent: (i) the young person will understand the professional's advice; (ii) the young person cannot be persuaded to inform their parents; (iii) the young person is likely to begin, or to continue having, sexual intercourse with or without contraceptive treatment; (iv) unless the young person receives contraceptive treatment, their physical or mental health, or both, are likely to suffer; and (v) the young person's best interests require them to receive contraceptive advice or treatment with or without parental consent.

20. There is some dispute as to whether this appellation is sufficiently accurate for it to persist as common currency (57).

21. This too has been my experience in conducting ongoing focus groups and interviews with over 80 elite 16–21 year old athletes in the UK (41 unpublished; 42 unpublished).

22. The others being that the substance is performance enhancing and against the spirit of sport. The code adopts a policy of banning where at least two of the three criteria are met (or where a masking agent has been used to avoid detection of a product or substance under that rule).

23. Notwithstanding this generalization it is necessary to note that very considerable disquiet has been registered by political and religious communities about the validity and reliability of the statistics on mortality rates in abortive surgery. This would need to be borne in mind if com-

paring doping with more serious medical interventions.

24. For example, one study of high school American football players found students starting even earlier, the average age of first time users 14 years. More alarmingly, 15 per cent of the athletes began taking AAS before they were aged 10 (51).

25. I develop this point in the context of risky activities in McNamee (37).

26. As I have indicated above, the statistics bear out the masculine reference.

27. See, for example, Gregory and Fitch (25).

28. I record my gratitude to Andrew Bloodworth, Steve Edwards and Verner Møller for the helpful comments on earlier drafts, and also to the anonymous reviewers and Editor of the Journal.

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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 contestable 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 positivist, 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.[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).

The precise nature of these 'controversies over fundamentals' begs questions regarding their relative status in the social and natural sciences as well as to their causes and effects. So it could be argued that either social scientists should either (i) reject the use of Kuhnian 'paradigms' altogether; or (ii) make it clear that when they use that term they have a particular meaning in mind other than simple theoretical and methodological diversity and conflict (in which case they should desist from referencing him!). If the latter is meant – and it is surely

applicable – then perhaps the least confusion would arise in ignoring the term except for analogous reference.

Third, in his articulation of this simpler divide between the two senses of 'paradigm', Kuhn also attempted to put clear water between himself and Popper. One of the chief ways of doing this, and borne out by his later elaboration of paradigm-talk, was the relations between language and nature as they affected the initiation or apprenticeship of scientists into their respective scientific communities. In this regard, Kuhn was developing a central idea of the work of Michael Polanyi (1958) that we know more than we can tell; what in scientific contexts this implied was that scientists come to understand and order the world in special ways that are not as volitional or as deliberate as the theory-choice that Popper sets out. This idea, even as loosely hinted at as this, cuts across categories (i) and (iii) of Masterman's senses. Kuhn (1972) argues that it would have been better not to confuse matters by grouping the constellation of ideas that surround this one with the label 'paradigm' or 'paradigmatic'. He suggests that what enables scientists to agree upon and solve puzzles with such a uniform approach would be better captured in the phrase 'disciplinary matrix' (1972: 271; 1977: 297). His use of that term is justified by arguing that the disciplinary identity of scientists rests in part upon the shared possession of a matrix or ordered body of symbolic generalizations, models and exemplars of concrete problem-solutions (see for examples here: Chapter 5, McFee; Chapter 7, Yeadon; Chapter 8, Noakes; Chapter 9, Brackenridge *et al.*).

Relativism and absolutism in research

In the trail of Kuhn's seminal book came a catalogue of others both more and less critical and supportive. One of the key elements of critical discussion related to the purported relativism of his idea that paradigms were themselves incommensurable. They were effectively competing and therefore not combinable intellectual currencies. Others (notably social scientists) have taken the idea much more strongly to argue that paradigms effectively locate scientists in different worlds. All knowledge becomes essentially located within competing paradigms. These stances betray a strong and a weak sense of incommensurability.

Harré and Krausz justify a weaker reading of the incommensurability thesis. They argue that the kind of wholesale upheaval that relativists latched onto in Kuhn's writings is not typical of western intellectual traditions. Of the authors cursorily cited here, I have at least gestured to the fact that their views stand on the shoulders of others, which is more or less true *mutatis mutandis*. Kuhn is not to be read in the radical fashion of someone such as Feyerabend, who discerns no historical rules that are universally applied in scientific reasoning and method. Of course some revolutions are of greater significance than others. But Kuhn can be understood as arguing that, for example, no scientist could be completely committed to both Aristotle and Newton in their approach to mechanics. Harré and Krausz write:

> If paradigm shifts necessitated complete shifts in subject matter there would be no possibility for hunting for rational choice among paradigms. Paradigm shifts within the framework of that Western scientific tradition which had its origins in the ancient Greek traditions of enquiry, have never involved root and branch transformations of their generic ontologies. The deep, generic ontologies of individual substances and their attributes, located in the manifolds of space and time, has persisted however radically versions of this ontology have differed from one another. Incommensurability between paradigms has never been so radical and deep that it has not been possible to recognise this generic ontology embedded in every successive world view. (1996: 79.)

Related however, recent social scientists have certainly tested out the categories that are typically used to organize knowledge. Poststructuralists and postmodernists deny the givenness of boundary-setting disciplines. What they offer in return, however, is typically labelled relativistic. What does such a claim amount to? Harré and Krausz helpfully distinguish two sorts of relativism: sceptical and permissive.

It cannot be denied that these epistemological currents have scarcely dented the traditional topography of our universities and governmental institutions for research funding. Nor should it go without comment that their critique, in whatever clever clothes they are fashioned and in whatever language they are shrouded, is scarcely new. The idea of relativism goes back at least as far as Democritus and the ancient Greeks, whereas the idea of disciplines has a slightly more modern turn – but even then it reaches back to the 1600s. Toulmin remarks:

> The invention of disciplines, a change that began in the seventeenth century, involved both intellectual and institutional factors. Intellectually, Descartes's use of geometry as the model for knowledge provided its slogans; institutionally, the division of labour into professions and disciplines gave it wings. But the change did not happen quickly, and it has reached its peak only in the twentieth century. (2003: 29.)

Discussion of the non-givenness of disciplines, while having historical foundation, can easily be overblown. It is scarcely accidental. The challenge of relativism, founded in the power of the boundaries and the boundary-setters, often conflates a malign and a benign sense of the term respectively: as Harré and Krausz observe, the sceptical and the permissive.

1. *Skepticism*: no point of view is privileged, no description is true, and no assessment of value is valid.
2. *Permissive*: all points of view are equally privileged, all descriptions are true and all assessments of value are equally valid. (Harré and Krausz 1996: 3.)

The idea that there is one truth or none is a stark choice indeed. But it is no more pernicious than the idea that there is no route to true knowledge: the scientific method. The very idea of a dichotomy – one standard or none – is scarcely helpful. It has not been uncommon in multidisciplinary research seminars I have attended, organized or spoken at, to find natural and social scientists alike who polarize around these extremes. It is not merely that their languages, models and techniques differ, but that the very criteria that might make rational for preference or aversion appears to be contested. Is the reliability eschewed by social scientists using focus groups justified? Is validity invalidated in the recent trend for autoethnographic/autobiographical research (see Chapter 10, Allen Collinson and Hockey)? Is parsimony always criterial in theory-preference? Can narratives be treated as data in the same way that particles or propositions are? Is there no more to investigative sociology than journalism (see Chapter 11, Sugden)? Does the serious entertaining of these questions entertain the demise of science in which we can trust? The triumph of scientists and philosophy of science alike was in part due to the development and refinement of the scientific method. And in most social science and certain natural science its place at the head of the high table is now questioned with more or less promiscuity. Of course a more subtle analysis of relativisms is needed, as is a more patient explication of the kinds of absolutism that they kick out against.

Finally, we might ask, are there not merely epistemic but also ethical criteria that can help us to clarify where scientific endeavours overlap with other professional concerns to do with bytes, data, information? I shall turn to two final remarks on the relations between values and science in the context of traditional, positivistic, science.

Science as value free (or: two-and-a-half cheers for Popper)

Putnam (1993) remarks that both philosophers and scientists have at different times wanted to cleave apart fact and values. Yet in many enquiries and experiments into exercise, health and sport *inter alia* the entanglement is unavoidable. And still many scientists claim to uphold the values of autonomy, neutrality and impartiality in their scientific labours (Lacey 1999). Others, such as critical feminists, insist on a stronger thesis that the social practices of science are inherently political and that notions such as these must be given up as hangovers from a precritical phase to be replaced by partiality and commitment in a more or less explicit way. Again, the picture that is being either supplanted or exculpated is the positivistic one. Putnam writes that positivism was 'fundamentally a denial of entanglement, an insistence on sharp dichotomies: science-ethics, science-metaphysics, analytic-synthetic' (1993: 15).

Much critical work was done in the beginnings of the philosophy of science that demonstrated – from a range of different directions and indeed disciplines – that values entered scientific processes. We can see how showing what now seems obvious to so many, was then so powerful. In their defence, positivists stressed the contexts of discovery were to be set aside from the contexts of

justification. Very roughly put, while the latter were amenable to subjective influences (from cultural to psychological) the latter, it was claimed, assured scientific objectivity. The view is typically attributed to the empiricist Reichenbach (1938) but is also shared by Popper and other positivists, though some philosophers are sceptical of this claim (Shapere 2001). Ladyman (2002: 75) quotes Popper, however, in precisely such a vein:

> The act of conceiving or inventing a theory seems to me neither to call of logical analysis nor to be susceptible to it . . . the question of how it happens that a new idea occurs . . . may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of knowledge (Popper 1934: 27).

While, therefore, anti-positivists all of persuasions have tended to celebrate the moves that were made by Popper in destabilizing logical positivism, we should be wary too of lionizing him. He too is a product of his age even if, unlike most of us, he was able to lift his head that much higher than most and gain a more critical perspective of the philosophical prejudices of his day. In general philosophical terms, merely to argue that the source of an argument is good/bad is inadequate. We may cite great authors who have been bad authorities in given topics or otherwise despicable writers whose views are soundly argued. In a sense Popper is right; from whom theories arise is not something that should enter the evaluation of their ideas. In another sense, and with an increasingly sophisticated awareness of the economic (see Chapter 8, Noakes), political, social and gender-driven biases (see Chapter 9, Brackenridge *et al.*) that enter into the processes of the production of scientific knowledge, the distinction loses some of its sharpness (see Shapere 2001: 416–17).

A final manner in which values have entered discussions of research is in what is termed 'research ethics'. In recent years, with the professionalization of research and the economically driven agendas of efficiency and effectiveness of research production and dissemination, the thought that science is an activity beyond morals – that is to say, amoral – is heard with less frequency. Both the processes of research and the products are increasingly open to ethical scrutiny. Despite, however, the emergence of various institutionalized responses to the wrongdoings of researchers, there is in inherent difficulty in the application of general ethical considerations to particular persons, practices and policies (see Chapter 12, Homan). What appears to be driving these agendas is a mix of the threat of legal redress and a genuine attempt to professionalize research (see Chapter 11, Sugden) in a manner that will at least in principle secure minimal standards of conduct and character, even if their application will always be a thorny affair.

At a more fundamental epistemic level, the traditional juxtaposition of fact and values is, as Putnam notes, due to an improper contrast between, for example, ethics and cultural knowledge generally and science in discussion of absolutism and relativism. In the contexts of health and well-being the effects of this contrast are at the centre of contemporary debates in philosophy as much as

in politics. He notes that the contrast is often made in the following way: first one sets out highly generalized examples on the one hand (whether this culture or moral practice is good or bad as a whole – an eminently disputable affair) and then one opposes them with a specific and uncontested fact (whether this is green or that is hot – the truth of which is denied by no-one). The kind of absoluteness *assumed* in the latter case embodies the positivistic 'view of the world from nowhere in particular within it' (to use Nagel's celebrated phrase) and is wholly illusory. In making judgements in both cases we stand within traditions of thought and action: the conceptual frameworks are just so taken for granted in the latter case. Neither scepticism about knowledge, nor the scientistic or positivistic conception of science as the only method to secure knowledge, is satisfactory. Rather, he writes:

> The third possibility is to accept the position we are fated to occupy in any case, the position being who cannot have a view of the world that does not reflect our interest and values, but who are, for all that, committed to regarding some views of the world – and for, that matter, some interests and values – as better than others. This may be giving up a certain picture of objectivity, but it is not giving up the idea that there are what Dewey called 'objective resolutions to problematical situations' – objective resolutions to problems which are *situated*, that is, in a place, at a time, as opposed to an 'absolute' answer to 'perspective-independent' questions. And that is objectivity enough. (Putnam 1993:156.)

Concluding remarks: an introduction to philosophical issues in science

The volume does not aim at comprehensiveness. Nor is it an introduction to the standard fare of philosophy of science texts. Though there is discussion of theories of science, of tolerance and ignorance thereof, and of the benefits and biases that attend positivistic science and philosophy, there is no space for more fundamental discussions of the nature of causality, determinism, explanation, realism and anti-realism and so on. What has driven my selection of the material is from my own professional experiences of topics that have caused fruitful professional frictions – in the various departments that I have worked in across the fields of exercise, health and sport sciences. This is one of the reasons why each chapter is in the form of a question – whether they are obvious or obscure is up to the reader to decide ultimately. I hope that the range of issues addressed here is not dismissed as mere philosophical intrusion – that much should be ensured by the standing of the authors and the quality of what they have written. *Ad hominem* argumentation is typically unacceptable, but academics of all persuasions still go in for it.

The aim of the book then is twofold. It is first to demonstrate the need for philosophical enquiry in the sciences of exercise, health and sport, whether that be undertaken by philosophers and/or philosophically minded scientists. Second,

a certain hegemony is enjoyed by the natural sciences in these fields. I have often been struck by (some friends might add 'and struck back at') the hubris of the naïve natural scientist who believed uncritically in the deserved dominance of natural *science* and *the* scientific method. Such scientists too readily pour scorn on the alleged relativistic promiscuity of humanities scholars and social scientists in their attempts to embrace 'mere' scholarship or 'soft – qualitative' research. I prefer some rapprochement not least through dialogue in and between philosophers and other disciplinarians – irrespective of whether they identify themselves with that professional designation or not. Scientists have no choice but to engage in conceptual clarification and the coherence and justification of their data, methods, theories and, yes, paradigms too.

Finally, I hope this book will serve as an introduction for undergraduate or postgraduate students, teachers or researchers alike, in the sense of 'introducing' that is perhaps peculiar to philosophical work. For while the work introduces it should also offer some friction to those more experienced in the subject. Possibly the first philosophy text I purchased, A.D. Woozley's *Theory of Knowledge*, includes the following in its introductory remarks: 'controversy is desirable, only unreflective and passive acquiescence are to be avoided' (1949: 11). It is fitting that the book might be a minor 'Amen' to that.[8]

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Notes

1 Hacking (1993: 246–51) for one certainly objects to such a label, even though it would appear commonplace in some introductory texts.
2 Of course, historians would remind me here that modernity is in part characterized by the rational disavowal of religious hegemony and the rise of science, which is consistent with the claim that the first sense of positivism here is modern. I merely mean that the latter are more contemporary.
3 For those interested, Hacking observes four epochs of positivism. After Hume (1739) and Comte (1830–42) come logical positivism (1920–40 – of which A.J. Ayer's classic 'Language, Truth and Logic' was the bible); and the contemporary philosopher Bas Van Fraasen (1980).
4 Whether Kuhn is properly to be thought of as a philosopher – his doctoral work was in physics, while he claims that his famous Structure of Scientific Revolutions is first and foremost a historical treatise on the development of science – is a point typically remarked upon in philosophical texts on science.
5 For a sympathetic account of this see Magee's autobiographical work, which weaves in a history of much modern-century philosophy in order to contextualize his own philosophical debts and commitments (Magee 1997: 46–55).
6 For further description and critique of Popper, see Chalmers' classic (1999: 59–86 and 87–103 respectively).
7 The original references Masterman cites are from the 1962 volume.
8 I am most grateful, and not for the first time, to my good friend Graham McFee for his insightful observations and critical comments on this chapter.