



The governance of science:
ideology and the future
of the open society



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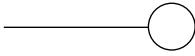
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Series editor's foreword



The social sciences contribute to a greater understanding of the dynamics of social life and to explanations for the workings of societies in general. They are often not given due credit for this role and much writing has been devoted to why this should be the case. At the same time, we are living in an age in which the role of science in society is being re-evaluated. This has led to both a defence of science as the disinterested pursuit of knowledge and an attack on science as nothing more than an institutionalized assertion of faith, with no greater claim to validity than mythology and folklore. These debates tend to generate more heat than light.

In the meantime, the social sciences, in order to remain vibrant and relevant, will reflect the changing nature of these public debates. In so doing, they provide mirrors upon which we can gaze in order to understand not only what we have been and what we are now, but also in order to inform ideas about what we might become. This is not simply about understanding the reasons people give for their actions in terms of the contexts in which they act, as well as analysing the relations of cause and effect in the social, political and economic spheres, but also concerns the hopes, wishes and aspirations that people, in their different cultural ways, hold.

In any society that claims to have democratic aspirations, these hopes and wishes are not for the social scientist to prescribe. For this to happen it would mean that the social sciences were able to predict human behaviour with certainty. One theory and one method, applicable to all times and places, would be required for this purpose. The physical sciences do not live up to such stringent criteria, while the conditions in societies which provided for this outcome, were it even possible, would be intolerable. Why? Because a necessary condition of human freedom is the ability to have acted otherwise and thus to imagine and practise different ways of organizing societies and living together.

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It does not follow from the above that social scientists do not have a valued role to play, as is often assumed in ideological attacks upon their place and function within society. After all, in focusing upon what we have been and what we are now, what we might become is inevitably illuminated. Therefore, while it may not be the province of social scientists to predict our futures, they are, given not only their understandings and explanations, but equal positions as citizens, entitled to engage in public debates concerning future prospects.

This new international series was devised with this general ethos in mind. It seeks to offer students of the sciences, at all levels, a forum in which ideas and topics of interest are interrogated in terms of their importance for understanding key social issues. This is achieved through a connection between style, structure and context that aims to be both illuminating and challenging in terms of its evaluation of those issues, as well as representing an original contribution to the subject under discussion.

Given this underlying philosophy, the series will contain books on topics which are driven by substantive interests. This is not simply a reactive endeavour in terms of reflecting dominant social and political preoccupations, it is also proactive in terms of an examination of issues which relate to and inform the dynamics of social life and the structures of society that are often not part of public discourse. Thus, what is distinctive about this series is an interrogation of the assumed characteristics of our current epoch in relation to its consequences for the organization of society and social life, as well as its appropriate mode of study.

Each contribution will contain, for the purposes of general orientation, as opposed to rigid structure, three parts. First, an interrogation of the topic which is conducted in a manner that renders explicit core assumptions surrounding the issues and/or an examination of the consequences of historical trends for contemporary social practices. Second, a section which aims to 'bring alive' ideas and practices by considering the ways in which they directly inform the dynamics of social relations. A third section will then move on to make an original contribution to the topic. This will encompass possible future forms and content, likely directions for the study of the phenomena in question, or an original analysis of the topic itself. Of course, it might be a combination of all three.

In *The Governance of Science*, Steve Fuller steers a course between the two views on science that I alluded to in the first paragraph of this Foreword. He is neither 'pro' nor 'anti' science. At the same time he asks a simple, frequently neglected, but fundamental question: given that science seeks universal knowledge, how is it that so few 'unelected' practitioners may claim to speak in the name of all? While defining science as the systematic pursuit of such knowledge and so encompassing the social sciences, he takes the experimental sciences as the ideal to which all aspire. Yet in his interrogations, influenced by Karl Popper, science is not found to live up to such canons. However, this conclusion leads him to celebrate neither artificial negativity nor positivity.

Artificial negativity represents the type of thinking associated with the Frankfurt School of Social Research and elements of postmodernism that leads to a high level of abstract theorizing without engagement with the object of its attention. Artificial positivity, on the other hand, is representative of the work of those such as Thomas Kuhn, for whom the ideal of the open society is already present within scientific practices. Indeed, it may be argued that Kuhn's work was a catalyst for the earlier social studies of science in terms of being a means of reconciling the differences between C.P. Snow's now famous 'two cultures'. Nevertheless, the result of Kuhn's approach is to endorse the status quo *as if* that were representative of the open society. Therefore, these approaches, albeit in different ways, fail to engage with science policy and the mediation of scientific activities through prevailing economic conditions. The material basis for the realization of the republican ideal is thus left unexplicated.

The author then moves on to argue that the conception and application of science should be open to democratic accountability for the purpose of enabling a greater understanding of the implications of scientific endeavours within the public domain. This necessitates a resort to normative arguments about the governance of science by way of an interrogation of liberal and communitarian ideas. Finding both wanting, in the sense that they deny the 'right to be wrong', he argues for republicanism as representative of the ideal of the open society.

One of the central sites of the knowledge production process is the university. It is for this reason that Steve Fuller turns his attention to this institution in the second part of the book. Returning to the question of science claiming to speak for all in the pursuit of universal knowledge, it may be the bureaucratic status quo within universities that enables this endeavour. From this point of view multiculturalism represents a challenge as does what he terms the 'military-industrial metaphor'. This may be witnessed, for example, in terms of a tension between the instrumental and goal-orientated nature of knowledge production and that approach which regards knowledge production as a never-ending quest. Here, new recruits undertake apprenticeships in order to acquire the tools to produce yet more knowledge.

The university is thus a site of ambivalence exemplified in the divide that they seek to bridge between their research and teaching activities, as well as between ways of knowing and the claims of particular academics to epistemic superiority based upon the status of the group to which they belong in general. This ambivalence is also manifest in the ways in which publishers increasingly dictate what can reach the public domain and between the pursuit of academic credibility via citation indices and the curiosity that must inform the scientific ethos in which, to paraphrase Bertrand Russell, one comes to recognize that the more one learns, the more one realizes how little one knows. Steve Fuller considers these issues with much insight and never hesitates to furnish the reader with a clear perspective on these important matters.

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With the above in mind, Part 3 of this book is devoted to the secularization of science: that is, the deconsecration of state funding, along with the promotion of alternative programmes of research as contributions to the public understanding of science. In this discussion the author remains spirited in his defence of an ideal, while never failing to engage with material reality. This is exemplified by the justification of a discovery being a consideration of the diversity of its applications. The claim to speak in the name of all is thereby tempered by how applications may be of benefit to different peoples. It is by virtue of such arguments that this book deserves a wide readership not only among scientists, but administrators, policymakers and the general public.

Tim May



Introduction



This book is my attempt to make science policy interesting and important to not only students of science but also students of democratic social and political theory. To be sure, there has been a long tradition critical of abuses of science, as defined by its technological applications. However, it has been much rarer to question the very constitution of ‘science’ as a polity. This is an issue that should be of interest to humanists, social scientists and natural scientists alike. Few doubt that the character of knowledge production has radically changed since the advent of the atomic bomb. Not only have physics and biology acquired a scaled-up industrial presence previously reserved for chemistry, but even certain lines of social science research now sport the forbidding technicality, spiralling research costs and cutthroat competition of ‘Big Science’. Moreover, and most importantly, these developments are routinely seen – even by sociologists of science – as signs of *health* in the knowledge enterprise, perhaps even worthy of emulation throughout academia.

When I speak critically of ‘science’, I am generally referring to this normative orientation, which takes Big Science as the standard against which other forms of inquiry are judged and to which they are supposed to aspire. Invariably, institutional and intellectual aspects of ‘science’ are intertwined in this definition. The historic success of the natural sciences in explaining and legitimating socially significant phenomena is often taken to mean that research conducted under their rubric can do no wrong. In this book I aim to reverse this tendency by disentangling the intellectual core of science as organized inquiry from the other institutional roles it has added. However, I do this not to preserve an impossibly idealized version of inquiry, but rather to articulate the material conditions under which the admirable features of science – especially those relating to its critical vision – have been both realized and perverted. In short, to adapt Harold Lasswell’s famous

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definition of politics: in the republic of science, who should be doing what, with what means and to what ends? These are the questions that the book before you addresses.

Many of the ideas that went into composing this book were conceived in the early 1990s, often as a follow-up to my 'Social epistemology and the research agenda of science studies' (in Pickering 1992). Presupposed in much of the discussion is the importance of rhetoric in breaking down disciplinary and other social barriers that prevent full participation in the science policy process. This point is explicitly developed in my earlier book, *Philosophy, Rhetoric and the End of Knowledge* (Fuller 1993a). Two other books written in roughly the same period should be consulted for the larger historical and philosophical vision that informs my general orientation: *Science* (Fuller 1997) and *Thomas Kuhn: A Philosophical History for Our Times* (Fuller forthcoming).

The event that galvanized my interest in treating the governance of science in some depth was the termination of public funding for the Superconducting Supercollider, which, if built, would have been the world's largest particle accelerator. According to its proponents, this machine would have been capable of revealing the ultimate nature of matter and motion, thereby providing the key for unlocking the great mysteries of the physical universe. In its 1992–3 session, the US Congress deemed that the project failed to meet the benefit-to-cost challenge in a tight budgetary regime. The protracted and highly publicized debate over the Supercollider drew attention to factional divisions within the physics community, the historical shallowness of 'cultural' arguments for science funding and the diminished political significance assigned to basic research in the post-cold war era.

The demise of the Supercollider also marked the first time that scientists openly declared that science and technology studies (STS) – the interdisciplinary field that studies the social production of scientific knowledge – was instrumental in promoting public disaffection with science. Noteworthy in this vein was Steven Weinberg's *Dreams of a Final Theory* (1992), which is also fairly regarded as one of the opening salvos in the ongoing 'science wars'. Readers may judge for themselves the justice of the charge against STS in the pages that follow, as I have been actively promoting the field for over ten years, in connection with my own research programme of 'social epistemology'. All I would say at this point is that one person's 'funding cut' is another's 'resource reallocation': in normative disputes, point of view is almost everything.

In keeping with the format of books in the 'Issues in Society' series, this one is divided into three parts. Part 1 presents the conceptual framework, which is drawn mostly from normative political theory. Here I explain the republican ideal of science as the 'open society' and science's failure to live up to this ideal as its scale and scope have expanded. However, in this respect, the problems of 'Big Science' are not much different from those facing 'Big Democracy'. In Part 2, I focus the discussion on the most concrete site for the governance of science, the university. The coherence

of this institution is increasingly challenged by multiculturalism and capitalism, which can be seen as representing the opposing pulls of communitarian and liberal ideologies introduced in Part 1. Part 3 presents the prospects for the future governance of science, which I see in terms of a continuation of the process of 'secularization' that decouples state power from the authorization of knowledge claims. I consider both historical precedents and experimental proposals for this process, which together offer the elements for renegotiating science's social contract.

Readers will find the references wide ranging. However, those wishing a broader overview of the 'essential tension' between science and democracy in American political thought should consult *Social Epistemology*, 7(1) (1993), which is a symposium around a piece by David Guston. On the tensions surrounding the contemporary university as a site of knowledge production, readers are directed to *Social Epistemology*, 12(1) (1998), which centres on an article by Gerard Delanty. Finally, readers should note that despite periodic allusions to the commodification of expertise, information technology and intellectual property, I have *not* used this occasion to collect together my ten years' worth of writings in what I now call 'the sociology of virtual knowledge'. Fuller (1998) represents my most recent thinking on this topic and includes a bibliography that will form the basis of a book in the near future.

There have been many occasions for presenting the material contained in these pages, including my inaugural lecture as Professor of Sociology at Durham University (30 November 1995), where I introduced the idea of 'secularizing' science discussed in Part 3 and in Fuller (1997: Ch. 4). Among those who made these occasions possible were J. Anthony Blair, Roy Boyne, Richard Harvey Brown, Jim Collier, Bill Dunn, Joan Leach, Jan Nederveen Pieterse, Zia Sardar and Ullica Segerstrale. I would also like to thank Amitai Etzioni and Alf Lawrie who helped clarify my ideas about republicanism and 'the right to be wrong'. Tim May deserves the credit for persuading me to write this book and for reading through the entire manuscript with a sharp editorial eye. Many of the ideas discussed in these pages were born of discussions with Sujatha Raman. However, my biggest debt during the difficult period this book was written is to Stephanie Lawler, who was an unflagging source of emotional support.

 PART ONE

The political and material conditions of scientific inquiry



The pursuit of knowledge, ‘science’ for short, has undergone significant material changes over the past century, probably more so than at any other point in its history. Yet, the political rhetoric surrounding science – especially the ideology of the open society – remains largely unchanged. In the first two chapters of this book, we uncover what is masked by the continued use of this rhetoric. The discussion in Chapter 1 is framed by three political theories of science: liberalism, communitarianism and republicanism. The open society is possible only in a republican regime, where, unlike liberal or communitarian regimes, a clear distinction is drawn between staking an idea and staking a life. This distinction underwrites the fundamental principle of the open society: the right to be wrong. Chapter 2 moves from defining this ground to showing how it has come to be eroded with the scaling-up of the scientific enterprise into what is nowadays routinely called ‘Big Science’. Today too many other things seem to be bound up with the organized pursuit of inquiry to enable it to function in the critical capacity demanded by the ideal of the open society. Part 1 ends with a rejection of ‘science literacy’ as a strategy for opening up science to the public: at best, it secures a receptive attitude without provision for greater public participation. However, the current popularity of science literacy campaigns reveals the extent to which the central political issues facing science are treated as a matter of remedying certain ‘cognitive’ deficits suffered by the public.

Science as the open society and its ideological deformations

Introduction: the artifice of science as the open society

Most of the debilitating effects of political regimes come from people feeling they cannot either admit their own errors or reveal the errors of others – that is, unless the errors are minor ones. (Postmodernists who balk at talk of ‘error’ should ask instead about the capacity to change one’s own and others’ minds in public.) Of course, those who propose claims about the errors of others may themselves be in error. However, for most of history (including the present), people have been afraid even to speak in terms of their own or others’ ‘errors’ because of what they fear to be the consequences of such talk. The result is that a self-imposed authoritarianism can remain in force even in avowedly liberal and communitarian societies – the two major philosophies in the western political tradition. The former finds the prospect of errors too risky to bear individually, whereas the latter portrays the admission of error as the betrayal of duty to the collective. In both cases, people lose their ‘right to be wrong’, which is the essence of the open society, the ideal projected by the elusive middle ground of political theory known as *republicanism*.

In the pages that follow, I treat the governance of science as a branch of normative political theory, so that all of the above terms and issues are made central to an understanding of the people, processes and products associated with ‘science’. By ‘science’ I normally mean the systematic pursuit of knowledge, in the German sense of *Wissenschaft*, which includes all the academic disciplines, not just the natural sciences. The famous precedent for this usage is Max Weber’s (1958) address to new sociology graduate students, ‘Science as a vocation’, which I shall revisit in Chapter 5. However, in the twentieth century, the experimental natural sciences have increasingly become the paradigm case of ‘science’, the standard against

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which other academic disciplines and even non-academic social practices are evaluated (Fuller 1997: Ch. 3). We shall see that often this standard is regarded so uncritically that less than ideal knowledge practices in the natural sciences are simply assumed to be normatively acceptable. For that reason, we need at the outset a clear normative vision that can be specified independently of its alleged exemplifications; hence, the appeal to political theory, and more specifically republicanism.

Why call this topic the ‘governance of science’ rather than, say, the ‘politics of science’? Often the latter expression refers to science’s entanglement with the larger – invariably conflicted – interests in society, typically in matters of technology that have serious impacts on individuals’ lives, the environment and/or the economy. In those contexts, science is treated as an instrument that can be used for good or ill, but little attention is paid to the constitution of scientific knowledge itself or the people who produce it. The ‘governance of science’ is meant to cover this relatively neglected set of concerns. I say ‘governance’ instead of ‘government’ because, seen from a political standpoint, the accountability relations in science – the analogues of elections, referenda, trials and audits – are remarkably informal. (For more on the theory of governance, see Power 1997.) Strictly speaking, science is a representative body in which a few speak for the many. Were this not literally true, then science’s claim to ‘universal knowledge’ would lose its meaning, given that not everyone can be involved in scientific inquiry at any given moment. Yet there is no parliament of scientists, and the days are long gone when academics were granted special voting privileges in national assemblies.

More to the point, scientists are not elected by the populace, or even a representative sample of the populace. Rather, they are ‘self-selected’, which means that people who are already scientists – and relatively few of those – decide who is fit to hold the title of ‘scientist’, through examination and publication policies that proceed with little external scrutiny. Therefore, the mystery surrounding science as a political concept lies less in its day-to-day business (i.e. ‘research’) than in its capacity to speak on behalf of the whole of humanity in a way that transcends national differences as well as other cultural and economic barriers. In that sense, science is a vehicle of global governance. This point is most readily seen in the efforts taken to standardize the public provision of education and health around the world. That science both governs and is governed without being formally constituted as a government implies a paradox. Scientific authority is currently founded on a principle of ‘mutually tolerable ignorance’: while the public understands little of the science it believes, scientists often have no first-hand knowledge of the experiences over which they pronounce.

On the one hand, the public trusts, or at least defers to, scientists, though few non-scientists have ever witnessed how scientists come up with the knowledge on which their judgements and explanations are based, and not many more can recite the catechism presented in science textbooks (Durant *et al.* 1989). People seem to live quite comfortably believing in Newton’s

theory of gravity or Darwin's theory of evolution, even though they are incapable of saying what the theory is or even what does and does not follow from it. Indeed, many philosophers nowadays regard this attitude as a sound epistemic strategy (Goldman 1999). On the other hand, the phenomena that scientists are authorized to judge and explain on the public's behalf typically have been experienced more directly by ordinary members of the public than by the scientists themselves. Thus, we find white, male medical scientists authorized to speak on biological topics ranging from childbirth practices to 'genetic planning', not to mention straight middle-class biochemists and psychiatrists explaining drug-taking and homosexuality. Very few, if any, religions have commanded such blind loyalty on the basis of such little mutual personal understanding between the speakers and the spoken for and about. The reason, of course, is that most religions include a pastoral mission that involves the flock in mastering a watered-down mythical version of orthodox theology and the ministry in regular contact with the spiritual and sometimes even physical needs of its flock. The constitution of science is remarkable in lacking any sense of pastoral mission or, in more secular terms, party politics.

It is also worth recalling a more conventional sense in which the constitution of science has political implications. Consider *The End of History and the Last Man*, where Francis Fukuyama (1992) declared that a liberal democratic future awaited all the world's peoples, courtesy of capitalism's systemic beneficence. Fukuyama was one with his Marxist opponents in pointing to the 'logic of natural science' as plotting an inevitable course that both transcends and transforms even the most historically entrenched of cultural differences. In that sense, science puts an end to history: once the natural trajectory of science is appropriately harnessed to the future of one society, history then simply consists of the rest of the world catching up by repeating the steps originally taken by that society. Until quite recently, this was how both capitalists *and* socialists in the first two 'Worlds' thought that the Third World would be 'modernized'. Socialists pointed to science's role in the creation of labour-saving technologies that eventually undermine the basis for any sharp distinction between the workers and their bosses. For their part, capitalists emphasized the role of science in enhancing people's innovative capacities and hence their ability to compete more effectively in the market-place. The roles assigned to science in the two political economies were different, but both were meant to have globally liberating consequences. Indeed, sometimes it seemed that 'science' was little more than the name given to the putative source of whatever progress the history of politics or economics was said to display.

Thomas Kuhn had a characteristically equivocal way of capturing accounts of science that straddle a description of its actual conduct and the standard it sets for the rest of society. Kuhn professed an interest in accounting for science 'when it functions as it should' (cf. Kuhn 1970: 237). Tactfully omitted from this aspiration was any judgement about how often, if ever, science lives up to its own standards of rationality and objectivity, the

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standards that then provide the normative basis for the 'knowledge-based societies' in which we allegedly live today (Stehr 1994). Consequently, Kuhn, like so many other social theorists of science, suspended his account of science in what may be called a state of 'artificial positivity', i.e. the (dubious) assumption that the clarity with which 'science' can be articulated as a normative ideal is indicative of the ideal's realizability in today's world. Let me briefly explain this concept, as it will suggest the distance between most current thinking about science and the mindset needed to address the governance of science properly.

The concept of artificial positivity is modelled on 'artificial negativity', an expression associated with the arch scepticism of the Frankfurt School's version of critical theory, which saw capitalist complicity in every form of cultural production. Such conspiracy theorizing, albeit conducted at a very high level of abstraction, ends up winning intellectual battles while losing the political war, since it induces the critic's withdrawal from the public sphere, lest the critic be sullied by capitalist conspirators by prematurely endorsing reformist measures (Geuss 1999: Ch. 4). In my usage, 'artificial positivity' represents the complementary attitude that there is nothing for the critic to do because the ideal is already presupposed in everyday practice. In that case, any perceived discrepancies between the ideal and the real are treated as localized incidents, the remediation of which will occur in the long run, either because the system naturally corrects itself or people come to see the discrepancies as systemic virtues in disguise. Sometimes this attitude is cloaked in philosophical high-mindedness. Ironically, the man who has done the most to debunk the wishful thinking that normally passes for such high-mindedness, Theodor Adorno, passed down the Frankfurt School's legacy to someone who has increasingly displayed just that attitude, Jürgen Habermas (Fusfield 1997).

Normative visions of science as the 'open society' are typically subject to artificial positivity in this sense, the result being a bland endorsement of the status quo. For example, the fact that many cases of research fraud are eventually caught by the scientific community is taken to vindicate the self-critical function of science, not to signify a deeper, more systemic problem with the conduct of scientific research. Also, the fact that science displays a pecking order of researchers, institutions and even subject areas that rivals that of any class-based society is presumed to be the desired outcome of processes involving the free and open participation of all members of the scientific community. The fact that these processes cannot be easily specified and that many scientists are clearly dissatisfied with their place in the pecking order are treated as areas 'in need of further empirical investigation', not indirect proof of the artificially positive assumptions made about the realizability of the open society in science today.

Perhaps the most thorough recent defence of a vision of science in this artificially positive mode is Cole (1992), who may be uncharitably read as arguing that everyone who deserves recognition in science eventually gets it – including women and minorities. However, characteristic of this kind

of research, Cole considered only scientists who actually managed to place publications in the leading journals of their fields. He might have told a different story had he considered the number of inquirers who dropped out even before reaching that stage of minimal scientific recognition. Nevertheless, over the past quarter-century, Cole has been one of the most widely consulted sociologists by the US National Science Foundation on science policy decision making.

In light of the above, it is no surprise that the arch sceptics of our own time, the postmodernists, have rejected the open society ideal of science as just so much wish-fulfilment. Nevertheless, the ideal remains sufficiently alive in policy circles and is sufficiently admirable on its own terms to deserve a rearticulation, one that deals squarely with the political and economic conditions that are necessary for its realization. This project goes very much to the heart of my own programme of social epistemology. When I began this programme, just over a decade ago, I wanted to lay the foundations of a sort of welfare economics of science, or 'knowledge policy' (Fuller 1988: 289, 1993b, 1997). While this still captures my general normative sensibility, it has become increasingly clear that the political implications of my work vacillate between liberalism and socialism, roughly depending on whether I have drawn my disciplinary resources from the humanities or the social sciences, respectively. However, implicit throughout has been a commitment to the republican values associated with Karl Popper's (1945) original popularization of the open society.

Republicanism as the political philosophy of an open science

The history of science can be told as a narrative of successive reconstitutions of the scientific polity: i.e. changing definitions of the rights and obligations of both scientific inquirers and the societies housing them. Republicanism represents the ideal state, in that it allows people to speak their mind with impunity. However, this is possible only under specific social and material conditions. When Michael Polanyi (1962) famously articulated the 'republic of science' as an ideal in the 1950s, he crucially failed to specify the relevant background conditions needed to realize this ideal. Specifically, there are communitarian and liberal 'excesses' between which a republican science would need to navigate. However, as we shall see, both are mediated by economic conditions.

Communitarian excess

If scientific utterances carry too much *prima facie* authority in the larger society, then it is easy to see how 'political correctness' might emerge as a countervailing response, especially in communitarian societies where matters of group identity are paramount. For example, because scientific studies that purport to prove the cognitive inferiority of blacks can all too easily

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translate into anti-welfarist and even racist policies, communitarians are tempted simply to prevent the conduct of such studies at the outset; that is, to impose 'ideological censorship'. However, a republican science would take a different tack: to allow the research but sever any straightforward connection between research and policy. This may be done by requiring an alternative scientific analysis of the same data or public interrogation of the research from various interpretive perspectives. Because the republican privileges the expression of alternative and even unpopular views, either option would be preferable to simply preventing the conduct of the research because of its potential for anti-group policies. Moreover, in the particular example, a republican science policy would make it clear that the mere identification of persistent group deficiencies by no means licenses a policy of neglect, or worse, subjugation. After all, most affirmative action legislation was enacted in the USA because a century of formal political and economic freedom had not sufficiently improved the conditions of black people. Here too we have an admission to persistent group deficiencies, but a more positive policy orientation. I shall return to this point at the end of this chapter with the discussion of a 'social inheritance tax'. Of course, there are now budgetary pressures on the state to divest its welfare functions, and so 'scientific' arguments relating to IQ are often made to do the work of economic expediency (e.g. Herrnstein and Murray 1994). This would seem to strengthen the case of the communitarian, who could then turn to the republican and ask: How do you prevent the existence of, say, 'racial science' from turning into a springboard for a political movement like Nazism, which is then democratically brought into power on a wave of pseudo-scientific rhetoric? I shall return to this question at the very end of this book.

Liberal excess

This excess poses a larger and more insidious threat to republican science. It represents the incursion of market values into the sphere of free inquiry, to the point that 'free market' and 'free inquiry' are seen as one and the same. It is epitomized in the idea that you can pursue any research you want, as long as you can find someone willing to pay for it. Before the start-up costs of research became so enormous (especially in the natural sciences), money was something that typically entered the consciousness of scientists only after some research had been completed and its various material rewards were reaped. However, because money is an issue at the outset of research, only research that is likely to succeed (because it fits with a discipline's expectations or it fills a clear niche in the market for new technologies) is likely to be supported. This mentality undercuts the incentive for providing substantial criticism to the fundamental presuppositions of existing research: who is motivated to accept serious criticism of high-energy physics, if its implications include rendering billion-dollar particle accelerators obsolete and forcing thousands of physicists out of work? This question highlights the emergence of 'financial censorship' of inquiry. Ironically, researchers

wishing to pursue lines of inquiry that go against the grain of the status quo are increasingly forced to seek private investment, which only accelerates the flight of science from the realm of public to private knowledge.

In today's increasingly 'liberal' society, the biggest incentive for the support of independent lines of research is the prospect of intellectual property rights. From a republican standpoint, this situation is akin to plundering the commons for personal gain, the ultimate environmental nightmare (Hardin 1968). The tough questions that the republican must face from the liberal are: Is this privatization reversible? How else does one provide an incentive to do innovative research if personal gain is *not* involved? The answer to these questions would clearly involve regulating the financial side of research. In Chapter 8, I speak of 'epistemic fungibility' as an important part of the solution to this problem.

A list of republican regimes typically includes the West's iconic political settings: classical Athens, pre-imperial Rome, the city-states of Renaissance northern Italy, Whig Britain, and the US Constitution's rule by 'division of powers' and 'checks and balances'. Why has it been so difficult to establish republicanism on a long term and worldwide basis? These difficulties revolve around the need for societies to provide the economic security and cultural resources needed to protect their members' right to be wrong, which at a more abstract and collective level amounts to a policy of preventing the past from overdetermining the future. (We shall return to this point at the end of this chapter, when considering the role of social inheritance in science's claims to progress.)

Republicanism's underlying idea is that true freedom requires the expression, not merely the toleration, of different opinions. In that sense, the right to be wrong is not really a right at all but an obligation, much as the 'right to vote' is interpreted in countries where failure to vote is penalized. Thucydides' account of Periclean democracy in fourth-century BC Athens, Kant's definition of the Enlightenment's 'public use of reason' and John Stuart Mill's defence of free speech all share this obligatory character – and here we might add Popper's exhortation that scientists falsify their hypotheses (Haworth 1998: 217 ff.). In all these cases, the idea seems to be that in the performance of the very same speech act – an open critical engagement with peers – individuals simultaneously demonstrate their autonomy and help improve the collective body of knowledge.

If it seems counterintuitive to hold that one is not merely permitted but obliged to speak one's mind, then that is only because we have a polarized image of liberty that occludes the republican alternative. 'Negative liberty', associated with liberalism, is simply the absence of individual enslavement, whereas 'positive liberty', associated with communitarianism, is the realization of one's social station (Berlin 1958). Missing from this dualism is the prospect that the recognition of individual distinction might define one's social position – as in a society that compels its members to take risks, say, by participation in such competitive arenas as games, warfare, the market, or for that matter, science. I suspect that this social vision is omitted because

Figure 1.1 A matrix of socially sanctioned risks

	High separability	Low separability
High closure	Sport	War
Low closure	Science	Business

High v. Low separability: how separable are the activity's goals from the goals of other activities?

High v. Low closure: does the activity have a natural sense of closure?

of the communitarian ideology that underwrites contemporary welfare-state thought, which assumes that the collective absorption of individual risk is a desirable if not an ultimate goal of state policy. However, as we shall see below, this overriding sense of risk aversion – which pervades science policy just as much as state policy – has debilitating consequences for the pursuit of free inquiry.

In Figure 1.1 I have represented the domain of socially sanctioned risk-taking in terms of two dimensions that enable us to see how scientific practice shades off into other activities. The element 'science' in the figure approximates the republican ideal. Here 'science' means inquiry conducted in artificial settings, such as laboratories, by which the inquirer directly studies one thing that is regarded as an indirect indicator of something of greater import. For example, an isolated laboratory experiment may be seen as a kind of match one wins by correctly second-guessing the outcome. Only once it is made clear how the outcome can be used to make inferences about the 'real world' does the experiment become an episode in scientific inquiry. But at that point the import of the particular experiment loses much of its initial sense of closure, since its outcome may be interpreted in various ways, depending on the theoretical context of inquiry and/or the other experimental findings within which it must be situated. In this way, science loses its gamelike character.

However, while an experiment does not become science unless placed in a larger context, that context must always be defined by science itself and not by other activities with which it may become materially involved. This is increasingly difficult, as science requires more human and material resources for its pursuit. Under those circumstances, it is tempting to judge a line of inquiry exclusively as a long-term investment strategy, such that the critical spirit comes to be diverted by the research manager's need to keep a balanced ledger and a full employment record. Thus, science turns into a business, and both share a certain open-endedness whereby no amount of knowledge or money ever seems to be enough. (We shall explore this transformation in the next chapter.) Moreover, one can easily imagine this business mentality acquiring a more focused goal orientation, as in the race to build the atomic bomb or cure cancer, in which all other activities and

resources are swept up. At that point, the conduct of science has mutated into a state of war.

The elusive material basis of republicanism

The realization of the republican ideal presupposes certain material conditions:

- that people's opinions might change for the better as a result of hearing opposing opinions;
- that people need not fear the consequences of their expressed opinions on their material well-being;
- that there is a 'public good' or 'civic ideal' to which people may appeal in deliberation which transcends specific individual and group interests.

Republican policies aim to ensure that all citizens are sufficiently secure in their material circumstances that they are not inhibited from speaking their minds. If you can express your mind with impunity then your ideas can die in your stead, to recall a phrase of Goethe's that Popper liked to use to epitomize the open society. The significance of this capacity should not be underestimated. A frequently remarked obstacle to instituting 'deliberative democracy' schemes (e.g. citizens juries, electronic town meetings) is the tendency for people to reinvent patterns of deference even in arrangements that have been designed as much as possible to be egalitarian (Fishkin 1991; Bohman 1996). While some trace this tendency to the inherently hierarchical nature of human beings (especially when large populations make it 'efficient' to sharply distinguish between leaders and followers), more likely it has to do with the fear of humiliation that comes from making one's mistakes in public (Elster 1993).

The political psychology surrounding the inhibition of republican sentiments is complex. While a measure of economic security is required for the realization of the republican ideal, it should not be assumed that the poorest members of society are the most easily inhibited from expressing dissent. On the contrary, it may be argued that the inveterate poor have little to lose and hence are more willing to speak their minds than those who regard the middle class with striking distance. For example, academia is one social environment that perpetuates a sense of bourgeois self-containment. Despite the clarity with which its 'haves' and 'have nots' are marked in terms of funding, publications and institutional location, very few academics stuck at the bottom of the pecking order ever believe that they are consigned to the dustbin of history (Fuller 1997: Ch. 4). Consequently, even the lowliest contract researchers believe that they are still contenders for tenured posts and hence think they potentially have something to gain by biting their tongues in deliberative settings. In this case, continued faith in an ideology of meritocracy is sustained by the vagueness of criteria for success and failure, combined with the smattering of success stories that compare

slightly favourably with winners of the national lottery. If one wanted to make a case for the irrationality of individuals who opt for the pure pursuit of knowledge in our times, then this would be the place to begin.

The historic virtue of republicanism is its concerted efforts to mitigate, if not outright eliminate, most of the hereditary bases for wealth and power that have all too frequently overdetermined any given generation's level of achievement (Unger 1996). In past republican regimes, people who simply lived off their inheritance were despised and, when possible, dispossessed of their holdings through the levy of a heavy inheritance tax. The beneficiaries of this redistributed wealth were those who were likely to increase the wealth of all in the future (e.g. entrepreneurs) or those who had already prevented it from decline in the past (e.g. military and civil service pensioners). Property ownership, typically a requirement for full citizenship, reflected less a deference to wealth as such than a basic political competency test: how can people be trusted to exercise independent judgement in the forum, and potentially offer their leadership to the entire polis, if they cannot even manage their own affairs? At the same time, the dispossession of inherited wealth was never total, since the market had yet to govern all forms of social interaction. The classical Greek sense of economy as *oikonomikos* still ruled, and so even the most inefficient and unproductive among the wealthy were left enough to maintain a household with some dignity. In that important sense, republicanism was 'pre-liberal' (Polanyi 1944).

A contemporary proposal in the same spirit is the 'guaranteed minimum citizen's income', which reflects that now, after 200 years of liberalism, there is a greater need to protect the poor than the rich from indignity. Moreover, wage labour has gone the way of land ownership in failing to capture the economic preconditions for making a meaningful contribution to society. In general, the value that republican regimes have invested in powers of self-maintenance has related to the source of such regimes' leaders, who may well be chosen by lot, as in the case of classical Athens. No doubt, citizens of our own 'democratic' societies would be gladly taxed much more heavily for improving education if their leaders were selected in this manner. To his credit, John Stuart Mill had figured this out, when he originally called for publicly funded mass education coupled with the use of education as the main criterion for political participation.

Reproducing republicanism has proven elusive, mainly because its identity is so closely tied to its catalytic role in the West's acceptance of the capitalist way of life. Philosophical histories of European politics present republicanism as a transitional phase between the close-knit communitarianism of agricultural societies and the dispersed liberalism of commercial societies (Pocock 1985). There is a comparable philosophical history of science, whereby the ideal of the open society appears briefly realized in the Enlightenment's 'republic of letters' between the clerical authoritarianism of the feudal era and the privatization of intellectual property rights that began in the eighteenth century with the institution of copyright, and accelerated in the nineteenth and twentieth centuries with the expansion of

Figure 1.2 The scientific matrix

Is risk-seeking encouraged?	Is there a public good?	
	Yes	No
Yes	Popperian bold conjecturer	Schumpeterian entrepreneur
No	Kuhnian normal scientist	Cartesian cogito

Figure 1.3 The political matrix

Is risk-seeking encouraged?	Is there a civic ideal?	
	Yes	No
Yes	Republican	Liberal
No	Communitarian	Robinson Crusoe

patent law. According to the tacit conventions by which history is reified as ‘theory’, communitarianism and liberalism – or normal science and technological innovation, respectively – are regarded as ‘pure types’ of governance, while republicanism is taken to be an unstable ‘hybrid’ (especially in the biological sense that hybrids are infertile). For, no sooner had republican regimes eradicated the privileges enjoyed by the Church and the landed aristocracy, than social instability dissolved republicanism’s own civic ideal. In its place emerged, over a couple of generations in the nineteenth century, a new industrial élite nurtured by laws permitting the transmission of acquired wealth to offspring, as justified by a liberal’s new-found sense of individualism – i.e. ‘I am entitled to dispose of my hard-earned wealth as I please’. These legal arrangements begat the great dynasties and monopolies, perhaps most notoriously symbolized by the Rockefellers and Standard Oil. In response, governments had to invent new regulatory powers for themselves that either assimilated industry into the state apparatus (the continental European route) or cast the state in the role of ‘trust-buster’ (the American route). Both routes were designed to recapture, however artificially, the lost world of republicanism’s civic ideal.

My argument so far is summarized in Figures 1.2 and 1.3, which pursue the analogy between the organization of scientific and political life. To conclude this section it will be useful to elaborate upon the two dimensions that define the terms of the analogy: the presence of a civic ideal/public good and the degree of risk seeking.

The presence of a civic ideal/public good

Is there a commonly recognized ‘court of appeal’ that cannot be reduced to special interests? Both special interests and private property presuppose the inviolability of a civic ideal/public good whose subversion would be self-defeating. Special interests are never favoured in a large polity for their own sake, but only to the extent that they can serve the interests of others. Private property is not *sui generis* but the product of a transformed commons. Intellectual property may be regarded in similar terms, namely as the application of universally available principles, or the economist’s sense of ‘public good’ – that is, a good to which access would cost more to restrict than to keep freely available. In this way, the epistemologist’s distinction between ‘pure’ and ‘applied’ knowledge is transformed, in the economist’s hands, into ‘public goods’ versus ‘intellectual property’. Another example of the presupposition of a commons in science is that you cannot oppose a scientific theory without abiding by the same rules of evidence and method that are allowed to your opponent; to do otherwise would be to opt out of the scientific field altogether (Bourdieu 1975). If you want to alter these rules, you must propose the changes openly, which then makes them subject to public scrutiny. The presence of a civic ideal/public good provides an external boundary (or ‘demarcation’) to the political/scientific enterprise (say, by nation or discipline) which enables its internal changes to be clearly tracked and reckoned. At any given point, they constitute what Popper called ‘conventions’ and Kuhn a ‘paradigm’, which are recognized by the republican as necessary evils. (The mistake made by communitarians like Kuhn is to regard the evil as a good, whereas the mistake by liberals is to suppose that this ‘evil’ can be eliminated without negative consequences.)

The degree of risk-seeking

Once the boundary of the polity/science has been set, are there incentives to change its internal constitution? Where no such boundary exists – i.e. societies governed merely by the dictates of individual self-interest – one gets radically polarized responses. Those inclined to risk their lives for their ideas appear as heroic figures, whereas those who do not are seen as self-sufficient, not a burden on anyone else: Schumpeter’s intrepid entrepreneur versus Descartes’ isolated thinker. For their part, republican societies have tended to use military conscription as the link between defending the polity from external threat and enabling citizens to fortify themselves in case their own lives become imperiled by the claims they express in public. This Italian innovation is captured by the second amendment to the US Constitution: the individual’s right to bear arms (Pocock 1975). Without endorsing vigilantism, one can see merit in the idea that tools honed to defend the polity can then be used to encourage the contestation of issues within the polity. Of course, one would like a less violent means of transmitting the relevant

skills and attendant attitudes. Indeed, compulsory citizen education of the sort advocated by John Stuart Mill in the UK and John Dewey in the USA may be seen as sublimating the republican society's interest in cultivating the martial arts. This is perhaps most evident in the rhetoric surrounding science education, which suggests that students are training to enable the nation to be more competitive in the global economic arena, while enhancing their own employment prospects at home.

The slippery slope from republicanism to liberalism

The most obvious difference between republicanism and liberalism is that the liberal does not recognize any sense of collective interest beyond aggregated self-interest; hence, liberalism has found the idea of a 'civic ideal' elusive, if not a complete fantasy. This is because societies dominated by the market mentality – as liberalism invariably is – make it rational for individuals to measure what they say against its likely consequences for their own well-being, assuming no social buffer from the repercussions of having made claims that are ultimately deemed mistaken. Thus, sometimes the distinction between 'liberal' and 'republican' regimes is drawn in terms of the types of freedom their citizens enjoy. In liberal regimes, it is supposed that sheer lack of physical interference constitutes freedom. In other words, if I am not enslaved, I must therefore be free to do what I want, there is no other alternative. However, republican regimes do not accept the premise as sufficient to warrant the conclusion. In addition, republicans hold that people need to act in an environment where there is a good chance that what they say and do will be taken seriously by others, and not simply ignored or become the grounds for the curtailment of their speech and action in the future.

Some aspects of the historical difference between liberalism and republicanism are worth recalling. Here the master is Quentin Skinner (1997), the leading historian of political thought in our time. Skinner regards the seventeenth-century philosopher Thomas Hobbes as an originator of the liberal mentality. Hobbes periodically spoke of people's 'freedom to obey' once the social contract was in place. For Hobbes, the social contract was an artificially enforced situation, in which peace was achieved in a combative 'state of nature' by all parties agreeing to abide by the dictates of some third party, designated as the 'sovereign'. The sovereign was traditionally imagined to be an absolute monarch but could equally be a legislature. In either case, the sovereign enjoys a monopoly on force in the society. Of course, one would technically remain free to violate the sovereign's dictates, but given the low chances of success in Hobbes' ideal polity, such a person would normally be seen as irrational. On the contrary, in a Hobbesian polity, it is more rational to shape one's goals so they have a reasonable chance of being achieved within the constraints laid down by the sovereign. Hobbes' republican opponents objected to the self-debasement bred

by this situation, namely, second-guessing and pandering to the whims of the sovereign: free agents would be reduced to mere courtiers.

In the late eighteenth century, the Hobbesian polity metamorphosed into the ‘constrained optimization’ model of rational action characteristic of classical and neoclassical economics. This change depersonalized the sovereign and devolved its powers, resulting in an ‘invisible hand’ governance of dispersed markets. The objectionable second-guessing and pandering of the Hobbesian regime came to be sublimated into the skills needed for speculating in the stock market and marketing goods at a mass level. In the second half of the twentieth century, this model was enhanced in two respects. The first, due to Herbert Simon (1945), internalized the constraints so that awareness of one’s own finite cognitive capacities in the face of overwhelming tasks set the parameters within which ‘bounded rational’ action could occur. The second is associated with Leon Festinger’s cognitive dissonance theory. It turned the adaptation of ends to fit the available means into an elaborate but unconsciously adopted survival strategy in a hostile world (Elster 1984). Thus, in keeping with the therapeutically oriented welfare state culture, the courtier and the trader yielded to the bureaucrat with an overflowing ‘in box’ in search of mental stability as he or she offered excuses as to why nothing ever seemed to go to plan.

Each stage in this potted history of the liberal mentality shares the assumption that ‘rationality’ – the all-embracing term for normatively adequate human action – can be relativized to virtually any set of constraints. From a republican standpoint, this implies an unwholesome intimacy between the selection of a course of action and the environment in which one is forced to act. For example, a constrained optimization model of rationality can countenance situations in which it would be ‘rational’ to disclose the identity of Jews to the Nazis (if one’s own life were threatened, etc.). In contrast, a republican would say that, in such situations, what one says and does is inappropriately influenced by the anticipated consequences for one’s own well-being. Simply put, it becomes impossible to propose an idea without staking one’s life, a situation which is *intrinsically* irrational. In this respect, republicanism is exceptionally sensitive to the social and economic conditions required for institutionalizing the metaphysical distinction between reality and its representations: I should be able to oppose your ideas without threatening your life. In both liberal and communitarian regimes, this distinction is collapsed, albeit with rather opposing results, as suggested by my brief survey of their ‘excesses’ in the previous section.

In science today, at most its élite members live in a republican regime, while the rest live in a liberal regime where their freedom is in practice severely constrained by whether they will offend a prospective employer or grant reviewer. I say ‘at most’ because the symbolic capital accumulated by scientists has come to be so bound up with ordinary economic capital (i.e. better = richer) that even dissenting members of the élite can have their right to be wrong seriously threatened by the ‘liberalization’ of inquiry.

One such élite researcher is Peter Duesberg, Professor of Cell Biology at the University of California, Berkeley. Duesberg was stripped of his outstanding researcher status at the US National Institute of Health (which had given him virtually a blank cheque for research) for having publicized his scepticism that AIDS is caused by HIV, arguing that AIDS may be a straightforward public hygiene problem related to the lifestyle of gay men that then leads to the breakdown of their immune systems. Serious consideration of his hypothesis would clearly challenge the American medical establishment's backing of a strong HIV-AIDS link, while rekindling an unwanted public debate on the ethics of homosexuality. Of course, in keeping with US First Amendment rights to 'free speech', Duesberg did not lose his job or get thrown in jail, but his research funding and book contract were withdrawn, making it difficult for him to develop and advocate his position effectively.

In contrast, consider the case of John Bockris, Professor of Chemistry at Texas A & M University, an élite dissenter who has successfully adapted to the erosion of republican science. He has steadfastly supported cold fusion as an important key to alternative energy research, even in the face of withering critiques from the physics community (Close 1991). Though widely criticized as high-tech alchemy, Bockris' lavishly and privately funded research has insulated him from calls by colleagues to divest him of his professorship. However, were Bockris to discover a commercially viable cold fusion process, it would be immediately patented as a technology and hence taken out of the domain of pure inquiry. The point here is that the motives for long-term investment in wild ideas should not be confused with the pursuit of knowledge for its own sake; hence the distinction between liberal and genuinely republican science.

Fear and anxiety infiltrates the day-to-day operations of science even when scientists are not proposing controversial hypotheses. This fact is obscured by the norm that Robert Merton (1973: 267–78) euphemistically labelled 'communism', which in practice means that scientists have no choice but to share data and credit if they expect to be supported in the future. On closer inspection, a mafia mentality turns out to be at work, itself another by-product of the liberalization of science. Thus, in good Hobbesian fashion, the supposed 'community' of science refers to little more than the fact that everyone equally suffers under the same threat.

Consider the peer review processes that govern scientific publications. They essentially provide insurance against risk: an individual scientist is allowed to say only as much as his or her peers can tolerate, and in return they absorb collective responsibility so that he or she does not have to bear the burden of proof alone. Readers can simply rely on the judgement of the journal's editors to vouch for the veracity of the author. However, this insurance is purchased at a cost: namely, that the contestation of already existing claims to knowledge is kept to a minimum and, wherever possible, pre-empted by gestures to portray one's work as cultivating a domain charted by previous researchers. After all, a print forum that publicized

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knowledge claims only to have them seriously challenged in subsequent issues would appear to be promoting more a clash of ideologies than the collective search for truth. In that sense, science is a society designed to suppress conflict rather than resolve it through either a peaceful consensus or open warfare.

Increasingly, the main field for containing the possibility of open dispute in science is 'research ethics' (Penslar 1995). The next section provides a brief introduction to the thinking that informs the field, which attempts to remedy the worst excesses of liberalism without addressing the background conditions that prevent the constitution of science as a republican regime.

Research ethics as the liberal ideology of scientific governance

Once upon a time, 'research ethics' were what protected the rights of subjects in the research conducted by psychologists and medical scientists. Now, it seems, the rights of the scientists themselves require protection. Reflecting the complex social environment in which science is practised these days, research ethics has expanded to become a field in its own right. In the USA and Scandinavia, one can even hold a chair in research ethics. A research ethicist is expected to be expert in disputes involving the assignment of intellectual property rights, as well as in the detection and prevention of professional misconduct. However, their academic credentials need not be in a laboratory science. Indeed, many are primarily trained in philosophy, theology or anthropology.

In 1995, the American Association for the Advancement of Science conducted a computer interactive project, 'Science Conduct Online', which lasted for two months and promoted research ethics as a component of the professional science curriculum. The project was heralded by the 23 June 1995 issue of *Science* magazine, which reported on the different contexts in which research ethics is currently practised in the USA: week-long workshops that teach scientists how to teach research ethics to their postgraduate students, high-profile lecture series in which leading scientists air their ethical quandaries, or simply informal lunchtime 'bull sessions'.

Five 'experts' (as *Science* dubbed the ethicists) were asked to present hypothetical ethical problems or 'scenarios' that can arise in the conduct of research. These were put online and publicized throughout the Internet. People were invited to propose solutions, to which the research ethicists would offer their reactions, hopefully leading to some deep and interesting insights about the research process. In the project's two months, its website was consulted over 3000 times, with over 30 people openly engaged in dialogue with the experts. Though mainly American, they represented a cross-section of the settings in which science is conducted today: academia, business and government, as well as student and unemployed scientists. Few had any trouble relating the scenarios to their own experiences. I was

one of the 30 who openly engaged with the research ethicists, and perhaps the only one not professionally trained as a natural scientist. However, as a philosopher and sociologist of science, I wanted to determine the exact nature of the research ethicist's 'expertise', and its relationship to the scientists' own views about good and bad research conduct. The use of scenarios to demonstrate the need for research ethics was familiar as the method used to justify the existence of other branches of applied philosophy, such as medical ethics and business ethics.

The five scenarios had certain common features. First, they often lacked details that would probably make a difference to whether an act was deemed ethical or unethical. However, this sketchiness was partly deliberate, since the cases were meant to function like Rorschach inkblot tests that encourage the viewer to 'read in' their own perspective. Second, all the scenarios were set in large university laboratories, where ethical concerns emerged because of uncertainties over the ownership of intellectual property. A third and subtle feature was that the narratives tended to portray the ethical quandary as symptomatic of a communication breakdown. Finally, and subtlest of all, even when women were presented as authority figures, they were generally portrayed as being in the ethically most vulnerable position.

One scenario featured the plight of Jessica Banks, a recent PhD who was about to take up a tenurable appointment and wanted to take the notes she had made while working on a research project with her major professor. However, the professor refused to grant permission and chastised Banks for the mere suggestion that she take something that belonged to the laboratory. Afterwards, a colleague of Banks said that she should just copy her notebooks at night when no one was around. Was this good advice?

Most respondents thought that Banks should copy the notebooks. Disagreement arose over whether she should do it on the sly, as her colleague suggested. Few believed that the notebooks belonged to 'the laboratory', since it was easy to imagine the professor taking the notebooks were he to start up a lab at another university. While he would probably be prevented from doing so in an industrial setting, as a senior academic authority his word was basically law – that is, unless there were explicit guidelines. Here we see a potential role for the research ethicist in laying down the proprietary relationship between the knowledge produced in a laboratory and the knowledge producers.

However, there is a difference between preventing unfortunate situations like the one described in the Banks scenario and dealing with such situations once they have arisen. The online experts were generally more adept at the former than the latter task. In the Banks scenario, the resident expert suggested that Banks offer to collaborate with her major professor. While some found this strategy persuasive, others wondered whether this was a realistic option, given the power differences between Banks and her major professor. Indeed, some respondents went so far as to suggest that Banks cut her losses and move on to another project, rather than risk receiving a negative letter of evaluation from the professor.

An interesting assumption made by both the expert and the respondents was that copying the notebooks would be tantamount to taking them, at least in terms of the professor's ability to use the data to the same effect. This assumption feeds into a larger set of issues touched upon by another scenario. In this case, one scientist wanted to publish a paper that analysed and drew scientifically interesting conclusions from data originally collected by another scientist who never published his results. This situation is common whenever scientists do contract-based research for government. What government considers useful data may not strike the contracted researcher that way. Nevertheless, the researcher may be well positioned to provide the data and may need the money to supplement her grant for research that she finds scientifically interesting. But suppose, three years after the information is made generally available, another researcher decides to capitalize on it? According to the scenario, the scientist who originally collected the data wanted to stop publication of the second scientist's paper, which had already been accepted by a major professional journal. Once again, the contrast with the business world was instructive. It would seem that the original scientist's contract with government was fairly straightforward. It required that services be rendered to the client in exchange for money – nothing more and nothing less. However, because these 'services' involved the production of knowledge, several people thought that the scientist had additional proprietary rights over his 'products' even after the terms of the agreement had been satisfied.

My own view was that the first scientist's failure to publish in a timely fashion implied that he had not seen sufficient scientific merit in his own data until his rival came along. Now, to hold up the second scientist's article would only serve to encourage scientists to start up many projects but only complete those that attract the interest of other researchers. After all, science is risky business. Still, several respondents wanted some kind of intellectual property 'insurance'. The research ethicist behind this scenario wisely argued that scientists need to learn that analysing data is just as much serious scientific work as collecting data. And given that few scientists are equally good at collecting and analysing data, the two tasks should be clearly separated in the design of grant proposals.

The most widely discussed scenario concerned a lab director who, in the course of reviewing a grant proposal, learned of a technique that could help one of her postdoctoral fellows make significant progress on an experiment. Should she tell him? The issue is complicated because a grant proposal's 'confidentiality' often allows the reviewer to seek technical advice from a colleague at her own institution. Both the resident expert and most respondents felt that confidentiality had to be protected at all costs, and many suspected that some of their best ideas had been 'stolen' in similar situations. This reaction struck me as excessive, especially once it was made clear that the technique was already public knowledge and not the grant proposer's innovation. However, it does highlight the extent to which scientists think of the products of their labours – including their literature

reviews – as personal property, even if that attitude only serves to impede the collective pursuit of knowledge. Instead of questioning the social conditions that have made this proprietary mentality second nature, research ethicists seem content with sheltering scientists from its harsher consequences. Is that all we can hope for from a field that calls itself ‘ethics’? Perhaps in a liberal regime, but not in a republican one.

The cardinal republican strategy: shoring up the commons by taxing social inheritance

In contrast to the fear and anxiety that is barely governed by research ethics in liberal regimes of science, republican science is inquiry in a state of ‘permanent revolution’, an expression that Popper (1970) originally adapted from Trotsky to distinguish his position from Kuhn’s (1970) more sanguine attitude to normal science. To be sure, the expression is an oxymoron, albeit a suggestive one. If people are always encouraged to be critical of their current situation and to propose better alternatives (rather than simply wait until the status quo fails on its own accord, as Kuhn suggested), then revolution loses its violent character, mainly because people come to realize just how arbitrary the status quo really is – perhaps little more than a historically entrenched accident. In that case, they will be less inclined to superstitiously associate longevity with destiny. They will realize that there is just as much risk in continuing the status quo as in breaking with it, at which point the past comes to be treated more as a contingent resource than a necessary burden or even a legacy.

In economic terms, the permanent revolutionary is thus concerned with ‘opportunity costs’ – that is, how many possible futures are closed off by my own action? Under these circumstances, induction and its rhetorical twin, presumption, lose their force as principles of rational inference. At the level of economics, republicans treat initial investments into a particular line of inquiry as a ‘sunk cost’ whose continuation must be gauged on the likelihood of future return and not merely on the sheer irretrievability of what was initially invested. Such a strategy would immunize science policy-makers against the belief in self-fulfilling prophecies and, still worse, the idea that wrongs sufficiently compounded over time add up to a right, as in the supposition that the vast majority of scientists whose work goes unrecognized have probably not contributed a great deal.

This brings us to what Robert Merton (1973: 439–59), in another of his classic euphemisms, called the ‘principle of cumulative advantage’. A republican science would treat this principle as comparable to the untaxed inheritance of acquired wealth. We shall periodically revisit this principle in the course of this book, especially in Chapter 5. Merton originally had in mind the selective advantage afforded to scientists trained at elite institutions, considered in relation to their ability to accumulate grants, publications, posts and honours. Scientists who distinguish themselves early in their

careers – a group that nearly coincides with those who participate in the ‘old boy networks’ of the prestigious research universities – tend to be the ones who distinguish themselves later on. As in capitalism, so too in science: the rich get richer, the poor get poorer. Of course, such a pattern is common to numerous large organizations. However, the difference is that in those other cases we often say that the organization suffers from a ‘structural bias’ that systematically underutilizes the pool of available talent. Why not the same diagnosis in the case of science? Indeed, the American policy scientist William Dunn has neatly characterized this problem as ‘the tragedy of the epistemic commons’, whereby the reinforcement of individual success results in overgrazing by the scientific élite.

Nevertheless, Merton’s followers have raised this tendency to a kind of ‘invisible hand’ at work in science (not unlike the classical political economists who, in the eighteenth century, justified the conversion of pastures to industrial sites). Since the distinction between those at the cutting edge of research and the rest of the pack is usually not the result of any central planning board, it must be the result of the collective spontaneous judgements of scientists themselves: so say the Mertonians. Neglected here, of course, is the role that the sheer size of the initial investment into a line of research (in terms of both labour and capital) plays in giving it forward momentum. Thus, the quick-and-dirty indicator of scientific progress in our times – its irreversibility (i.e. the inconceivability of turning back on an inquiry once begun) – is measured more in terms of the amount that has so far been invested than in terms of the benefits that have so far been realized. This point is central to the scale and scope of the scientific enterprise, to be considered in the next chapter. However, we can already appreciate that, from a standpoint critical of the principle of cumulative advantage, the US Congress’s decision in 1993 to halt the construction of what would have been the world’s largest particle accelerator, the Superconducting Supercollider, struck a blow for republican science against the superstition of irreversible progress. But generally speaking, what is the antidote to this pernicious mentality?

My first piece of advice is to regard the predictability that an Ivy League or Oxbridge degree will bring scientific success in the same spirit as Marxist and institutionalist economists have regarded the stability of the major corporate dynasties or oligopolies in capitalism – that is, as symptomatic of inequities in the way science has come to be institutionalized, not a sign that science has managed to sort out the wheat from the chaff. The appeal of both ‘Big Science’ and ‘Big Business’ is grounded in a myth of continuity, be it of a research programme or a corporate track record. In the case of the former, one speaks of the regularity with which such a programme can come up with novel findings; in the latter, the corporation’s experience in manufacturing or, more likely these days, marketing new products. To political economists such as Thorstein Veblen and Joseph Schumpeter who, in their different ways, placed a greater value on the spirit of enterprise – that is, the periodic destabilization of stagnant markets – than on the sheer accumulation of wealth, the illusion of a ‘weight of the past’ sustained by

the absence of a redistribution policy appears as more millstone than milestone to the continued existence of capitalism. Basically, capitalism's competitive edge is gradually eroded, as governments take the soft option of allowing wealth to be perpetuated in ways that resemble its current form.

Here history provides a valuable lesson. In the early days of capitalism, before high productivity was realized, inheritance taxes were seen as the only equitable policy for redistributing wealth to ensure that markets remained free to all enterprising individuals. However, with the advent of high productivity came the modern welfare argument for taxation, which assumes the possibility of benefitting the poor without disadvantaging the rich. Thus, the rich would be taxed mainly for what the state regarded as surplus income that could be used more efficiently for the overall benefit of society by being given to the poor. Arguments then revolved around the relative efficiency of taxing the rich in this way *vis-à-vis* providing them with incentives to invest their surplus income.

But of course, seen in historical perspective, this debate amounts to little more than cosmetic accounting, whereby income levels are narrowed but the difference in relative advantage that the poor and rich receive from each unit of income remains unchanged. Given that on modern welfare schemes the rich do not decline in what is nowadays called 'social capital' (Coleman 1990: Ch. 12), they are able to do much more than the poor with whatever income they earn, no matter how much their surplus is then taxed. In that case, a social inheritance tax is called for, the exemplar of which today is affirmative action legislation (Cahn 1995). In the pages that follow, I periodically return to this general strategy for redressing the inequities of Big Science. But in concluding this chapter, let me focus on its implications for the distribution of academic posts.

It is not sufficient for graduates of first-rate institutions to be hired by, say, third-rate institutions and vice versa. The difference in social capital remains and, in the long term, one would expect the former group to regain much of their original advantage. Instead, redistribution must occur in terms of who is trained at those different quality institutions in the first place, with an eye toward enabling graduates to enter the employment market with roughly equivalent amounts of social capital and hence to be evaluated solely on their own merits. Here it is worth recalling that a person's social capital is marked mainly by whether their background and training enables them to connect with others so as to realize their own goals. In other words, the same knowledge content may contain different amounts of social capital, depending on the situations in which knowledge is able to bring about action. The closest that science policy has come to recognizing and treating the issues surrounding social capital occurred during the American New Deal of the 1930s, which unsurprisingly coincided with the period in which science was most organically integrated in public policy. We shall revisit its initiatives in Chapter 7. However, in what immediately follows, we consider the current situation, in which science policy and public policy appear in collusion to concentrate social capital.