



## Unacceptable Images

WHAT IS THE ROLE OF THE SCIENCES in a democratic society? Some people, let us call them the “scientific faithful,” say this: “The sciences represent the apogee of human achievement. Since the seventeenth century, they have disclosed important truths about the natural world, and those truths have replaced old prejudices and superstitions. They have enlightened us, creating conditions under which people can lead more satisfying lives, becoming more fully rational and more fully human. The proper role of the sciences today is to continue this process, by engaging in free inquiry and by resisting attempts to hobble investigations for the sake of any moral, political, or religious agenda.”

The faithful do not believe that scientific research is completely free of moral constraints. They would agree that investigators must be honest in the presentation of their findings, and they would concede that some methods of inquiry cannot be tolerated. Mindful of the appalling activities undertaken by the Nazi doctors and of the Tuskegee syphilis study (in which black men were left untreated “for the sake of science”), they recognize that the conduct of experiments cannot override human rights—or, perhaps, even the rights of some animals. However great the intellectual benefits of disentangling the roles of nature and nurture in human development, it would be morally monstrous to breed “pure lines” of children and rear them in carefully calibrated environments. So when it is claimed that inquiry must be free, what seems to be intended is that moral, political, and religious judgments should not enter into two important contexts of decision: the formulation of projects for scientific inquiry and the appraisal of evidence for conclusions. The questions investigators address should not be limited by the ideals and the fears that happen to be prevalent in human societies. Nor should we deceive ourselves by believing

what we find comfortable when that belief would be undermined by available evidence. *Sapere aude* remains our proper motto.

Others think differently. On their account, the vision just sketched is a myth. Would-be debunkers believe that it is very much in the interests of those who are currently in power in affluent societies to cultivate the idea of a pure science that stands free of moral, political, and religious values, and that the myth serves as a tactic for excluding viewpoints that the powerful would like to marginalize. The minimal criticism is that decisions about which inquiries are to be pursued are always made by invoking judgments of value. Many would add to this the suggestion that there is no objective notion of “the evidence,” that decisions about which “scientific conclusions” to accept are always made on the basis of moral, political, and religious values. A more extreme critique would argue that the idea that the sciences deliver to us truths about nature is another part of the myth. In the end, institutionalized science comes to seem like an effective propaganda machine, serving the interests of the elite classes and imposing its doctrines, ideals, and products on the marginalized masses in much the way that politico-religious institutions of the past managed so successfully. Science (with a capital “S”) is the heir of the Catholic Church and the Party.

Neither of these images is acceptable. Each contains elements that can be used in crafting a more adequate vision. My aim is to articulate that vision. In rejecting both the image of the scientific community as secular priesthood and its polar opposite, I offer a conception of the scientist as artisan, as a worker capable of offering to the broader community something of genuine value, whose contributions can be, and should be, responsive to a much wider range of concerns than are usually taken to be appropriate. That, of course, is only a sketch. The plausibility of the full picture will depend on the details.

Let us begin again, more concretely. There are several places at which contemporary scientific research inspires reflective people to ponder the value of lines of inquiry that are proposed and to invoke one of the images I have characterized as unacceptable. Without any suggestion that all the nuances of complex debates will be captured, it will be worth reviewing a few examples.

Consider first the Human Genome Project. Governments throughout the affluent world, but most particularly in the United States, have contributed large sums of money so that researchers will map and sequence the genome of our species (or, more exactly, a bundle of segments of DNA drawn from a small number of human beings) and the genomes of certain carefully selected other organisms. Public defense of the project often emphasizes the medical benefits that will flow from the expansion of detailed knowledge about human genes. Privately, policy-makers and politicians who favor the project talk more frequently of the economic benefits of engaging in it, the advantages of building or maintaining a lead in biotechnology, while scientific researchers, away from the

microphones and cameras, point out the ways in which a huge archive of sequence data will help the “basic biology” of the new century. All this is readily comprehensible. For the economic consequences and the consequences for biological research are far more definite than the nebulous payoffs for human health.

It is already clear from cases in which we have achieved molecular insights about the causes of disease that there may be no obvious way to apply those insights in treating, curing, preventing, or ameliorating the malady in question. The molecular details underlying sickle-cell anemia have been known for half a century without yielding any successful strategies for tackling this disease. Yet there are stories of small advances. Thanks to our ability to identify alleles implicated in cystic fibrosis, it is now possible to diagnose children more quickly and to use techniques that reduce the frequency and intensity of the crises to which those who have the disease are subject. So long as one emphasizes improvements in diagnostic testing and partial gains in coping with some diseases, it is quite reasonable to claim that the genomes project (as it would more aptly be called) can bring some medical benefits. Furthermore, as we look into the future, enhanced understanding of basic biology *may* bring, several decades or a century hence, significant breakthroughs in the treatment or prevention of diseases that cause suffering and premature death for millions. It would be unwise either to rule out that possibility or to stake the (research) farm on it.

Unfortunately, as should by now be abundantly obvious, the explosion of genetic knowledge will have immediate consequences of a much darker kind. Within a decade, biotechnology companies will be offering hundreds, if not thousands, of predictive genetic tests. Given the character of the practice of medicine in much of the affluent world, it is highly likely that a significant number of people will confront information that is psychologically devastating, or be excluded from a job on genetic grounds, or be denied insurance through genetic discrimination, or face an acute dilemma about continuing a pregnancy. These consequences have been amply discussed by knowledgeable and well-meaning people, and, in all cases except the last, the solutions, in principle, to the problems are not too hard to find. Nevertheless, over a decade after the genomes project began, virtually nothing has been done to alleviate readily foreseeable harms. That fact is especially noteworthy, given the decision, made at the beginning of the project, to undertake a thorough exploration of its ethical, legal, and social consequences. In the United States that commitment was expressed in setting aside a small percentage of the (very large) funds expended on the project, and many of the suggestions for avoiding the difficulties of the new age of genetic testing come from research that has been supported in this way.

Turn to a second example. At about the same time that molecular biologists were persuading the U.S. Congress to fund the genomes project, an extremely

prestigious group of physicists failed in their attempt to obtain public money to build the superconducting supercollider. In this case, the character of the public defense coincided with the private justifications given by the scientists involved. The request for a sum an order of magnitude larger than that expended on mapping and sequencing genomes was to build a facility in which minute constituents of matter could be smashed into one another at velocities considerably greater than those ever previously achieved, in the hope of discovering a rare and evanescent product of the collisions. Some politicians were probably swayed by the thought that their local constituents would benefit from jobs created by the project, but, for the majority, the decision turned on whether a considerable sum of public money should be spent in the hope of confirming and developing an esoteric theory about the ultimate constituents of matter. Physicists were eloquent in explaining how the facility they proposed was needed to continue probing the character of fundamental particles, how their planned investigations extended a line of inquiry that had given rise, successively, to the atomic theory, to conceptions of atoms as composed of elementary particles (electrons, protons, neutrons), to the discovery of quarks and the partial unification of accounts of the basic physical forces, but those who held the purse-strings were eventually unmoved by dreams of a final theory, perhaps viewing the accelerator as an expensive plaything that would generate nothing outsiders could appreciate or understand. They judged other demands to be more urgent.

In other instances, a line of proposed scientific research may be evaluated not as insufficiently beneficial but as genuinely harmful. For at least a century, the general public has been periodically informed that careful biological investigations have revealed unpleasant truths about the natural differences among members of particular groups. Inequalities in performance with respect to tasks that are socially valued have been unmasked as the result of unmodifiable characteristics, and, more or less regretfully, the investigators and those who have popularized their findings have maintained that any policy of eradicating inequalities is doomed to failure. No matter how hard we may try, there are limits to our power to boost I.Q. or to make the upper echelons of the professions available to groups that have been historically disadvantaged (people with two X chromosomes or with a tendency to produce melanin in their skin cells). In many instances, those who champion this kind of research claim that the problems they are addressing are too important to ignore, and that an enlightened social policy must be based on an awareness of the fixed obstacles that block paths we would like to take. When these defenses are challenged, the advocates can fall back on the importance of knowledge in general, and of self-knowledge in particular, independent of any practical consequences. Their opponents sometimes argue that research of so sensitive a kind must be held to stringent standards of evidence, and that socially consequential claims should not be accepted on what they see as the flimsy reasoning being offered. More fundamen-

tally, they may draw from the dismal history of efforts to trace a biological basis for social inequality the conclusion that we have good reason to believe the appropriate standards of evidence to be simply unattainable. Taking an even more radical step, they may suggest that, even if true, these are not matters about which we should want to gain knowledge. The proposals for more research on differences due to sex, race, or class thus face the charge that the envisaged inquiries are morally suspect.

The long sequence of investigations to which I have just alluded gives rise to moral debate because the acceptance of some scientific doctrines would affect the lives of people in very obvious ways. My final example steps away from the mundane consequences, the everyday shocks that types of human beings are differentially heir to. A commonplace about the growth of the sciences is that, at various times, a new proposal has profoundly disturbed reflective people, causing them to re-evaluate, and even abandon, some of the central beliefs that have given shape and significance to their lives. The impact of Darwin's ideas on human aspirations and self-conceptions is reflected in his first disclosure of his theory to his close friend Joseph Dalton Hooker: "It is like confessing a murder," he wrote (and he meant it). Even today, of course, people continue to resist the claim that there is overwhelming evidence that Darwin was right about the history of life, and their struggles with his doctrines often take the form of conjuring a conspiracy against religion and suggesting that this is a place in which science has been distorted by prejudice. Ironically, the conception of Darwinism itself as a religion masquerading as science is not far from some academic suggestions that *all science* is permeated by prejudices and social values, concretely expressed in the example of evolutionary theory by claiming that we should understand Darwin's triumph not in terms of his evidence and sound arguments but in his ability to resonate the values of competitive, Victorian, bourgeois capitalism.

The scientific faithful have familiar ways of responding to the issues posed by the examples I have offered. Consider, first, the genomes project. The beginning of wisdom, the faithful will insist, is to distinguish sharply between science and technology. There are scientific findings about the relative positions of genetic loci on chromosomes and about the structures of the alleles at those loci, and there are technological applications of those findings within agriculture, medicine, criminology, and other social ventures. Science proposes, society disposes. On one forthright conception of the proper role of the sciences, although we may appraise the moral status of technological ventures, the scientific research itself is entirely neutral. So resolute a stance might provoke doubts once it is recognized that there are extreme cases in which it appears that *any* application of a piece of research, within the kinds of societies we can plausibly envisage, would prove destructive: consider, for example, research that reveals how

cooking just the right combination of broccoli, bananas, and bluefish (or other readily obtainable ingredients) would generate an explosion that would make Hiroshima and Nagasaki appear as damp squibs. When the only consequences of applying a scientific result are so clear, and so clearly awful, then even the faithful may allow that there is a moral imperative to desist. Yet this, they are likely to suggest, is truly an extreme case. For virtually all scientific research, the consequences are unpredictable and the harms and benefits of technological applications incalculable. In such circumstances the value of the knowledge, for its own sake and for the sake of future developments to which it may lead, should prove decisive. So, in the particular instance of the genomes project, we cannot say in advance what the balance of good and bad results will be. Scientists act responsibly in gaining deeper biological knowledge and in deferring to others the problem of making the best use of what they find.

An extension of the same line of argument portrays the decision not to fund the superconducting supercollider as myopic. The value of a scientific inquiry cannot be identified just by considering the set of technological applications to which it gives rise. To discover the Higgs vector boson (the elusive particle that the apparatus was designed to hunt) would be to take a further step in the great intellectual adventure of uncovering the structure of matter. Independently of any practical spinoffs from the experiments, achieving a clearer picture of the fundamental constituents of the universe would be worthwhile for its own sake, just as it is valuable for us to know the major characteristics of our galaxy, the processes that formed our planet, and the history of life on earth. Not only do such cognitive accomplishments vastly outweigh the kinds of pragmatic concerns that figure in budgetary decisions, but they often point in unexpected ways toward future lines of scientific research that will ultimately bequeath to our descendants a vastly wider range of practical options. As the faithful like to emphasize, the history of science is full of examples in which work that initially appeared to lack any practical value proved to be crucial for subsequent developments that spawned a host of welcome technological applications: abstract approaches to computation gave birth to the word processor and the internet; breeding experiments on fruit flies eventually yielded medical genetics; and so it goes. The decision against the supercollider both ignored the intrinsic benefit of the knowledge it would bring and forgot the historical lesson that the pursuit of fundamental science brings long-term dividends.

Elaborating the argument still further, the faithful approach my third and fourth examples. They recognize that, in the short term, the articulation of unpleasant scientific truths may cause pain and suffering, and may even affect most those who have been victims of discrimination in the past. It is important, they will agree, to do whatever can be done to ensure that findings about human nature are translated into social policy in ways that are sensitive to the needs of the disadvantaged. Yet to produce an enlightened social policy we require the

clearest possible account of human beings and their needs. As in other scientific cases, there is an intrinsic benefit from arriving at knowledge—perhaps an especially rich benefit when the subject is ourselves—but here there is also the practical gain of an ability to design social institutions that does not go astray because of illusory hopes. Much as we might like to believe particular things, we gain from knowing the truth about ourselves and from putting our knowledge to work.

My final example encourages a development of the theme. Even when there is no practical benefit from applying our new self-knowledge in social policies and even when the knowledge may deprive us of comforting illusions, we are still better for having it. Human beings participate in a common enterprise of fathoming nature, and that enterprise is one of the chief glories of our species. Or, to put the point differently, to shun knowledge because it might appall us is to betray an important aspect of our humanity.

In the responses I have put in the mouths of the scientific faithful, we find a number of philosophical theses. The sciences can provide us with knowledge of nature. They have a definite aim, namely to offer knowledge that is as systematic and complete as possible. That knowledge can be used for practical ends, but the moral appraisal of the uses is properly directed at technology and public policy, not at science itself. Besides its practical benefits or harms, the knowledge has intrinsic value, and that value typically overrides mundane practical concerns.

The most popular recent criticisms of “scientism” focus on the first thesis. In many academic circles, it has become increasingly popular to deny the claims of the sciences to yield knowledge (truth, we are often told, is either unattainable or a notion that is *passé*). As I shall try to show, this is an unfortunate way to join the debate, for the serious concerns about the credo of the faithful should focus on the subsequent theses. Can we really make sense of the idea that sciences have a single definite aim? Can we draw a morally relevant distinction between science and technology? Can we view the kind of knowledge achieved by the sciences as having overriding value?

My eventual aim is to address these questions. First, however, we must become clearer about the notions of truth, knowledge, and objectivity. So I begin with them.

## SEVEN



# The Myth of Purity

CONSIDER A STANDARD DEFENSE of unpopular scientific research. Moral, social, and political concerns, it's frequently suggested, should not be invoked in the appraisal of investigations. The sciences seek to establish truths about nature. How the resultant knowledge is used is a matter for moral, social, and political debate, but it is intrinsically valuable for us to gain knowledge. If the circumstances in which knowledge is applied are likely to generate harmful consequences, then that is a sign of defects in the social milieu that surrounds the sciences, and, ideally, we should try to gain the knowledge and remove the defects.

As I remarked in chapter 1, everyone will agree to qualify this defense in one way. Investigations involving procedures that would violate the rights of subjects (systematic torturing of neonates to measure their capacity for pain, for example) are properly rejected on moral grounds. Critics of the defense often seek further concessions on two grounds, first by supposing that moral, social, and political values affect decisions about which projects are worth pursuing, and, second, by claiming that such values partially determine which statements are accepted as "true."

In the preceding chapters, I have been disentangling the criticisms. There is no need to abandon the everyday conception that inquiry yields truth about independent objects. Nor should we suppose that ideals of objectivity are misguided and that, because of rampant underdetermination, scientific decisions are made, perforce, by invoking moral, social, and political values. Yet my account of the ways in which our evolving interests draw new boundaries in nature and of how the concept of scientific significance reflects contingent interests enables us to see how to develop the first criticism. The standard defense of

the last paragraph depends on a view of the aims of the sciences we ought to abandon.

All kinds of considerations, including moral, social, and political ideals, figure in judgments about scientific significance, and hence in the evolution of significance graphs. Inquiries that appeal to us today, and that we characterize as epistemically significant, sometimes do so because of the practical projects our predecessors pursued in the past. With our eyes focused on the present, it's easy to deny that these inquiries are in any way connected with broader values. A longer view would reveal that the questions we pose, the apparatus we employ, the categories that frame our investigations, even the objects we probe, are as they are because of the moral, social, and political ideals of our predecessors.

It's hard not to sympathize with the physical chemist who dismisses the idea that his research is suffused with the values of bourgeois males of European descent, and who bluntly declares that he's just out to analyze (or synthesize) the molecules. When the critique is directed against modest realist claims, that is when it's suggested that the values in question are reflected in the structures the chemist presents in his research reports, then the charge lapses into absurdity. Yet when we formulate the worry as one about scientific significance, matters are different. Why have those molecules been selected for analysis or synthesis? Or, in some instances, why do those molecules exist at all? A significant number of contemporary investigations go forward because entrepreneurs believe that studying *just these* molecules will help increase their profits. Even when such direct links are absent, however, some lines of chemical inquiry take the form they do because of the practical decisions of earlier generations. It seemed morally legitimate to previous researchers to find ways of ameliorating the debilitating effects on workers of the hazardous environments in which they labored, and so to focus on the problem of understanding certain complex molecules and their interactions. Some generations later, the chemist wrestles with the theoretical problem of fathoming a molecular structure without any conception of the filiations that connect his research with a past policy of "protecting" a group of workers, or with the future applications to which his findings may give rise.

We need to scrutinize the myth of purity. The most popular form of the myth supposes there is a straightforward distinction between pure and applied science, or between "basic research" and technology. I shall try to show that these divisions are not so simple.

Pure science isn't differentiated from applied science or technology by the sites at which it is practiced. Industrial laboratories contain "pure" researchers, and academic environments harbor people dedicated to technological ventures. Nor can we make a separation in terms of products. Basic science produces devices as well as knowledge, and technology sometimes yields knowledge as well as devices—indeed it's tempting to argue that the manufacture of a device inevitably

brings with it new knowledge about the ways in which parts of nature can be harnessed to work together. Instead of looking at external signs, like places or products, we do better to try to mark the distinction in terms of aims. The aim of science (pure science, basic research) is to find truth; the aim of technology (applied research) is to solve practical problems.

There is surely something right about this, but it will not do as it stands. The aim of science is not to discover any old truth but to discover significant truths. Recognizing the distinction between epistemic and practical significance, we might propose that pure science aims to find those truths whose only significance is epistemic. Yet this is vulnerable to the possibility that inquiries undertaken solely for the sake of satisfying curiosity might turn out to have practical payoffs, and would thus be debarred from counting as parts of pure science.

We do better to deploy the notion of aims in its most natural home, referring to the aims of individual agents rather than those of some abstraction (like science or technology). Let's say, then, that an investigator is practicing pure science just in case the investigator's aim is to address a question in the significance graph solely because of the epistemic significance that that question inherits. We can explain what this scientist does simply by adverting to the epistemic significance that would come from her success and seeing her as motivated by her perception of this significance. She wants to find some elusive particle, say, solely because she sees that the discovery of this particle would answer theoretical questions about the structure of matter; whether its discovery would have any practical implications is of no concern to her. Her technologist colleagues, by contrast, do the things they do solely with the intent of resolving practical problems and have no interest in whatever epistemic significance may accrue to the truths they discover.

There are obvious complications. The pure scientist we've envisaged is extraordinarily high-minded. Considerations of fame or fortune (or grant renewal) are no part of her motivation. When such personal motives are present, how should they be classified? Are there virtually always practical concerns hovering in the background, if only in the conscientious researcher's concern to give satisfaction to employers or funding agencies? Perhaps the simplest response is to suppose that these kinds of motivations occur equally in pure scientists and in those who practice technology, so that they can be ignored for the purposes of drawing the distinction. In any event, I shall henceforth ignore them.

Deeper difficulties come from the multifarious interconnections of the epistemic and the practical in significance graphs. Dolly's significance derives in part from connections to broad issues in development, in part from her agricultural and medical promise. After Dolly, investigators may undertake ventures in nuclear transfer using different donor cells in different mammals. Their inquiries satisfy curiosity—Are some mammals easier to clone than others? Are

some cells especially good for supplying nuclei?—but they may also advance practical projects. How should mixed inquiries be classified?

One response is to interpose a third category between science and technology: there's basic science (the pursuit only of epistemic significance), applied science (with both epistemic and practical significance), and technology (only of practical significance).<sup>1</sup> Ventures in mammalian cloning would be taken to belong to applied science. This, however, seems doubly unsatisfactory, for it lumps together the investigator who is out to fathom the molecular changes involved in cellular differentiation and the researcher who wants to find reliable methods of breeding superior livestock. Once again, we need to attend to the particular intentions of the scientists involved. There's no all-purpose tag that can be pinned on particular constituents of significance graphs.

With respect to cloning, it's easy to envisage two extreme cases and to classify them by attending to the researchers' aims. Yet an inquirer's motives can genuinely be mixed. Would-be cloners might want both to address broad questions about development and to produce a better pig. In such cases, the intermediate category of "applied scientist" (or something similar) seems an attractive idea, exposing the double nature of the lines of connection in the significance graph. On further reflection, we should appreciate that not all that is mixed is mixed equally. In the middle sits an investigator equally devoted to embryological insight and porcine perfection. Just to her technological right is a colleague who gives slightly greater weight to bringing home the bacon, while just to her scientific left is another colleague whose priorities are the reverse. Indeed, we can envisage a chain of researchers extending from the pure embryologist at the one end to the animal breeder at the other. Where along this chain do we want to mark the boundaries of however many categories we propose to introduce?

An appropriate answer to that question would point out that, despite the difficulty of fixing transition points, it may still be valuable to distinguish the extremes. Even though we can't find a sharp distinction between "pure science" and technology, we can still use a vague distinction that separates certain very clear cases—the imaginary embryologist and animal breeder, for example. We understand the easy cases and the hard cases by disclosing the structures of the pertinent significance graphs and the ways in which different investigators respond to those structures and seek to extend the nets. "Pure science" is what

1. An approach along these lines is developed by Ilkka Niiniluoto, "The Aim and Structure of Applied Research," *Erkenntnis*, 38, 1993, 1–21. Niiniluoto is firmly in the tradition of seeking context-independent aims for inquiry, but he offers an account of the science–technology distinction that is unusually sensitive to some of the hard cases. Another important attempt to fashion an intermediate category is offered by Donald Stokes in *Pasteur's Quadrant* (Washington D.C.: Brookings Institute Press, 1997). Stokes's discussion demonstrates quite convincingly that the motives of tackling a practical problem and contributing to "basic science" aren't incompatible.

pure scientists do, and pure scientists are those people whose research is guided by the lines along which epistemic significance flows.

There are two further complications, however, both prefigured in earlier parts of this discussion. First, we may look backward and recognize that the historical explanation for the current epistemic significance accruing to a line of inquiry turns in part on some practical project from the past. Second, we may look forward and recognize that there are readily envisageable ways of linking the results of the inquiry (or the possible results if the inquiry develops in a particular foreseeable way) to practical projects that others could be expected to pursue. If research is to be genuinely pure, how should the investigator's aims accommodate these filiations to the practical?

There are natural answers. A chemist, working on a molecule of current "theoretical" interest, may well not know or care that the molecule came to scientific attention because of past efforts to find a cheap way to appease the public about conditions in mines. Unlike others who work on the molecule because it will speed up a commercially important industrial process (albeit at an environmental cost), our chemist has no ties to entrepreneurs and no concern for the practical applications. As he never tires of explaining, he simply finds the problem of figuring out the structure a fascinating challenge. Since his aims are only to achieve results of epistemic significance, he is a pure researcher. Or is he?

Once again, we can contrast extreme cases. When any links to practical projects are buried in a distant past, with no bearing on contemporary applications, and when it's very hard to forecast how results from this inquiry could be adapted to solve practical problems, then researchers can quite legitimately declare their intentions to be thoroughly epistemic. However, when only a little curiosity is needed to see that the current significance graph has been shaped by dubious ventures from the past, or when the propensity of others to engage in morally consequential applications ought to be obvious, the researcher who proclaims solely epistemic intent is guilty of self-deception (at the very least). Tom Lehrer made the point in a witty lyric:

"When the rockets go up, who cares where they come down?  
That's not my department," says Werner von Braun.

Pure researchers, then, are not simply those whose intentions are entirely to promote epistemic significance but whose lack of interest in the practical can be justified.

We've been considering the complexities of the distinction between science and technology, and it's worth stepping back to remind ourselves of why the distinction has seemed so important. As I noted at the beginning of this chapter, the fundamental point seems to be to limit the scope of moral, social, and political appraisal. If a clear separation can be made, then the line of defense

considered at the beginning of this chapter can be articulated. Beyond requiring that researchers pursue their experiments in morally appropriate ways (treating their experimental subjects properly, dealing honestly with fellow scientists, and so forth) there are no further moral, social, and political standards to which the practice of science is accountable. Such standards arise only in the context of applied science or of technology.

The myth of purity proposes that there is a distinction that fulfills these purposes. The considerations of this chapter oppose the myth. We may be able to identify certain people as practicing “pure research,” but our classification depends not only on their intentions but also on whether those intentions can be justified. In other words, insofar as the distinction between pure science and technology can be drawn, it depends upon a *prior* judgment to which moral considerations are pertinent. The claim that a particular inquiry is a piece of pure science can only be evaluated in light of the character of the significance graph, the intentions of the investigator(s), and the possibility of justifying a practice of ignoring any connections to practical concerns. Very frequently, the complex intertwining of the epistemic and the practical and the mixed motivations of actual researchers will make the application of any simple distinction (or set of distinctions) impossible, but, even when we separate out these complications, the links to past projects and to future possibilities have to be assessed *before* we can count the inquiry as a piece of pure science. Flourishing the badge of purity isn’t automatic. The label has to be earned.

I’ll conclude the discussion by illustrating my point with one of the most obvious examples of pure research, one that may initially seem to vitiate many points of the past two chapters. At the frontiers of contemporary theoretical particle physics, researchers explore extremely abstract mathematics in trying to find a unified account of fundamental forces and the elementary constituents of matter. Surely in this instance the line of thought with which we started the chapter seems to work: practical consequences, for good or ill, are too remote to be specified; rather the inquiries are pursued because of the value of uncovering the deepest (“most beautiful”) truths about our universe.

Let’s accept the claim that practical consequences are indeed remote, that, unlike the comparable situation at earlier stages of atomic physics, there are no relatively straightforward ways to try to deploy principles and theories that are likely to emerge from the investigation in order to generate vast amounts of energy. We can still ask why the project is assigned such high value. With the demise of the Unity-of-Science view, the answer can’t be that we’re going to arrive at a theory from which all other parts of science are destined to flow. Rather the significance of the work lies in the interest for us of identifying the ultimate constituents of matter. At various stages in past inquiry, attempts to answer that question have been connected to all sorts of practical concerns, but, even if we set those to one side, there’s a fundamental point about the justification of fur-

ther inquiry. To concentrate on the epistemic significance of a unified treatment of gravitation and the other three “fundamental forces” is to presuppose a judgment about the relative value of answering a particular set of questions in mathematical physics and alternative ways of extending the collection of significance graphs that the current generation of researchers has inherited from the past. There are any number of ways in which we might go on from where we are (including some that would revoke past decisions), and the resources of equipment, time, and talent are finite.<sup>2</sup> Engaging in research that does no foreseeable harm may be unjustified because of the good that the researchers who carry it out fail to do. Once we have abandoned the idea of a context-independent conception of epistemic significance, we see that judgments about lines of inquiry inevitably weigh the rival merits of various practical goals and various ways of satisfying human curiosity. This applies to the “purest” cases just as it does to the areas of science that are obviously intertwined with applications.

None of this is to suggest either that attempts at a theory of superstrings (or similar ventures at the theoretical reaches of physics) are impure or that they are unjustified. It seems to me eminently possible that researchers who undertake this project are motivated by concerns of epistemic significance alone and that they are entitled to ignore any practical linkages. But my hunch that their research is pure and well motivated depends on supposing that the results of a moral, social, and political appraisal would vindicate it. The myth of purity is the claim that gesturing at the absence of any practical intent is enough to isolate a branch of inquiry from moral, social, or political critique.

I shall elaborate upon this theme later. First, however, I want to consider an example in which an ideal of pure inquiry has been invoked to ward off political objections.

2. Similar points were made during the 1960s, at a time when there was serious debate about the foundations of issues in science policy. For a forthright statement, see Alvin M. Weinberg, “Criteria for Scientific Choice,” in *Criteria for Scientific Development: Public Policy and National Goals*, ed. Edward Shils (Cambridge, Mass.: MIT Press, 1968).

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## Constraints on Free Inquiry

IN THE MID-1970S, A GROUP OF SCHOLARS, including prominent biologists as well as academics from other disciplines, wrote scathing indictments of conclusions they claimed to find in E. O. Wilson's much-lauded book, *Sociobiology: The New Synthesis*. Wilson had argued that a Darwinian analysis of human social behavior revealed that certain features of contemporary societies were deeply rooted in human nature, and thus unmodifiable by adjusting the environments in which people develop. In particular, he suggested that current sex roles are inevitable, that xenophobia cannot be eradicated, and that we can expect that any society will be based on intense competition that generates inequalities. The Sociobiology Study Group of Science for the People charged that these conclusions were both unwarranted and politically dangerous in their apparent support of reactionary policies. In two replies, Wilson disavowed many of the conclusions, claiming his critics had misinterpreted his book, and he ended each article by recalling a traditional liberal theme. The linking of explicitly political considerations to the scientific discussion was an instance, he averred, of "the kind of self-righteous vigilantism which not only produces falsehood but also unjustly hurts individuals and through that kind of intimidation diminishes the spirit of free inquiry and discussion crucial to the health of the intellectual community."<sup>1</sup> In a more expansive treatment, he closed a second reply with the following paragraph:

All political proposals, radical and otherwise, should be seriously received and debated. But whatever direction we choose to take in the

1. E. O. Wilson, "For Sociobiology," *New York Review of Books*, 11 Dec. 1975; reprinted in Arthur Caplan, ed., *The Sociobiology Debate* (New York: Harper, 1978); see p. 268.

future, social progress can only be enhanced, not impeded, by the deeper investigation of the genetic constraints of human nature, which will steadily replace rumor and folklore with testable knowledge. Nothing is to be gained by a dogmatic denial of the existence of the constraints or attempts to discourage public discussion of them. Knowledge humanely acquired and widely shared, related to human needs but kept free of political censorship, is the real science for the people.<sup>2</sup>

Wilson thus cast his critics as attacking precepts about the value of free inquiry that have a rich heritage in the liberal democratic tradition, and, consequently, are typically accepted without question.

Wilson's critics quickly disavowed the charge that they were trying to hedge free inquiry with political constraints.<sup>3</sup> Suppose, however, they had queried the traditional precepts, asking why the value of free inquiry should outweigh other moral, social, and political concerns. In that case, the debate would have turned to the tradition on which Wilson tacitly drew, probably to the writings of the most eloquent defender of free expression, John Stuart Mill, and to the second chapter of *On Liberty*.

There Mill advances four arguments in favor of free expression. He begins with the fallibility of human opinion, pointing out that even though we may feel certain of the truth of our beliefs we may still be mistaken, and claiming that it is important to guard against error by allowing open discussion of rival points of view. Secondly, he notes that views that are, as a whole, false, may contain some truth, even some truth that orthodoxy currently fails to recognize, so that free discussion may guide us to improved opinions. Furthermore, received beliefs that are not subject to criticism from alternative perspectives may come to be held dogmatically, "in the manner of a prejudice"; and, finally, Mill notes that the meaning of the doctrines may become lost. Now it's noteworthy that all of these considerations depend on an ideal: Mill seems to hold out before us the vision of a person who aims at, and achieves, true beliefs held with an understanding both of their content and of the grounds on which they rest. If that ideal can be questioned, by juxtaposing it with other things we are inclined to value and revealing tensions, then there will at least be room for debating Mill's defense of freedom of expression.

When Mill's arguments are transferred directly to the context of scientific research it does appear that there are various ways of probing the ideal: Is it realistic to suppose that inquirers today must continually confront the discarded

2. Wilson, "Academic Vigilantism and the Political Significance of Sociobiology," reprinted in *The Sociobiology Debate*, ed. Caplan; see p. 302.

3. See Stephen Jay Gould, "Biological Potentiality vs. Biological Determinism," in *Ever Since Darwin* (New York: Norton, 1977), 258, and my own *Vaulting Ambition* (Cambridge, Mass.: MIT Press, 1985), 7.

doctrines of the past, rather than forging forward? Is the attainment of scientific truth so significant that it overrides any countervailing considerations from the effects of research on human welfare? The latter question is underscored by the discussions of the past two chapters, for, if the arguments I have offered are correct, the significance of scientific results is entangled with practical concerns, and we cannot appeal to some overarching project whose value transcends all others.

Mill would not have been perturbed by these observations. Although his writings are often viewed as a general defense of free inquiry, the types of opinions under consideration in his arguments are quite special. Behind *On Liberty* stands the long sequence of debates about freedom of religious expression. Mill places those debates on a more general level, taking as his principal topic the opinions that are central to people's articulations of their goals and values, of their main projects and the significance of their lives. Chapter 2 of *On Liberty* follows chapter 1, where Mill makes the foundations of his defense completely explicit: "The only freedom which deserves the name is that of pursuing our own good in our own way, so long as we do not attempt to deprive others of theirs or impede their efforts to obtain it."<sup>4</sup> The importance of free expression and open debate is thus to promote individuals' reflective decisions about the ends of their own lives—so to advance "the permanent interests of man as a progressive being"<sup>5</sup>—and the Millian ideal of the agent who recognizes the grounds of his beliefs, fully understands the content of those beliefs, and has had the opportunity to test those beliefs against rivals, acquires its importance precisely because the beliefs in question are those that structure his projects and aspirations.<sup>6</sup> When Mill is understood in this way, the questions of the last paragraph become irrelevant. But, by the same token, there is no longer a direct argument from the precepts he elaborated and defended to the freedom of scientific inquiry.

In fact, we can go further. To take seriously Mill's point that the freedom to which we aspire is the freedom to define and pursue our own vision of the good is to recognize the possibility that the unconstrained pursuit of inquiry might sometimes interfere with the most important kind of freedom, at least for some members of society. So we can envisage a Millian argument *against* freedom of inquiry, one that proceeds by trying to show that certain types of research would be likely to undermine a more fundamental freedom. I aim to articulate this argument, to expose its force and its limits.

4. Mill, *On Liberty* (Indianapolis: Hackett, 1992), 16–17.

5. Mill, *On Liberty*, 14.

6. Here I am in agreement both with Alan Ryan's insightful essay "Mill in a Liberal Landscape," in *The Cambridge Companion to Mill*, ed. J. Skorupski (Cambridge: Cambridge University Press, 1998) (see especially pp. 507, 509, 510), and with Isaiah Berlin's "John Stuart Mill and the Ends of Life," in *Four Essays on Liberty* (Oxford: Oxford University Press, 1969).

Concerns about the social impact of research can be developed in at least three different ways. The strongest, and most ambitious, version of the argument proceeds directly from the difference between the ideal of formulating and pursuing one's own plan and the goals of scientific inquiry, without any further epistemological assumptions. Thus it might be suggested that, were we to recognize certain kinds of truths, the impact on some people would be to erode their sense of worth and to make it difficult, even impossible, for them to frame a conception of their lives as valuable. For the moment, I shall set such considerations on one side; they will occupy us later in a more general context (chapters 12–13).

Alternatively, instead of supposing inquiry will (eventually) deliver the truth, we can take a more realistic (less rosy) view of our epistemic prospects than Mill, his predecessors, and many of his successors are inclined to do. When the expression of unpopular doctrines is defended on the grounds that the clash of views is healthy, it often seems that the defenders take for granted that “truth will out,” at least in the long run. Recognizing that research is fallible, as well as socially consequential, we may start to elaborate a critique of some lines of inquiry.

In its most minimal form, the critique need not challenge the value of free inquiry. Those who replied to Wilson's defense of human sociobiology often pointed out that they were concerned with the evidence for the controversial conclusions, and that political considerations were relevant precisely because when the potential consequences are grave, standards of evidence must go up. I'll now try to show that the sociobiology debate offers an opportunity for developing a more ambitious line of argument.

Suppose we envisage scientific investigations as taking place within a society in which there are significant inequalities with respect to well-being. Members of a particular group within this society, a group I'll refer to as “the underprivileged,” have lives that go substantially less well than is typical in the rest of the society. This relative assessment of the quality of their lives may turn on obvious economic disadvantages, lower life expectancy, or restricted access to coveted opportunities and positions. Moreover, the reduced average quality of life for the underprivileged is partially caused by the fact that, in the past, it was widely believed that those with characteristics prevalent within the group were naturally inferior or that such people were only fitted for a narrow range of opportunities and positions. Residual forms of this belief are still present, although the belief is repudiated in most public discourse.

Imagine further that some scientific investigations conducted within the society might be taken to support conclusions that bear on the officially discarded belief. Specifically, let the belief in question be, “People with a particular characteristic (call it *C*) are naturally less well-suited to a particular role (call it *R*),”

and suppose that an area of science *S* might yield evidence for or against this view. The impact of pursuing *S* and uncovering the evidence is *politically asymmetrical*, in that both the following conditions obtain:

- (a) If the evidence is taken to favor the hypothesis that those with *C* are naturally less well-suited to *R*, then there will be a change in the general attitudes of members of the society toward those with *C*, constituting (at least) a partial reversion to the old state of belief; if the evidence is taken to favor the negation of this hypothesis, there will be no significant further eradication of the residues of the old belief.
- (b) If the belief that those with *C* are naturally less well-suited to *R* again becomes widespread, then the quality of the lives of those with *C*—the underprivileged—will be further reduced, partly through the withdrawal of existing programs of social aid, partly through the public expression of the attitude that those with *C* are inferior to those who lack *C*; unless there is significant further eradication of the residues of the old belief, there will be no notable improvement in the lot of the underprivileged from pursuit of *S*.

Recognition of the political asymmetry lies behind the modest argument, outlined above, according to which standards of evidence must go up when the consequences of being wrong are more serious.

However, assume also that the society's pursuit of *S* will be *epistemically asymmetrical*, in that people will always take the belief to have more support than it deserves. More precisely:

- (c) There will be significant differences between the probabilities assigned to the hypothesis that people with *C* are less well-suited to *R* and the probabilities that would be assigned by using the most reliable methods for assessing evidence; the probabilities assigned to the hypothesis by members of the society will typically exceed the probabilities that reliable methods would yield, and the probabilities assigned to the negation of the hypothesis will be correspondingly deflated.

Although there are already hints of danger for the underprivileged, troublesome consequences aren't inevitable. Evidentiary matters about the effects of having *C* might be clear-cut, favoring the egalitarian conclusion to a large enough extent to outweigh the bias towards the hypothesis.

Suppose, however, this isn't so. If the issues surrounding the impact of having *C* are confusing or complicated, and if the bias towards overestimating the support for an antiegalitarian answer is sufficiently strong, then the underprivileged are indeed threatened by the pursuit of *S*. Specifically, assume that

- (d) With high probability, the evidence obtained from pursuit of *S* will be indecisive, in that the most reliable methods of assessing that evidence would assign a probability of roughly 0.5 to the hypothesis.
- (e) The bias in favor of the hypothesis is so strong that most members of the society will take evidence that, when assessed by the most reliable methods, would yield a probability for the hypothesis of roughly 0.5 to confer a probability close to 1 on the hypothesis.

If all these conditions are met, there's a significant probability that the antiegalitarian hypothesis will be taken to be extremely well supported, even though the evidence leaves the issue open, with consequent harm to the underprivileged. There is no chance of any genuine benefit for the underprivileged. From the perspective of the underprivileged, the expected utility of pursuing *S* is thus clearly negative. If we shouldn't engage in ventures that can be expected to decrease the well-being of those who are already worse off than other members of society, we should therefore refrain from engaging in *S*.

This argument is abstract and general. Its burden is that when a certain constellation of conditions is satisfied—the conditions (a)–(e)—the pertinent inquiries ought not to be pursued. I strongly suspect that there are cases in which the conditions obtain, and, indeed, that some of the disputes about human sociobiology and human behavioral genetics satisfy the conditions. If we were to take the underprivileged to be the set of women, the characteristics to consist of biological traits uncontroversially possessed by women and not by men, and the role *R* to be any of a number of prominent, and sought after, positions in American or European society, we could generate plausible instances. Even more obviously the assumptions appear to apply to members of various minority groups—African-Americans in the United States, West Indians in Britain, immigrants from North African and Near Eastern countries in European nations.

Consider, first, the political asymmetry. What would be the likely impact of widespread acceptance of inegalitarian conclusions—say that women, “by their nature,” lack the competitive urge or the commitment to career to occupy challenging positions, or that minorities have genetic predispositions to lower intelligence? Surely the most predictable results would be the withdrawal of resources from any current efforts to try to equalize opportunity for filling *R*, and a diminution of self-respect and of motivation among the underprivileged. It is hardly cynical to believe that the supposedly scientific findings would inspire policymakers to “stop trying to do the impossible”—instead of “rubbing against the grain of human nature” they would save money now spent on wasteful public programs. Nor is it unreasonable to think that the psychological effects on members of the underprivileged would be damaging, either because they accede to the conclusion that they are less worthy than other members of society, or because they view this as a common perception of their status and

thus develop a sense of alienation. At best, these deleterious consequences would be offset by an allocation of public funds to respond to what would now be regarded as the *real* needs and potential of the underprivileged—although it's not entirely obvious what programs of this type would do. Not only are the hypothetical gains extremely nebulous, but it's also far from clear that contemporary affluent societies have much political will for this type of expenditure.

Recent debates about inegalitarian claims support other aspects of the political asymmetry. When evidence is announced in favor of equality, the effect is only to offset whatever damage has been done by more flamboyant presentations of the case for inequality. Defenses of “natural inequalities” typically out-sell the egalitarian competition. Furthermore, when rejoinders are published there is no groundswell of enthusiasm in favor of investing more resources in attempts to equalize social roles.

These remarks amount only to a *prima facie* case. A lot of detailed sociological work would be needed to show that (a) and (b) are satisfied in the scientific controversies about sex and race. Hence it would be reasonable for a defender of research into “the biological bases of social inequality” to protest the application of the argument *if that person were prepared to take on the burden of demonstrating that the consequences I have alleged do not ensue*. That is not, of course, how the defense usually goes, and, in what follows my chief aim will be to consider complaints that the general form of argument by appeal to political and epistemic asymmetries is invalid because it overlooks important aspects of inquiry.

With respect to the epistemic asymmetry it's possible to be more definite about the applicability of the argument, for here a wealth of historical studies hammers home the same moral. First, there is ample evidence of a bias in inegalitarian conclusions: patterns clearly discernible in the history of measuring those traits associated with cognitive performance, from the nineteenth century to the present, from the craniometers to the high priests of heritability, display one version of inegalitarianism (typically seen as preposterous by later generations) widely accepted until painstaking work exposes its lack of evidential support, followed by an interval of agnosticism until the next variation makes its appearance. Second, uncovering the flawed inferences underlying claims of a scientific basis for uncomfortable conclusions typically reveals just how complex are the issues with which investigators are trying to wrestle: analytical study of the methods of trying to show genetic differences in intelligence brings out what would be required to support responsible conclusions; examination of ventures in human sociobiology exposes how hard it would be to do it properly. Reliable knowledge about the topics is hard to come by. Combining this observation with the pattern that emerges from the history, the obvious explanation is that, in an epistemically cloudy situation, the probabilities assigned to the inegalitarian hypotheses are inflated, so that sincere investigators incorrectly be-

lieve themselves to have found a scientific basis for socially acceptable conclusions. So I think there's good evidence for the pertinent versions of (c), (d), and (e).

I turn now to some obvious criticisms. First comes the worry that the argument I've presented is myopic. Perhaps in focusing on a particular situation, we fail to understand the more general import of defending free inquiry. Recall Wilson's defense of free inquiry in terms of promoting the intellectual health of the community. He can easily be interpreted as warning of the dangerous effects that blocking sensitive investigations might have on a more general policy whose overall consequences are beneficial. So we might indict the argument for failing to recognize the disutility of closing down particular inquiries, a disutility that results from undermining a society-wide practice of fostering free discussion. Our choices ought to have been framed (so the accusation goes) in terms of a social context for scientific research that is thoroughly committed to a policy of free inquiry, and which occasionally encounters the unfortunate consequences my arguments expose, and a social context for research that hampers the freedom of inquiry, that avoids some local unfortunate consequences, but also suppresses valuable inquiries with appreciable losses in utility.

The obvious answer to this challenge is to deny that our choice is between these two contexts. The objection proposes to evade the argument by mimicking a familiar strategy: faced with the fact that breaking a promise might sometimes maximize expected utility, moral philosophers sometimes suggest that the *rule* of keeping promises promotes well-being and that breaking a promise on a particular occasion would undermine the rule. Unfortunately, the suggestion faces an obvious reply: why should we not adopt a practice of promise-keeping except in situations where it's clear that breaking a promise would maximize expected utility? In similar fashion, the scientific community might be committed to a practice of free inquiry except in situations in which it's clear that certain investigations will be socially disadvantageous (or disadvantageous for those who are underprivileged).

If it were genuinely difficult to distinguish situations in which pursuing some lines of inquiry could be expected to be socially damaging, there might be reason to think that a policy of admitting limits on inquiry would quickly decay to the detriment of society's intellectual health. We begin with good intentions to bar certain investigations but, in allowing the social consequences of an inquiry to determine its legitimacy, we enter a zone in which it's easy to lose our way, ultimately retreating from lines of research that would have proved valuable. Yet the arguments of the form we're considering plainly allow for definite instances, cases in which it's possible to judge that the expected utility of the pursuit of an inquiry is negative (or negative for those who are worst off), and we could block the alleged slide by adopting a policy of only abandoning inquiries when it's

clear that the social consequences of pursuit are deleterious (free inquiry would be given the benefit of any doubt). The objection is right to remind us of the broader context in which decisions about the value of free inquiry should be made, but, so far at least, it seems possible to accommodate the point while allowing that some instantiations of the argument are cogent.

Consider a second objection, one that tries to subvert the argument by recalling the historical sources of the beliefs it employs. At any number of stages in the history of the sciences, people with values that were threatened by a particular line of investigation could have contended that the inquiry in question was likely to bring nothing but loss. Imagine committed Aristotelians campaigning against further efforts to determine the earth's motions, or devout Victorians objecting to "speculations" on the origins of species. Had the argument I've given been influential at earlier stages of inquiry, we would have forfeited enormous epistemic advantages. Precisely because we have liberated ourselves from the ideas of our predecessors, through allowing inquiry to undermine accepted beliefs, we are now in a position to make the kinds of evaluations on which the argument depends. Our values have themselves been shaped by the overthrow of previous systems of belief, systems that would have accepted the inequalities in contemporary society with equanimity. Consider, for example, the version of the argument that attacks research into racial differences in intelligence. The recognition that there would be costs if people classified as belonging to minority races were told that authoritative science had established their intellectual inferiority itself depends on a process through which people with particular superficial features and of particular descent were recognized fully as people, a process that depended on the possibility of free inquiry into unpopular topics.

Although this line of reasoning appears plausible, it rests on a number of controversial assumptions. The final step can be debated by questioning the role the sciences have actually played in fostering the acceptance of disadvantaged minorities. The chief defect of the objection lies, however, in the similarity it suggests between the heroic scientific liberators of the past and those who would investigate natural inequalities in the present. People who publish findings purporting to show that behavioral differences stem from matters of race or sex often portray themselves as opposing widely held views in the interest of truth. But do Galileo's would-be successors don his mantle legitimately?

Of course, what matters is *significant* truth, and there are serious issues about why the favored lines of inquiry should count as significant. At this stage, however, I want to focus on a different presupposition of the attempted defense. In understanding the epistemic asymmetry, we recognize a bias towards accepting inegalitarian conclusions because they resonate with attitudes publicly denied but nonetheless present in contemporary societies. Many champions of unpopular inquiries correctly believe their conclusions oppose doctrines affirmed

by their colleagues (perhaps even by almost all of those working in the areas related to their discussions) and often upheld by the parts of the media with the strongest intellectual credentials. Their defenses typically fail to mention that there is a broad tendency to believe quite contrary things in private, that the views defended conform to inclinations that voters and public officials harbor and that may even be espoused by those who profess quite different views. In consequence, there's a deep disanalogy between contemporary investigators of racial difference (say) and the scientists of the past who defied the orthodoxies of their age.

Let's say that a belief is part of a *total* consensus just in case almost everyone in the pertinent society accepts it (or is prepared to defer to people who accept it), that a belief is part of an *official* consensus if it is publicly professed by everyone (or if people are at least prepared to defer publicly to people who publicly profess it), that it is part of an *academic* consensus if it is held by almost everyone within the academic community, and that it is part of a *lay* consensus if it is held by almost everyone outside the academic community. Galileo and Darwin opposed total consensus in their communities, and there were powerful biases *against* adopting their conclusions; thus the conditions for applying the argument I've reconstructed to them are not satisfied, and the social disutility of their inquiries can no longer be calculated in the same fashion. Contemporary investigators who claim important differences due to race or sex surely oppose an official academic consensus, and perhaps are at odds with both official consensus and academic consensus. It would be too strong to claim that there is a lay consensus on an inegalitarian conclusion inconsistent with the official academic consensus, but, outside the academy, there are sufficiently powerful inclinations to accept inegalitarian beliefs, held by sufficiently powerful people, to suggest that there will be an epistemic bias in favor of the inegalitarian conclusions, and that these conclusions are likely to be implemented in social policies. Furthermore, there may well be scientists whose embrace of egalitarian claims is sufficiently shallow that they too will be disposed to take indecisive evidence as demonstrating important differences.

Scientists quite understandably bridle at the thought that their research will have to conform to standards of "political correctness," so it's important to understand the exact nature of the argument. Recognizing that some types of research bear on struggles to achieve social justice, *and that there is a schizophrenic moral consciousness in which public "politically correct" attitudes coexist with inclinations to quite opposite beliefs*, we should see the impact of the research as affected by both a political asymmetry and an epistemic asymmetry. Instead of lumping together quite disparate examples from the history of science, it's important to focus on the special conditions the argument discerns in our contemporary predicament. The Millian arena, in which conflicting ideas battle for public approval on epistemically equal terms, and in which the bystanders are

never hurt by the nature of the conflict, is a splendid ideal, but it would be quite naïve to think that all pieces of controversial research are discussed in anything like this ideal arena.

The last objection I'll consider may be the most obvious. Perhaps all that the argument shows is the error of a consequentialist treatment of these questions—we go astray in thinking that decisions about the merits of inquiry can be judged by attending to the expected consequences. Of course, the main versions I've considered already incorporate the most prominent concerns about utilitarianism, in that they base judgments on the expected utility for the least fortunate (the underprivileged). Ironically, consequentialism is most sympathetic to inquiry into socially charged topics when we *ignore* the objections. If the response is to succeed, it must propose there's a moral basis for pursuing investigations independently of the impact on the underprivileged. One way to develop that idea is to suppose we have a duty to try to ascertain significant truths about nature. Can this duty override worries about the consequences for the unfortunate?

I think not. Far less controversial than any duty to seek the truth is the duty to care for those whose lives already go less well and to protect them against foreseeable occurrences that would further decrease their well-being. We should recognize a clash of duties whose relative importance must be assessed. To oppose the argument, one must believe that the duty to seek the truth is so strong that it is binding, even in situations that will adversely affect the underprivileged, that will offer little prospect for gaining knowledge, and that will afford considerable opportunity for error.

A different way of opposing the consequentialist framework would be to insist that the project of improving the well-being of the disadvantaged can't be allowed to interfere with rights to free inquiry. This libertarian response would abandon both the consequentialism of the argument and the attempt I've made to avoid typical foibles of consequentialism by focusing on the well-being of the least well-off. Any libertarian defense would thus have to claim that the distribution of rights doesn't matter, that if, through historical contingencies, subgroups of the population have been deprived of various rights we can't seek to remedy the situation by abridging the rights others enjoy, even if doing so would limit rights in small ways to enhance dramatically the ability of the disadvantaged to exercise rights others take for granted. It would also have to argue that the right to free inquiry is fundamental, that it overrides important rights of those who suffer from the pursuit of inquiries that reinforce incorrect stereotypes. I think it doubtful either of these challenges (let alone both) can be met, but, in any event, there is a simpler antilibertarian argument. Respecting rights comes at a price, and it's important that the price be distributed fairly. In situations where free inquiry would unfairly increase the burden on those who are already disadvantaged, there can be no right to free inquiry.

If one seeks to reject the argument by abandoning its utilitarian framework, the best approach seems not to be to invoke implausibly strong collective duties or unfringeable rights, but to suggest instead that freedom of expression is required for the deepest and most important kind of human well-being, to return, in effect, to Mill's own conception of "the permanent interests of man as a progressive being." Couched in Mill's own terms, where the focus is on our capacity for choosing our own vision of the good and for planning how to achieve it, this is quite promising. One might argue that free inquiry is needed if we are to discover what is best or most worthwhile and how to create the conditions most conducive to its realization.<sup>7</sup> As I hinted earlier, these considerations do support the ideal of freedom of inquiry *to the extent that it promotes human reflection and deliberation*. But they do not provide any escape from the argument about free scientific inquiry.

The difficulty on which earlier attempts at evasion have foundered is the conflict between a relatively abstract value (the attainment of truth) and the concrete ways in which some people's lives are diminished by the purveying of inegalitarian conclusions. It would be easy to conceive the latest version in the same terms, taking it to oppose the fundamental interests that ground freedom of expression to the demands of equality, and so heading for a familiar stalemate. But this would be mistaken. The issue isn't how we weigh competing fundamental values (freedom vs. equality) but rather how we require whatever values are seen as most fundamental to be distributed. Champions of free inquiry often view it as a precondition of human well-being because they think in terms of an abstract human subject whose deliberative capacities are enhanced by open discussion—all is calm, serene and unthreatening. How representative is this abstract subject of the actual people whose deliberations would be affected by the actual pursuit of the forms of inquiry about which there is dispute? Once this question is posed, we begin to understand that the structure of the argument in its consequentialist form can be replicated precisely because the *absence* of particular kinds of inquiries would enhance the deliberative capacities of those for whom deliberation is currently most constrained. We can agree with Mill and his successors that the freedom to deliberate is fundamental, and, in consequence, *adopt just the argument I have given on the grounds that it promotes a fair distribution of this fundamental freedom*. To make this more concrete, we can compare the controversial inquiries to other instances in which the value of free expression is undermined by the importance of securing the deliberative freedom of the disadvantaged. Consider hate speech. For certain kinds of verbal performances it's at least arguable that the effect isn't to broaden the possibili-

7. Variations of this general approach are offered by Joshua Cohen, "Freedom of Expression," *Philosophy and Public Affairs*, 22, 1993, 207–263, and David Brink, "Millian Principles, Freedom of Expression, and Hate Speech," *Legal Theory*, 7, 2001, 119–57.

ties of dialogue but rather to exclude the victims of the speech from discussion, thus cramping their deliberative opportunities. Obscene racial epithets don't invite a calm rejoinder that would open up new intellectual vistas for both aggressor and victim; they are intended to banish some people from public fora, and they often succeed in doing so.

Mill's own principles thus support the conclusion that certain forms of inquiry ought not to be pursued, undercutting the popular—and, I believe, well-intentioned—view that the free pursuit of inquiry is always a good thing. Yet, as Mill saw as clearly as anyone, the fact that we ought not to pursue a particular course of action doesn't mean that there should be a publicly enforceable ban. We can thus distinguish two potential conclusions of the argument I've offered, one which supposes that certain types of research should not be undertaken and another that takes the further step of claiming these inquiries should be proscribed. The distinction is important. For the argument that, when conditions (a)–(e) are satisfied, there are moral grounds for refraining from inquiry is cogent. Demanding a ban on inquiry under such conditions would be to take a further, illegitimate, step.

Mill proposed that the scope of law is limited to those instances in which the prohibited action would cause harm to others, and one might initially think that this proposal clears the way for a ban on some types of inquiry, namely those likely to erode further the status of the underprivileged. The problem with a ban does not stem from the Millian proposal, but from the consequences of instituting it: in short, the “cure” is worse than the “disease.” More exactly, the very conditions that underlie the asymmetries on which the argument draws ensure that officially restricting free inquiry would exacerbate the social problems.

In a world where (for example) research into race differences in I.Q. is banned, the residues of belief in the inferiority of the members of certain races are reinforced by the idea that official ideology has stepped in to conceal an uncomfortable truth. Prejudice can be buttressed as those who oppose the ban proclaim themselves to be gallant heirs of Galileo. When the Caucasian child asks why research into differences between racial groups is not allowed, a superficially plausible answer will be that everyone knows what the research would show and that people are unwilling to face the unpleasant truth. Proscribing the research has consequences of the same general kind as allowing it—except that they are probably worse. *So long as the epistemic asymmetry is not clearly appreciated*, champions of the research will always ask (rhetorically), “If there is genuine equality, why not try to demonstrate it?” From the perspective I've been defending, there's an answer to the question, and it lies in pointing out conditions (c), (d), and (e). If the answer were widely accepted, there would be no need for a ban; when the answer is not widely accepted, any ban would be seen as illegitimate.

I doubt that more limited solutions, such as the withdrawal of public funds from support of certain kinds of projects, will fare any better. So long as the conditions driving the argument are not appreciated, champions of the forms of inquiry that should be eschewed can always make use of the rhetoric of freedom to portray themselves as victims of an illegitimate public policy of stifling the truth. The consequences of *any* type of official intervention are thus likely to be counterproductive—and this may even extend to presentations (like this one) of the harms inquiry may engender.

These gloomy reflections do not touch the argument that the research under scrutiny is morally unjustified, although they leave us with a dilemma about what to do once we recognize the point. One obvious suggestion is that we view the conclusion simply as a moral imperative. Responsible scientists ought to ponder the consequences of their own work and refrain from research into areas to which the conditions of the argument apply. Yet the very considerations that inspire the argument make it apparent that we can't hope that scientific self-scrutiny will be efficacious. The pressures that build the epistemic asymmetry, most prominently the temptation to gain a large audience and to influence public opinion by defending "unpopular" views, make it highly likely that scientists fascinated by the thought of exploring differences due to sex, gender, or race will read the evidence very differently: some will deny that the history of research into these areas has revealed a pattern of shoddy work, widely accepted as supporting inegalitarian conclusions until critics expose the deficiencies, seeing instead the tragedy of bold predecessors pilloried for peccadilloes by a politically biased establishment; others may admit the troubles of the past, insisting that things are different *this time*, that the brave new method will finally deliver the goods. I believe a sober review of the history of research into racial and sexual differences supports the view recorded in the argument, and thus any attempts to read that history differently embody just that epistemic bias that the argument diagnoses. Moreover, the sense that investigators now have new tools for conducting their inquiries should be coupled with a clear understanding that others have been similarly optimistic in the past, and that epistemic bias may lead one to overrate the force of the "latest findings." Nonetheless, from the perspective of the argument, it's to be expected that those attracted to research in these areas will find ways of denying key premises, so their attempts at moral reflection, however sincere, will not persuade them that they should abstain.

If this is correct, we can neither use the argument to support public discouragement of certain types of inquiry, nor expect that private moral reflection will make the problem go away. So what implications, if any, does the argument have? What's to be done?

To repeat: the Millian conception of the arena in which doctrines compete equally and in which public expression of those doctrines causes no harm is a splendid ideal. Unfortunately, it sometimes inspires people to take a naively op-

timistic view about the actual pursuit of inquiry. To accept the argument I have offered should *not* be to conclude that there ought to be public censorship of inquiry. Instead, it ought to provoke us to pursue questions about the social surroundings of our investigations: What are the conditions under which the Millian arena functions properly? What kinds of social factors cause those conditions to break down? What can be done to prevent the breakdown?

There are two polar views, both of which ought to be resisted. One claims the Millian arena always functions properly, yielding eventually secure knowledge whose value outweighs whatever harm has been caused in the fray. The other denies that the Millian arena ever works in the way it is supposed to, contending that what we take for a competition among ideas on their evidential merits is always a thoroughly political struggle. The reasoning of this chapter is intended to show what is wrong with the first view. My quarrel with the second begins with the modest realism espoused in chapters 2 and 3. The position I favor is that we sometimes achieve true beliefs about nature, that, when we do so, we often proceed by reliable means and gain knowledge, and that, in some of these instances, lively debate between partisans of different positions is instrumental in our attaining knowledge—in much the way Mill envisaged. Unfortunately, this is not always possible, and, as the argument recognizes, we're sometimes victims of epistemic biases. But our plight isn't hopeless and we may be able to identify those epistemic biases, thus avoiding those clashes of ideas that would be genuinely harmful. The failures of the Millian arena are local. With respect to some issues, open debate will generate opinions with the virtues Mill so lucidly characterized, opinions that are closer to the truth, held with understanding on the basis of reliable methods that are clearly recognized. Using the history of our inquiries as a guide, we can come to distinguish these instances from others in which the arena fails to function, and, perhaps, on the basis of the distinction, we can improve our epistemic condition.

We have reason to be confident in the claims we make in some areas of inquiry, but there's no guarantee that the methods that spawned those claims will apply generally to yield knowledge in all the fields we'd like to investigate. Once this point is appreciated, we obtain just the perspective on the Millian arena I wish to commend. Instead of believing that the "contest of ideas" will always guide us to the truth, we can see that, in some instances, the contest might indeed be helpful, while in others it will not—whether because the problems are too hard or we suffer from biases we haven't yet learned to eradicate (and which may be ineradicable). Our best strategy is not to start from the assumption that free inquiry will always be a good thing, but rather to use the style of argument I've developed, in tandem with serious analysis of the successes and failures of our past ventures to try to improve our methods of inquiry (and to abstain from those investigations that can be predicted to bring only trouble).

The passages from Wilson with which I began make it apparent that this is not a standard way of considering scientific research. We aren't used to thinking about the ways in which our attempts to achieve knowledge, and the track records of their successes and failures, impinge on people's values and interests. I suggest that this is because of a tension between the science that is practiced in democratic societies and the underlying ideals of those societies. I formulate this as the thesis that *science is not well-ordered*. The task of the next three chapters is to understand what well-ordered science would be.