
1

INTRODUCTION

I

By the end of the Spring semester in 1975, I badly needed a break. For four years, I had been teaching three or four courses a semester at the University of Massachusetts at Boston while writing my dissertation, "The Politics of the Property Tax," and readying it for publication. So, to reward myself, I signed up to work and study at the UMass field station on Nantucket Island. I agreed to mend nets, wash bottles, collect algae, cook, and do a host of other odd jobs in return for the opportunity to indulge an old enthusiasm for natural history.

Linking my political science and natural history interests was the last thing on my mind. On the contrary, the appeal of a summer's work in field biology lay in its very distance from my academic interests, then focused on political economy, public policy, and the history of political theory.

By the time the summer ended, my enthusiasm for natural history had turned into an interest in biology. On returning to the university, I registered for a formal course in the subject. And then for another. And another. Each time I told myself that I would just take one more course, but I couldn't stop. Eventually it became

clear that I had to find a way to make these apparently disparate interests mesh: I told my astonished Department that I intended to return to school and, afterward, to write about science rather than taxes. That they tenured me anyway was an act of faith that I hope the essays in this volume go at least some way to justify.

In the years that followed, my disciplinary identity shifted from political scientist to historian of science. But my interests in public policy and political theory did not fade. They continue to inform, and I hope enrich, all my writing on the history of evolution and genetics.

The essays collected in this volume (as well as others written over the same period) explore specifically political dimensions of science. Of course, much recent work in science studies is also concerned with the political. Numerous ethnographic studies of laboratory life and detailed historical case studies have focused on the ways in which truth may be politically “negotiated,” often emphasizing the role of social interactions in determining scientific success. Steven Shapin has recently summed up the body of work in the sociology of scientific knowledge and related history and philosophy “as concerned to show, in concrete detail, the ways in which the making, maintaining, and modification of scientific knowledge is a local and mundane affair.”¹ But contrary to the old adage, not all politics is local.

Marxists have always recognized that fact. However, many scholars are made understandably nervous by accounts of scientific change and the reception of scientific ideas that invoke large sociopolitical forces. They associate such accounts with generalizations about society and class that are too broad to be defensible. But as I hope these essays help to demonstrate, analyses of politics outside the laboratory need not be crude. My aim is to contribute to a growing body of historical work on scientific knowledge and practice that is simultaneously broad in scope, politically relevant, and sensitive to nuance.

II

The essays collected in this volume, which were written over a period of a dozen years, explore the history of eugenics, biomedicine, and the nature-nurture debate. Many show how political factors underlie various apparent (and otherwise inexplicable) changes

in scientific and biomedical perspectives. They address such questions as the following: Why did assumptions about the role of genes in human behavior that were taken for granted in the 1960s come under vehement attack in the 1970s? Why did the same scientific principles that in the 1930s seemed to demonstrate a desperate need for eugenics come in the 1950s instead to explain its futility? Why was the distinction between good and bad eugenics abandoned in favor of a distinction between medical genetics and the “pseudo science” of eugenics? Why was carrier detection viewed in the interwar period as a means to root out defective genes and in the postwar period as a means to mask their effects? How was the history of screening for phenylketonuria transformed from the tale of a troubled program to the greatest success story of applied human genetics? But the essays also show that some apparently profound shifts were quite superficial; that changes in rhetoric may obscure the stability of core underlying beliefs.

The first essay in the collection, “Eugenics and the Left,” was published in 1984. It contests the then-conventional association of socialism with opposition to eugenics, noting that Marxist and Fabian condemnation of the race and class bias of the mainstream movement should not be equated with in-principle opposition to the rational control of reproduction. Indeed, to many Left intellectuals, especially in the sciences, eugenics seemed to follow logically from the rejection of *laissez-faire*. In the 1930s, 1940s, and 1950s, geneticists of every political persuasion were convinced that individuals varied in their genetic value and that the worthiest should be encouraged to have more children and the least worthy fewer or none.

A second argument challenges the (still common) claim that the post-World War II demise of eugenics is explained by the progress of science. It argues that the scientific findings said to have undermined eugenics in fact occurred much too early to account for postwar developments. In a recent essay, co-written with Hamish Spencer, entitled “Did Eugenics Rest on an Elementary Mistake?” I examine that argument with respect to the Hardy-Weinberg theorem (see chapter 7 in the present volume). An implication of that theorem is that, when genes are both recessive and rare, the number of carriers will vastly exceed those actually affected. Since segregation and sterilization do not reach the clinically asymptomatic, it might seem that programs of eugenical

selection are beside the point. But contrary to the conventional wisdom, few geneticists drew this conclusion. Instead, they saw the lesson of Hardy-Weinberg to be the need for research to detect apparently healthy carriers and prevent them from breeding. Nor is there any scientific reason that this theorem should have carried the same implications for them as it does for us. The lessons that now seem so plain follow only in the context of values that, though widely held today, were disdained by an earlier generation of geneticists.

Shifts in the meaning of “eugenics” and the struggles to demarcate it from genetics are explored in a number of essays. In “The Rockefeller Foundation and the Origins of Behavior Genetics,” we see that genetics has sometimes been equated with basic research and eugenics with applied research. Thus, to Foundation officers even a proposed Institute for Race Biology, the ultimate aim of which was to improve the “biological constitution” of the population, had nothing to do with eugenics. “Eugenic Anxieties, Social Realities, and Political Choices” identifies other conventional lines of demarcation, including motivation (where eugenics is equated with social goals, whereas medical genetics is identified with individual aims) and means (where eugenics is equated with coercion, whereas medical genetics is associated with freedom of choice). Reflecting my interests in political theory, this essay also analyzes the protean meanings of freedom and coercion, explores how “autonomy” came to trump every other value in the sphere of biomedicine, and probes some of the consequences.

“Eugenic Origins of Medical Genetics” provides a historical perspective on efforts to demarcate eugenics from other practices and also challenges the conventional view that eugenics fell into disrepute following World War II. In the 1950s and 1960s, medical geneticists often characterized their work as “eugenics”—though of a kind sharply distinguished from the “bad” eugenics of the past. But by the 1970s, the term had become disreputable. As a consequence, geneticists largely abandoned their attempt to distinguish good eugenics from bad. Medical genetics was now contrasted instead with the “pseudo science” of eugenics. At the same time, critics moved in the opposite direction, toward expansive definitions that associate genetic medicine with a host of now-despised practices.

“Genes and Contagious Disease” extends the arguments both of “Eugenic Origins of Medical Genetics” and “Did Eugenics Rest on an Elementary Mistake?” It explores what happened when eugenicists’ hopes of identifying clinically asymptomatic carriers were

finally realized. In the 1950s, the first reliable methods of carrier detection were employed not to efficiently root out defective genes, but rather to mask their effects—a strategy to reduce the immediate burden of genetic disease at the cost of increasing the incidence of disease-causing genes. The essay traces the scientific and social developments that explain why the new technology was turned in such a different direction than eugenicists had once expected. It also illustrates the difficulty of agreeing on a definition of eugenics. From one perspective, to embrace a masking strategy is to abandon eugenics, defined in this case as a concern with the future of the gene pool. From another perspective, it is seen to mark a turn from one kind of eugenics to another.

The final essay in this collection, “PKU Screening: Competing Agendas, Converging Stories,” concerns the first treatable genetic disease: phenylketonuria, or more simply, PKU. In 1948, the British geneticist Lionel Penrose used the disease to illustrate the futility of eugenical selection against rare genes. PKU has ever since been employed as a potent symbol.

Since the early 1960s, it has been possible to identify affected newborns, who can be placed on a diet that prevents the severe mental retardation associated with the disease. This therapeutic success is frequently cited by enthusiasts for genetic medicine, for whom it illustrates the good that screening can do. But the same case is as often invoked by skeptics of genetic medicine. For them, the treatability of the disease carries a different message—that genes are not destiny. While they draw disparate lessons from the PKU case, enthusiasts and critics have a joint interest in portraying treatment as simple and wholly effective. Alas, it is neither. As a result of these converging interests, an appealing but deceptive story about PKU has become entrenched in popular literature. And as “The Nine Lives of Discredited Data” shows, once entrenched, stories that carry moral or political messages are almost impossible to uproot.

III

These essays are political in more than one sense of the term. Most obviously, they are *about* the connections, at various levels, between politics and science. But they are also intended, in the broadest sense, as political interventions. I have entertained the hope that such historical work might be relevant to contemporary

debates about public policy. This political commitment has entailed efforts to write for disparate audiences: the general public as well as specialists, scientists as well as historians, and individuals who would contest, as well as those who share, my own broadly socialist perspective. To this end, I have written for journals as diverse as the *Journal of the History of Ideas*, *Science for the People*, *Newsweek*, *Scientific American*, the *Quarterly Review of Biology*, and *Nature*. Indeed, this diversity is one reason for collecting some of the essays in one volume.

For some time now, I have been distressed by the increasingly self-referential turn that much academic work in science studies seems to be taking. The influence of postmodernism has been productive in some respects but counterproductive in others. It has taught us the important lesson that the truth of our views is guaranteed neither by God nor by Nature nor by the laws of history. While the recent *Social Text* affair has generated a certain amount of romantic nostalgia for the old-fashioned, plain-speaking academic Left,² postmodernist excesses cannot be countered by reasserting old certainties.³ There are good reasons why foundationalism has fallen from favor. But taken to extremes, the romance with "theory" has rendered much work in science studies unintelligible both to scientists and the public.

At the same time, many scientists confuse the purpose of historical and sociological studies with advocacy: they expect deference from outsiders and dismiss even the most plain-speaking scholars who fail to oblige.⁴ It is not easy to speak across this science/humanities divide. Moreover, the mutual incomprehension that characterizes the current "science wars" has its analogue in the politics of many nonscientific debates. On the one hand are those who believe that they possess a privileged access to truth. On the other are those who have no grounding at all for their opinions; their claims are reduced to expressions of taste. Either stance renders political discussion pointless. There is no arguing either with ordained Truth or with likes and dislikes. We do not judge subjective preferences to be right or wrong, valid or invalid.⁵ But these essays were written on the assumption that there remains a role for reasoned argument in current debate, that it is still worth trying to reach across the profound and, unfortunately, deepening professional and political divides.

IV

Given my background and professional and political aims, I have naturally been interested in a related issue that has been the subject of passionate debate within the science studies community: Is a wholly social epistemology compatible with moral and political critique? It has often been charged that the symmetry principle—which holds that true and false beliefs need equally to be explained sociologically—undermines or even precludes an evaluative stance. Is the accusation justified?

The claims of such sociology of knowledge (SSK) practitioners as David Bloor, who adopts a stance of “moral neutrality,”⁶ or Harry Collins, who asserts that SSK leaves science exactly as it is,⁷ certainly lend it credence. In *The Golem: What Everyone Should Know about Science*, Harry Collins and Trevor Pinch tell the story of cold fusion. Their tale revolves around scientists and administrators whose motivations are crassly commercial. The desire to patent and market the discovery leads scientists to hype their results, which are announced at press conferences rather than in journals, and to withhold details of their experiments. But Collins and Pinch do not draw the conventional moral. If the market has fully penetrated the scientific arena, that is fine with them. “It is our image of science which needs changing,” they conclude, “not the way science is conducted.”⁸

Such an apolitical stance is unappealing to many SSK scholars. As both partisans and critics of current trends in science studies have noted, a “self-conscious radicalness” informs much of the literature, indicating that its authors are motivated at least in part by normative considerations. Bruno Latour writes of many postmodern critics of science that “they maintain the will to denounce and debunk, but have no longer any grounds to do so.”⁹ Politically engaged practitioners, on the other hand, insist that methodological relativism does not require evaluative restraint; indeed, that SSK can and should contribute to progressive or “emancipatory” goals. They believe these aims to be best served by a focus, not on whether scientific views are true or false, but on whether they are empowering or disempowering. In their view, it is possible to judge theories as emancipatory or oppressive, independent of any evaluations about their fit with the world.¹⁰ They want “not truth, but justice.”¹¹

That approach in effect reduces science, not only to politics, but to a highly subjective sort of politics. It thus ensures that critique will be solipsistic. Judgments about science would have meaning that is internal only to groups whose members already agree. For political minorities to be effective, they must show that those they criticize have violated widely shared norms. In arguing that Cyril Burt's work was sloppy or faked—that it was bad science—Marxists were able to appeal to individuals with very disparate political convictions. Does anyone think that labelling Burt's work "disempowering" would have been equally effective? Preaching to the already converted sometimes has a point. But for minorities, simply rallying the troops will never win wars. Jane Flax argues that "there may be more effective ways to attain agreement or produce change than to argue about truth. Political action and change require and call upon many human capacities including empathy, anger, and disgust."¹² But the efficacy of appeals to emotion cannot and should not be dissociated from beliefs about what is, in fact, the case. Even if we were comfortable with the view that scholarship should simply subserve politics, how will minorities provoke these politically useful emotions if they are barred from grounding appeals for justice in "reason, knowledge, or truth"?

Reducing science to politics is not a new endeavor. In the 1940s and 1950s, orthodox Marxists argued that the natural sciences are superstructural in the same way as politics and law. It seemed to follow that scientific theories serve the interests of either proletarians or the bourgeoisie. Looking back on the role of French communist intellectuals in the early days of the Cold War, Louis Althusser wrote:

In our philosophical memory it remains the period of intellectuals in arms, hunting out error from all its hiding-places; of the philosophers we were, without writings of our own, but making politics out of all writing, and slicing up the world with a single blade, arts, literature, philosophies, science with the pitiless demarcation of class—the period summed up in caricature by a single phrase, a banner flapping in the void: "bourgeois science, proletarian science."¹³

The theory of two sciences found its most brutal expression in the condemnation of genetics (and geneticists) as "Menshevizing," "idealist," and "undialectical."

In part reacting to that history, many Marxists have adopted an (Althusserian) approach that wholly dissociates science and politics. In this perspective, good science is uncontaminated by ideology. Associating themselves with the cause of science, Marxists have often denied the political dimensions of their own work. But if they are naive to view politics simply as a contaminant, they are right to resist the claim that science *is* politics. Unlike politics (or religion or art or, to take a recent famous example, baseball¹⁴) the *raison d'être* of science is the understanding of nature. To omit nature from our evaluations is to render the enterprise unintelligible. But there is a political reason as well to judge theories by their "fit with the world." For the Left, as for any minority, to accept the alternative is to court disaster.

Notes

1. Steven Shapin, "Here and Everywhere: Sociology of Scientific Knowledge," *Annual Review of Sociology* 21 (1995): 304.
2. Katha Pollitt, "Pomolotov Cocktail," *The Nation*, June 10, 1996.
3. Alan Sokal, "A Physicist Experiments with Cultural Studies," *Lingua Franca*, May–June 1996, pp. 62–64.
4. Cf. Peter Gross and Norman Levitt, *Higher Superstition: The Academic Left and Its Quarrels with Science* (Baltimore: Johns Hopkins University Press, 1994); Louis Wolpert, *The Unnatural Nature of Science: Why Science Does Not Make (Common) Sense* (London: Faber and Faber, 1992).
5. Charles Taylor, *Philosophical Arguments* (Cambridge: Harvard University Press, 1995), p. 254.
6. *Knowledge and Social Imagery* (London: Routledge and Kegan Paul, 1976), p. 10.
7. "An Empirical Relativist Programme in the Sociology of Scientific Knowledge," in K. Knorr-Cetina and M. Mulkay, eds., *Science Observed: Perspectives on the Social Study of Science* (London: Sage, 1983), p. 98.
8. Harry M. Collins and Trevor Pinch, *The Golem: What Everyone Should Know about Science* (Cambridge: Cambridge University Press, 1993), p. 78.
9. *We Have Never Been Modern*, trans. Catherine Porter (Cambridge: Harvard University Press, 1992), p. 291.
10. See Jane Flax, "The End of Innocence," in J. Butler and J. W. Scott, eds., *Feminists Theorize the Political* (London: Routledge, 1992); William T. Lynch, "Ideology and the Sociology of Scientific Knowledge,"

Social Studies of Science 24 (1994): 198; Kelly Oliver, "Keller's Gender/Science System: Is the Philosophy of Science to Science as Science Is to Nature?" *Hypatia* 3 (1989): 137–48; Bruce Robbins and Andrew Ross, "Mystery Science Theater," *Lingua Franca*, July–August 1996, p. 57; Steve Woolgar and Keith Grint, "A Further Decisive Refutation of the Assumption that Political Action Depends on the 'Truth' and a Suggestion that We Need to Go beyond This Level of Debate: A Reply to Rosalind Gill," *Science, Technology, and Human Values* 21 (1996): 353–57.

11. Flax, as cited by Rosalind Gill, "Power, Social Transformation, and the New Determinism: A Comment on Grint and Woolgar," *Science, Technology, and Human Values* 21 (1996): 351.

12. Flax, p. 458.

13. *For Marx* (London: Allen Lane, 1969), p. 1.

14. Stanley Fish, "Professor Sokal's Bad Joke," *New York Times*, May 21, 1996, p. A23.