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Why and How: Reflections in an Autobiographical Key

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My first book, *The Physicists,* was conceived when I. I. Rabi visited Princeton in 1961–1962 as a Shreve Fellow in the History Department (see Kevles 1995). Some two years earlier C. P. Snow had published his influential provocation, *The Two Cultures and the Scientific Revolution,* and the academic world was abuzz with initiatives aimed at achieving better literacy in science among liberal arts majors. Rabi was a Nobel laureate in physics at Columbia University and his visit was one of Princeton's efforts to this end. It had been arranged by my mentor, Eric F. Goldman, a historian of modern America who had gotten to know Rabi through their mutual engagement with current issues in American democracy.

Rabi was an impishly warm man with a shrewd understanding of human affairs and an avid interest in history. He was extensively experienced in the high councils of science and government, having been a key figure in the wartime mobilization of physics, including microwave radar and the atomic bomb project, and having served on the General Advisory Committee of the Atomic Energy Commission and on the President's Science Advisory Committee. The Shreve Fellowship had been established to foster historical studies that would ascertain the reasons for the "decay, degeneracy, extinction and destruction" of nations, as Eric gleefully pointed out to me. The idea behind appointing Rabi to the post – he was the first non-historian to hold it – was that he would help the humanists at Princeton learn how better to integrate science and its culture into their courses (presumably, one supposed, to forestall the decay of American civilization in the late twentieth century).[1]

I had majored in physics at Princeton and encountered Big Physics even as an undergraduate, working as a research assistant to Gerard K. O'Neill, a member of the physics faculty who was involved with the Princeton-Penn Accelerator, and then spending a summer with Gerry at Stanford, where he was developing the apparatus for colliding beam experiments with Wolfgang Panofsky. I was fascinated by the enterprise of Big Physics – my first published article was a profile of Gerry in the *Princeton Alumni*

[1] Cf. Press Release announcing Rabi's appointment, Sept. 19, 1961, Isidor Isaac Rabi file, Faculty A-107, Box 418, folder 1, Princeton University Archives.

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Weekly – by its resources, capacities, and scientific purposes, but I was made restless by the prospect of a career in large-scale, corporatized science.

I found history and writing more appealing, in no small part as the result of taking Eric's celebrated course in Modern America (my friends in the liberal arts had told me that it was a must, even for a physics major). I first got to know Eric by participating in a course project to rewrite the U.S. Constitution, adapting it to the realities of the welfare state and the country's commitments to collective security in the nuclear age (the idea of modifying the sacred document aroused scandalized comment in some quarters of the country). The experience prompted me to think about becoming a historian. Eric put me in touch with Charles C. Gillispie, who taught persuasively that science has a history that merits study. But not least – though not only – because of Eric, I wanted to work in American history. After getting my degree, in 1960, I spent a year studying history at Oxford – it seemed like a good idea to learn some European history, too – with the good fortune of having the historian Alan Bullock, Hitler's biographer, as my tutor and Alistair Crombie, a leading scholar of medieval science, as my guide in the history of science.

In 1961, I returned to graduate work in history at Princeton to study under Eric, thinking in some vague way that I might combine my interests and knowledge by studying the history of science in America. The subject was of little concern to most historians at the time, including historians of science, not least because science in the United States was taken to be a historical backwater, operating without distinction in the shadow of Europe. But Eric encouraged my inclination towards it. His graduate seminar, the year I took it, was devoted to the domestic history of World War I and was organized around writing a major research paper. Mine concerned the wartime use and postwar impact of intelligence testing in the American army and it eventually became my first scholarly article (see Kevles 1968).

What prompted my focus on the history of physics in the United States was sitting in on an informal discussion group of scientists and historians that Eric and Rabi established during Rabi's visiting year to explore the rise to power of science, especially physics, in the United States and its relationship to the larger society. The group, which met every couple of weeks or so at Rabi's Princeton home, included J. Robert Oppenheimer, who in the 1930s had created the nation's first school of theoretical physics, led the wartime effort to build an atomic bomb, and through the early 1950s was an exceptionally high-profile adviser in nuclear policymaking; Henry DeWolf Smyth, the author of the authoritative postwar report on the technical aspects of the atomic bomb and later a member of the Atomic Energy Commission; John A. Wheeler, one of the world's leading theorists of general relativity; the chemist Donald Hornig, who would later serve as Science Adviser to President Lyndon B. Johnson; George F. Kennan, perhaps the most influential figure in the shaping of postwar American foreign policy; and Charles Gillispie.

Eric had arranged for me to attend the gatherings and to write a paper for the group sketching out a course in modern American history that would integrate the

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development and role of science into the national narrative. The group's meetings were eye-opening, rare encounters with living history and proved to be one of the most influential elements in my graduate education. I gained specific information from them, of course, but, still more important, insight into their culture and experience as the scientists among them attempted to account for how they had progressed so rapidly in both task and sensibility from the exploration of subjects such as nuclear structure and quantum field theory to building atomic bombs, dramatically escalating the destructiveness of war, and riding the bronco of the postwar nuclear arms race.

By the time the meetings ended, I was fixed on the idea of writing my dissertation, and eventually a book, on how physicists in the United States had risen to world prominence and become an important power group in American society. This story, which reached back into the nineteenth century, was largely untold, and I thought that if it was populated by the kind of people in the Rabi discussion group, the tale would surely be fascinating in and of itself and perhaps bridge the two cultures. Towards that latter purpose, I intended that the book would both satisfy the rigorous requirements of historical scholarship and appeal to a general, educated audience. I saw no conflict between the two goals then, nor have I discerned any since.

I left Princeton, Ph.D. in hand, in June 1964, first for a brief stint with Eric on the White House staff – he was Special Consultant to President Johnson – and then to an assistant professorship in the humanities faculty of the California Institute of Technology (Caltech). Once I managed to devise the courses I was to teach and learned how to teach them, I began to develop my dissertation, which covered physics in the United States from the Civil War to World War I, into what became *The Physicists.*

My aim was to write a history of the physics community that would combine multiple subjects – professional, intellectual, social, institutional, economic, and political history. I knew of no models to follow, especially for such a topic set in the United States, so I invented my way forward. In my dissertation I had defined the community for myself by establishing a prosopographic profile *avant la lettre,* at least in the history of science, identifying its members and analyzing their career trajectories, using whatever biographical materials I could get.

The numbers told me that, while the American physics community grew a good deal in the later nineteenth century, it was still small in the early 1900s, a fact that, along with a lack of theory and an alleged national indifference to basic science, seemed to account for the weakness of physics in the United States. But then Paul Forman, John Heilbron, and Spencer Weart published their *Physics circa 1900*, a quantitative survey of the number of physicists at work in Germany, France, the United Kingdom, and the United States as well as of the funds and laboratories available for research (see Forman, Heilbron, and Weart 1975). Its results were striking, revealing that the number of physicists in the U.S. and the resources available to them were not much different from numbers and resources in Europe. Neither, according to my own statistical survey, were the research topics – for example, electricity, magnetism, and heat – on which they were publishing.

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The results prompted me to think about the structure and practices of the American physics community in the later nineteenth century with an eye to finding in its arrangements for intellectual governance reasons beyond the lack of theory for its relative weakness in the world rankings, even in experimental physics. That exploration led to the sociology of science and history done with sociological sensitivity, particularly the works of Robert K. Merton and his students at Columbia, especially Harriet Zuckerman and Jonathan Cole, and the books of Joseph Ben-David, Sally Kohlstedt, and Thomas Kuhn (cf. Zuckerman 1977, Merton 1966; Cole and Cole 1973; Ben-David 1971; Kohlstedt 1976; Kuhn 1962). The take-away for me was that science is an elite enterprise, in which a large fraction of intellectual advance in a field is produced by a small fraction of its practitioners. I added to the take-away my own view that the members of the elite tended to possess what Rabi called a "taste for good science" and that they needed some institutional means to encourage good taste in their disciplines. In the late nineteenth century, geology in the United States was not a backwater but was practiced with world-class distinction, and a comparison of the structures of intellectual governance in physics and geology showed why the one was strong and the other comparatively weak.

Since I aimed to integrate multiple topics into the book, it made good sense to draw on a broad range of secondary works that went beyond the history of science as such and – reflecting, I suppose, my training under Eric, in a history department – of diverse primary sources, including private manuscript collections, university and philanthropic archives, scientific papers, memoirs, and newspapers and magazines. Serendipity, in the form of my happening to be at Caltech, provided access to two crucially significant caches of manuscript – the papers of the astrophysicist George Ellery Hale, an enormously influential statesman of American science, and Robert A. Millikan, the nation's second Nobel Laureate in physics and a key figure at the intersection of physics, academia, industry, and government. (The Hale papers, which were at the Pasadena offices of the Mount Wilson Observatory, had been somewhat used by scholars; the Millikan papers, which occupied multiple old file cabinets in the basement of a Caltech lab, to my knowledge, not at all. Along with using both collections extensively, I edited a 100-reel microfilm edition of the Hale papers that is now available in a number of libraries around the world and arranged to have the Millikan papers organized and catalogued. I also obtained a small grant to establish a Caltech archive from Lee DuBridge, the Institute's president, who responded immediately and with enthusiasm to the proposal.)[2]

I also made use of interviews, some of which had been done by other scholars – of special importance were those in the *Archive for the History of Quantum Physics*,

[2] The Hale project was funded by the National Historical Publications Commission, and, as I recall, so was the Millikan cataloguing project. Caltech appointed Judith R. Goodstein, a recent Ph.D. in the history of science from the University of Washington, as head of the Archives. Over the years she built it into major archives for the history of science.

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a joint effort of Thomas Kuhn, Paul Forman, and John Heilbron – a number of which I conducted myself. My first interviews were not all that productive because my questions were too general ("Tell me about your career in physics"), yet at least one of them – a conversation with Edward Teller, in the spring of 1962 when I was writing my dissertation – was altogether the opposite, in more ways than one. Teller had recently been a guest on Eric's "Open Mind," a weekly NBC television talk show that he moderated and that was sometimes given over to a conversation with a single individual. Teller, a hard-line anti-Communist, was known as the father of the hydrogen bomb and, among liberal scientists, as a knife-in-the-back witness against Oppenheimer during his security hearing, in 1954. Eric asked Teller about Oppenheimer as a security risk. Teller sat tight-lipped, saying nothing for the better part of a minute, after which Eric turned to other subjects. Teller demanded that Eric's question and his silence be cut from the program, which had been taped, before it was broadcast. NBC agreed over Eric's vigorous objections, but when reporters called to ask what had been edited out, Eric told them, making Teller's silence front-page news the next day.[3]

When I got to Berkeley, Teller invited me for our chat to his home. We sat in his living room, a large round coffee table between us, and his wife, Mitzi, served me freshly baked chocolate chip cookies and a glass of cold milk. Our talk went pleasantly enough until Teller discovered that Eric was my doctoral supervisor. He told me that he had been under no obligation to answer Eric's question about Oppenheimer. I said that I agreed, thinking, though, that he had no right to suppress the fact that he had refused to answer the question. Not wanting to antagonize him, I kept that thought to myself, preferring to ask him about his testimony against Oppenheimer at the security hearings. He declared that Oppenheimer had been a pro-Soviet Communist. I countered that no one had ever produced a shred of evidence that Oppenheimer had been a card-carrying member of the Communist Party. He drew himself up and, wagging his finger at me over the milk and chocolate chip cookies, averred in a low but heated voice, "Young man, you don't understand that the Communists don't give cards to their most important agents."

The conversation taught me that interviews could be dramatically informative, not least about the beliefs, attitudes, and character of the interviewee. I also eventually realized that what made the encounter with Teller so revealing was that it centered on specific, concrete topics – Eric's program and Robert Oppenheimer's alleged political commitments. I thus sought to make interviews useful by coming to them with specific questions that arose from extensive previous research, and I adhered to that approach in the numerous additional interviews I've conducted for most of my books since.

Trying to integrate all the material I was gathering into a readable history that would appeal to both historical scholars and a broad general audience was a challenge all its

[3] Cf. *The New York Times*, April 16, 1962, p.1. In 1962, "The Open Mind" was awarded an Emmy, and in 1966 it won a second.

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own. I found inspiration and guidance to this end in Eric and his work. He was a brilliant writer whose books and essays were grounded in meticulous scholarship and appealed to a wide readership. If you want to write well, he would tell me, read good books, pay attention to how the authors do it, and write a lot. He often stressed that the reader could close the book at any time.

While attending to scholarly rigor, I made a point of trying to keep the reader interested by writing history that told a story while weaving analysis into the tale. I drew literary instruction from first-rate historians but also from novelists and journalists, some of them fast friends. Particularly influential among them have been Hugh Nissenson and Jane Kramer, each a master of the melding of craft, content, and the revelation of character. I sought to enliven my narratives by incorporating vignettes of historical actors high and low, bearing in mind the advice that Charles Gillispie once offered – that it is far better to see into people rather than through them.

I was advantaged in my scholarly purposes by being at Caltech. The Institute, while a powerhouse in science and engineering, was the opposite of intimidating. It was an informal, welcoming place, a kind of republic of science, where faculty at all ranks from the president on down were on a first-name basis and people cared not about who you were but what you had to say. I knew from the Rabi discussion group that many physicists were remarkably interesting and thoughtful human beings, and my life at Caltech, where I regularly lunched and partied with scientists, steadily reinforced that impression. To be sure, some were indifferent to the writing of history – Richard P. Feynman seemed, uniquely, contemptuous of it – but many of them liked to talk history, including history of science. A number of the physicists – for example, Lee DuBridge, Robert Bacher (the Institute provost), Carl Anderson (the chair of the Division of Physics, Astronomy, and Mathematics), and Robert Christy (a later provost) – were prominent members of what I came to call the Los Alamos generation. I picked up from them a rich store of their experience and lore, some of which made its way into my book but much of which informed my historical understanding.

The content and interpretation of the book was also shaped by what was happening in the world at large. It was clear from the outset of my work, if only from talking with Rabi, that the entrance of Jews into American physics was a salient part of the story of physics in the United States of the twentieth century. But the emergence of the civil rights and the women's movements prompted me to address in the book the issue of why through the second third of the century American physics included so few blacks, women, and Roman Catholics. Here I profited from the pioneering work of Margaret Rossiter on women in American science and from studies by several sociologists on the historical relationship of Roman Catholics to learning in the United States. I was also sensitive to the social upheavals of the 1960s and early 1970s as they bore upon the power and standing of physicists in American society, notably the indictment of the military-industrial complex, the open resistance to the development of anti-ballistic missiles, the attack against nuclear power, and President Richard M. Nixon's dissolution of the President's Science Advisory Committee. Physicists

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in the United States obviously commanded a fragile power and I was stimulated to deepen my thinking about the broad sweep of the community's development, including the problems that an elite of knowledge had long faced in the American democracy.

A significant part of *The Physicists* deals with science and government in the United States. Some of my most rewarding research was in the records of federal agencies and the congress, including congressional hearings. I was avidly interested in politics (I remain a political junkie to this day) and knew something about the workings of politics and government from direct experience – my stint on the White House staff and then in California my intermittent involvement in presidential and state campaigns. On the scholarly front, A. Hunter Dupree's pioneering classic, *Science in the Federal Government,* provided a solid historical starting point, and my thinking was also shaped by the political scientist Don K. Price, who was attentive to the inevitable tensions in the United States arising from the preference for autonomy of an elite of special knowledge and lay insistence on democratic control of the affairs of government (see, for example, Price 1965).

Then, too, at Caltech I was in the Division of Humanities and Social Sciences (the Institute, which has no departments, is organized into six interdisplinary divisions). I learned a great deal from my colleagues about economic and political ways of thinking – concerning, for example, investment in public and private goods and interest-group politics at both the congressional and federal levels – that contributed to my understanding of science in the American democracy. Since Caltech had no graduate program in history and its undergraduate history curriculum was marked by a lot of flexibility, I was able to teach the book I was writing, a practice I have followed with subsequent books. Trying to explain things to undergraduates imposes its own clarifying discipline on the presentation of a subject.

I spent some ten years writing *The Physicists*, and being among a small vanguard of the social history of science I ran into discouragements of various kinds. I was then – and for more than a decade afterward – the only historian of science at Caltech. I found professional sustenance elsewhere, notably in an invisible college of several sympathetic historians, especially John Heilbron, whom I first met when he heard me clattering away on an old manual typewriter in the archives of the American Philosophical Society; Paul Forman, to whom John introduced me; and Martin Sherwin, whom I first encountered in the Manuscripts Division of the Library of Congress and whom I noticed because he had jotted down in the registry book that his topic was Robert Oppenheimer. All were working on subjects complementary to my own. They took an interest in my project, as did I in theirs; John and Paul read drafts of chapters, and provided criticism and suggestions of indispensable value. Eric was particularly encouraging, too, listening to my ideas, giving critical readings to drafts of the manuscript, and bolstering my sometimes flagging resolve. And I had the good fortune to have in Ashbel Green at Alfred A. Knopf, an editor who was at once tough and seasoned but patient and forbearing.

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Among the reasons that I turned to a career in history was that you could write on any topic that piqued your interest. Having spent so long on the history of physics in the U.S., I wanted to pursue something else. The something else proved to be eugenics and genetics, a choice that led to my second book, *In the Name of Eugenics* (INE) (cf. Kevles 1985).

The inspiration for the choice came from contemporary events rather than noticing any holes in the historiography. What compelled my attention was the controversy over recombinant DNA. The heated debates over this new technology in molecular genetics reached across academia, the U.S. Congress and state legislatures, the media, and lay activists. For molecular geneticists, the upheavals over rDNA raised both challenges to the moral responsibilities of science and rich prospects for understanding gene function and commerce in biotechnology. For the critics, it posed threats to health and the environment and the possibility of a new eugenics. In all, the controversy formed another chapter, only this time in biology, in the book of tensions between the autonomy of professional science and democratic control of its activities.

Once I began to focus on the history of eugenics and genetics, I discovered that both subjects were sites of abundant historiographic opportunity. Several fine books had been published on both topics, but those in genetics ended before the era of biochemical and molecular genetics and those on eugenics dealt with the subject through the lens of the Nazis and the Holocaust. I realized that it would be advantageous to explore eugenics before 1933 on its own terms, recognizing that its advocates were ignorant of the future. That strategy led me to see that pre-Nazi eugenics was not only about social prejudice, that it involved left-wing radicals and progressives as well as conservatives, that it was about health as well as degeneration, and that it provided a way for women to enter the public sphere and without shame publicly to discuss, among other topics, sex, contraception, and pregnancy.

Several other scholars were beginning to explore the history of eugenics and genetics, among them Ruth Schwartz Cowan, Loren Graham, Bernard Norton, Donald MacKenzie, Kenneth Ludmerer, Garland Allen, and Nils Roll-Hansen. While I learned from all of them, Bernard, Donald, and Gar in particular formed my invisible college for this project. Having discovered in writing *The Physicists* the value of comparisons across national and disciplinary boundaries, I conceived what became *In the Name of Eugenics* from the outset as a cross-national treatment of its subject in the United States and Great Britain that would be grounded in part on a quantitative profile of the scientific actors in each country. The approach paid off quickly, providing fresh insight (with the help of an article on the ecology of genetics in the U.S. by Charles Rosenberg) on why the debate between the biometricians and the Mendelians in the United Kingdom was so heated when in the U.S. it was not (see Kevles 1980). The U.S.-U.K. comparison also explained why eugenic sterilization laws were enacted in the States but not in Great Britain.

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While I had fancied myself a young Turk in enlarging the history of science to include its social history, I sided with the older Turks in believing that the content of science belonged in the genre. I had incorporated the development of modern physics into *The Physicists*, in proportion and detail appropriate to the central subject of the book (the American physics community). I thought it all the more important to deal with the science in INE. Eugenicists were then (and are now) often written off as pseudoscientists, but in fact the biologists among them considered themselves serious scientists seeking to develop and apply knowledge of human heredity. They took their cues from the rest of genetics as it advanced, forging methods to study heredity in an organism that, unlike, say, fruit flies, reproduces infrequently and in limited numbers and cannot be subjected to controlled breeding experiments.

I tooled up for writing INE by reading genetics textbooks, talking to geneticists at Caltech, and taking several courses, including Edward Lewis' genetics course and Max Delbruck's course in molecular biology for physicists. I found it unnecessary – as well as impractical – to deal with the entire history of genetics in the book, but I did consider it essential to treat the evolving content of genetics in general as it related to eugenics and to account for the emergence of human genetics from the socially prejudiced and epistemologically flawed tangle of eugenic science. Here surveying a sample of journals to see who was publishing in human genetics and where the publishers were located paid off handsomely. Statistical analysis of the results demonstrated that the lead in human genetics was taken by scientists in the U.K., especially in the London region, and a cross-national comparison of their circumstances with those of their American counterparts revealed why this was the case. Analysis of the papers by the most productive publishers revealed the methods they developed to overcome the fact that you could not do breeding experiments with human beings and the characteristics to which they applied the methods in trying to forge a socially unbiased human genetics.

While I was working on INE, the development of the Sociology of Scientific Knowledge (SSK), whose roots trace back at least to the 1930s, was flourishing, advanced notably in France by Bruno Latour and in Great Britain, especially at the University of Edinburgh, by Barry Barnes and Donald MacKenzie (later joined by Steven Shapin). The formal articulation that the construction of scientific knowledge is a social process was helpful in analyzing the conditions that led to the early preeminence of the British school of human genetics. I also profited from the work of SSK because if any body of knowledge had been shaped by social commitments it was surely eugenics. Early eugenic science was utterly suffused with biases arising from class and racial prejudice.

I remain convinced that statistical analysis and theories or quasi-theories such as SSK about the making of scientific knowledge are helpful tools, and that so are Kuhn's insights. But in my experience, they are best seen as suggestive of lines of inquiry and interpretation rather than as ends in themselves. One reason is that they lack universality. Take Kuhn's views: a number of "revolutions" in science have

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arisen not from the breakdown of paradigms but from acquisition of data enabled by the deployment of new instruments. Think, for example, of the impulse given by microwave detectors to radioastronomy, by electrophoresis, radio-isotopes, and high-speed centrifuges to molecular biology, and by fast sequencers and computers to genomics.

Another reason is that overemphasis on the sociological, as distinct from the natural, in the construction of knowledge can all too easily undermine regard for the importance of the truth-content of knowledge as such. Nature can take care of itself. But what respect we accord – or do not accord – to various claims about nature matters a great deal, especially nowadays, when science – often contested for political, economic, and religious reasons – pervades so many areas of public policy and private decisions. Without giving adequate respect and attention to the truth-content of science – freely challengeable, as it must be, with evidence and argument – it is hard to see how one can, for example, beat back a racist or Lysenkoist biology or counter denials of anthropogenic global warming?[4]

To be sure, the prejudiced science of early eugenics was overturned in part by geneticists, psychologists, and anthropologists who were inclined by their social liberalism or radicalism or by their ethnicity to subject its claims to critical scrutiny. But their success was not the product of ideological declarations. It arose from extensive empirical research and evidence-based argument, the processes by which scientific knowledge has tended to develop.

I suppose that much of my own work has been in the end the product of a similar process. To be sure, since INE current events have continued to inspire my choice of topics for books I've subsequently edited or written, notably *The Baltimore Case* (see Kevles 1998), which concerned the persecution of a collaborator of the biologist David Baltimore by government investigators. Still, while that book was an exploration in contemporary history, I brought to the writing of it all the tools and rules of historical craft and scholarship I could muster. It was deeply grounded in evidence gathered from laboratory notebooks, scientific articles, interviews, congressional hearings, and government investigations, among other sources. (I obtained important parts of this material through the Freedom of Information Act, a tool that more historians of science might well learn to use.) In the end, I found the case a rich subject not only because I gained access to so much documentary material but because I was able to interview several dozen of the actors, including all the principals.[5]

[4] See Nils Roll-Hansen's work on Lysenko (2005) and Bruno Latour's reflections on these matters (2004), including his suggestion that SSK might be reformed and strengthened by approaching its subjects through multiple perspectives, what he calls "gatherings." It may be that I have long practiced the strategy of "gatherings" without knowing it.

[5] I have deposited the transcripts of the interviews along with the rest of my files for the book in the Rockefeller Archives, in Tarrytown, New York, for the use of future historians, who in time will be unable to talk to the principals.

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I have sometimes let ideas for books marinate until they are ready for active attention. I eventually made good on my assignment for the Rabi group, coauthoring a college text in American history that integrates the history of science and technology into the main national narrative (cf. Maier, Smith, Keyssar, and Kevles [2003] 2006). I am now completing a history of innovation and intellectual property (IP) protection in living organisms that originated in 1987, when the United States Patent and Trademark Office ruled that living animals were eligible for patents. The ruling aroused considerable controversy and attention in the media and the U.S. Congress.

I initially thought of the book as a way of clarifying the issues and interests involved, making it a contemporary exploration at the intersection of genetic engineering, the biotechnology industry, the social responses to both, and the law. But I set aside the project to write *The Baltimore Case* and my part of the U.S. history text. Whenever I took up work on the IP book, I kept getting forced back in time, eventually reaching the starting point of the story in the American colonial period. In recent years IP has commanded increasing attention among lawyers, economists, and historical as well as literary and art-historical scholars, and I have profited greatly by attending to their ideas My thinking has been especially enriched by colleagues and students in the Law School at Yale – I joined the University's faculty in 2001 – and by legal scholars elsewhere, including Lori Andrews, Rochelle Dreyfuss, Jon Harkness, and Christopher Beauchamp. Curiously, IP has captured little attention among historians of science. Exceptions to the rule include Mario Biagioli, Jean-Paul Gaudillière, Berris Charnley, Greg Radick, Adrian Johns, and Myles Jackson. They, especially Mario and Jean-Paul, have formed a large part of my current invisible college and acted as fellow evangels for the subject.

Why write the history of scientific subjects of contemporary salience? My fundamental reason is the belief that it is difficult to know where you are and where you may be going without knowing and understanding where you've been. Yet once I've chosen a contemporary subject, I have found that the pursuit of its history takes on a life of its own, poses its own demands – engaging with other scholars and their writings, mastering the scientific content of the story, obtaining evidence wherever it can be found, reading and interpreting it in the context of its own time, trying to understand the actors and their actions, and, ultimately, integrating what I learn into a narrative that is at once readable and analytic.

Moving from one major subject to another has sometimes been daunting, but I have found it tonic on the whole, a way of sustaining zestful engagement with the craft and content of history. Nowadays I find myself perusing nineteenth-century illustrations of animals, fruits, and vegetables and reading histories of food and agriculture, botanical explorations, seed and nursery catalogues, horticultural journals, business records, and court cases. All this is a long way from quantum physics and atom bombs, but if you relish the writing of history for its tempting new directions, the topical turns can lead to intellectually delicious places you never expected to be.

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References

Ben-David, Joseph. 1971. *The Scientist's Role in Society: A Comparative Study*. Englewood Cliffs NJ: Prentice-Hall.

Cole, Jonathan R., and Stephen Cole. 1973. *Social Stratification in Science*. Chicago: University of Chicago Press.

Forman, Paul, John Heilbron, and Spencer R. Weart. 1975. *Physics circa 1900: Personnel, Funding, and Productivity of the Academic Establishments.* Princeton: Princeton University Press (= vol. 5 of *Historical Studies in the Physical Sciences*).

Kevles, Daniel J. 1968. "Testing the Army's Intelligence: Psychologists and the Military in World War I." *Journal of American History* 50:565–81.

Kevles, Daniel J. 1980. "Genetics in the United States and Britain, 1890–1930: A Review with Speculations." *Isis* 71:441–455.

Kevles, Daniel J. 1985. *In the Name of Eugenics: Genetics and the Uses of Human Heredity*. New York: Alfred A. Knopf.

Kevles, Daniel J. 1995. *The Physicists: The History of a Scientific Community in Modern America*. Cambridge MA: Harvard University Press.

Kevles, Daniel J. 1998. *The Baltimore Case: A Trial of Politics, Science, and Character.* New York: W. W. Norton.

Kohlstedt, Sally Gregory. 1976. *The Formation of the American Scientific Community: The American Association for the Advancement of Science, 1848–1860*. Champaign: University of Illinois Press.

Kuhn, Thomas. 1962. *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.

Latour, Bruno. 2004. "Why Has Critique Run out of Steam? From Matters of Fact to Matters of Concern," *Critical Inquiry*, 30, 225–248.

Maier, Pauline, Merritt Roe Smith, Alexander Keyssar, and Daniel J. Kevles. [2003] 2006. *Inventing America: A History of the United States*, 2nd edition. New York: W. W. Norton.

Merton, Robert King. 1966. *The Sociology of Science: Theoretical and Empirical Investigations*, edited by Norman W. Storer. Chicago: University of Chicago Press.

Price, Don K. 1965. *The Scientific Estate*. Cambridge MA: Harvard University Press.

Roll-Hansen, Nils. 2005. *The Lysenko Effect: The Politics of Science*. Amherst NY: Humanity Books.

Zuckerman, Harriet. 1977. *Scientific Elite: Nobel Laureates in the United States.* New York: Free Press.

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