



Who is the Industrial Scientist? Commentary from Academic Sociology and from the Shop-Floor in the United States, ca. 1900-ca. 1970

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*This volume is dedicated
to the memory of
Keith Pavitt*

The Science–Industry Nexus

History, Policy, Implications

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SVEN WIDMALM

Editors

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Who is the Industrial Scientist?

*Commentary from Academic Sociology
and from the Shop-Floor in the United
States, ca. 1900-ca. 1970*

STEVEN SHAPIN

HERE ARE TWO STORIES about the identity, condition, and state of mind of the industrial scientist that circulated in America from the early 20th century to the 1960s or thereabouts. The first story had it that he—almost always “he” during this period—was unhappy, anxious, and maladjusted to his lot. Powerfully socialized as a young man into a unique set of scientific values associated with the university, he took industrial employment because suitable academic research careers were in short supply or because they were just too low-paid to keep body and soul together for those not keen on an ascetic way of life. He found adaptation to incompatible industrial moral economies difficult, sometimes smoldering with resentment throughout his career. As the twig was bent so grew the tree: socialization into such values was strong and consequential. The industrial scientist deeply disliked, if he did not actively rebel against, the violation of scientific values he found in industry: secrecy, regimentation, hierarchy, constraint, and short-termism. The money—for the money was on the whole good—never made up for it. The scientist was being forced into a gray flannel lab coat, and it just didn't fit. That was his central problem and it was a problem that industrial research managers would have to deal with as best they could.

This is a story about “conflict of interest,” though here the texture and vector of conflict are rather different from what they are understood to be in the present-day American research university. In this story, the emotional pull of the unique scientific ethos is so strong that there are grounds for worry that industry, or indeed governmental laboratories which share some of industry's characteristics, can obtain a sufficient supply of such men, or, when they are recruited, that they can be kept happy and productive. Of course, in the current state of affairs, concerns about “conflict of interest” usually take other forms. Now, university administrators and ethics committees understand that the pull of commercial lucre

is so strong that standards of proper academic behavior are seriously liable to corruption.

The historical sources of story number 1 are familiar to sociologists and historians of science. While such sentiments were fairly widely distributed in American culture in the early to mid-20th century,¹ the most influential systematic version of this story was elaborated by the late Robert K. Merton in essays of the early 1940s—modified and developed in the 1950s and 1960s by such students and colleagues as Bernard Barber, Norman Storer, Walter Hirsch, and Warren Hagstrom—and institutionalized in the canon of American sociology of science. The “norms of science” into which the scientist was socialized became, as Merton said, “internalized,” where they formed the scientist’s “conscience” or “super-ego.” Even though Merton was then understandably pre-occupied with such threats to scientific integrity as those posed by the Nazi idea of “Jewish physics” or the Soviet concept of “bourgeois genetics,” his initial description of the “institutional ethos” of science also remarked upon the tensions between scientific and commercial values. In science, Merton pointed out, *Die Gedanken sind frei*, and the “rationale of the scientific ethic” whittles down “property rights in science” to the “bare minimum” needed to secure recognition and esteem to the originator of a scientific idea. Science is public knowledge or it is not science at all, and the very idea of secrecy offends those who have internalized its values:

The communism of the scientific ethos is incompatible with the definition of technology as ‘private property’ in a capitalistic society. [...] Patents proclaim exclusive rights of use and, often, nonuse. The suppression of invention denies the rationale of scientific production and diffusion.

And Merton here specifically alluded to late nineteenth-century litigation between the Federal Government and the Bell Telephone Company which established the inventor’s “absolute property” in his invention and his right to withhold crucial knowledge of it from the public. For mid-twentieth-century scientists, Merton wrote, this was a “conflict-situation,” and, while different scientists were more or less uneasily responding to it in different ways (by taking out patents, by becoming entrepreneurs, or, indeed, by advocating socialism), nevertheless the fundamental friction arose from a conflict of values between pure science and commerce. Conflict was always likely to appear whenever science came into contact with institutions whose values differed from those needed for the pursuit of certified objective knowledge and which attempted to enforce “the centralization of institutional control.”²

From these early statements of the norms of science, there emerged what seems to be a *prediction* about what empirical research would eventually show, if, indeed, systematic empirical research was deemed necessary to confirming such a matter-of-course state of affairs: scientists socialized into this value system would suffer the “pain of psychological conflict” when presented with situations which required or encouraged them to behave in ways that violated the norms they

had acquired. To avoid or free themselves of this “pain,” it was “to their interest” to conform to the ethos in which they had been socialized. Should the internalized “pure science sentiment” be put under pressure by “other institutional agencies” committed to the application of knowledge and concomitant organizational control, the result will be “the persistent repudiation by scientists of the application of utilitarian norms to their work. [...] The exaltation of pure science is thus seen to be a defense against the invasion of norms which limit directions of potential advance and threaten the stability and continuance of scientific research as a valued social activity.”³ So it might be deduced that, for both psychological and social-functional reasons, scientists would vigorously resist assimilation into the value-system of commerce—such was the strength of the psychological grip of scientific values and such was the functional dependence of science on the embrace of these values. The scientist in industry, or in the military, would be an unhappy, awkward, and possibly even a disloyal figure, in constant conflict with commercial values and organizational structures. As a result of scientists’ unique pattern of socialization, their personalities were such as would not tolerate organizational constraints: scientists were too fiercely independent and mindful of their individual integrity, too skeptical, too hostile to authority structures, too loyal to science and too disloyal to local organizational values. Such persons would pose a major problem for the smooth running of commercial organizations.⁴

The work of Merton, Barber, Storer, and, to a lesser extent, of Hagstrom was largely programmatic, but by the 1960s academic sociologists—of whom the most prominent were William Kornhauser at Berkeley and Simon Marcson at Rutgers—were producing extended quasi-empirical studies of the “scientist in industry,” centering on the problems of normative “conflict” and assessing the extent to which both industry and the scientists themselves had been obliged to come to some sort of in-practice “accommodation” between value systems that were in such fundamental in-principle conflict.⁵ In 1956, the founding of the academic journal *Administrative Science Quarterly* (ASQ) proceeded from a desire for a “general theory of administration,”⁶ while a number of contributions to early issues—notably the work of Merton’s student Alvin Gouldner on “locals and cosmopolitans” in organizational life—fit within the general framework that diagnosed conflicting loyalties between the scientist and other employees of commercial organizations.⁷

Certainly by the early 1950s, the conflicted and unsatisfactory position of the scientist in American industry and in governmental research facilities appeared to commentators of several sorts as an enormous practical problem in the context of the Cold War: how to recruit such people in numbers sufficient to respond to the massive Soviet threat? The low birth rates of the 1930s Depression Era combined with the burgeoning demand for scientists and engineers to drive the Cold War arms race, with the result that the “scientist-gap” and the “engineer-gap” preceded the “missile-gap” in American anxieties during the 1950s and 1960s.⁸ Few publications from this period dealing with recruitment and with allied problems of

retention, creativity, morale, and the efficient use of research workers omitted to justify the salience of such problems to the Soviet threat, and Federal government agencies, led by the Office of Naval Research, were prime sources of funding for social science work in this area. (Thomas Kuhn's paper on "The Essential Tension," which was the precursor to key ideas in his seminal 1962 *The Structure of Scientific Revolutions*, was given in 1959 to a conference on scientific creativity that included contributions from officials in the Pentagon's Advanced Research Projects Agency, the Air Force Personnel and Training Research Center, as well as the Dow Chemical Company.)⁹ Already by the late 1940s, the Steelman Report to the President's Scientific Research Board devoted much attention to problems of recruitment, creativity, and morale, and worried *inter alia* that scientists socialized into a preference for the freedom, autonomy, openness, and collegiality of academic settings might constitute a substantial obstacle to Cold War scientific and technological mobilization against the Soviet threat.¹⁰

In this Cold War context, while academic social scientists elaborated a picture of the industrial scientist made-unhappy-through-early-socialization, they had allies in university departments of industrial relations and in business schools, as well as in strands of non-academic cultural commentary. So, for example, the Harvard Business School professor Charles Orth wrote in 1959 that corporate managers completely fail to understand "the kind of people who work in the world of science and the influence on their values of the special training which made them scientists." Socialized in academic environments, scientists all want to do basic research; they are accustomed to, and value, "an independence of thought" and a "permissive, low-pressure atmosphere;" they care deeply about the good opinion of their disciplinary colleagues and less about the approval of their corporate superiors. That's just what scientists are like, whether the cause is academic socialization or the selective recruitment into a scientific career of personality types (Orth didn't especially care): "the resultant personality matrix of the scientist is familiar to anyone who has associated at all closely with these men." Put these sorts of people in industry, and "the fact that they think and behave differently makes life difficult for practically everyone in an industrial organization who must deal with them. Businessmen find it hard to understand almost everything such men say and do." Indeed, scientists don't enter industry "because they like the idea of working for an industrial organization"; they are drawn to it against their own inclinations, because they need the facilities, resources, and "somewhat larger salaries" that industry can offer and that academia cannot. The scientist "typically does not really want to work for an organization."¹¹

Here Orth acknowledged indebtedness to the most widely distributed 1950s picture of the disastrous, but necessary, engagement between scientists and industry, William H. Whyte's *The Organization Man*. The centerpiece of Whyte's book was a set of three chapters on "The Organization Scientist," which identified the pressures brought on scientists in American industry to conform to industrial values, work conditions, and structures of authority—pressures which were well on their way to eroding the nation's capacity for technological and commercial inno-

vation at just the historical conjuncture when those capacities were most needed. For Whyte, as for Merton, the crux of the matter was a conflict of values between science and those institutions called upon to support and enlist science, and in particular a failure on the part of sustaining institutions to comprehend the unique values which alone would encourage scientific geese to lay their utilitarian golden eggs. In the relevant sections of *The Organization Man*, the target was an obtuse, and ultimately self-destructive, unwillingness on the part of industrial managers to recognize authentic scientific values and to accommodate those values in the organizational life of the industrial research laboratory. His fundamental argument, Whyte said, was that "between the managerial outlook and the scientific there is a basic conflict in goals." The current "orgy of self-congratulation over American technical progress" attributed it overwhelmingly to "the increasing collectivization of research." But this was a mistake that was storing up enormous trouble for the national future. If America effectively organized the individual researcher out of existence, it would eventually put an end to creativity, and, by extension, to America's ability to compete, commercially and militarily.¹²

We are, Whyte wrote, currently lectured by bureaucrats and industrial managers on "how the atom bomb was brought into being by the teamwork of huge corporations of scientists and technicians." Only occasionally does someone have the good sense "to mention in passing" that the creative impulse for the atomic bomb was an individual, unorganized, and spontaneous act of genius—indeed, something that "an eccentric old man with a head of white hair did back in his study forty years ago." American industrial management was working remorselessly to "mold the scientist to its own image; indeed, it saw the accomplishment of this metamorphosis as the main task in the management of research." If it succeeded, Whyte judged, it would be committing suicide, for "every study" has demonstrated that the "dominant characteristic of the outstanding scientist" is "a fierce independence" that will not tolerate control, interference, or collectivization: "In the outstanding scientist [. . .] we have almost the direct antithesis of the company-oriented man." American industry's aversion to the necessary eccentricity of genius was, for Whyte, signaled by a Monsanto recruiting film in which the voice-over announced "No geniuses here; just a bunch of average Americans working together."¹³

So that is story number 1, or at least a plausible enough précis of it for present purposes, since the story is still so familiar. There is, however, quite a different story—more precisely, a set of stories—about the identity, condition, and state of mind of the American scientist-in-industry during the 20th century, and these stories are not at all well known among academic social scientists and historians. The literature from which these stories emerge is seldom cited, and, from what I can tell, scarcely read, by relevant sociologists and historians, with the result that the sensibilities represented there are rarely, if ever, confronted by academic writers. And probably the major reason why this literature is little known, and why, therefore, its lessons are little engaged with, is that this commentary emerges not from the academy but from industry itself, or, more specifically, from those research

managers and administrators whose job it was, from early in the century through the 1960s, to recruit, manage, coordinate, and motivate the work of scientists in industry.

Sources for story 2 can be found in a wide variety of places. From the early 1950s, industrial consortia and managers established journals of their own in which to trade experiences about problems encountered in the new, and fast-changing, world of commercial research: *Industrial Laboratories* in 1949, then *Research Management* in 1957. The *I[nstitute of] R[adio] E[ngineers] Transactions on Engineering Management* intermittently published practical commentary on problems of research management in issues from 1955 to the early 1960s, as did *Chemical and Engineering News*, *The Journal of Industrial and Engineering Chemistry*, *Mechanical Engineering*, *The Technology Review*, *Personnel* (the periodical of the American Management Association), and similar periodicals. Conferences and publications on research management convened and sponsored by such Big Business firms as Standard Oil of New Jersey, Standard Oil of California, and the Big Business consortium, the Industrial Research Institute, are further sources for such stories; and, from the 1920s through the 1960s, a small number of the more reflective research executives published books on the subject of organizing industrial research facilities and administering scientists. I call these sources "shop-floor" commentary, even if there must be some pertinent differences in experience and perspective between research managers and the research workers they manage.

In marked, if unsurprising, contrast to the academic commentary on research management, shop-floor writing displayed no interest whatever in making points of general sociological interest, in using passages of research management as "case-studies" for any other purpose than coming to some more-or-less robust findings about recurrent problems in and about the industrial laboratory and in proffering some more-or-less plausible practical solutions to those problems. So, for example, a research director at RCA in the early 1950s rejected the pertinence of general theories of administration in terms typical of practical administrators, insisting that the requirements and problems of research management "will vary among different units of industry" and will even depend upon the "differing outlooks towards research" taken by "individual research administrators."¹⁴ As an agricultural research administrator succinctly put it in 1926, "It is a condition and not a theory that confronts us."¹⁵ This material is not academic in tone or appearance: there are rarely, if ever, any footnotes or literature references—for writing in this genre that is contemporary with, or subsequent to, the work of Merton and his followers, it is as if such work never existed—and the evident purpose is not apologetic, defensive, or even celebratory, but, rather, in the spirit of trading "war stories" among congenial colleagues. The question here is always practical management, not sociological generalization: how can one make industrial scientists more productive, more creative, happier, more likely to stay with the firm? what forms of organization work best for the industrial research laboratory, or for particular types of laboratories and in different industries and in firms of certain

sizes? how does one attract the most able research workers and how can one recognize the signs of ability in potential recruits? That is to say, while the shop-floor literature cannot possibly be confused with academic social science, it is concerned with social arrangements and relations which overlap massively with those of the academic genre. It is warrantably *about* the same social world, and, therefore, one would expect that social problems identified as referentially central in the academic tradition ought to preoccupy writers of the practical literature as well. Unhappy, value-conflicted industrial scientists were, in part, what theory predicted, but the same unhappy scientists, and the same grounds for their unhappiness, should be an enormous practical problem for research managers and their corporate associates.

However, with vanishingly few exceptions—exceptions which may dissolve on further investigation—unhappy industrial scientists, made unhappy by the strength of their socialization into unique academic values, *just do not exist* in the commentary of research managers and allied executives.¹⁶ And some of the rare reflections on the transition-process produced by practicing research workers themselves draw attention to disorientation in leaving the *more* "regimented" world of thesis-research, in which you had just one supervisor to satisfy, for an industrial laboratory in which the lines of control were not nearly so clear. True, orientation to commercial outcomes is not something that the newly-minted Ph.D. was accustomed to, but those who chose industry might either embrace or accept those goals, and "In general," as one newly recruited electrical engineer said, "industrial research is well-respected in the academic world."¹⁷ It is not that the industrial research laboratory is seen as problem-free: far from it; story 2 is about nothing *but* problems and their possible resolution—problems of recruiting, remunerating, retaining, motivating, organizing, and directing the labors of research workers. It was very widely recognized that research workers moving from university to industry may go through processes of adaptation, but such adaptation was usually seen in terms of getting people and their families settled in (schools for the kids, canasta-partners for the wives, etc.); getting research workers familiarized with organizational culture, routines, and expectations; and sometimes, indeed, getting them accustomed to a more team-orientated style of work than was typical in academia.¹⁸ Nor is it that members of industrial research facilities were necessarily seen as "one big happy family"—though such attitudes were sometimes expressed—or that serious tensions were not recognized to exist between companies' research functions and such of their other arms as accounting and production.¹⁹ In shop-floor commentary, the industrial research laboratory was full of tensions, just as its place in overall corporate culture continued to be problematic throughout my period of interest. But to recognize such tensions and conflicts was much the same sort of thing as it was to recognize endemic tensions and conflicts between, say, firms' production and marketing divisions or, on the production floor, between supervisors and skilled workers.²⁰ Just as these sorts of tensions and conflicts are acknowledged and dwelt upon in internal business commentary, so the research managers whose writings make up story 2 were

obsessed by the organizational problems of the industrial research laboratory. It is just that the persistent and consequential problem of socialization so precisely and persistently identified in the academic literature did not exist in shop-floor commentary.

Indeed, there are important and pervasive strands of such commentary that portrayed the quotidian realities of industrial research in ways that make academically predicated role-conflict highly problematic. Doing major violence to the heterogeneous nature of this literature, I draw attention here to just a few of these realities and associated values. First, the related questions of autonomy and planning. Autonomy of research work is always a relative matter. If the university was an institution marked by the value it placed on intellectual autonomy—and, indeed, a number of research managers vigorously insisted that it was, and that this marked a fundamental difference between academia and industry²¹—nevertheless the substantial reality was that some scientists might choose industry because they would in fact be freer to do the work they wanted. In 1905, Willis Whitney recruited William Coolidge from MIT, and, more famously, in 1909, Irving Langmuir from the Stevens Institute of Technology, to conduct fundamental research at General Electric, if not precisely free as birds, then certainly freed from heavy teaching loads and academic colleagues' lack of interest in research, with the resultant commercial bonanza from improvements in the tungsten-filament incandescent light-bulb and the incidental benefit of the 1932 Nobel Prize in chemistry for Langmuir.²² The chemist Wallace Carothers, later the discoverer of nylon, reckoned Harvard to be the "academic paradise" for teaching, but Du Pont was able to woo him away in 1928 because, as Hounshell and Smith say, "he really did not like to do it. He preferred research." Carothers cared about the higher salary that industry offered, but worried about the potential loss of "the real freedom and independence and stability of a university position." However, weeks after his move to Du Pont, he had no regrets, writing to a friend: "Already I am so accustomed to the shackles that I scarcely notice them. [. . .] Regarding funds, the sky is the limit. [. . .] Even though it was somewhat of a wrench to leave Harvard [. . .], the new job looks just as good from this side as it did from the other."²³ Reflecting on university conditions for research in 1916, Charles Steinmetz, chief consulting engineer for GE, wrote about the "false commercialism," which figured professors' "output" in their pedagogical production rate, and which "wasted the universities' best assets, its professors":

Thus we find in our colleges men who had shown themselves capable as investigators to do scientific research work of the highest order, overloaded with educational or administrative routine, and deprived of the time for research work. Private industries rarely commit such crimes of wasting men on work inferior to that which they can do: industrial efficiency forbids it.²⁴

Many commentators, both in academia and industry early in the century, reckoned that research, even of the fundamental sort, might not have a future in the American university because of its primary commitment to teaching and because

of its poverty of resources to support research. Autonomy doesn't mean much—then or now—if you can't get the time or funds to do the research you want to do.

Of course, industrial research managers made no bones about the fact that the scientists in their employ were free only within limits, and that those limits were ultimately defined by their company's commercial concerns. Recognition of that constraint was consistent from the origins of industrial scientific research. In 1919, the physicist Frank Jewett of science-friendly AT&T remarked that "The performance of industrial laboratories must be money-making. [. . .] For this reason they cannot assemble a staff of investigators to each of whom is given a perfectly free hand."²⁵ And a few years later, his colleague John J. Carty insisted that "Unless the work promises practical results it cannot and should not be continued." Corporate scientists must ask themselves, and be required by their superiors to answer, the fundamental question "Does this kind of scientific research pay?"²⁶ And in the 1950s, the Director of Research at Minneapolis-Honeywell was one of many in his position underlining the requirement that "*the results of research must pay*."²⁷ Research administrators at Owens-Illinois Glass Company noted in the 1960s that there was no point in directing fundamental research if commercial benefit was not reasonably to be expected of it. Research executives were firm in their position: there was no reason for a company to support fundamental research, so to speak, for its own sake, nor for industrial research workers' enjoying the freedom to pick any kind of problem they pleased: glass companies could not be expected to support employees' fundamental research in oceanography.²⁸ Creative people were, of course, always likely to be tempted into "fascinating but irrelevant side alleys," and then it was the supervisor's task to get them back on organizational track, but not before checking that the by-ways were really devoid of commercial potential.²⁹ At the same time, there was no reason for research workers to imagine a conflict of agendas where none necessarily existed: as James Fisk of Bell Labs put it, "Because a man considers the needs of the organization that employs him, there is no reason to think that this makes for any lesser contribution in the scientific sense."³⁰ "Our fundamental belief," Fisk wrote, "is that there is no difference between good science and good science relevant to our business," and by the late 1950s Bell Labs had the Nobel Prizes to support their claim.³¹

Managers commonly acknowledged it to be in the nature of genuine research that the unexpected sometimes turned up, and that these unexpected outcomes might be commercially consequential. Research worthy of the name was always to a degree unpredictable, and expressions of that sort of sentiment were absolutely standard among industrial research managers. In 1950, Kenneth Mees and John Leermakers of the Eastman Kodak Research Laboratory insisted that "It is really not possible to foresee the results of true research work," and in 1958 a GE administrator writing about "Free Inquiry in Industrial Research" defined research as "systematic inquiry into the unknown," drawing from that bland definition the conclusion that the "detailed course of a scientific inquiry" is always subject to unpredictable vicissitudes, and, consequently, that "a certain amount of

freedom on the part of the investigator" is implied by the very idea of research.³² Even if such a thing were possible, it was very widely understood that the firm would get few benefits from research whose outcome *was* wholly predictable. Moreover, it was appreciated that some (not all) research workers were the sorts of people from whom one could not get the best without finding ways to give them their heads. In the ongoing, and highly politicized, debates over the extent to which industrial research, or research of any kind, could and should be *planned*, some research managers made a distinction between the possibility and even necessity of planning what they called the "function" of research over a number of years—that is, organizing commitments to it and its place in corporate activities—and planning the "act" or "conduct" of research, in which considerable freedom of action was deemed simply necessary.³³ David Noble's tendentious survey of early American industrial research correctly cites occasional corporate rhetoric pointing to the desirability of tightly managing the creative process, while eliding any distinction between such rhetoric and shop-floor realities and ignoring a large body of managerial rhetoric that frankly acknowledged limits to any such control.³⁴

Research managers defended their own autonomy by persuading their bosses—to the extent they could—that the research function could *not* be held to the same system of cost-benefit accountability as other corporate activities. On the one hand, they wanted, and sometimes obtained, financial and temporal flexibility. Kenneth Mees at Eastman Kodak secured a commitment of ten years' worth of research funding from George Eastman in 1912; a consortium of oil company executives counseled against expecting returns from industrial research in less than five to seven years; and one survey of industrial research managers in the late 1960s found expected average "payback" times from R&D of about four years—with big firms having a more generous time-horizon than smaller ones—though another survey by the management consultancy firm Booz, Allen & Hamilton noted with alarm that less than a quarter of American large companies had *any* formal method for evaluating research, still less calculating a payback time.³⁵ In 1916, Mees wrote that "Those with the most experience of research work are all agreed that it is almost impossible to say whether a given investigation will prove remunerative or not."³⁶ And Charles "Boss" Kettering, when he was recruited from National Cash Register by Alfred Sloan to be director of research at General Motors, made sure that Sloan understood "You can't keep books on research, because you don't know when you are going to get anything out of it or what it is going to be worth when you get it."³⁷ RAND Corporation economists in the late 1950s drew importantly upon such sentiments in arguing vigorously against imposing rigorous cost-benefit regimes on research and development.³⁸

On the other hand, allowing research workers a significant amount of company time to do their own research is definitely not the late 20th-century invention of the Silicon Valley high-tech and biotech "knowledge economy." Mees encouraged such autonomous research in the 1910s, and a survey of industrial practices in 1950 found that an allowance of 10% to 20%—that is, a half-day to a day a week, with company resources to match—was then common, even in American

"smoke-stack" industry.³⁹ The president of Dow Chemicals said that he had "learned that if a research laboratory is to produce results, the men must be allowed the freedom to be a bit crazy," and the American Cyanamid research director quoting this remark approved up to 20% paid company time for research personnel "to work out their own ideas."⁴⁰ For very highly prized scientific employees that free time could amount to considerably more: the terms that won William Coolidge for GE in 1905 included one-third—in other versions one-half—of his time to continue an existing personal research project.⁴¹ When Du Pont tried to recruit the organic chemist Louis Fieser from Bryn Mawr in 1927, Fieser was struck by the research freedom on offer: "I never expected to go into industrial work but the thing which makes a decision so difficult in this case is that I don't have to sell my soul at all; they even said I could bring my quinones along and continue my present work."⁴² And a 1952 survey reported that, for some individuals, in some industrial organizations, "no plans are made as to how they shall spend their time."⁴³ While David Hounshell is surely right in identifying this sort of research latitude as a recruiting and retention tactic for some "academic elitists" who saw industrial research "as a poorer career option than that offered by a university or a private basic research institute," he does not claim that all research workers felt that way, and there is abundant evidence of *commercial* justifications for considerable degrees of industrial research freedom.⁴⁴

Eastman Kodak's Kenneth Mees became famous in management circles for his celebration of research *disorganization*. The epigraph to his influential 1920 book on *The Organization of Industrial Scientific Research* boldly stated that "There is danger in an organization chart—danger that it be mistaken for an organization," the source of which was not a scientist fed up with corporate bureaucracy but one of the founders of American technical management consultancy, Arthur D. Little.⁴⁵ Disorganization, and the research autonomy consequent on recognition that planning in these areas was naturally constrained, were justified as conditions of commercial functionality. Mees was quite hard-headed enough to insist that "the primary business of an industrial research laboratory is to aid the other departments of the industry," and that its central responsibility was to contribute to the corporate bottom-line,⁴⁶ but he rejected the distinction—central to the writings of the British anti-planning Society for Freedom of Science—that it was only "pure" science that was incompatible with planning. Commenting on early writings by Michael Polanyi, Mees said that "I take issue with [the] description of applied science as a field in which freedom of science might conceivably be undesirable. I have been engaged in applied science for forty years, and in that period I have come very definitely to the conclusion that the prosecution of applied science in its most efficient form is identical with that of pure science. I don't think for a moment that it is desirable that applied science should be directed *except in times of emergency*."⁴⁷ Mees' own much-quoted aphorisms include: "When I am asked how to plan, my answer is 'Don't,'" and "No director who is any good ever really directs any research. What he does is to protect the research men from the people who want to direct them and who don't know anything about

it."⁴⁸ And the only remark by Mees that finds its way into the reference books is his most eloquent condemnation of research control:

The best person to decide what research work shall be done is the man who is doing the research. The next best is the head of the department. After that you leave the field of best persons and meet increasingly worse groups. The first of these is the research director, who is probably wrong more than half the time. Then comes a committee, which is wrong most of the time. Finally, there is the committee of company vice presidents, which is wrong all the time.⁴⁹

This sort of sensitivity to the pathological consequences of attempts to control research was, according to the President of Bell Labs in the 1940s and '50s, something that "All successful industrial research directors know [...] and have learnt by experience:" the "one thing a director of research must never do is to direct research, nor can he permit direction of research by an supervising board."⁵⁰ The same Minneapolis-Honeywell research director who insisted on the bottom-line criterion for judging research results concluded his piece in *Industrial Laboratories* by conceding that, while "the amount of freedom or control of research projects is probably the most difficult question in its administration, [...] I tend toward the principle expressed by Thomas Jefferson for government: 'The least government is the best government.'"⁵¹

In practical terms, such sensibilities translated into a significant tolerance for the spontaneous and unpredictable emergence of novel research agendas among industrial research workers who enjoyed significant freedom to formulate their courses of work. In his own laboratory, Mees had to warn a scientist who developed an interest in high-vacuum pumps and gauges that this was work in no way compatible with the concerns of a photographic company, and one lesson drawn from this story was that scientists in Eastman Kodak's research laboratory could *not* do just anything they wanted. But Mees then repeatedly went on to relate that when he saw what a splendid technology was being developed, he secured the resources to spin off a distinct commercial laboratory, one which ultimately became a highly profitable vitamin-producing joint venture with General Mills. Here, as elsewhere, the general lesson that Mees wanted research managers to learn was that autonomy was after all good business. There was no alternative to a high degree of autonomy, and attempts to be unremittingly hard-headed were ultimately self-defeating.⁵² Intriguingly, in the very same year that Whyte's *Organization Man* appeared, with its eloquent condemnation of industry's commitment to crushing scientific genius under the dead weight of teamwork and management control, his own *Fortune* magazine ran a piece celebrating changed industrial sensibilities towards basic research and its demands for individualism, under a title—"geniuses now welcome"—that gave the lie-oblique to Whyte's story.⁵³

A second basis for industrial scientists' supposed discontent, and consequent role-conflict, was the secrecy that was a necessary feature of commercial research.

And, indeed, no research manager commenting on what is now called intellectual property thought that scientists could possibly be allowed freely to publish trade secrets. Their work was understood to belong to the company, and it was for the company to decide what could or could not be published in the open scientific literature.⁵⁴ Scientists joining Du Pont, for example, were obliged to sign non-negotiable agreements that all "inventions, improvements, or useful processes" made while in the company's employ were the "sole and exclusive property" of Du Pont, and they agreed not to "disclose or divulge confidential information or trade secrets."⁵⁵ However, in practice many research managers vigorously endorsed the commercial prudence of a quite free publishing policy and argued for the barest minimum of secrecy. The aphorism "When you lock the laboratory door, you lock out more than you lock in" comes not from Robert Merton or Michael Polanyi but—in the same temporal and cultural context—from "Boss" Kettering at General Motors.⁵⁶ The free flow of technical information, or, at least, the freest flow compatible with broad corporate interests, was widely, if not universally, acknowledged in these circles as a net benefit to all parties.⁵⁷

Research managers had to advertise their laboratories as workplaces attractive to the most talented research workers, many of whom were found in academia, and there was no better way to do this than to encourage their scientists to participate in professional society meetings and to publish in the same journals as their academic disciplinary colleagues. So in 1948 the Director of Research at Sylvania wrote that "The reputation of the organization whose research men do publish their findings will be enhanced according to the caliber of the work done"; an oil company research executive agreed that encouraging research workers to publish their results—subject, of course, to "adequate patent protection"—brings "substantial indirect value to a company, which gets a reputation for being willing to have its men present papers and for being progressive. It is not just a matter of humoring the man, but can usually be justified from the company viewpoint"; a Vice President of Union Carbide noted that "Forward-looking companies have come to realize that they can attract better men if they are willing to permit publication at the earliest moment compatible with a proper regard for economic advantage"; and a research manager at the General Electric Research Laboratory pointed out (without qualification) the likelihood and advantages of reciprocity: "Any laboratory that must do good basic scientific research must also encourage open publication of research results because this policy insures access to the work of other laboratories." For these, and many other reasons, a chemical company research director insisted that "a liberal company policy on publication" was simply a "must": "Only a blind or short-sighted management curbs or prohibits publication."⁵⁸

Even at Du Pont—a company that some academic scientists reckoned to be unduly secretive—official policy was massaged by research directors who recognized that a liberal publication policy was just necessary for attracting first-rate chemists and maintaining their morale. So in the late 1920s two leading Du Pont research managers recruited scientists by assuring them that "the work of these

[fundamental researchers] shall be published almost without restriction," and, as Hounshell and Smith document, that open policy with respect to Du Pont's fundamental research was effectively realized.⁵⁹ In addition, publication was understood as a vehicle for interesting disciplinary communities to take up your problems, whether in their relatively pure or relatively applied forms. The more people working away in your area, the better for you. (And that is one, sometimes unappreciated, reason why Edward Teller at Livermore in the 1950s urged the freest possible dissemination of information concerning thermonuclear weapons.) Patentable findings, of course, had to be legally protected, and, while patents had the desired effect of securing property rights to the company, they were understood also as a way of ensuring that the findings were in the public domain, thus satisfying, through a different route, those scientists who craved publication and consequent recognition from their peers. An RCA research director writing about recruitment problems noted that publication was important to young scientists, but so were patent and incentive awards, as each of these was a sign that the company recognized individual contributions.⁶⁰ Moreover, in fast-moving fields, the trick was not to secure advantage by locking up all possible intellectual property but by keeping one step ahead of the competition. Secrecy in such fields was of little concrete value. And a Sylvania research manager noted that whatever necessity there might be, so to speak, to embargo publication was usually only "temporary": commercial commitment to publication could and should remain as a principle and as a substantial reality.⁶¹ Such a widely accepted principle could and did co-exist with the practical possibility that publication might, at any moment, be prohibited or delayed for commercial reasons—and industrial research workers understood this very well. Some potential recruits to industry bridled at this state of affairs, but many others did not: Reich writes that even with substantial "restrictions on communication and publication, Whitney had little trouble finding researchers willing to work at GE," with even such a prestigious and well-equipped institution as MIT being a rich hunting-ground.⁶²

Finally, and most interestingly from the point of view of academic theories of socialization, a number of industrial research managers were worried that their newly recruited academic scientists became *too quickly and too totally accepting of the values and research agendas of what they took to be corporate, as opposed to academic, culture*. At Eastman Kodak, Mees judged it very important that personal credit for research be given to individuals and publication under his name. "The publication of the scientific results obtained in a research laboratory is quite essential in order to maintain the interest of the laboratory staff in the progress of pure science. [...] When the men come to a laboratory from the university they are generally very interested in the progress of pure science," Mees wrote, "but they rapidly become absorbed in the special problems presented to them, and without definite direction on the part of those responsible for the direction of the laboratory there is great danger that they will not keep in touch with the work that is being done in their own and allied fields. Their interest can be stimulated by journal meetings and scientific conferences, but the greatest stimulation is

afforded by the publication in the usual scientific journals of the scientific results which they themselves obtain."⁶³ The practical problem pointed to here was not the strong and persistent socialization into academic values predicated by sociologists but its opposite—the matter-of-fact willingness of research workers trained in universities to *abandon* such putatively distinct values. And it was that spontaneous abandonment which concerned research managers like Mees—not for moral or ideological, but for wholly practical, reasons.⁶⁴

One could go on painting this sort of picture of American industrial research from the beginning of the 20th century to the 1960s or so. It would be a picture that, minimally, makes the academically predicated role-conflict problematic and, if it were taken to an extreme, would argue the total illegitimacy of traditionally established contrasts between institutional values, just as it would open up the possibility of a more "evolutionary" than "revolutionary" account of widely imitated practices in contemporary American knowledge-intensive industry. But even at this superficial level of detail the evidence presented here licenses some brief speculations about the relations between strands of rhetoric and institutional realities in this area.

First, despite this evidence, I cannot see any reason to dispute *all* versions of academic theories taking as their task the identification of distinctive values attached to universities and to commercial undertakings. If the question is one that asks what values *distinguish* academia from industry, then I can see no better response than that which points to academic values clustering around disinterestedness, autonomy, spontaneity, and openness, versus commercial values centering on concrete economic outcomes, organization, planning, and the control of intellectual property. In academic institutions, it might plausibly be said, the "Mertonian" values can be publicly celebrated as institutional essence, while in industrial research they are more often asserted tactically as reminders to the uninformed that research is, to a great extent, an uncertain business, not to be subjected to the accountability regimes of other corporate activities. Yet, a theory of ideal-typical *differences* between institutional cultures is one thing and a description of quotidian realities in complex institutional environments is quite another. Those in the practical business of managing research enterprises have tended to acknowledge the intractable problems of distinguishing between these institutional environments, for theorizing essential differences has been of little concern to them. A paper company research director, given the task of speaking about such things in the mid-1960s, threw up his hands in despair: "Neither industrial nor academic research is, as it were, monochromatic; by whatever criteria we choose for judgement, both yield very broad spectra." He found intra-group differences at least as great as inter-group differences, and thought it "quite possible that there are no such unique species as industrial *vis-à-vis* academic research."⁶⁵ Just as nominalism or particularism in these matters seems the natural attitude of the practical actor, so theorizing essential differences has an affinity with the ideological work of distributing value across the institutional landscape. So among academic commentators, most especially in the social sciences, there has been a persistent ten-

dency to elide a distinction between ideal-typifications and accounts of institutional realities. The task of defending and criticizing institutional kinds is thereby made so much easier.

So far as possible, apples should always be compared with apples and not with oranges. It may well be that the best response to the questions "What makes university science different from industry?" and "What are scientists like?" is that offered in story 1. For all that, certain obstinate facts remain: (1) by the middle of the 20th century the majority of academically trained American scientists did not work in universities, and neither industrial nor government laboratories had any problems recruiting as many as they wanted, even if there was always a struggle for the best and brightest;⁶⁶ (2) American universities, certainly at the beginning of the century, and arguably to the present day, were not globally regarded as natural homes for research: most were under-resourced; most had a primary commitment to teaching;⁶⁷ many experienced cultural, political, and religious pressures that seriously compromised any notion that universities, as such, were communities of free, open, and suitably resourced inquirers.⁶⁸ With due respect, the University of Southern Mississippi is not MIT, and, as we have seen, General Electric was well able to attract distinguished researchers away from even MIT because there was more effective freedom of action at the company.⁶⁹ Free action in research is a matter of material resources and time as well as of rhetoric and ideals. A comparison between conditions at Harvard and at the scientific laboratory of a small chemical company might sustain story 1, but many highly pertinent comparisons between particular institutions of higher education and many particular industrial research facilities definitely do not. Such comparisons are as available in the 1920s as they remain today. And it is good to remember the restrictions on free research problem-choice that were, and remain, endemic in even the best universities. As a member of a sociology department, for example, I am free in principle to conduct research in any area I like, but I cannot expect, and I do not receive, credit from my colleagues for work they do not recognize as sociology, nor am I institutionally well placed to obtain support for any research I might propose to do in psychology or economics or particle physics. Moreover, as a research director for the Carrier Corporation noted in the early 1960s, "I know of some cases where competent Ph.D. candidates left the universities of their choice [for industry] because they either were not allowed to work on a topic picked by them, or they could not get as advisor the man they wanted in their own field."⁷⁰ These, and other, autonomy-restricting states of affairs are not at all uncommon in academia, nor are they inconsistent with saying that universities have an in-principle association with the ideal of research freedom.⁷¹

Second, as I have already noted, some academic sociologists, and most explicitly those involved in the early ASQ, were in the business of elaborating what they called "a general theory of administration," and in that cause both ideal types and the most robust possible generalizations of organization similarities and differences were central. It is in such a theoretical context that questions like

"What makes the academy different from industry?" take on special salience. The shop-floor commentary, by contrast, was not about offering an opposing theory of administration, nor, even, of asking "What makes the academy similar to industry?" Such commentary took problems as they presented themselves to managers concerned to cope with their own quotidian circumstances and to learn from others' experience if and when such experience seemed warrantably pertinent. The research managers, so to speak, played Simplicio to Galileo's Salviati: they wanted to know how to deal with *this* research laboratory; they were not much interested in specifying "the nature of science," "the nature of the university," or "the nature of industry." There is, of course, no escaping the science studies principle that "it's rhetoric all the way down"—how else would you have access to past organizational realities except through the heterogeneous commentaries of various participants and observers? But one reason why I feel somewhat more confident in the empirical grip of the shop-floor version is that there is no evident commitment to theory-building and no evident apologetic and justificatory flavor to such commentary. If the scientists were being awkward for the reasons predicated by the academic sociologists, I feel reasonably confident that the research managers would have said so.

So there are, indeed, problems with the empirical adequacy of the academic story, problems similar to those pointed out in the late 1960s and early 1970s by such British sociologists of science as Barry Barnes, Steven Box and Stephen Cotgrove, and even by such renegade Merton students as Norman Kaplan, whose work in this area now appears outstandingly sensitive and prescient.⁷² In this connection, criticism of academic theorizing culpably disengaged from empirical realities can be joined to an historical appreciation of the circumstances in which such a story emerged and secured some local credibility. On the one hand, an academic social science strongly committed to establishing its scientific bona fides was actively seeking for theories of great predictive power, just as it was looking for typologies that allowed practitioners to sort institutions into their species and genres. To that extent, I consider that much academic social science which dealt with "unhappy industrial scientists" was in large part (not wholly) *deducing its objects from theory*. At this distance, one has to take on trust the interview-responses recorded by academic sociologists documenting substantial "role conflict" among industrial scientists in the 1960s, but it is not impossible to give alternative plausible interpretations to some of this evidence, and one wonders why a post-World War II survey of scientists' attitudes saw no problems of bias in posing such questions as this: "*Aside from money considerations*, where do you think a person can get most satisfaction from a career in science—in the Federal Government, in an industrial laboratory, in a university, or somewhere else?"⁷³ Nor is it any part of my argument that all, or even a certain high percentage, of industrial scientists were happy as clams at high tide—that would be a remarkable state of affairs for members of any organization, much less organizations with such a high degree of organizational uncertainty as industrial research laborato-

ries. Nor is there any reason to ignore occasional—though scarce—expressions of unhappiness among industrial scientists that drew upon cultural repertoires that circulated in the cultural neighborhood and that might lend some credibility to story 1. However, what is undeniable is the remarkable gap between the world as seen by academic sociologists in mid-century and the same bit of the world as seen by research managers.

Moreover, the cultural and political conjunctures from which this academic commentary emerged had characteristics that made the story about role-conflict especially appealing to American social scientists and humanists. If natural scientists and engineers in the Cold War period had institutional options—many could jump from the university to industry or government if they wished—the great majority of social scientists and humanists did not.⁷⁴ The university was their natural home in a much stronger sense than it was the natural scientists' home. And, as we well know, during the Cold War that home, and the values that were most cherished by the humanists, were under serious attack. Many of the social scientists and their allies did indeed defend themselves against McCarthyism and the militarization and commercialization of the academy, but, more to the point, some emerged as the most articulate defenders of *science*, whether or not they identified what *they* did as science. So, for example, in 1958, Merton's colleague Paul Lazarsfeld wrote that college professors as a body tend to be naturally selected from a "permissive" and "liberal" environment: "Once he is on campus, economic and social circumstances also militate against a conservative affirmation by the academic man." But for that minority who came into the academy from a different background, there might be problems adjusting:

For a young person coming from a business background, a teaching career involves a break in tradition. The business community has an understandable affinity with the conservative credo, with its belief in the value of tradition and authority, its corresponding distrust of people who critically scrutinize institutions like religion and the family, and its beliefs in the social advantages of private property and the disadvantages of state interference in economic affairs.⁷⁵

Role-conflict was thus naturalized by some sociologists as a feature of the academic identity, whether humanist, social scientist, or natural scientist. The message was in effect "We are all in this together," and Oppenheimer's struggle in the 1954 security hearings was assimilated to their own. If you wanted to fight external control, and, of course, many humanists and social scientists during the Cold War did *not*, then damaged scientific allies were nonetheless important allies. The sociologists developed a picture of the nature of the scientific community that invited scientists to consider themselves allies of their non-scientific colleagues. That picture was *inter alia* a resource for healing emerging fissures in elite American universities. It was a cause as noble as it eventually proved to be futile.⁷⁶

ENDNOTES

1. Polemical writing against the obstruction or perversion of pure science by the interests and norms of business appeared in the late 19th and early 20th centuries; see notably Henry Rowland, "A Plea for Pure Science," in *The Physical Papers of Henry Augustus Rowland* (Baltimore: The Johns Hopkins Press, 1902; art. orig. publ. 1883), pp. 593–613; Thorstein Veblen, *The Higher Learning in America: A Memorandum on the Conduct of Universities by Business Men* (New York: Sagamore Press, 1957; orig. publ. 1918).
2. Robert K. Merton, "The Normative Structure of Science," in idem, *The Sociology of Science*, ed. Norman W. Storer (Chicago: University of Chicago Press, 1973; art. orig. publ. 1942), pp. 267–278, (pp. 273, 275, 278). See also Bernard Barber, *Science and the Social Order* (New York: Collier Books, 1952); Warren O. Hagstrom, *The Scientific Community* (Carbondale: Southern Illinois University Press, 1975; orig. publ. 1965); Norman W. Storer, *The Social System of Science* (New York: Holt, Rinehart & Winston, 1966); idem, "Science and Scientists in an Agricultural Research Organization: A Sociological Study" (Cornell University, Unpublished Ph.D. thesis, 1961); Walter Hirsch, *Scientists in American Society* (New York: Random House, 1968).
3. Merton, "Normative Structure," p. 260.
4. Here I acknowledge the continuing interest of my former colleague Barry Barnes' early publications, identifying in the Mertonian story empirical and theoretical awkwardnesses that flowed from a specific theory of socialization, and pointing to an alternative such theory in the work of Howard Becker: Barry Barnes, "Making Out in Industrial Research," *Science Studies* 1 (1971): 157–175; cf. David Bloor and Celia Bloor, "Twenty Industrial Scientists: A Preliminary Exercise," in *Essays in the Sociology of Perception*, ed. Mary Douglas (London: Routledge & Kegan Paul, 1982), pp. 83–102; Michael Aaron Dennis, "Accounting for Research: New Histories of Corporate Laboratories and the Social History of American Science," *Social Studies of Science* 17 (1987): 479–518. However, my purpose here is not the same as Barnes's: though there is, indeed, criticism of the Mertonian account in this essay, my major impulse here is to show the interest of interpreting it as a historical product.
5. William Kornhauser, *Scientists in Industry: Conflict and Accommodation* (Berkeley: University of California Press, 1962); Simon Marcson, *The Scientist in American Industry: Some Organizational Determinants in Manpower Utilization* (New York: Harper & Row, 1960).
6. E.g., Edward H. Litchfield, "Notes on a General Theory of Administration," *Administrative Science Quarterly* 1 (1956): 3–29; see also James D. Thompson, "On Building an Administrative Science," *ibid.*: 102–111; Talcott Parsons, "Suggestions for a Sociological Approach to the Theory of Organizations (I) and (II)," *ibid.*: 63–85, 225–239.
7. Alvin W. Gouldner, "Cosmopolitans and Locals: Toward an Analysis of Latent Social Roles," *Administrative Science Quarterly* 2 (1957–1958): 281–306, 444–480.
8. For representative practical expressions of such anxieties, see, e.g., John R. Steelman, *Science and Public Policy: A Report to the President by John R. Steelman, Chairman, The President's Scientific Research Board*, 5 vols. (Washington, DC: Government Printing Office, 1947), Vol. 3, p. 28 (also p. 141); Phillip N. Powers, "Industrial

- Research Workers and Defense," in *Selection, Training, and Use of Personnel in Industrial Research*, Proceedings of the Second Annual Conference on Industrial Research, June 1951, eds. David B. Hertz and Albert H. Rubenstein (New York: King's Crown Press, 1952), pp. 94–112, esp. p. 98; Donald M. Laughlin, "The Engineer Shortage: Challenge to Management," *Personnel* 29 (1952): 418–421; Charles D. Orth, 3rd, "More Productivity from Engineers," *Harvard Business Review* 35, no. 2 (March–April 1957): 54–62 (esp. p. 58).
9. *Scientific Creativity: Its Recognition and Development: Selected Papers from the Proceedings of the First, Second, and Third University of Utah Conferences: "The Identification of Creative Scientific Talent,"* eds. Calvin W. Taylor and Frank Barron (New York: John Wiley & Sons, 1963), see especially contributions by Maury H. Chorness of the Air Force Personnel Research Laboratory; J. H. McPherson of Dow; and N. E. Golovin of ARPA, who made his concern with the U.S.–U.S.S.R. creativity-gap quite explicit. The research of one of this volume's editors (Calvin Taylor) was funded by the Air Force Personnel Laboratory and that of other contributors by the ONR.
 10. Steelman, *Science and Public Policy*, Vol. 3, pp. 28, 141, 205–252.
 11. Charles D. Orth, 3rd, "The Optimum Climate for Industrial Research," *Harvard Business Review* 37, no. 2 (March–April 1959): 55–64 (pp. 55–56, 64); reprinted in *Science and Society*, ed. Norman Kaplan (Chicago: Rand-McNally, 1965), pp. 194–210. For insistence that Progressive Era industrial scientists identified very strongly with the companies that employed them and embraced their values, see Patrick J. McGrath, *Scientists, Business, and the State, 1890–1960* (Chapel Hill: University of North Carolina Press, 2002), esp. pp. 19–21.
 12. William H. Whyte, Jr., *The Organization Man* (New York: Simon and Schuster, 1956), pp. 205–206.
 13. *Ibid.*, pp. 206–207, 211, 214. (No such "studies" were actually cited.)
 14. Elmer W. Engstrom, "What Industry Requires of the Research Worker," in *Human Relations in Industrial Research, Including Papers from the Sixth and Seventh Annual Conferences on Industrial Research: Columbia University, 1955 and 1956*, eds. Robert Teviot Livingstone and Stanley H. Milberg (New York: Columbia University Press, 1957), pp. 69–79 (p. 69). (Engstrom was a radio engineer who had been a director of research at RCA since the 1940s, and in 1951—when his talk was delivered—was Senior Executive Vice-President of that company).
 15. Carleton R. Ball, "Personnel, Personalities and Research," *Scientific Monthly* 23 (1926): 33–45 (p. 35).
 16. The closest thing I can find to an expression of role-conflict-through-academic-socialization from an industrial source is Lowell W. Steele, "Rewarding the Industrial Scientist: A Problem of Conflicting Values," in *Human Relations in Industrial Research*, eds. Livingstone and Milberg, pp. 163–175. But while Steele was indeed an executive at General Electric, it is relevant that he was not a natural scientist or engineer by training and had no direct shop-floor experience. Steele was an industrial personnel manager at the GE Research Laboratory, in charge of salary practices. He took a Harvard M.B.A. in 1948 (where he may well have encountered the views of the previously mentioned Charles Orth) and, later, an MIT Ph.D. in economics and social science under the supervision of Herbert A. Shepard. Whyte's *Organization Man* (pp. 211–212) used Steele's earlier views prominently as an authority on the crushing demands for loyalty and conformity rightly made on scientists by commercial organizations: Lowell W. Steele, "Personnel Practices in Industrial Laboratories," *Personnel* 29 (1953): 469–476 (p. 471). Steele evidently had a change of heart on this issue between 1953 and 1957.
 17. I. Gorog, "Successful Adjustment to Industrial Research: How Can University and Industry Assist," *Research Management* 9 (1966): 5–13 (pp. 7, 10–11). Gorog was a 1964 Berkeley Ph.D. in electrical engineering, then working at RCA. See also J. J. Tietjen, "The Transition from Graduate School to Industrial Research," *ibid.* 9 (1966): 109–113.
 18. Horace A. Secrist, Director of Research at The Kendall Company, noted that industrial management "often does not fully realize how great an adaptation the scientist does succeed in making when he moves from the university to the industrial laboratory," drawing attention here not to programmatic norms but to some mundane aspects of adjusting to *teamwork*: Secrist, "Motivating the Industrial Research Scientist," *Research Management* 3 (1960): 57–64 (pp. 58–59), and a research manager at Sun Oil noted the importance of showing recruits—through organization charts—where they fit in the corporate scheme of things: E. M. Kipp, "Introduction of the Newly Graduated Recruit to Industrial Research," *ibid.*: 39–47. On the pragmatics of adjustment, see, for example, Warner Eustis (of The Kendall Company), "Personnel Policies and Personality Problems," in *Research in Industry: Its Organization and Management*, ed. C. C. Furnas (New York: D. Van Nostrand, 1948), pp. 277–294; John A. Van Raalte, "Reflections on the Ph.D. Interview," *Research Management* 9: 307–317 (esp. pp. 307, 315).
 19. See Tom Burns, "Research, Development and Production: Problems of Conflict and Cooperation," in *Administering Research and Development: The Behavior of Scientists and Engineers in Organizations*, eds. Charles D. Orth, 3rd, Joseph C. Bailey, and Francis W. Wolek (Homewood, IL: Richard D. Irwin, Inc. and the Dorsey Press, 1964), pp. 112–129 (orig. publ. *IRE Transactions on Engineering Management* [March 1961], pp. 15–23), for a study of tensions between the research facility and production functions in British companies, and p. 123 for a participant's idyllic account of relations within the laboratory: "In the lab, we're very happy—sort of happy family relationships. The lab chief must select people on the grounds of getting on with others; they certainly do get on with everybody."
 20. Steele, "Personnel Practices in Industrial Laboratories," p. 471.
 21. There are countless examples of such sentiments among industrial research administrators. For a representative example, see C. E. Kenneth Mees, *The Organization of Industrial Scientific Research* (New York: McGraw-Hill, 1920), pp. 20–21; also John J. Carty (of AT&T), "Science and Business: An Address to the Chamber of Commerce of the United States, May 8, 1924," *Reprint and Circular Series of the National Research Council* (Washington, DC: National Research Council, 1929), pp. 1–2; David F. Noble, *America by Design: Science, Technology, and the Rise of Corporate Capitalism* (New York: Alfred A. Knopf, 1977), pp. 129–131; Willis Jackson (of Metropolitan-Vickers [UK]), "Discussion," in *The Direction of Research Establishments: Proceedings of a Symposium held at the National Physical Laboratory [Teddington] on 26th, 27th & 28th September 1956* (New York: Philosophical Library, 1957), p. A.3: insisting that "the primary function of university research was in its relation to teaching," he urged that the goals of academic research should never be defined by any outside sponsor, and "a clear distinction should be made between universities and national as well as industrial laboratories;" John A. Leermakers, "Basic Research

- in Industry," *Industrial Laboratories* 2, no. 3 (March 1951): 2-3: "It is in the universities that true intellectual freedom, so essential to the development of the scientific attitude, is found." Note, however, that Leermakers was then an associate of Eastman Kodak's Kenneth Mees, whose views on the freedom required by industrial research are quoted below.
22. Leonard S. Reich, *The Making of American Industrial Research: Science and Business at GE and Bell, 1876-1926* (Cambridge: Cambridge University Press, 1985), pp. 69-70, 75, 82-83, 92, 111, 126-127; also Daniel J. Kevles, *The Physicists: The History of a Scientific Community in Modern America* (New York: Vintage Books, 1979), pp. 99-101.
 23. David A. Hounshell and John Kenly Smith, Jr., *Science and Corporate Strategy: Du Pont R&D, 1902-1980* (Cambridge: Cambridge University Press, 1988), pp. 230-231.
 24. Charles P. Steinmetz, "Scientific Research in Relation to the Industries," *Journal of the Franklin Institute* 182 (1916): 711-718 (pp. 711-712).
 25. Frank B. Jewett, "Industrial Research," *Reprint and Circular Series of the National Research Council*, #4 (Washington, DC: National Research Council, 1919), p. 7 (quoted in Kevles, *The Physicists*, p. 100).
 26. Carty, "Science and Business," quoted in Noble, *America by Design*, p. 115.
 27. Waldo H. Kliever, "Design of Research Projects and Programs," *Industrial Laboratories* 3, no. 10 (October 1952): 6-13 (p. 6); see also McGrath, *Scientists, Business, and the State*, pp. 12-14.
 28. James W. Hackett and B. L. Steierman, "The Organization of a Fundamental Research Effort at Owens-Illinois Glass Company," *Research Management* 6 (1963): 81-92 (pp. 85-86).
 29. C. George Evans, *Supervising R&D Personnel* (New York: American Management Association, 1969), p. 24.
 30. Hackett and Steierman, "The Organization of a Fundamental Research Effort at Owens-Illinois Glass Company," pp. 85-86; see also James B. Fisk (Bell Labs), "Basic Research in Industrial Laboratories," in *Symposium on Basic Research, Sponsored by the National Academy of Sciences, the American Association for the Advancement of Science, and the Alfred P. Sloan Foundation*, ed. Dael Wolfe (Washington, DC: American Association for the Advancement of Science, 1959), pp. 159-167 (p. 163).
 31. Quoted in Francis Bello, "The World's Greatest Industrial Laboratory," *Fortune* (November 1958): 148-157, 208, 212, 214, 219-220 (pp. 212, 214).
 32. C. E. Kenneth Mees and John A. Leermakers, *The Organization of Industrial Scientific Research*, Second Edition (New York: McGraw-Hill, 1950), p. 223; Malcolm H. Hebb, with Miles J. Martin, "Free Inquiry in Industrial Research," *Research Management* 1 (1958): 67-83 (p. 67). Both Hebb and Martin were physicists who moved to the GE Research Laboratory from academic appointments sometime after 1955.
 33. E.g., Merritt L. Kastens, "Research—A Corporate Function," *Industrial Laboratories* 8, no. 10 (October 1957): 92-101; D. L. Williams, *Planning of Research and Development* (New York: Wallace Clark, 1947), p. 5; David B. Hertz, *The Theory and Practice of Industrial Research* (New York: McGraw-Hill, 1950), p. 202; Harvey Brooks, "Can Science Be Planned?" in idem, *The Government of Science* (Cambridge, MA: MIT Press, 1968), ch. 3, esp. pp. 59-72.
 34. Noble, *America by Design*, ch. 7, esp. p. 118: "As the industrial research laboratories grew in size, the role of the scientists within them came more and more to resemble

- that of workmen on the production line and science became essentially a management problem."
35. Arthur W. Baum, "Doctor of the Darkroom," *The Saturday Evening Post* (25 October 1947): 15-17, 47, 50, 52 (p. 17). (I thank Ray Curtin of the Eastman Kodak Company for this reference.) See also Mees, *The Organization of Industrial Scientific Research*, p. 102; idem, *From Dry Plates to Ektachrome Film: A Story of Photographic Research* (New York: Ziff-Davis, 1961), p. 50; Darrell H. Voorhies, *The Co-ordination of Motive, Men and Money in Industrial Research, A Survey of Organization and Business Practice Conducted by the Department on Organization of the Standard Oil Company of California* (San Francisco: Standard Oil Company of California, 1946), pp. 53-54; Arthur Gerstenfeld, *Effective Management of Research and Development* (Reading, MA: Addison-Wesley, 1970), pp. 18-23 (for 4-year payback and large- versus small-company figures); C. Wilson Randle, "Problems of Research and Development Management," *Harvard Business Review* 37, no. 1 (January-February 1959): 128-136 (p. 131) (for lack of formal evaluating criteria).
 36. C. E. Kenneth Mees, "The Organization of Industrial Scientific Research," *Science* 43, no. 1118 (2 June 1916): 763-773 (p. 768).
 37. Quoted in T. A. Boyd, *Professional Amateur: The Biography of Charles Franklin Kettering* (New York: E. P. Dutton, 1957), p. 118.
 38. See, notably, Richard R. Nelson, "The Economics of Invention: A Survey of the Literature," *The Journal of Business* 32 (1959): 101-127; cf. David A. Hounshell, "The Medium is the Message, or How Context Matters: The RAND Corporation Builds an Economics of Innovation, 1946-1962," in *Systems, Experts, and Computers: The Systems Approach in Management and Engineering, World War II and After*, eds. Thomas P. Hughes and Agatha C. Hughes (Cambridge, MA: The MIT Press, 2000), pp. 255-310.
 39. Mees and Leermakers, *The Organization of Industrial Scientific Research*, pp. 235-237; see also F. Russell Bichowsky, *Industrial Research* (Brooklyn, NY: Chemical Publishing Co., 1942), p. 106 (for the advisability of keeping a "slush fund"—say, 5% of the research budget—for individual researchers, with the assent of group leaders, to do with pretty much as they pleased).
 40. Norman A. Shepard (American Cyanamid), "How Can We Build Better Teamwork within Our Research Organizations?" *Chemical and Engineering News* 23, no. 9 (10 May 1945): 804-807 (p. 805, quoting Willard Dow).
 41. Reich, *The Making of American Industrial Research*, p. 75 (for one-third); Kendall Birr, *Pioneering in Industrial Research: The Story of the General Electric Research Laboratory* (Washington, DC: Public Affairs Press, 1957), p. 37 (for one-half).
 42. Letter from Fieser to James B. Conant, 26 November 1927, quoted in David A. Hounshell, "The Evolution of Industrial Research in the United States," in *Engines of Innovation: U.S. Industrial Research at the End of an Era*, eds. Richard S. Rosenbloom and William J. Spencer (Cambridge, MA: Harvard University Press, 1996), pp. 13-85 (p. 27); see also Hounshell and Smith, *Science and Corporate Strategy*, pp. 228, 299. (Fieser ultimately turned down the Du Pont position.) But see McGrath, *Science, Business, and the State*, pp. 35 and 208 n. 6, for vigorous dissent from Hounshell's sketch of Conant as a typical academic "snob" about industrial research. Conant did indeed maintain that "first-rate" students should wind up in universities rather than industrial laboratories, but he was himself heavily involved in industrial research and saw nothing awkward in these relationships.

43. Robert N. Anthony, *Management Controls in Industrial Research Organizations* (Boston: Graduate School of Business Administration, Harvard University, 1952), pp. 134-136.
44. Hounshell, "Evolution of Industrial Research," pp. 26-27. Evidently, the then-common practice of allowing industrial research workers free time could be unknown to social scientists commenting on the condition of scientists in industry during the 1950s. A Columbia professor of education, for example, wrote of his impression that such an idea was regarded by industrial research administrators as "a fantastic" and even laughable notion: Lyman Bryson, "Researchers in Industry," *Human Relations in Industrial Research*, eds. Livingstone and Milberg, pp. 129-137 (p. 130).
45. Quoted again in Mees and Leermakers, *The Organization of Industrial Scientific Research*, p. 244. (This so-called "second edition" of 1950 was in fact an almost totally different book than the 1920 original.) I have not yet been able to locate the source of this quotation in Little's publications. Suspicion of the descriptive relevance of organization charts continued to be vigorously expressed by A. D. Little management consultants well after World War II: see, e.g., Sherman Kingsbury, in association with Lawrence W. Bass and Warren C. Lothrop, "Organizing for Research," in *Handbook of Industrial Research Management*, ed. Carl Heyel (New York: Reinhold Publishing Corporation, 1959), pp. 65-99 (pp. 65, 77-78).
46. Mees, *The Organization of Industrial Scientific Research*, pp. 67-68.
47. C. E. Kenneth Mees, "Discussion [of Michael Polanyi, 'The Foundations of Freedom in Science']," in *Physical Science and Human Values*, ed. E. P. Wigner (Princeton, NJ: Princeton University Press, 1947), pp. 140-141 (emphases in original). In these connections, it is interesting to note that, around this time, Mees was collaborating with the English zoologist John R. Baker on a semi-popular history of science: Baker had been a leading light, and associate of Polanyi, in the Society for Freedom of Science: see C. E. Kenneth Mees, with the co-operation of John R. Baker, *The Path of Science* (London: Chapman & Hall, 1946).
48. Mees' discussion of a piece by Thomas Midgley in Standard Oil Development Company, *The Future of Industrial Research: Papers and Discussion* (New York: Standard Oil Development Company, 1945), p. 48; cf. Mees and Leermakers, *The Organization of Industrial Scientific Research*, p. 233. Many research directors felt obliged to take a position—for, against, or qualifying—Mees' aphorisms: see, e.g., Norman Shepard, "How Can We Build Better Teamwork?," p. 804.
49. This has been quoted many times, including Kliever, "Design of Research Projects," p. 11; Nelson, "The Economics of Invention," p. 125; Rob Kaplan, *Science Says: A Collection of Quotations on the History, Meaning, and Practice of Science* (New York: Stonesong Press, 2001), pp. 105-106; John Jewkes, David Sawers, and Richard Stillerman, *The Sources of Invention* (London: Macmillan, 1958), p. 138.
50. O. E. Buckley, quoted in Jewkes, Sawers, and Stillerman, *The Sources of Innovation*, pp. 137-138.
51. Kliever, "Design of Research Projects and Programs," p. 13.
52. Mees, "Discussion of Midgley," p. 49; idem, *From Dry Plates to Ektachrome Film*, pp. 291-292; idem and Leermakers, *The Organization of Industrial Scientific Research*, p. 149; Baum, "Doctor of the Darkroom," p. 47. The scientist concerned was K. C. D. Hickman, and the new company founded to manufacture vitamins A and E using this technology was Distillation Products, Inc.

53. Francis Bello, "Industrial Research: Geniuses Now Welcome," *Fortune* (January 1956): 96-99, 142, 144, 149-150. Bello, like Whyte, exempted the industrial research "stars" (Bell, GE, Eastman Kodak) from his criticisms, and, while he reckoned that "There are still a good many laboratories where signs might still be posted: 'No geniuses wanted,'" he applauded big industry's willingness—perhaps, he thought, prompted by *Fortune's* arguments over the years—to develop "more flexibility in accommodating unusual personalities," *ibid.*, p. 96.
54. Noble, *America by Design*, ch. 6.
55. Hounshell and Smith, *Science and Corporate Strategy*, pp. 300-301.
56. Quoted in Ralph T. K. Cornwell, "Professional Growth of the Research Man," in *Research in Industry*, ed. Furnas, pp. 295-307 (p. 301).
57. See, for example, the vigorously pro-publication, and wholly pragmatic, views of a research manager at GE: R. W. Schmitt, "Why Publish Scientific Research from Industry?" *Research Management* 4 (1961): 31-41.
58. Cornwell (Sylvania), "Professional Growth of the Research Man," p. 301; Robert E. Wilson (Standard Oil, Indiana), "The Attitude of Management toward Research," *Chemical and Engineering News* 27, no. 5 (31 January 1949): 274-277 (p. 277); H. B. McClure (Union Carbide), "External Communication of Research Results," in *Selection, Training, and Use of Personnel in Industrial Research*, eds. Hertz and Rubenstein, pp. 161-176 (pp. 163, 166); Schmitt (General Electric), "Why Publish Scientific Research from Industry?," p. 32. (Uniquely, in my experience, this research manager offered evidence that he had read some works of Robert Merton, citing Merton's claims about the essential openness of genuine science while completely setting aside Merton's characterization of what industrial research was like: *ibid.*, pp. 36, 39-40.) See also Mees and Leermakers, *The Organization of Industrial Scientific Research*, pp. 277-280, 313; Hertz, *The Theory and Practice of Industrial Research*, pp. 347-350; Fisk, "Basic Research in Industrial Laboratories," p. 164. For a "must" policy on open publication, see Norman Shepard, "How Can We Build Better Teamwork?" p. 805.
59. Hounshell and Smith, *Science and Corporate Strategy*, pp. 223, 301.
60. John A. Van Raalte, "Reflections on the Ph.D. Interview," *Research Management* 9 (1966): 307-317 (p. 310).
61. Cornwell, "Professional Growth of the Research Man," pp. 301-302.
62. Reich, *The Making of American Industrial Research*, p. 110. A 1961 survey of publication practices for basic research findings among 174 U.S. companies showed that 14% of them published "substantially all" basic research findings; 26% published "most," 45% published "some," and 16% published "none." *Publication of Basic Research Findings in Industry* (Washington, DC: National Science Foundation, 1961), pp. 11 ff., quoted in Hirsch, *Scientists in American Society*, p. 64.
63. Mees, "The Organization of Industrial Scientific Research," p. 771; idem, *The Organization of Industrial Scientific Research*, p. 100 (my emphases); see also D. E. H. Edgerton, "Industrial Research in the British Photographic Industry, 1879-1939," in *The Challenge of New Technology: Innovation in British Business Since 1850*, ed. Jonathan Liebenau (Aldershot: Gower, 1988), pp. 106-134 (p. 122).
64. An industrial research director interviewed by Lowell Steele in the early 1950s remarked that "he was constantly amazed at the speed at which his scientists became interested in the welfare of the company." Steele was then aware—though his later

- change of mind has been noted—that many commentators thought it impossible to get scientists to become “company conscious,” but cited such evidence to argue that “this criticism is unsound.” Steele, “Personnel Practices in Industrial Laboratories,” p. 471.
65. Alfred H. Nissan, “Similarities and Differences between Industrial and Academic Research,” *Research Management* 9 (1966): 211–219 (pp. 211–212).
 66. As early as the late 1920s, James Bryant Conant was noting the change: among chemists awarded the Ph.D. at Harvard between 1907 and 1917, practically all intended an academic career; a decade later, “the majority of those who received the advanced degree would enter industrial employment either at once or after a few years.” James B. Conant, *My Several Lives: Memoirs of a Social Inventor* (New York: Harper & Row, 1970), p. 25. Government statistics showed that in the late 1940s 54% of all U.S. Ph.D. chemists worked in industrial laboratories, 10% in government laboratories, and only 33% in academia, without specifying the percentage of the latter active in research: cited in Hagstrom, *The Scientific Community*, p. 38. The 1950 Census was the first to specify personnel in scientific fields other than chemistry, and it showed that of about 150,000 total “natural and physical scientists”—including those without higher degrees—only 33,000 (or a little over a fifth) were working as teachers in institutions of higher education, the only notable exception being mathematicians, 80% of whom were in higher education: figures reproduced from National Manpower Council, *A Policy for Scientific and Professional Manpower: A Statement by the Council with Facts and Issues Prepared by the Research Staff* (New York: Columbia University Press, 1953), p. 47. See also, for example, Hirsch, *Scientists in American Society*, pp. 4–5, 60, and Albert E. Hickey, Jr., “Basic Research: Should Industry Do More of It?” *Harvard Business Review* 36, no. 4 (July–August 1958): 115–122 (figures on pp. 116–117).
 67. Writing in 1914, Kenneth Mees gave reasons why the universities were unlikely ever to become natural homes for scientific research: “For the last fifty years it has been assumed that the proper home for scientific research is the university, and that scientific discovery is one of the most important—if not the most important—function which a university can fulfill. In spite of this only a few of the American universities, which are admittedly among the best equipped and most energetic of the world, devote a very large portion of their energies to research work, while quite a number prefer to divert as little energy as possible from the business of teaching, which they regard as the primary function of the university.” C. E. Kenneth Mees, “The Future of Scientific Research [Editorial],” *Journal of Industrial and Engineering Chemistry* 6 (1914): 618–619 (p. 618).
 68. E. g., Veblen, *The Higher Learning in America*; Robert E. Kohler, “The Ph.D. Machine: Building on the Collegiate Basis,” *Isis* 81 (1990): 638–662.
 69. John W. Servos, “The Industrial Relations of Science: Chemical Engineering at MIT, 1900–1939,” *Isis* 71 (1980): 531–549, esp. p. 532, for poor research resources at MIT in the early twentieth century.
 70. J. F. Downie Smith, “Academic and Industrial Research,” *Research Management* 5 (1962): 257–275 (p. 261).
 71. By the late 1960s, troubled by the changes accompanying Big Science, some academic sociologists nervously broached the idea that, even for the academic scientist, “autonomy is a relative matter: his ‘role set’ involves at the very least the judgment of his peers, not to speak of the constraints imposed on him by his need to have a continuous flow of funds for equipment and research assistance—or just plain time to

- think.” Hirsch, *Scientists in American Society*, p. 67. And, for more full-blooded skepticism, see Norman Kaplan, “Organization: Will It Choke or Promote the Growth of Science?” in *The Management of Scientists*, ed. Karl Hill (Boston: Beacon Press, 1964), pp. 103–127, esp. p. 114.
72. See, notably, Kaplan, “Organization: Will It Choke or Promote the Growth of Science?”; also Stephen Cotgrove and Steven Box, *Science, Industry and Society: Studies in the Sociology of Science* (London: George Allen and Unwin/New York: Barnes & Noble, 1970), and Barnes, “Making Out in Industrial Research.”
 73. Steelman, *Science and Public Policy*, Vol. 3, pp. 142, 208 (my emphases). Of all groups responding, 11% preferred government, 31% industry, and 48% academia. Note that when industrial scientists alone were asked to put money considerations aside, 58% thought industry gave most satisfaction.
 74. For such perceptions, see, for example, Hirsch, *Scientists in American Society*, p. 53.
 75. Paul F. Lazarsfeld and Wagner Thielens, Jr. (with a field report by David Riesman), *The Academic Mind: Social Scientists in a Time of Crisis* (Glencoe, IL: The Free Press, 1958), pp. 148–149.
 76. See, e.g., David A. Hollinger, “Money and Academic Freedom a Half-Century after McCarthyism: Universities and the Force Fields of Capital,” in *Unfettered Expression: Freedom in American Intellectual Life*, ed. Pegg J. Hollingsworth (Ann Arbor: University of Michigan Press, 2000), pp. 161–184.