

The Matthew Effect in Science, II

Cumulative Advantage and the Symbolism of Intellectual Property

By Robert K. Merton*

THE SUBJECT OF THIS ESSAY is a problem in the sociology of science that has long been of interest to me. That problem, a candid friend tells me, is somewhat obscured by the formidable title assigned to it. Yet, properly deciphered, the title is not nearly as opaque as it might at first seem.

Consider first the signal emitted by the Roman numeral II in the main title. It informs us that the paper follows on an earlier one, "The Matthew Effect in Science," which I finally put into print a good many years ago.¹ The ponderous, not to say lumpy, subtitle goes on to signal the direction of this follow-on. The first concept, cumulative advantage, applied to the domain of science, refers to the social processes through which various kinds of opportunities for scientific inquiry as well as the subsequent symbolic and material rewards for the results of that inquiry tend to accumulate for individual practitioners of science, as they do also for organizations engaged in scientific work. The concept of cumulative advantage directs our attention to the ways in which initial comparative advantages of trained capacity, structural location, and available resources make for successive increments of advantage such that the gaps between the haves and the have-nots in science (as in other domains of social life) widen until dampened by countervailing processes.

The second phrase in the subtitle directs us to the distinctive character of intellectual property in science. I propose the seeming paradox that in science, private property is established by having its substance freely given to others who might want to make use of it. I shall argue further that certain institutionalized aspects of this property system, chiefly in the form of public acknowledgment of the source of knowledge and information thus freely bestowed on fellow scientists, relate to the social and cognitive structures of science in interesting ways that affect the collective advancement of scientific knowledge.

That is a long agenda for a short disquisition. Since that agenda can only be

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This article contains the main part of the inaugural lecture of the George Sarton Leerstoel, 28 November 1986, University of Ghent. [See p. 669—Eds.] By request it includes detailed bibliographical references to this strand of my work since I served as an apprentice to George Sarton. The full lecture, with its prefatory pages devoted to Sarton, is to appear in translation in the Ghent review *Tijdschrift voor Sociale Wetenschappen*. Earlier versions were presented at the New York Hospital-Cornell Medical Center, Yale University, Bell Laboratories, the College of Physicians and Surgeons of Columbia University, Smith College, Washington University, and the Fox Chase Cancer Center.

¹ Robert K. Merton, "The Matthew Effect in Science," *Science*, 5 January 1968, 159(3810):56-63; rpt. in Merton, *The Sociology of Science*, ed. Norman W. Storer (Chicago: Univ. Chicago Press, 1973), Ch. 20.

discharged by dealing with these matters in the large, I shall not attempt to summarize the detailed findings that derive from a now widely dispersed program of research on cumulative advantage and disadvantage in the social stratification of science.

An obscure title can also have a latent function: to keep one from assuming that the title truly speaks for itself, and thus to make it necessary to elucidate one's intent. As for the main title: what, you may well ask, does "The Matthew Effect in Science" refer to? A mercifully short reprise of the work introducing this notion will get us into its further elucidation.

THE MATTHEW EFFECT

We begin by noting a theme that runs through Harriet Zuckerman's hours-long interviews with Nobel laureates in the early 1960s. It is repeatedly suggested in these interviews that eminent scientists get disproportionately great credit for their contributions to science while relatively unknown ones tend to get disproportionately little for their occasionally comparable contributions. As a laureate in physics put it: "The world is peculiar in this matter of how it gives credit. It tends to give the credit to already famous people."² Nor are the laureates alone in stating that the more prominent scientists tend to get the lion's share of recognition; less notable scientists in a cross-sectional sample studied by Warren O. Hagstrom have reported similar experiences.³ But it is the eminent scientists, not least those who have received the ultimate contemporary accolade, the Nobel Prize, who provide presumptive evidence of this pattern. For they testify to its occurrence, not as aggrieved victims, which might make their testimony suspect, but as "beneficiaries," albeit sometimes embarrassed and unintentional ones.

The claim that prime recognition for scientific work, by informed peers and not merely by the inevitably uninformed lay public, is skewed in favor of established scientists requires, of course, that the nature and quality of these diversely appraised contributions be identical or at least much the same. That condition is approximated in cases of full collaboration and in cases of independent multiple discoveries. The distinctive contributions of collaborators are often difficult to disentangle; independent multiple discoveries, if not identical, are at least enough alike to be defined as functional equivalents by the principals involved or by their informed peers.

In papers jointly published by scientists of markedly unequal rank and reputation, another laureate in physics reports, "the man who's best known gets more credit, an inordinate amount of credit." Or as a laureate in chemistry put it, "If my name was on a paper, people would remember *it* and not remember who else

² Harriet Zuckerman, "Nobel Laureates: Sociological Studies of Scientific Collaboration" (Ph.D. diss., Columbia Univ., 1965). The later fruits of Zuckerman's research appear in Zuckerman, *Scientific Elite: Nobel Laureates in the United States* (New York: Free Press, 1977); an account of the procedures adopted in these tape-recorded interviews appears in Zuckerman, "Interviewing an Ultra-Elite," *Public Opinion Quarterly*, 1972, 36:159-175. This is occasion for repeating what I have noted in reprinting the original "Matthew Effect in Science": "It is now [1973] belatedly evident to me that I drew upon the interview and other materials of the Zuckerman study to such an extent that, clearly, the paper should have appeared under joint authorship." A sufficient sense of distributive and commutative justice requires one to recognize, however belatedly, that to write a scientific or scholarly paper is not necessarily sufficient grounds for designating oneself as its sole author.

³ Warren O. Hagstrom, *The Scientific Community* (New York: Basic Books, 1965), pp. 24-25.

was involved.”⁴ The biological scientists R. C. Lewontin and J. L. Hubby have lately reported a similar pattern of experience with a pair of their collaborative papers, which have been cited often enough to qualify as “citation classics” (as designated by the Institute for Scientific Information). One paper was cited some 310 times; the other, some 525 times. The first paper described a method; the second

gave the detailed result of the application of the method to natural populations. The two papers were a genuinely collaborative effort in conception, execution, and writing and clearly form an indivisible pair, . . . published back-to-back in the same issue of the journal. The order of authors was alternated, with the biochemist, Hubby, being the senior author in the method paper and the population geneticist, Lewontin, as senior author in the application paper. Yet paper II has been cited over 50 percent more frequently than paper I. Citations to paper I virtually never stand alone but are nearly always paired with a citation to II, but the reverse is not true. Why? We seem to have a clearcut case of Merton’s “Matthew Effect”—that the already better known investigator in a field gets the credit for joint work, irrespective of the order of authors on the paper, and so gets even better known by an autocatalytic process. In 1966 Lewontin had been a professional for a dozen years and was well known among population geneticists, to whom the paper was addressed, while Hubby’s career had been much shorter and was known chiefly to biochemical geneticists. As a result, population geneticists have consistently regarded Lewontin as the senior member of the team and given him undue credit for what was a completely collaborative work that would have been impossible for either one of us alone.⁵

At the extreme, such misallocation of credit can occur even when a published paper bears only the name of a hitherto unknown and uncredentialed scientist. Consider this observation by the invincible geneticist and biochemist, J. B. S. Haldane (whose *not* having received a Nobel Prize can be cited as prime evidence of the fallibility of the judges sitting in Stockholm). Speaking with Ronald Clark of S. K. Roy, his talented Indian student who had conducted important experiments designed to improve strains of rice, Haldane observed that

Roy himself deserved about 95 per cent of the credit. . . . “The other 5 per cent may be divided between the Indian Statistical Institute and myself,” he added. “I deserve credit for letting him try what I thought was a rather ill-planned experiment, on the general principle that I am not omniscient.” But [Haldane] had little hope that credit would be given that way. “Every effort will be made here to crab his work,” he wrote. “He has not got a Ph.D. or even a first-class M.Sc. So either the research is no good, or I did it.”⁶

It is such patterns of the misallocation of recognition for scientific work that I described as “the Matthew effect.” The not quite foreordained term derives, of course, from the first book of the New Testament, the Gospel according to Matthew (13:12 and 25:29). In the stately prose of the King James Version, created by what must be one of the most scrupulous and consequential teams of scholars in Western history, the well-remembered passage reads: “For unto ev-

⁴ Zuckerman, *Scientific Elite* (cit. n. 2), pp. 140, 228.

⁵ R. C. Lewontin and J. L. Hubby, “Citation Classic,” *Current Contents/Life Sciences*, 28 Oct. 1985, No. 43, p. 16.

⁶ Ronald W. Clark, *J.B.S.: The Life and Work of J. B. S. Haldane* (New York: Coward-McCann, 1969), p. 247.

eryone that hath shall be given, and he shall have abundance; but from him that hath not shall be taken away even that which he hath."⁷

Put in less stately language, the Matthew effect is the accruing of large increments of peer recognition to scientists of great repute for particular contributions in contrast to the minimizing or withholding of such recognition for scientists who have not yet made their mark. The biblical parable generates a corresponding sociological parable. For this is the form, it seems, that the distribution of psychic income and cognitive wealth in science also takes. How this comes to be and with what consequences for the fate of individual scientists and the advancement of scientific knowledge are the questions in hand.

THE ACCUMULATION OF ADVANTAGE AND DISADVANTAGE FOR SCIENTISTS

Taken out of its spiritual context and placed in a wholly secular context, the Matthew doctrine would seem to hold that the posited process must result in a

⁷ The term and concept "Matthew effect" has diffused widely since its coinage a quarter century ago. Geographically, it has become common usage in the West and, my colleague Andrew Walder informs me, has traveled to mainland China where it is known as "mati xiaoying." Substantively, it has diffused into diverse domains other than the sociology and history of science. As examples, it has been adopted in welfare economics and social policy (e.g., by Herman Deleeck, *Het Matteüeffect: De ongelijke verdeling van de sociale overheidsuitgaven in België* [Antwerp: Kluwer, 1983]); in education (Herbert J. Walberg and Shioh-Ling Tsai, "Matthew Effects in Education," *American Educational Research Journal*, 1983, 20:359-373); in administrative studies (James G. Hunt and John D. Blair, "Content, Process, and the Matthew Effect among Management Academics," *Journal of Management*, 1987, 13:191-210); and, to go no further, in social gerontology (Dale Dannefer, "Aging as Intracohort Differentiation: Accentuation, the Matthew Effect and the Life Course," *Sociological Forum*, 1987, 2:211-236.)

Despite that wide diffusion, it is also the case that the term "Matthew effect," though not the concept, has been questioned from the start on several grounds. In 1968, soon after its first appearance in print, my colleague and later collaborator, David L. Sills, based his reservations about the term upon "(1) the issue of the priority of the words in Matthew 25:29 (Mark 4:25 said them first [to say nothing of Luke 8:18 and 19:26 probably being indebted to them both]); (2) the authorship issue (it is almost certain that Matthew did not write the Gospel According to Matthew); (3) the attribution issue (the words are Christ's, not those of the author-compiler of the gospel); and (4) the interpretation issue (it is quite unlikely that the point of the parable is 'the more, the more')": Sills to Merton, 29 Mar. 1968.

These objections have been variously reiterated over the years. Thus the astronomer Charles D. Geilker (*Science*, 1968, 159:1185) maintains that since the three evangelists were all quoting Jesus, I might just as well have called it "the Jesus effect." But then, this would have precluded my having neutralized or reduced the Matthew effect of the term by the very act of calling it "the Matthew effect." Most recently, I am indebted to M. de Jonge, professor of theology at the University of Leyden, who has made some of the same observations as Sills. He notes further that "it is highly likely that [Jesus] took over a general saying, current in Jewish (and/or Hellenistic) Wisdom circles—see, e.g., Proverbs 9:9, Daniel 2:21, or Martialis, Epigr. V 81: 'Semper pauper eris, si pauper es, Aemiliane. Dantur opes nullis [nunc] nisi divitibus.'" And de Jonge concludes: "The use made of this sentence [in Matthew] by modern authors neglects the eschatological thrust inherent in the saying in all versions, and (in all probability) in Jesus's own version of it. It links up, however, with the Wisdom-saying taken over by Jesus: 'Look around you and see what happens: If you have something, you get more; if you have not a penny, they will take from you the little you have.'" M. de Jonge, summary of lecture, "The Matthew Effect," 24 July 1987.

It is not for me to adjudicate these matters. The priority question of Matthew, Mark, Luke, Q, or still earlier proverbial wisdom had best be left to historians specialized in the matter. In coining the term, I was plainly transferring the pertinent sentence from its theological context into a secular one. Having studied the various interpretations of the five similar passages in the synoptic gospels—principally as summarized and advanced by Ronald Knox, *A Commentary on the Gospels* (New York: Sheed & Ward, 1952)—I decided to give public expression to my preference for Matthew. It was a comfort to learn recently that Wittgenstein had chosen Matthew as his favorite gospel: M. O'C. Drury, "Conversations with W.," in *Ludwig Wittgenstein: Personal Recollection*, ed. Rush Rhees (Oxford: Blackwell, 1981), p. 177.

boundlessly growing inequality of wealth, however wealth is construed in any sphere of human activity. Conceived of as a locally ongoing process and not as a single event, the practice of giving unto everyone that hath much while taking from everyone that hath little will lead to the rich getting forever richer while the poor become poorer. Increasingly absolute and not only relative deprivation would be the continuing order of the day. But as we know, things are not as simple as all that. After all, the extrapolation of local exponentials is notoriously misleading. In noting this, I do not intend nor am I competent to assess the current economic theory of the distribution of wealth and income. Instead, I shall report what a focus upon the skewed distribution of peer recognition and research productivity in science has led some of us to identify as the processes and consequences of the accumulation of advantage and disadvantage in science.

(Unkind readers will no doubt describe this part of my report as rambling; critical ones, as convoluted; and kindly, understanding ones, as complex. Myself, I should describe it as the slow, laborious emergence of an intellectual tradition of work in the evolving sociology of science.)

I first stumbled upon the general question of social stratification in science in the early 1940s. One paper of that period alludes to “the accumulation of differential advantages for certain segments of the population, differentials that are not [necessarily] bound up with demonstrated differences in capacity.”⁸ It would not be correct or, indeed, just to say that that text is no clearer to me now than the notoriously obscure *Sordello* was clear to Robert Browning, when he confessed: “When I wrote that, God and I knew what it meant, but now God alone knows.”⁹ However, I can report that the notion of the accumulation of advantage just rested there as only a proto-concept—inert, unnoticed, and unexplicated—until it was taken up, almost a quarter century later, in my first paper on the Matthew effect. Until then, the notion of cumulative advantage in science had led only a ghostly existence in private musings, sporadically conjured up for oral publication rather than put in print.¹⁰

Further investigation of the process of cumulative advantage took hold in the latter 1960s with the formation of a research quartet at Columbia consisting of Harriet Zuckerman, Stephen Cole, Jonathan R. Cole, and myself. There has since emerged an “invisible college”—to adopt the brilliant terminological recoinage by Derek Price—which has grown apace in contributing to a program of research on cumulative advantage and disadvantage, in social stratification generally and in science specifically. Notably including Price himself until his recent lamented death, that college also numbers Paul D. Allison, Bernard Barber, Ste-

⁸ Robert K. Merton, “The Normative Structure of Science” (1942), rpt. in Merton, *Sociology of Science* (cit. n. 1), p. 273.

⁹ There are other versions of that confession. Edmund Gosse reports that he “saw him [Browning] take up a copy of the first edition, and say, with a grimace, ‘Ah! the entirely unintelligible “Sordello”’: Gosse, “Robert Browning,” *Dictionary of National Biography*, First Supplement (London: Smith, Elder, 1901), Vol. 1, pp. 306–319, on p. 308. The remark has also been attributed to the eighteenth-century poet Friedrich Klopstock and to Hegel. Once again, it is not for me to adjudicate priority claims.

¹⁰ The central idea was presented briefly in the National Institutes of Health Lecture in February 1964 and later that year in expanded form at the annual meeting of the American Association for the Advancement of Science. It then underwent several more editions in a succession of public lectures, notably one at the University of Leyden in 1965, before it found its way into print in *Science* (see n. 1).

phen J. Bensman, Judith Blau, Walter Broughton, Daryl E. Chubin, Dale Dannefer, Simon Duncan, Mary Frank Fox, Eugene Garfield, Jerry Gaston, Jack A. Goldstone, Warren O. Hagstrom, Lowell L. Hargens, Karin D. Knorr, Tad Krauze, J. Scott Long, Robert McGinnis, Volker Meja, Roland Mettermeyer, Edgar W. Mills, Jr., Nicholas C. Mullins, Barbara Reskin, Leonard Rubin, Dean K. Simonton, Nico Stehr, John A. Stewart, Norman W. Storer, Stephen P. Turner, and Herbert J. Walberg, among others.¹¹

This, surely, is not the occasion for providing a synopsis of that now considerable body of research materials. Rather, I shall only remind you of a few of the marked inequalities and strongly skewed distributions of productivity and resources in science, and then focus on the consequences of the bias in favor of precocity that is built into our institutions for detecting and rewarding talent, an institutionalized bias that may help bring about severe inequalities during the life course of scholars and scientists.

First, then, a quick sampling of the abundance of conspicuously skewed distributions and inequalities identifiable at a given time.

- The total number of scientific papers published by scientists differs enormously, ranging from the large proportion of Ph.D.s who publish one paper or none at all to the rare likes of William Thomson, Lord Kelvin, with his six hundred plus papers, or the mathematician Arthur Cayley, publishing a paper every few weeks throughout his work life for a total of almost a thousand.¹²
- The skewed distribution in the sheer number of published papers is best approximated by variants of Alfred J. Lotka's "inverse square law" of scientific productivity, which states that the number of scientists with n publications is proportional to n^2 . In a variety of disciplines, this works out to some 5 or 6 percent of the scientists who *publish at all* producing about half of all papers in their discipline.¹³
- The distributions are even more skewed in the use of scientists' work by their peers, as that use is crudely indexed by the number of citations to it. Much the same distribution has been found in various data sets: typical is Garfield's finding that, for an aggregate of some nineteen million articles published in the physical and biological sciences between 1961 and 1980,

¹¹ Price had extended Robert Boyle's seventeenth-century term "invisible college" to designate the informal collectives of scientists interacting in their research on similar problems, these groups being generally limited to a size "that can be handled by interpersonal relationships": Derek J. de Solla Price, *Little Science, Big Science . . . and Beyond* (New York: Columbia Univ. Press, 1986; 1st ed., 1963), pp. 76–81, *passim*. For a key paper on cumulative advantage see Price, "A General Theory of Bibliometric and Other Cumulative Advantage Processes," *Journal of the American Society for Information Science*, 1976, 27:292–306. For detailed analysis and history of the idea and a substantial bibliography see Harriet Zuckerman, "Accumulation of Advantage and Disadvantage: The Theory and Its Intellectual Biography," paper presented to the Amalfi Conference of the Associazione Italiana di Sociologia, 1987; forthcoming in *L'opera di Robert K. Merton e la sociologia contemporanea*, ed. Carlo Mongardini (Rome).

¹² Silvanus P. Thompson, *The Life of William Thomson, Baron Kelvin of Largs*, 2 vols. (London: Macmillan, 1910), Vol. II, pp. 1225–1274; J. D. North, "Arthur Cayley," *Dictionary of Scientific Biography*, ed. Charles C. Gillispie (New York: Scribners, 1970–1980), Vol. III, p. 163.

¹³ Alfred J. Lotka, "The Frequency Distribution of Scientific Productivity," *Journal of the Washington Academy of Sciences*, 1926, 16:317–323; and Price, *Little Science, Big Science . . . and Beyond* (cit. n. 11), pp. 38–42.

0.3 percent were cited more than one hundred times; another 2.7 percent between twenty-five and one hundred times; and, at the other extreme, some 58 percent of those that were cited at all were cited only once in that twenty-year period.¹⁴ This inequality, you will recognize, is steeper than Pareto-like distributions of income.

When it comes to *changes* in the extent of inequalities of research productivity and recognition during the course of an individual's work life as a scientist, the needed longitudinal data are much more scarce. Again, a few suggestive findings must serve.

- In their simulation of longitudinal data (through disaggregation of a cross section of some two thousand American biologists, mathematicians, chemists, and physicists into several strata by career age), Paul D. Allison and John A. Stewart found "a clear and substantial rise in inequality for both [the number of research publications in the preceding five years and the number of citations to previously published work] from the younger to the older strata, strongly supporting the accumulative advantage hypothesis."¹⁵
- Allison and Stewart also confirmed the Zuckerman-Merton hypothesis that decreasing research productivity with increasing age results largely from differing rates of attrition in research roles and that this approximates an all-or-none phenomenon. The hypothesis held that "the more productive scientists, recognized as such by the reward-system of science, tend to persist in their research roles," while those with declining research productivity tend to shift to other indispensable roles in science, not excluding the conventionally maligned role of research administrator.¹⁶
- Derek Price pointedly reformulated and developed that hypothesis, "because there is a very large but decreasing chance that any given researcher will discontinue publication, the group of workers that reaches the [research] front during a particular year will decline steadily in total output as time goes on. Gradually, one after another, they will drop away from the research front. Thus the yearly output of the group as a whole will decline, [and now comes the essential point Zuckerman and I tried to emphasize] even though any given individual within it may produce at a steady rate throughout his [or her] entire professional lifetime. We need, therefore, to

¹⁴ Eugene Garfield, *The Awards of Science and Other Essays* (Philadelphia: ISI Press, 1985), p. 176.

¹⁵ Paul D. Allison and John A. Stewart, "Productivity Differences among Scientists: Evidence for Accumulative Advantage," *American Sociological Review*, 1974, 39:596-606. But see Michael A. Faia, "Productivity among Scientists: A Replication and Elaboration," *Amer. Sociol. Rev.*, 1975, 40:825-829, and the following Allison-Stewart "Reply," pp. 829-831; also Roland Mettermeir and Karin D. Knorr, "Scientific Productivity and Accumulative Advantage: A Thesis Reassessed in the Light of International Data," *R & D Management*, 1979, 9:235-239. A later study by Paul D. Allison, J. Scott Long, and Tad Krauze, based on actual rather than simulated age-cohort data for chemists and biochemists, finds increasing inequalities in research publication as a cohort ages but, as yet inexplicably, finds no such increases in rates of citation: "Cumulative Advantage and Inequality in Science," *Amer. Sociol. Rev.*, 1982, 47:615-625.

¹⁶ Allison and Stewart, "Productivity Differences" (cit. n. 15); Harriet Zuckerman and Robert K. Merton, "Age, Aging and Age Structure in Science" (1972), rpt. in Merton, *Sociology of Science* (cit. n. 1), pp. 497-559, on pp. 519-537.

distinguish this effect [of mortality at the research front] from any differences in the actual rates of productivity at different ages among those that remain at the front."¹⁷

With regard to the Matthew effect and associated cumulation of advantage,

- Stephen Cole found, in an ingeniously designed study of a sample of American physicists, that the greater their authors' scientific reputation, the more likely that papers of roughly equal quality (as assessed by the later number of citations to them) will receive rapid peer recognition (by citation within a year after publication). Prior repute of authors somewhat advances the speed of diffusion of their contributions.¹⁸
- Cole also found that it is a distinct advantage for physicists of still small reputation to be located in the departments most highly rated by peers: their new work diffuses more rapidly through the science networks than comparable work by their counterparts in peripheral university departments.¹⁹

ACCUMULATION OF ADVANTAGE AND DISADVANTAGE AMONG THE YOUNG

I now focus on the special problems in the accumulation of advantage and disadvantage that derive from an institutionalized bias in favor of precocity. The advantages that come with early accomplishment taken as a sign of things to come stand in Matthew-like contrast to the situation confronted by young scientists whose work is judged as ordinary.²⁰ Such early prognostic judgments, I suggest, lead in some unknown fraction of cases to inadvertent suppression of talent through the process of the self-fulfilling prophecy. Moreover, this is more likely to be the case in a society, such as American society, where educational institutions are so organized as to put a premium on relatively *early* manifestations of ability—in a word, on precocity. Since it was that wise medical scientist Alan Gregg who led me to become aware of this bias institutionalized in our educational system, and since I cannot improve on his formulation, I transmit it here in the thought that you too may find it revealing.

By being generous with time, yes, lavish with it, Nature allows man an extraordinary chance to learn. What gain can there be, then, in throwing away this natural advantage by rewarding precocity, as we certainly do when we gear the grades in school to chronological age by starting the first grade at the age of six and college entrance for the vast majority at seventeen and a half to nineteen? *For, once you have most of your students the same age, the academic rewards—from scholarships to internships and residencies—go to those who are uncommonly bright for their age.* In other

¹⁷ Derek de Solla Price, "The Productivity of Research Scientists," *1975 Yearbook of Science and the Future* (Chicago: Encyclopedia Britannica, 1975), pp. 409–421, on p. 414. Stephen Cole's studies of age cohorts in various sciences confirm this pattern of a steady rate of publication by a significant fraction of scientists; see Cole, "Age and Scientific Performance," *Amer. J. Sociol.*, 1979, 84:958–977.

¹⁸ Stephen Cole, "Professional Standing and the Reception of Scientific Discoveries," *Amer. J. Sociol.*, 1970, 76:286–306, on pp. 291–292.

¹⁹ *Ibid.*, p. 292.

²⁰ Jonathan R. Cole and Stephen Cole, *Social Stratification in Science* (Chicago: Univ. Chicago Press, 1973), pp. 112–122, *passim*.

words, you have rewarded precocity, which may or may not be the precursor of later ability. So, in effect, you have unwittingly belittled man's cardinal educational capital—time to mature.²¹

The social fact noted by Gregg is of no small consequence for the collective advancement of knowledge as well as for distributive justice. As he goes on to argue, "precocity thus may succeed in the immediate competitive struggle, but, in the long run, at the expense of mutants having a slower rate of development but greater potentialities."²² By suggesting that there are such slow-starting mutants who have *greater* potentialities than some of the precocious, Gregg is plainly assuming part of what he then concludes. But, as I noted almost thirty years ago, Gregg's

argument cuts deeply, nevertheless. For we know only of the "late bloomers" who have eventually come to bloom at all; we don't know the potential late bloomers who, cut off from support and response in their youth, never manage to come into their own at all. Judged ordinary by comparison with their precocious "age-peers," they are treated as youth of small capacity. They slip through the net of our institutional sieves for the location of ability, since this is a net that makes chronological age the basis for assessing relative ability. Treated by the institutional system as mediocrities with little promise of improvement, many of these potential late bloomers presumably come to believe it of themselves and act accordingly. At least what little we know of the formation of self-images suggests that this is so. For most of us most of the time, and not only the so-called "other-directed men" among us, tend to form our self-image—our image of potentiality and of achievement—as a reflection of the images others make plain they have of us. *And it is the images that institutional authorities have of us that in particular tend to become self-fulfilling images: if the teachers, inspecting our Iowa scores and our aptitude-test figures and comparing our record with [those] of our "age-peers," conclude that we're run-of-the-mine and treat us accordingly, then they lead us to become what they think we are.*²³

Of even more direct import for our immediate subject is the further observation back then that the institutionalized bias toward precocity, noted by Gregg, may have notably different consequences for comparable youngsters in differing social classes and ethnic groups.

The potential late bloomers in the less privileged social strata are more likely to lose out altogether than their counterparts in the middle and upper strata. If poor [youths] are not precocious, if they don't exhibit great ability early in their lives and so are not

²¹ Alan Gregg, *For Future Doctors* (Chicago: Univ. Chicago Press, 1957), pp. 125–126 (emphasis added).

²² *Ibid.*, p. 125.

²³ This sociological extension of Gregg's biopsychosocial observation remains as formulated in 1960: R. K. Merton, "'Recognition' and 'Excellence': Instructive Ambiguities," in *Recognition of Excellence: Working Papers*, ed. Adam Yarmolinsky (New York: Free Press, 1962), rpt. in *Merton Sociology of Science* (cit. n. 1), pp. 419–438, on p. 428 (emphasis added). Much theoretical debate and hundreds of empirical studies of this kind of self-fulfilling prophecy in American schools have resulted from the pioneering work of Robert Rosenthal. See, to begin with, Robert Rosenthal and Lenore Jacobson, *Pygmalion in the Classroom: Teacher Expectation and Pupils' Intellectual Development* (New York: Holt, Rinehart & Winston, 1968); the critical monograph by Janet D. Elashoff and Richard E. Snow, *Pygmalion Reconsidered* (Worthington, Ohio: Jones Publishing, 1971); and a monograph on the "decade of research and debate" by Harris M. Cooper and Thomas L. Good: *Pygmalion Grows Up: Studies in the Expectation Communication Process* (New York/London: Longman, 1983).

rewarded by scholarships and other sustaining grants, they drop out of school and in many instances never get to realize their potentialities. The potential late bloomers among the well-to-do have a better prospect of belated recognition. Even if they do poorly in their school work at first, they are apt to go on to college in any case. The values of their social class dictate this as the thing to do, and their families can see them through. By remaining in the system, they can eventually come to view. But many of their [presumably] more numerous counterparts in the lower strata are probably lost for good. The bias toward precocity in our institutions thus works profound [and ordinarily hidden] damage on the [potential] late bloomers with few economic or social advantages.²⁴

Such differential outcomes need not be intended by the people engaged in running our educational institutions and thereby affecting patterns of social selection. And it is such unanticipated and unintended consequences of purposive social action—in this case, rewarding primarily early signs of ability—that tend to persist. For they are *latent*, not manifest, social problems, that is, social conditions and processes that are at odds with certain interests and values of the society but are not generally recognized as being so.²⁵ In identifying the wastage that results from marked inequalities in the training and exercise of socially prized talent, social scientists bring into focus what has been experienced by many as only a personal problem rather than a social problem requiring new institutional arrangements for its reduction or elimination.

Mutatis mutandis, what holds for the accumulation of advantage and of disadvantage in the earliest years of education would hold also at a later stage for those youngsters who have made their way into fields of science and scholarship but who, not having yet exhibited prime performance, are shunted off into the less stimulating milieus for scientific work, with their limited resources. Absent or in short supply are the resources of access to needed equipment, an abundance of able assistance, time institutionally set aside for research, and, above all else perhaps, a cognitive microenvironment composed of colleagues at the research front who are themselves evokers of excellence, bringing out the best in the people around them. Not least is the special resource of being located at strategic nodes in the networks of scientific communication that provide ready access to information at the frontiers of research. By hypothesis, some unknown fraction of the unprecocious workers in the vineyards of science are caught up in a process of cumulative disadvantage that removes them early on from the system of scientific work and scholarship.²⁶

²⁴ Merton, " 'Recognition' and 'Excellence,' " pp. 428–429.

²⁵ On the first concept see R. K. Merton, "The Unanticipated Consequences of Purposive Social Action," *Amer. Sociol. Rev.*, 1936, 1:894–904; on the concept of manifest and latent social problems see R. K. Merton, *Social Research and the Practicing Professions*, ed. Aaron Rosenblatt and Thomas F. Gieryn (Cambridge: Abt Books, 1982), pp. 43–99, esp. pp. 55ff.

²⁶ Late-bloomer patterns in science remain a largely unexplored area of research. Jonathan R. Cole and Stephen Cole found (in a sample of 120 university physicists that by design overrepresents productive and eminent physicists) that "three-quarters of these physicists began their professional careers by publishing at least three papers soon after their doctorates. There are few 'late bloomers'; only five of the thirty physicists who started off slowly ever became highly productive (averaging 1.5 or more papers a year)": Cole and Cole, *Social Stratification in Science* (cit. n. 20), p. 112. Whether one writes that "only" five of thirty (17 percent) or "as many as" 17 percent proved to be late bloomers is, of course, a matter of tacit judgment. See also Stephen Cole, "Age and Scientific Performance" (cit. n. 17); Nancy Stern, "Age and Achievement in Mathematics: A Case-Study in the Sociology of Science," *Social Studies of Science*, 1978, 8:127–140; and Barbara Reskin, "Age and

Other social and cognitive contexts may make for such patterned differentials of cumulative advantage and disadvantage. Harriet Zuckerman suggests, as an example, that just as class origins may differentially affect the rates at which potential late bloomers remain in the educational system long enough to bloom, so academic disciplines may differ in an unplanned tolerance for late blooming. Disciplines in which scholars often develop comparatively late—say, the humanities—presumably provide greater opportunities for late bloomers than those in which early maturation is more common—say, mathematics and the physical and biological sciences. Generalized, these conjectures hold that *contextual differences* such as social class or fields of intellectual activity as well as *individual differences* in the pattern of intellectual growth affect the likelihood of success and failure for potential late bloomers.²⁷

Differences in individual capabilities aside, then, processes of accumulative advantage and disadvantage accentuate inequalities in science and learning: inequalities of peer recognition, inequalities of access to resources, and inequalities of scientific productivity. Individual self-selection and institutional social selection interact to affect successive probabilities of being variously located in the opportunity structure of science. When the scientific role performance of individuals measures up to or conspicuously exceeds the standards of a particular institution or discipline—whether this be a matter of ability or of chance—there begins a process of cumulative advantage in which those individuals tend to acquire successively enlarged opportunities for advancing their work (and the rewards that go with it) even further.²⁸ Since elite institutions have comparatively large resources for advancing research in certain domains, talent that finds its way into these institutions early has the enlarged potential of acquiring differentially accumulating advantages. The systems of reward, allocation of resources, and other elements of social selection thus operate to create and to maintain a class structure in science by providing a stratified distribution of chances among scientists for significant scientific work.²⁹

Scientific Productivity: A Critical Review," in *The Demand for New Faculty in Science and Engineering*, ed. Michael S. McPherson (Washington, D.C.: National Academy of Sciences, 1979).

²⁷ Zuckerman, "Accumulation of Advantage and Disadvantage" (cit. n. 11).

²⁸ In terms of a clinical rather than statistical sociology, I have tried to trace the process of accumulation of advantage in the academic life course of the historian of science and my longtime friend Thomas S. Kuhn, as I have done more recently in tracking my own experience as apprentice to the then world dean of the history of science who has been honored by the establishment of the George Sarton Chair in the History of Science at the University of Ghent. For the case of Kuhn see R. K. Merton, *The Sociology of Science: An Episodic Memoir* (Carbondale: Southern Illinois Univ. Press, 1979), pp. 71–109; for my own case see Merton, "George Sarton: Episodic Recollections by an Unruly Apprentice," *Isis*, 1985, 76:470–486.

²⁹ On processes of stratification in science see Harriet Zuckerman, "Stratification in American Science," *Sociological Inquiry*, 1970, 40:235–257; Zuckerman, *Scientific Elite* (cit. n. 2); Cole and Cole, *Social Stratification in Science* (cit. n. 20); Jonathan R. Cole, *Fair Science: Women in the Scientific Community* (New York: Free Press, 1979); Jerry Gaston, *The Reward System in British and American Science* (New York: Wiley, 1978); G. Nigel Gilbert, "Competition, Differentiation and Careers in Science," *Social Science Information*, 1977, 16:103–123; Hagstrom, *Scientific Community* (cit. n. 3); Lowell Hargens, Nicholas C. Mullins, and Pamela K. Hecht, "Research Areas and Stratification Processes in Science," *Soc. Stud. Sci.*, 1980, 10:55–74; Hargens and Diane Felmlee, "Structural Determinants of Stratification in Science," *Amer. Sociol. Rev.*, 1984, 49:685–697; Norman W. Storer, *The Social System of Science* (New York: Holt, Rinehart & Winston, 1966); Jack A. Goldstone, "A Deductive Explanation of the Matthew Effect in Science," *Soc. Stud. Sci.*, 1979, 9:385–392; and Stephen P. Turner and Daryl E. Chubin, "Chance and Eminence in Science: Ecclesiastes II," *Soc. Sci. Info.*, 1979, 3:437–449.

ACCUMULATION OF ADVANTAGE AND DISADVANTAGE
AMONG SCIENTIFIC INSTITUTIONS

Skewed distributions of resources and productivity that resemble those we have noted among individual scientists are found among scientific institutions. These inequalities also appear to result from self-augmenting processes. Clearly, the centers of historically demonstrated accomplishments in science attract far larger resources of every kind, human and material, than research organizations that have not yet made their mark. These skewed distributions are well known and need only bare mention here.

- In 1981, some 28 percent of the \$4.4 billion of federal support for academic research and development went to just ten universities.³⁰
- Universities with great resources and prestige in turn attract disproportionate shares of the presumably most promising students (subject to the precocity restriction we have noted): in 1983, two thirds of the five hundred National Science Foundation graduate fellows elected to study at just fifteen universities.³¹
- Those concentrations have been even more conspicuous in the case of outstanding scientists. Zuckerman found, for example, that at the time they did the research that ultimately brought them the Nobel Prize, 49 percent of the future American laureates working in universities were in just five of them: Harvard, Columbia, Rockefeller, Berkeley, and Chicago. By way of comparison, these five universities comprised less than 3 percent of all faculty members in American universities.³²
- Zuckerman also found that these resource-full and prestige-full universities seem able to spot and to retain these prime movers in contemporary science. For example, they kept 70 percent of the future laureates they had trained, in comparison with 28 percent of the other Ph.D.s they had trained. Much the same pattern, though less markedly, held for a larger set of sixteen elite institutions.³³

But enough about these details of great organizational inequalities in science. This only raises the question anew: If the processes of accumulating advantage and disadvantage are truly at work, why are there not even greater inequalities than have been found to obtain?

COUNTERVAILING PROCESSES

Or to put the question more concretely and parochially, why have not Harvard, rich in years-350 of them-and in much else, and Columbia, with its 230 years, and, to remain parochial, the Rockefeller, with its 75 years of prime reputation both as research institute and graduate university, jointly garnered just about *all* the American Nobel laureates rather than a “mere” third of them within five

³⁰ National Science Foundation, *Federal Support to Universities, Colleges, and Selected Nonprofit Institutions, Fiscal Year 1981* (Washington, D.C.: U.S. Government Printing Office, 1983), pp. 79-80.

³¹ National Science Foundation, *Grants and Awards for Fiscal Year 1983* (Washington, D.C.: U.S. Government Printing Office, 1984), pp. 215-217.

³² Zuckerman, *Scientific Elite* (cit. n. 2), p. 171.

³³ *Ibid.*, Ch. 5.

years after the prize?³⁴ Put more generally, why do the posited processes of accumulating advantage and disadvantage not continue without assignable limit?

Even Thomas Macaulay's ubiquitous schoolboy would nowadays know that exponential processes do not continue endlessly. Yet some of us make sensible representations of growth processes within a local range and then mindlessly extrapolate them far outside that range. As Derek Price was fond of saying in this connection, if the exponential rate of growth in the number of scientists during the past half century were simply extrapolated, then every man, woman, and child-to say nothing of their cats and dogs-would have to end up as scientists. Yet we have an intuitive sense that somehow they will not.

In much the same way, every schoolgirl knows that when two systems grow at differing exponential rates, the gap between them swiftly and greatly widens. Yet we sometimes forget that as such a gap approaches a limit, other forces come into play to constrain still further concentrations and inequalities of whatever matters are in question. Such countervailing processes that close off the endless accumulation of advantage have not yet been systematically investigated for the case of science-more particularly, for the distribution of human and material resources in research universities and of scientific productivity within them. Still, I would like to speculate briefly about the forms countervailing processes might take.

Consider for example the notion of an excessive density of talent. It is not a frivolous question to ask: How much concentrated talent can a single academic department or research unit actually stand? How many prime movers in a particular research area can work effectively in a single place? Perhaps there really can be too much of an abstractly good thing.

Think a bit about the patterned motivations of oncoming talents as they confront a high density of talented masters in the same department or research unit. The more autonomous among them might not entirely enjoy the prospect of remaining in the vicinity and, with the Matthew effect at work, in the shadow of their masters, especially if they felt, as youth understandably often comes to feel-sometimes with ample grounds-that those masters have seen their best days. Correlatively, some of the firmly established masters, in a pattern of master-apprentice ambivalence, may not relish the thought of having exceedingly talented younger associates in their own or competing research terrains, who they perceive might subject them to premature replacement, at least in local peer esteem, when, as anyone can see, they, the masters, are still in their undoubted prime.³⁵ Not every one of us elders has the same powers of critical self-appraisal, and the same largeness of spirit, as Isaac Barrow, the first occupant of the Lucasian Chair of Mathematics at Cambridge, who stepped down from that special chair at the advanced age of thirty-nine in favor of his twenty-seven-year-old student-a chap named Isaac Newton. In our time, of course, at least during the years of seemingly limitless academic affluence and expansion, Barrow would have stayed on and Newton would have been given a new chair. But again, as we

³⁴ *Ibid.*, p. 241.

³⁵ R. K. Merton and Elinor Barber, "Sociological Ambivalence" (1963), rpt. in Merton, *Sociological Ambivalence* (New York: Free Press, 1976), pp. 3-31, esp. pp. 4-6; Vanessa Merton, R. K. Merton, and Elinor Barber, "Client Ambivalence in Professional Relationships," in *New Directions in Helping*, ed. B. M. DePaulo *et al.* (New York: Academic Press, 1983), Vol. II, pp. 13-44, on pp. 26-27.

have ample cause to know, continued expansion of that kind in any one institution also has its limits.

Apart from such forces generated *within* universities that make for dispersion of human capital in science and learning, there is also the system process of social and cognitive competition *among* universities. Again, a brief observation must stand for a detailed analysis. Entering into that external competition is the fact that the total resources available to a university or research institute must be allocated somehow amongst its constituent units. Some departments wax poor even in rich universities. This provides opportunities to institutions of considerably smaller resources and reputation. These may elect to concentrate their limited resources in particular fields and departments and so to provide competitively attractive microenvironments to talents of the first class in those fields.

As another countervailing process, populist and democratic values may be called into play in the wider society, external to academic institutions and to science, and lead governmental largesse to be more widely spread in a calculated effort to counteract cumulating advantage in the great centers of learning and research.

But enough of such speculations. I must not further defer examination of the symbolism of intellectual property in science by continuing with observations on countervailing forces that emerge to curb the accumulation of advantage that might otherwise lead to a permanent institutional monopoly or sustained oligopoly in fields of science and the sustained domination of a few individuals in those fields. Just as there is reason to know that the preeminence of individual scientists will inexorably come to an end, so there is reason to expect that various preeminent departments of science will decline while others rise in the fullness of time.³⁶

THE SYMBOLISM OF INTELLECTUAL PROPERTY IN SCIENCE

To explore the forms of inequality in science registered by such concepts as the Matthew effect and the accumulation of advantage, we must have some way of thinking about the distinctive equivalents in the domain of science of income, wealth, and property found in the economic domain. How do scientists manage to perceive one another simultaneously as peers and as unequals, in the sense of some being first among equals—*primus inter pares*, as the ancients liked to say? What is the distinctive nature of the coin of the realm and of intellectual property in science?

The tentative answer to the coinage question I proposed back in 1957 seems to have gained force in light of subsequent work in the sociology of science.³⁷ The system of coinage is taken to be based on the public recognition of one's scientific contributions by qualified peers. That coinage comes in various denominations: largest in scale and shortest in supply is the towering recognition

³⁶ Surveys of the quality of graduate departments in American universities have been conducted from time to time, with the last three of them, in 1966, 1970, and 1982, having adopted more-or-less similar methods of inquiry. I am indebted to an unpublished study by Donald Hood that identifies patterns of substantial change in the assessed quality of academic departments in the course of quite short intervals.

³⁷ R. K. Merton, "Priorities in Scientific Discoveries" (1957), rpt. in Merton, *Sociology of Science* (cit. n. 1), pp. 286-324.

symbolized by eponyms for an entire epoch in science, as when we speak of the Newtonian, Darwinian, Freudian, Einsteinian, or Keynesian eras. A considerable plane below, though still close to the summit of recognition in our time, is the Nobel Prize. Other forms and echelons of eponymy, the practice of affixing the names of scientists to all or part of what they have contributed, comprise thousands of eponymous laws, theories, theorems, hypotheses, and constants, as when we speak of Gauss's theorems, Planck's constant, the Heisenberg uncertainty principle, a Pareto distribution, a Gini coefficient, or a Lazarsfeld latent structure. Other forms of peer recognition distributed to far larger numbers take further graded forms: election to honorific scientific societies, medals and awards of various kinds, named chairs in institutions of learning and research, and, moving to what is surely the most widespread and altogether basic form of scholarly recognition, that which comes with having one's work used and explicitly *acknowledged* by one's peers.

I shall argue that cognitive wealth in science is the changing stock of knowledge, while the socially based psychic income of scientists takes the form of pellets of peer recognition that aggregate into reputational wealth. This conception directs us to the question of the distinctive character of intellectual property in science.

As I suggested at the outset, it is only a seeming paradox that, in science, one's private property is established by giving its substance away. For in a long-standing social reality, only when scientists have published their work and made it generally accessible, preferably in the public print of articles, monographs, and books that enter the archives, does it become legitimately established as more or less securely theirs. That is, after all, what we mean by the expression "scientific contribution": an offering that is accepted, however provisionally, into the common fund of knowledge.

That crucial element of free and open communication is what I have described as the norm of "communism" in the social institution of science—with Bernard Barber going on to propose the less connotational term "communality."³⁸ Indeed, long before the nineteenth-century Karl Marx adopted the watchword of a fully realized communist society—"from each according to his abilities, to each according to his needs"—this was institutionalized practice in the communication system of science. This is not a matter of human nature, of nature-given altruism. Institutionalized arrangements have evolved to motivate scientists to contribute freely to the common wealth of knowledge according to their trained capacities, just as they can freely take from that common wealth what they need. Moreover, since a fund of knowledge is not diminished through exceedingly intensive use by members of the scientific collectivity—indeed, it is presumably augmented—that virtually free and common good is not subject to what Garrett Hardin has aptly analyzed as "the tragedy of the commons": first the erosion and then the destruction of a common resource by the individually rational and collectively irrational exploitation of it.³⁹ In the commons of science it is structurally the case that the give and the take both work to enlarge the common resource of accessible knowledge.

³⁸ R. K. Merton, "The Normative Structure of Science" (1942), rpt. *ibid.*, pp. 267-278, esp. pp. 273-275; and Bernard Barber, *Science and the Social Order* (New York: Free Press, 1952), pp. 130-132.

³⁹ Garrett Hardin, "The Tragedy of the Commons," *Science*, 1968, 162:1243-1247.

The structure and dynamics of this system are reasonably clear. Since positive recognition by peers is the basic form of extrinsic reward in science, all other extrinsic rewards, such as monetary income from science-connected activities, advancement in the hierarchy of scientists, and enlarged access to human and material scientific capital, derive from it. But, obviously, peer recognition can be widely accorded only when the correctly attributed work is widely known in the pertinent scientific community. Along with the motivating intrinsic reward of working on a scientific problem and solving it, this kind of extrinsic reward system provides great incentive for engaging in the often arduous and tedious labors required to produce results that enlist the attention of qualified peers and are put to use by some of them.

This system of open publication that makes for the advancement of scientific knowledge requires normatively guided reciprocities. It can operate effectively only if the practice of making one's work communally accessible is supported by the correlative practice in which scientists who make use of that work acknowledge having done so. In effect, they thus reaffirm the property rights of the scientist to whom they are then and there indebted. This amounts to a pattern of legitimate appropriation as opposed to the pattern of illegitimate expropriation (plagiarism).

We thus begin to see that the institutionalized practice of citations and references in the sphere of learning is not a trivial matter. While many a general reader—that is, the lay reader located outside the domain of science and scholarship—may regard the lowly footnote or the remote endnote or the bibliographic parenthesis as a dispensable nuisance, it can be argued that these are in truth central to the incentive system and an underlying sense of distributive justice that do much to energize the advancement of knowledge.

As part of the intellectual property system of science and scholarship, references and citations serve two types of functions: instrumental cognitive functions and symbolic institutional functions. The first of these involves directing readers to the sources of knowledge that have been drawn upon in one's work. This enables research-oriented readers, if they are so minded, to assess for themselves the knowledge claims (the ideas and findings) in the cited source; to draw upon other pertinent materials in that source that may not have been utilized by the citing intermediary publication; and to be directed in turn by the cited work to other, prior sources that may have been obliterated by their incorporation in the intermediary publication.

But citations and references are not only essential aids to scientists and scholars concerned to verify statements or data in the citing text or to retrieve further information. They also have not-so-latent symbolic functions. They maintain intellectual traditions and provide the peer recognition required for the effective working of science as a social activity. All this, one might say, is tucked away in the aphorism that Newton made his own in that famous letter to Hooke where he wrote: "If I have seen further, it is by standing on y^e shoulders of Giants."⁴⁰ The very form of the scientific article as it has evolved over the last three centuries normatively requires authors to acknowledge on whose shoulders

⁴⁰ George Sarton was long interested in the history of the aphorism. Since it says much in little about one of the ways in which scientific knowledge grows, I indulged in a Shandean account of its historical adventures: R. K. Merton, *On the Shoulders of Giants* (1965; New York: Harcourt Brace Jovanovich, 1985).

they stand, whether these be the shoulders of giants or, as is often the case, those of men and women of science of approximately average dimensions for the species *scientificus*. Thus, in our brief study of the evolution of the scientific journal as a sociocognitive invention, Harriet Zuckerman and I have taken note of how Henry Oldenburg, the editor of the newly invented *Transactions of the Royal Society* in seventeenth-century England, induced the emerging new breed of scientist to abandon a frequent long-standing practice of sustained secrecy and to adhere instead to “the new form of free communication through a motivating exchange: open disclosure in exchange for institutionally guaranteed honorific property rights in the new knowledge given to others.”⁴¹

That historically evolving set of complementary role obligations has taken deep institutional root. A composite cognitive and moral framework calls for the systematic use of references and citations. As with all normative constraints in society, the depth and consequential force of the moral obligation to acknowledge one's sources become most evident when the norm is violated (and the violation is publicly visible). The failure to cite the original text that one has quoted at length or drawn upon becomes socially defined as theft, as intellectual larceny or, as it is better known since at least the seventeenth century, as plagiarism. Plagiarism involves expropriating the one kind of private property that even the dedicated abolitionist of private productive property, Karl Marx, passionately regarded as inalienable (as witness his preface to the first edition of *Capital* and his further thunderings on the subject throughout that revolutionary work).

To recapitulate: the bibliographic note, the reference to a source, is not merely a grace note, affixed by way of erudite ornamentation. (That it can be so used, or abused, does not of course negate its core uses.) The reference serves both instrumental and symbolic functions in the transmission and enlargement of knowledge. Instrumentally, it tells us of work we may not have known before, some of which may hold further interest for us; symbolically, it registers in the enduring archives the intellectual property of the acknowledged source by providing a pellet of peer recognition of the knowledge claim, accepted or expressly rejected, that was made in that source.

Intellectual property in the scientific domain that takes the form of recognition by peers is sustained, then, by a code of common law. This provides socially patterned incentives, apart from the intrinsic interest in inquiry, for attempting to do good scientific work and for giving it over to the common wealth of science in the form of an open contribution available to all who would make use of it, just as the common law exacts the correlative obligation on the part of the users to provide the reward of peer recognition by reference to that contribution. Did space allow—which, happily for you, it does not—I would examine the special case of tacit citation and of “obliteration by incorporation” (or, even more briefly, OBI): the obliteration of the sources of ideas, methods, or findings by their being anonymously incorporated in current canonical knowledge.⁴² Many of

⁴¹ Harriet Zuckerman and R. K. Merton, “Patterns of Evaluation in Science: Institutionalization, Structure and Functions of the Referee System,” *Minerva*, 1971, 9:66–100.

⁴² I easily resist the temptation to begin a discourse on this pattern in the transmission of knowledge. Short proleptic discussions of “obliteration by incorporation” are found in Merton, *Social Theory and Social Structure* (New York: Free Press, 1968), pp. 25–38; Merton, foreword to Eugene Garfield, *Citation Indexing: Its Theory and Application in Science, Technology, and Humanities* (New York: Wiley, 1979); and Garfield, *Essays of an Information Scientist* (Philadelphia: ISI Press, 1977), pp. 396–399.

these cases of seemingly unacknowledged intellectual debt, it can be shown, are literally exceptions that prove the rule, that is to say, they are no exceptions at all since the references, however tacit, are evident to knowing peers.

Once we understand that the sole property right of scientists in their discoveries has long resided in peer recognition of it and in derivative collegial esteem, we begin to understand better the concern of scientists to get there first and to establish their priority.⁴³ That concern then becomes identifiable as a “normal” response to institutionalized values. The complex of validating the worth of one’s work through appraisal by competent others and the seeming anomaly, even in a capitalistic society, of publishing one’s work without being directly recompensed for each publication have made for the growth of public knowledge and the eclipse of private tendencies toward hoarding private knowledge (secrecy), still much in evidence as late as the seventeenth century. Current renewed tendencies toward secrecy, and not alone in what Henry Etzkowitz has described as “entrepreneurial science,”⁴⁴ will, if extended and prolonged, introduce major change in the institutional and cognitive workings of science.

Since I have imported, not altogether metaphorically, such categories as intellectual property, psychic income, and human capital into this account of the institutional domain of science, it is perhaps fitting to draw once again upon a chief of the tribe of economists for a last word on our subject. Himself an inveterate observer of human behavior rather than only of economic numbers, and also himself a practitioner of science who keeps green the memory of those involved in the genealogy of an idea, Paul Samuelson cleanly distinguishes the gold of scientific fame from the brass of popular celebrity. This is how he concluded his presidential address, a quarter century ago, to an audience of fellow economists: “Not for us is the limelight and the applause [of the world outside ourselves]. But that doesn’t mean the game is not worth the candle or that we do not in the end win the game. In the long run, the economic scholar works for the only coin worth having-our own applause.”⁴⁵

⁴³ For the claim that the race for priority derives from the culture of science itself see Merton, “Priorities in Scientific Discoveries” (cit. n. 37), pp. 286-308. It is further proposed (pp. 309-324) that the extreme emphasis upon significant originality in the culture of science can become pathogenic, making for such occasional side effects as the cooking of fraudulent evidence, the hoarding of one’s own data while making free use of others’ data, and the breaching of the mores of science by failing to acknowledge the work of predecessors one has drawn upon.

⁴⁴ Henry Etzkowitz, “Entrepreneurial Scientists and Entrepreneurial Universities in American Academic Science,” *Minerva*, 1983, 21: 198-233.

⁴⁵ Paul Samuelson, “Economics and the History of Ideas” (delivered in 1961), rpt. in *The Collected Scientific Papers of Paul A. Samuelson*, ed. Joseph E. Stiglitz (Cambridge, Mass.: MIT Press, 1966), Vol. II, pp. 1499-1516.