THE MATTHEW EFFECT IN SCIENCE. CUMULATIVE ADVANTAGE AND THE SYMBOLISM OF INTELLECTUAL PROPERTY

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A personal Prologue

I begin with a short and personal prologue. This is a special moment for all of us and not least for me, for reasons I shall confess and, in these friendly surroundings, confess quite freely. This grand occasion follows by two years the occasion on which hundreds of scholars from around the world gathered here to celebrate the centenary of George Sarton's birth. I was privileged there to tell of my complex and grateful apprenticeship served at Harvard University under the then grand master of the art and craft of the history of science just as I am privileged today to take part in inaugurating the first Chair established in his honor at any university.

We are, indeed, assembled here in what amounts to a manifold commemoration: Having lately celebrated the birth of George Sarton a hundred years ago, we can now commemorate his having received a doctorate from this University precisely 75 years ago. And further, we can regard this occasion as commemorating Sarton's own inaugural lecture for another University program in the history of science, that one the Seminary at Harvard 50 years ago (1) (at the very time, I add in a distinctly small footnote, that I was completing my doctorate under his direction with a dissertation in the historical sociology of science). Though ever the dedicated rationalist, George Sarton might nevertheless have been bemused by this numerological symmetry attending our collective remembrances of him.

To contribute further to this cluster of commemoration, it was also half-a-century ago, in 1935, that Sarton himself was engaged in celebrating a Centenary, in his beloved journal, *Isis*. That was the Centenary of the pathbreaking and pathmaking volume, *Physique sociale,* which won instant fame for its author, Adolphe Quetelet (who, of course, was the very first recipient of a doctorate from the newly founded University of Ghent). In that tribute and with passionate intensity [as you may recall] Sarton rightly credits Quetelet with being one of the spiritual fathers of modern statistics. More in point for me this day, he also credits Quetelet with being a co-founder if not, indeed, *the* founder of sociology. I can bear witness that, despite Sarton's longlasting devotion to the positivism generated by Auguste Comte, he had small regard for Comte the man and, for that matter, in some aspects, for Comte the scholar. Sarton acknowledged that Comte "was probably the first to speak of social physics (as early as 1822) and of sociology (1839)" (2). But, he went on to observe with undisguised scorn, he "wrote on these matters as on many others with unbearable prolixity and conceit ... Comte talked, strutted and soared, and apparently ignored the terre-à-terre activity of his fellow worker in 'social physics', but that activity was far more creative than his own. Comte was building proud castles on sand, Quetelet, humbler constructions on bedrock." I recall the passion with which Sarton composed these words for I then still occupied a desk in his famed workshop in Harvard's Widener Library. (As you see, I still treasure this rare offprint of that essay.) I also recall his assuring me, in a light moment of Flemish humor, that his preference for Quetelet did not at all derive from their being fellow Gentenaren, both by birth and by education. And to integrate these biographical and historical moments into further interpersonal networks, a generation later, Paul Lazarsfeld, my collaborator and friend for 35 years, was concluding a joint article on Quetelet in the *International Encyclopedia of the Social Sciences* with these words: "... it is difficult to dispute Sarton's description of *Sur l'homme [i.e. Physique sociale]* as 'one of the greatest books of the nineteenth century'; or, for that matter, his choice of Quetelet over Comte as the 'founder of sociology'" (3). In light of all this, you will understand that when a special chair was to be established at Columbia University for Paul Lazarsfeld, I urged that it be named, as indeed it was, the Quetelet Professor of Social Science. Truly, this sequence is an interweaving of personal linkages and scholarly traditions.

Sarton's interest in sociological matters was not shortlived or perfunctory; it was in evidence from his youth unward. And as I told here at his Centenary (4), it may have been that interest which led to his accepting me into his workshop as one of his very few graduate students. Now one knows that counterfactual history or biography is precarious; still, I indulge for a moment to express the belief that without George Sarton's support I might not have continued my work on a dissertation in what was then far from being a discipline, the historical sociology of science. That field of inquiry, to some extent then and much more since, seeks to merge history and sociology in an effort to understand the character and development of the sciences in both their social and their cognitive aspects: the sciences as institutionalized arrangements, evolving or designed, for the acquiring of scientific knowledge, and the sciences as that knowledge itself with identifiable properties and modes of change in various times and places. George Sarton had mixed feelings about the emerging historical sociology of science but his attitude was largely favorable. After all, as early as 1916, he could write that "the history of science in the main amounts to psycho-sociological investigation" (5). And as late as 1952, he could refer to "my sociology of science" (6). Further, I can scarcely forget that in the mid-1930s, he had created a post of an Associate Editor of *Isis* to deal with "social aspects of science" and later, of "sociology" which he assigned to this one-time student of his. All these years later, I still sense the symbolic importance that generous action must have had for a neophyte sociologist fully aware that, at its origin, *Isis* had numbered the master sociologist, Emile Durkheim, among its patrons (7).

It is within the context of those years shared with the man and the scholar we honor here today that I propose, for this inaugural lecture, a report on 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:

THE MATTHEW EFFECT IN SCIENCE II. Cumulative Advantage and the Symbolism of Intellectual Property.

Yet, properly deciphered, that title is not nearly as obscure 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 is a follow-on to an earlier one, "The Matthew Effect in Science," which I finally put into print a good many years ago (8). The ponderous, not say lumpy, subtitle signals the content of this lecture. 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 unless restricted 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. And I shall argue 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 lecture. Since that agenda can only be 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.

Only now does it occur to me that an obscure title can have its latent functions: to provide necessity for elucidating one's intent rather than allowing one to assume that the title truly speaks for itself. Which brings us, naturally enough, to 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 (9). 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 (Zuckerman, taped protocols): "The world is peculiar in this matter of how it gives credit. It tends to give the credit to [already] famous people." 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-section sample studied by Hagstrom have made similar observations (10). 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 are at least enough alike to be defined as functional equivalents by 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" (Zuckerman, p. 140). Or as a laureate in chemistry put it" "If my name was on a paper, people would remember it and not remember who else was involved" (Zuckerman, p. 228).

At the extreme, such misallocation of credit can occur even when published papers bear only the name of the hitherto unknown scientists. 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 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 percent of the credit":

> "'The other 5 percent 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'." (11).

It is these patterns of the misallocation of recognition for scientific work which I have described as "the Matthew effect." The foreordained term derives, of course, from the first book of the New Testament, the Gospel according to St. 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 everyone 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 (12).

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, which 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 some of the questions before the house this evening.

Accumulation of Advantage and Disadvantage for Scientists

Taken literally, the Matthew doctrine would result in a 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 while giving less or nothing at all unto him and her that hath not will of course lead to the rich getting forever richer while the poor get relatively and absolutely 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 examine 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 listeners 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 ... (13). It would be neither correct nor just to say that that text is no clearer to me now than that notoriously obscure passage in *Sordello* was clear to Robert Browning, when he confessed that "When I wrote that, God and I knew what it meant, but now God alone knows" (14). However, it is correct to say that the notion of cumulative advantage just rested there as only a proto-concept - inert, unexplicated, and unnoticed - 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 in print (15). Further investigation of the process of cumulative advantage took hold in the later 1960s with the formation of a research quartet at Columbia consisting of Harriet Zuckerman, Stephen Cole, Jonathan Cole, and myself. To adopt the brilliant terminological recoinage of Derek Price, a nationwide "invisible college" then emerged and has since grown apace that is engaged in developing a program of research on cumulative advantage and disadvantage, in social stratification generally and in science particularly. That invisible college (16) includes Derek Price himself, Paul Allison, Judith Blau, Jerry Gaston, Jack Goldstone, Lowell Hargens, Karen Knorr, Tad Krauze, J. Scott Long, Robert McGinnis, Edgar W. Mills, Jr., Barbara Reskin, Leonard Rubin, Jay Stewart, Nico Stehr and Volker Meja, H. J. Walbert, among others.

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 consequence 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 in the life-course of individual scholars and scientists.

First, then, a quick sampling of the abundance of conspicuous 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 Kelvin with his 600 papers or the mathematician Cayley, publishing a paper every few weeks throughout his life for a total of almost a thousand.
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> The skewed distribution in the sheer number of published papers is best approximated by variants of Lotka's so-called "inverse square law" which states that the number of scientists with *n* publications is proportional to ln^{2}". In a variety of disciplines, this works out to some 5 or 6 % of the scientists who *publish at all* producing about half of all papers in their discipline.
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> 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 datasets: typical is Garfield's finding that for an aggregate of some 10 million articles published in the physical and biological sciences between 1975-79,
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> .1% were cited more than 100 times; another
> 1.3% between 25 and 100 times; and, at the other extreme,
> 63.6% of those which were cited at all were cited only once.
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> This inequality, you will recognize, is steeper than most 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 2000 American biologists, mathematicians, chemists, and physicists into several strata by career age), Allison and

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" (17).

Allison and Stewart also confirmed the Zuckerman-Merton hypothesis (18) that decreasing research productivity with increasing age results largely from differing rates of attrition in research-roles; 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.

As Derek Price (19) ably reformulated 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. Graduallly, 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 [or even increasing] rate throughout his [or her] professional lifetime. We need, therefore, to 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".

With regard to the Matthew effect and associated cumulation of advantage, Stephen Cole (1970) found for a sample of American physicists that the greater their reputation, the more likely that their new publications will soon be recognized through citation (i.e., within a year after they appeared). Prior repute somewhat advances the speed of diffusion. 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.

As I have mentioned, I want to focus for a time on the special problems in the accumulation of advantage 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 early work is judged as ordinary. Such early prognostic judgments, I suggest, lead to the 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 ours, where our 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 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" [Gregg, For Future Doctors, 1973].

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 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". By suggesting that there are such slow-starting mutants who have *greater* potentialities than others, Gregg is plainly assuming part of what he then concludes. Nevertheless, his argument cuts deeply. For, of course, we know only of those late bloomers who eventually came to bloom; we don't know of the potential late bloomers who, cut off from positive response and support in their youth never managed to come into their own at all.

Judged inept or at best ordinary by comparison with precocious age peers, they are treated as youth of small capacity. They slip through the net of our institutional sieves for the location of potential performance since that selective net makes chronological age - not even occupational or professional age - the basis for assessing relative ability. Defined by the institutional system as incorrigible mediocrities, some of the potential late bloomers come to believe this of themselves, and act accordingly. They limit their pointless efforts or, at the extreme, retreat from the system altogether. At least what we know about the workings of the self-fulfilling prophecy in the formation of self-images suggests that this is so. Most of us most of the time, and not only the so-called "other-directed" men and women amongst us, tend to form our self-image - our image of potentiality and of achievement - as a reflection of the images significant others indicate they have of us. In particular, it is the images which institutional authorities have of us that tend to become self-fulfilling, for it is they who shape our micro-environments: thus, early on, if the teachers who inspect our intelligence tests and our aptitude tests and all the other institutionalized indicators of future performance, go on to compare our records with those of our age-peers, and concluding that we're merely run-of-the-mine or worse, then proceed to *treat us accordingly*, they can lead the less precocious amongst us to become what we have been led to think we are: condemned to mediocrity.

What's more, I think it likely that the institutionalized bias toward precocity has notably different consequences for comparative 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 youngsters aren't precocious, if they don't exhibit distinct ability early on and so are not rewarded by scholarships and other sustaining grants, economic pressures require significant numbers of them to drop out. In contrast, potential late bloomers among the well-do-do have a better prospect of belated recognition. Even when they do poorly in their school work at first, they frequently go on to college. 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, some fraction of these late bloomers eventually come to view. A far larger fraction of their counterparts in the much larger population of the less advantaged strata are by hypothesis lost for good, so far as certain forms of intellectual work are concerned. The bias toward precocity thus works profound and ordinarily hidden damage upon some of those subjected to it, this without any such intent on the part of the people engaged in running our institutions of education and thereby of social selection. And, as is usually the case, it is such unanticipated and unintended consequences of purposive social action that tend to persist. They are *latent*, not manifest, social problems (20), that is, social conditions and processes that are at odds with interests and values of the society but are not generally recognized as being so. 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 milieux for scientific work with their limited resources. Absent or in short supply are the scarce resources of access to needed equipment, an abundance of able assistance, time institutionally set aside for research and, above all else perhaps, a cognitive micro-environment 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 which removes them early on from the system of scientific work and productive scholarship.

In short, the processes of accumulative advantage and disadvantage accentuate various inequalities in science and learning: inequalities of recognition, inequalities of access to resources, and inequalities of scientific productivity. Antecedent differences in places of university study with their associated differences in access to outstanding and evocative research teachers, early or late publication, initial job placement, postponed citation and other modes of peer recognition combine multiplicatively in the course of time to produce a distribution of tastes, skills, rewards, facilities, and consequent opportunities that cumulate to produce highly skewed productivity of scientific work (21).

Thus, processes of individual self-selection and institutional social selection interact to affect successive probabilities of various locations in the opportunity structure. When the role performance or other attributes of the individual measure up to or conspicuously exceed the standards of the particular institution, this begins a process of cumulative advantage in which that individual acquires successively enlarged opportunities for advancing his work (and the rewards that go with it) even further. 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 (22).

Accumulation of Advantage and Disadvantage among Science Institutions

Skewed distributions of resources and productivity are found among science institutions that resemble those we have noted among individual scientists. 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 which have not yet made their mark. These skewed distributions are well known and need only bare mention here:

> In 1981, some 28 percent of federal support for academic research and development went to just ten universities (23).
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> In an earlier year, all divisions of the federal government allocated 29% of their funds for academic research and development in the physical sciences to a scant seven universities where, it turns out, the graduate departments had been rated by samples of scientists in those fields, as among the top-ranked five in astronomy, chemistry, geology, and physics.
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> Those composites of 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 National Science Foundation graduate fellows elected to study at just 15 universities.
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> Those concentrations have been even more conspicuous in the case of outstanding scientists. Zuckerman (1977) found, for example, that at the time they did the research that ultimately brought them the Nobel prize, 49% of the future laureates working in universities were in just five of them: Harvard, Columbia, Rockefeller, Berkeley and Chicago. By way of comparison, these five universities constituted less than 3% of all faculty members in American universities. Zuckerman also found that these resource-full universities seem able to spot and to retain these prime movers in contemporary science. For example, they

kept 70% of the future laureates they had trained in comparison with 28% of the other Ph.Ds they had trained.

But enough of these familiar details of great organizational inequalities in science. This only raises anew the question which must have been nagging at you for much of this evening: if the processes of cumulating advantage and disadvantage are truly at work, why aren't there even greater inequalities than have been found to obtain?

Countervailing Processes

Or to put the question more concretely and parochially, why haven't 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 at a particular time? Put more generally, why don't the processes of cumulating advantage and disadvantage continue without assignable limit?

Now even Macaulay's ubiquitous schoolboy would presumably 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 won't.

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 which 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 universities and of scientific productivity within them. But 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 abstractedly good thing.

Think further 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 feel, 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 the pattern of master-apprentice ambivalence may not relish the thought of having in their vicinity exceedingly talented younger associates 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. 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 august chair at the advanced age of 39 in favor of his 27-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 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 somehow be allocated 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 provide competitively attractive micro-environments 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 I must not further exploit the moments borrowed from a scheduled examination of the symbolism of intellectual property in science by continuing with observations on countervailing forces that emerge to curb the accumulation of advantage which might otherwise seem to lead inexorably to a sustained institutional oligopoly of fields of science and the sustained domination of a few individuals in those fields. Just as there is reason to expect that the preeminence of individual scientists will come to an end, so there is reason to expect that various departments of science will rise, disperse, and decline in the fullness of time.

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 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. 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, and shortest in supply, is the towering recognition symbolised 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 name of scientists to all or part of what they have contributed, are comprised by 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, or Thurstone and Guttman scales. Other forms of peer recognition distributed to far larger numbers take further graded forms: election to honorific scientific societies, medals and awards of varied kinds, named chairs in institutions of learning and research, and, moving to what is surely the most widespread and altogether basic form of 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 such peer recognition. This 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 longstanding social reality, only when scientists have published their work and made it generally accessible, preferably in the public print of journals and monographs 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.

Bernard Barber has proposed the less connotational term, "communalism". (24) Indeed, long before the 19th-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. Of course, 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 abilities, 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 (1968) 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. In the commons of science it is structurally the case that the give and take both work to enlarge the common resource of accessible knowledge.

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 access to enlarged human and material scientific capital, derive from it. But, of course, peer recognition can be widely accorded only when the correctly attributed work is widely known in the pertinent scientific community. All apart from the motivating *intrinsic* reward of finding a scientific problem and solving it, this kind of reward-system provides great incentive for engaging in the sometimes exceedingly demanding labors, and often much drudgery, involved in the sustained inquiry that may enlist the attention of qualified peers and be put to use by some of them.

This system of open publication that makes for the advancement of scientific knowledge can operate only if the practice of making one's work communally available 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 legitimatized *ap*propriation without illegitimate *ex*propriation.

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, endnote, or bibliographical parenthesis as a dispensable nuisance, it can be argued that they are in truth central to the incentive system that does 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 instrumental cognitive function involves directing readers to the sources of knowledge one has drawn upon in one's work. This enables research-oriented readers, if they are so minded, (1) to assess for themselves the knowledge claims (the ideas and findings) in the cited source; (2) to draw upon other pertinent materials in that source which may not have been utilized by the citing intermediary publication; and (3) to be directed in turn by the cited work to other, prior sources which may have been obliterated by incorporation in the intermediary publication.

But citations and references are not merely essential aids to scientists 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. All this, I 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 the shoulders of giants." (25). The very form of the scientific article as it has evolved over the last three centuries normatively requires authors to acknowledge on whose shoulders they stand, whether these be the shoulders of giants or, as is often the case, the 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 socio-cognitive invention, Harriet

Zuckerman and I have taken note of how Henry Oldenburg, the editor of the newly invented *Transactions* of the Royal Society in 17th-century England, induced the emerging new breed of scientists to abandon a longstanding practice of sustained secrecy and to adhere instead to "the new norm of free communication through a motivating exchange: open disclosure in exchange for institutionally guaranteed honorific property rights in the new knowledge given to others."

That historically evolving set of complementary role-obligations has taken deep institutional root. 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 which one has quoted at length becomes socially defined as theft, as intellectual larceny or, as it is better known since at least the 17th century, as plagiary. Plagiary involves appropriating the one kind of private property which even the dedicated abolitionist of private property, Karl Marx, passionately regarded as inalienable.

To recapitulate: the bibliographical footnote, the reference to a source, is not merely a grace note, affixed by way of erudite ornamentation. (That it can so be used, or abused, does not of course negate its core uses.) The citation 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 as pellet of peer recognition of the knowledge claim, accepted or expressly rejected, that was made in that source.

Intellectual property in the scientific domain which takes the form of recognition by peers is sustained then, by a code of common law. This provides socially patterned incentives, apart from the idiocyncratic ones, 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 citations to that contribution. Did time 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 (26). Many of 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.

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. 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 secrecy and private knowledge, still much in evidence as late as the 17th century. Renewed tendencies toward secrecy, and not alone in what Henry Etzkowitz (27) has described as "entrepreneurial science," will, if prolonged, introduce major change in the institutional 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 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 ideas, Paul Samuelson clearly 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" (28).

FOOTNOTES

(1) George Sarton, *The Study of the History of Science*. Cambridge, Massachusetts: Harvard University Press, 1936.

(2) George Sarton, "Preface to Volume XXIII of Isis: Quetelet", *Isis* 1935, 4-24, at 14.

(3) David Landau and Paul F. Lazarsfeld, "Adolphe Quetelet", in *International Encyclopedia of the Social Sciences* (David L. Sills, ed.). New York: The Macmillan Company and The Free Press, 1968, Vol. 13, pp. 247-257.

(4) Robert K. Merton, "George Sarton: Episodic Recollections of an Unruly Apprentice", *Isis* 1985, 76, 470-486.

(5) From "The History of Science", *The Monist* 1916, 26:321-365, as reprinted in George Sarton, *The Life of Science*. New York: Henry Schuman, 1948, p. 57.

(6) But by this time, as the following passage signals, he had restricted his concept of psycho-sociological inquiry: "What I call here sociology of science is implicitly defined in the preceding sentence ['the impact of society upon science and of science upon society']. It is somewhat different from the Wissenssoziologie [about which see Robert K. Merton: The sociology of knowledge (*Isis* 1937, 27, 493-503)]. Wissenssoziologie is more ambitious from the metaphysical and epistemological point of view than my sociology of science". George Sarton, *Horus: A Guide to the History of Science*. Waltham, Massachusetts: Chronica Botanica, 1952, p. 94.

(7) This calls back to mind that the very first scholarly paper I published

- "Recent French Sociology" (1934) - was largely focussed on Durkheim and the Durkheim school, while my second was devoted to Durkheim's fundamental work: *Division of Labor in Society*.

(8) In *Science*, 5 January 1968, Vol. 159, #3810, 56-63; reprinted in R. K. Merton, *The Sociology of Science*. Chicago: University of Chicago Press, 1973, Chapter 20.

(9) Harriet Zuckerman, *Nobel Laureates in Science: A Sociological Study of Scientific Collaboration*,, Ph.D. dissertation, Columbia University, 1965. The later fruits of that research appear in Zuckerman, *Scientific Elite: Nobel Laureates in the United States*. New York: The Free Press, 1977; an account of the procedures adopted in these tape-recorded interviews appears in her article, "Interviewing an Ultra-Elite," *Public Opinion Quarterly* 1972, 36, 159-175. This is occasion for repeating what I have noted in reprinting the article, "The 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 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.

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

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

(12) The astronomer Charles D. Geilker and the sociologist David L. Sills have separately pointed out to me that this might as well have been dubbed "the Mark effect" or "the Luke effect" since much the same passage occurs in all three synoptic gospels (Mark 4:5 and Luke 8:18, 19:26). As Geilker observes, my usage only goes to show the importance of being 'first author', in what amounts to a sequence of authors'

names in a tacit collaboration. Perhaps Mark should have first claim, since he probably published first; the evidence is fairly clear but not beyond dispute. Yet in a way, Mark is redundant, for there is nothing in his short, direct version of the Gospel that is not to be found in one or another of the other Gospels. Luke may be a more serious contender. There, of course, the key passage is imbedded in a parable much like that found in Matthew, albeit referring to higher rates of return to be gained from the trading or investment of 'talents'. But it is not essential in these matters that the last-placed of the three pertinent Gospels shall be first. Finally, both Geilker and Sills have a point in noting that since the three authors are all quoting Jesus, I might just as well have adopted the term, "the Jesus effect". But then, I should not have achieved the feat of neutralizing the Matthew effect by the very act of calling it "the Matthew effect". In any event, it is not for me to adjudicate the claims to priority of Matthew, Mark, or Luke. That can best be left to a higher court.

(13) "The Normative Structure of Science" [1942], reprinted in Merton, *The Sociology of Science*, p. 273.

(14) 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.

(15) It was first presented as the National Institutes of Health Lecture in February 1964 and later that year at the annual meetings of the American Association for the Advancement of Science. The paper then underwent several more editions in a succession of public lectures before it found its way into print in *Science* (1968).

(16) In his *Little Science, Big Science,* Price ([1962] 1986) extended Robert Boyle's 17th-century term, 'invisible college', to designate the informal collectives of scientists interacting in their research on particular problems which are generally limited to a size "that can be handled by interpersonal relationships".

(17) Paul D. Allison and John A. Stewart, "Productivity Differences Among Scientists: Evidence for Accumulative Advantage", *American Sociological Review* 1974, 39, 596-606, at 598-9.

(18) This hypothesis and its underpinnings remain as formulated a quarter-century ago: R. K. Merton, "'Recognition' and 'Excellence': Instructive Ambiguities", in Adam Yarmolinsky, ed., *Recognition of Excellence: Working Papers*. New York: The Free Press, 297-328, at p. 312; that volume now being out of print, the essay can be more readily found in Merton, *The Sociology of Science*, pp. 419-438, see in particular at 428. Under the stimulus of Robert Rosenthal, hundreds of empirical studies of this self-fulfilling effect in American schools have been conducted during the past two decades. See, to begin with, Robert Rosenthal, *"Pygmalion in the Classroom"*; Lenore Jacobson, *"Teacher Expectation and Pupils' Intellectual Development"* (New York: Holt, Rinehart & Winston, 1968); Janet Elashoff and Richard E. Snow, *"Pygmalion Reconsidered"* (Worthington, Ohio: Jones Pub., 1971); and Harris M. Cooper and Thomas L. Good, *"Pygmalion Grows Up: Studies in the Expectation Communication Process* (New York: Longman, 1983)

(19) Derek de Solla Price, "The Productivity of Research Scientists", *Yearbook of Science and the Future 1975*. Chicago: Encyclopedia Britannica, 1975, 409-421, at 414.

(20) On the concept of manifest and latent social problems, see Merton, *Social Research and the Practicing Professions*. Cambridge, Massachusetts: Abt Books, 1982, pp. 43-99, esp. pp. 55 ff.

(21) Harriet Zuckerman, *"Accumulation of Advantage and Disadvantage: The Theory and Its Intellectual Biography"*. in Carls Mongardini and Simonetta Tabboni, eds., *L'opera di Robert K. Merton e la sociologia contemporanea*. Roma: Associazione Italiana di Sociologia, in press.

(22) A while ago, I tried to trace in more-or-less clinical fashion, the pattern of accumulation of advantage in the academic life-course of the

historian of science and my longtime friend, Thomas S. Kuhn, just as I have done more recently in tracking my own experience as an apprentice to the then world dean of the history of science, George Sarton. For the case of Kuhn, see Merton, *The Sociology of Science: An Episodic Memoir*. Carbondale, Illinois: Southern Illinois University Press, 1979, pp. 71-109; for my own case, see Merton, "George Sarton: Episodic Recollections of an Unruly Apprentice," *Isis*, 1985, 75, 470-486.

(23) 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.

(24) Bernard Barber, *"Science and the Social Order"*. New York: The Free Press, 1952, p. 130.

(25) Some of us have long been interested in the historical adventures of that aphorism which says much in little about the ways in which scientific knowledge selectively accumulates. I have indulged that interest in the book, *On the Shoulders of Giants* (New York: Harcourt Brace Jovanovich, [1965] 1985).

(26) I easily resist the temptation to begin a discourse on this pattern in the world of learning. Short proleptic discussions of it are to be found in R. K. Merton, *Social Theory and Social Structure*. New York: The Free Press, 1968, 25-38 and Merton, Foreword to Eugene Garfield, *Citation Indexing: Its Theory and Application in Science, Technology, and Humanities*. New York: Wiley, 1979; Eugene Garfield, *Essays of an Information Scientist*. Philadelphia: ISI Press, 1977, 396-399.

(27) Henry Etzkowitz, "Entrepreneurial Scientists and Entrepreneurial Universtities in American Academic Science", *Minerva*, 1983, *21*, 198-233.

(28) Entitled "Economists and the History of Ideas," the address, given in

1961, is reprinted in Volume 2 of *The Collected Scientific Papers of Paul A. Samuelson*, edited by Joseph E. Stiglitz. Cambridge, Massachusetts: The M.I.T. Press, 1966, pp. 1499-1516.