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The Matthew Effect in Science

The reward and communication systems of science are considered.

Robert K. Merton

This paper develops a conception of ways in which certain psychosocial processes affect the allocation of rewards to scientists for their contributions—an allocation which in turn affects the flow of ideas and findings through the communication networks of science. The conception is based upon an analysis of the composite of experience reported in Harriet Zuckerman's interviews with Nobel laureates in the United States (*1*) and upon data drawn from the diaries, letters, notebooks, scientific papers, and biographies of other scientists.

The Reward System and "Occupants of the Forty-First Chair"

We might best begin with some general observations on the reward system in science, basing these on earlier theoretical formulations and empirical investigations. Some time ago (*2*) it was noted that graded rewards in the realm of science are distributed principally in the coin of recognition accorded research by fellow-scientists. This recognition is stratified for varying grades of scientific accomplishment, as judged by the scientist's peers. Both the self-image and the public image of scientists are largely shaped by the communally validating testimony of significant others that they have variously lived up to the exacting institutional requirements of their roles.

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A number of workers, in empirical studies, have investigated various aspects of the reward system of science as thus conceived. Glaser (*3*) has found, for example, that some degree of recognition is required to stabilize the careers of scientists. In a case study Crane (*4*) used the quantity of publication (apart from quality) as a measure of scientific productivity and found that highly productive scientists at a major university gained recognition more often than equally productive scientists at a lesser university. Hagstrom (*5*) has developed and partly tested the hypothesis that material rewards in science function primarily to reinforce the operation of a reward system in which the primary reward of recognition for scientific contributions is exchanged for access to scientific information. Storer (*6*) has analyzed the ambivalence of the scientist's response to recognition "as a case in which the norm of disinterestedness operates to make scientists deny the value to them of influence and authority in science." Zuckerman (*7*) and the Coles (*8*) have found that scientists who receive recognition for research done early in their careers are more productive later on than those who do not. And the Coles have also found that, at least in the case of contemporary American physics, the reward system operates largely in accord with institutional values of the science, inasmuch as quality of research is more often and more substantially rewarded than mere quantity.

In science as in other institutional realms, a special problem in the workings of the reward system turns up when individuals or organizations take on the job of gauging and suitably rewarding lofty performance on behalf of a large community. Thus, that ultimate accolade in 20th-century science, the Nobel prize, is often assumed to mark off its recipients from all the other scientists of the time. Yet this assumption is at odds with the well-known fact that a good number of scientists who have not received the prize and will not receive it have contributed as much to the advancement of science as some of the recipients, or more. This can be described as the phenomenon of "the 41st chair." The derivation of this tag is clear enough. The French Academy, it will be remembered, decided early that only a cohort of 40 could qualify as members and so emerge as immortals. This limitation of numbers made inevitable, of course, the exclusion through the centuries of many talented individuals who have won their own immortality. The familiar list of occupants of this 41st chair includes Descartes, Pascal, Molière, Bayle, Rousseau, Saint-Simon, Diderot, Stendahl, Flaubert, Zola, and Proust (*9*).

What holds for the French Academy holds in varying degree for every other institution designed to identify and reward talent. In all of them there are occupants of the 41st chair, men outside the Academy having at least the same order of talent as those inside it. In part, this circumstance results from errors of judgment that lead to inclusion of the less talented at the expense of the more talented. History serves as an appellate court, ready to reverse the judgments of the lower courts, which are limited by the myopia of contemporaneity. But in greater part, the phenomenon of the 41st chair is an artifact of having a fixed number of places available at the summit of recognition. Moreover, when a particular generation is rich in achievements of a high order, it follows from the rule of fixed numbers that some men whose accomplishments rank as high as those actually given the award will be excluded from the honorific ranks. Indeed, their accomplishments sometimes far outrank those which, in a time of less creativity, proved enough to qualify men for this high order of recognition.

The Nobel prize retains its luster because errors of the first kind—where scientific work of dubious or inferior worth has been mistakenly honored—are uncommonly few. Yet limitations of the second kind cannot be avoided. The small number of awards means that, particularly in times of great scientific advance, there will be many occupants of the 41st chair (and, since the terms governing the award of the prize do not provide for posthumous recognition, permanent occupants of that chair). This gap in the award of the ultimate prize is only partly filled by other awards for scientific accomplishment since these do not carry the same prestige either inside the scientific community or outside it. Furthermore, what has been noted about the artifact of fixed numbers producing occupants of the 41st chair in the case of the Nobel prize holds in principle for other awards providing less prestige (though sometimes, nowadays, more cash).

Scientists reflecting on the stratification of honor and esteem in the world of science know all this; the Nobel laureates themselves know and emphasize it, and the members of the Swedish Royal Academy of Science and the Royal Caroline Institute who face the unenviable task of making the final decisions know it. The latter testify to the phenomenon of the 41st chair whenever they allude to work of "prize-winning calibre" which, under the conditions of the scarcity of prizes, could not be given the award. And so it is that, in the case of the Nobel prize, occupants of the 41st chair comprise an illustrious company that includes such names as Josiah Willard Gibbs, Mendeleev, W. B. Cannon, H. Quincke, J. Barcroft, F. d'Hérelle, H. De Vries, Jacques Loeb, W. M. Bayliss, E. H. Starling, G. N. Lewis, O. T. Avery, and Selig Hecht, to say nothing of the long list of still-living uncrowned Nobel laureates (*10*).

In the stratification system of honor in science, there may also be a "ratchet effect" (*11*) operating in the careers of scientists such that, once having achieved a particular degree of eminence, they do not later fall much below that level (although they may be outdistanced by newcomers and so suffer a *relative* decline in prestige). Once a Nobel laureate, always a Nobel laureate. Yet the reward system based on recognition for work accomplished tends to induce continued effort, which serves both to validate the judgment that the scientist has unusual capacities and to testify that these capacities have continuing potential. What appears from below to be the summit becomes, in the experience of those who have reached it, only another way station. The scientist's peers and other associates regard each of his scientific achievements as only the prelude to new and greater achievements. Such social pressures do not often permit those who have climbed the rugged mountains of scientific achievement to remain content. It is not necessarily the fact that their own Faustian aspirations are ever escalating that keeps eminent scientists at work. More and more is expected of them, and this creates its own measure of motivation and stress. Less often than might be imagined is there repose at the top in science (see *12*).

The recognition accorded scientific achievement by the scientist's peers is a reward in the strict sense identified by Parsons (*13*). As we shall see, such recognition can be converted into an instrumental asset as enlarged facilities are made available to the honored scientist for further work. Without deliberate intent on the part of any group, the reward system thus influences the "class structure" of science by providing a stratified distribution of chances, among scientists, for enlarging their role as investigators. The process provides differential access to the means of scientific production. This becomes all the more important in the current historical shift from little science to big science, with its expensive and often centralized equipment needed for research. There is thus a continuing interplay between the status system, based on honor and esteem, and the class system, based on differential life-chances, which locates scientists in differing positions within the opportunity structure of science (*14*).

The Matthew Effect in the Reward System

The social structure of science provides the context for this inquiry into a complex psychosocial process that affects both the reward system and the communication system of science. We start by noting a theme that runs through the interviews with the Nobel laureates. They repeatedly observe that eminent scientists get disproportionately great credit for their contributions to science while relatively unknown scientists tend to get disproportionately little credit for comparable contributions. As one laureate in physics put it (*15*): "The world is peculiar in this matter of how it gives credit. It tends to give the credit to [already] famous people."

As we examine the experiences reported by eminent scientists we find that this pattern of recognition, skewed in favor of the established scientist, appears principally (i) in cases of collaboration and (ii) in cases of independent multiple discoveries made by scientists of distinctly different rank (*16*).

In papers coauthored by men of decidedly unequal reputation, another laureate in physics reports, "the man who's best known gets more credit, an inordinate amount of credit." In the words of a laureate in chemistry: "When people see my name on a paper, they are apt to remember *it* and not to remember the other names." And a laureate in physiology and medicine describes his own pattern of response to jointly authored papers:

> You usually notice the name that you're familiar with. Even if it's last, it will be the one that sticks. In some cases, all the names are unfamiliar to you, and they're virtually anonymous. But what you note is the acknowledgement at the end of the paper to the senior person for his "advice and encouragement." So you will say: "This came out of Greene's lab, or so-and-so's lab." You remember that, rather than the long list of authors.

Almost as though he had been listening to this account, another laureate in medicine explains why he will often not put his name on the published report of a collaborative piece of work: "People are more or less tempted to say: 'Oh yes, so-and-so is working on such-and-such in C's laboratory. It's C's idea.' I try to cut that down." Still another laureate in medicine alludes to this pattern and goes on to observe how it might prejudice the career of the junior investigator:

> If someone is being considered for a job by people who have not had much experience with him, if he has published only together with some known names—well, it detracts. It naturally makes people ask: "How much is really his own contribution, how much [the senior author's]. How will he work out once he goes out of that laboratory?"

Under certain conditions this adverse effect on recognition of the junior author of papers written in collaboration with prominent scientists can apparently be countered and even converted into an asset. Should the younger scientist move ahead to do autonomous and significant work, this work *retroactively* affects the appraisals of his role in earlier collaboration. In the words of the laureate in medicine who referred to the virtual anonymity of junior authors of coauthored papers: "People who have been identified with such joint work and who then go on to do good work later on, [do] get the proper amount of recognition." Indeed, as another laureate implies, this retroactive judgment may actually heighten recognition for later accomplishments: "The junior person is sometimes lost sight of, but only temporarily *if* he continues. In many cases, he actually gains in acceptance of his work and in general acceptance, by having once had such association." Awareness of this pattern of retroactive recognition may account in part for the preference, described by another laureate of some "young fellows [who] feel that to have a better-known name on the paper will be of help to them." But this is an expressive as well as a merely instrumental preference, as we see also in the pride with which laureates themselves speak of having worked, say, with Fermi, G. N. Lewis, Meyerhof, or Niels Bohr.

So much for the misallocation of credit in this reward system in the case of collaborative work. Such misallocation also occurs in the case of independent multiple discoveries. When approximately the same ideas or findings are independently communicated by a scientist of great repute and by one not yet widely known, it is the first, we are told, who ordinarily receives prime recognition. An approximation to this pattern is reported by a laureate who observes:

> It does happen that two men have the same idea and one becomes better known for it. E__, who had the idea, went circling round to try to get an experiment for. . . . Nobody would do it and so it was forgotten, practically. Finally, A__ and B__ and C__ did it, became famous, and got the Nobel Prize. . . . If things had gone just a little differently; if somebody had been willing to try the experiment when E__ suggested it, they probably could have published it jointly and he would have been a famous man. As it is, he's a footnote.

The workings of this process at the expense of the young scientist and to the benefit of the famous one is remarkably summarized in the life history of a laureate in physics, who has experienced both phases at different times in his career.

> When you're not recognized, he recalls, it's a little bit irritating to have somebody come along and figure out the obvious which you've also figured out, and everybody gives him credit just because he's a famous physicist or a famous man in his field.

Here he is viewing the case he reports from the perspective of one who had this happen to him before he had become famous. The conversation takes a new turn as he notes that his own position has greatly changed. Shifting from the perspective of his earlier days, when he felt victimized by the pattern, to the perspective of his present high status, he goes on to say:

> This often happens, and I'm probably getting credit now, if I don't watch myself, for things other people figured out. Because I'm notorious and when I say it, people say: "Well, he's the one that thought this out." Well, I may just be saying things that other people have thought out before.

In the end, then, a sort of rough-hewn justice has been done by the compounding of two compensating injustices. His earlier accomplishments have been underestimated; his later ones, overestimated (*17*).

This complex pattern of the misallocation of credit for scientific work must quite evidently be described as "the Matthew effect," for, as will be remembered, the Gospel According to St. Matthew puts it this way:

> For unto every one 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.

Put in less stately language, the Matthew effect consists in the accruing of greater increments of recognition for particular scientific contributions to scientists of considerable repute and the withholding of such recognition from scientists who have not yet made their mark. Nobel laureates provide presumptive evidence of the effect, since they testify to its occurrence, not as victims—which might make their testimony suspect—but as unwitting beneficiaries.

The laureates and other eminent men of science are sufficiently aware of this aspect of the Matthew effect to make special efforts to counteract it. At the extreme, they sometimes refuse to coauthor a paper reporting research on which they have collaborated in order not to diminish the recognition accorded their less-well-known associates. And, as Harriet Zuckerman has found (*18*), they tend to give first place in jointly authored papers to one of their collaborators. She discovered, moreover, that the laureates who have attained eminence before receiving the Nobel prize begin to transfer first-authorship to associates earlier than less eminent laureates-to-be do, and that both sets of laureates—the previously eminent and not-so-eminent—greatly increase this practice *after* receiving the prize. Yet the latter effort is probably more expressive of the laureates' good intentions than it is effective in redressing the imbalance of credit attributable to the Matthew effect. As the laureate quoted by Harriet Zuckerman acknowledges: "If I publish my name first, then everyone thinks the others are just technicians. . . . If my name is last, people will credit me anyway for the whole thing, so I want the others to have a bit more glory."

The problem of achieving a public identity in science may be deepened by the great increase in the number of papers with several authors (*1*, chap. 3; *19; 20*, p. 87) in which the role of young collaborators becomes obscured by the brilliance that surrounds their illustrious co-authors. So great is this problem that we are tempted to turn again to the Scriptures to designate the status-enhancement and status-suppression components of the Matthew effect. We can describe it as "the Ecclesiasticus component," from the familiar injunction "Let us now praise famous men," in the noncanonical book of that name.

It will surely have been noted that the laureates perceive the Matthew effect primarily as a problem in the just allocation of credit for scientific accomplishment. They see it largely in terms of its action in enhancing rank or suppressing recognition. They see it as leading to an unintended double injustice, in which unknown scientists are unjustifiably victimized and famous ones, unjustifiably benefited. In short, they see the Matthew effect in terms of a basic inequity in the reward system that affects the careers of individual scientists. But it has other implications for the development of science, and we must shift our angle of theoretical vision in order to identify them.

The Matthew Effect in the Communication System

We now look at the same social phenomena from another perspective—not from the standpoint of individual careers and the workings of the reward system but from the standpoint of science conceived of as a system of communication. This perspective yields a further set of inferences. It leads us to propose the hypothesis that a scientific contribution will have greater visibility in the community of scientists when it is introduced by a scientist of high rank than when it is introduced by one who has not yet made his mark. In other words, considered in its implications for the reward system, the Matthew effect is dysfunctional for the careers of individual scientists who are penalized in the early stages of their development, but considered in its implications for the communication system, the Matthew effect, in cases of collaboration and multiple discoveries, may operate to heighten the visibility of new scientific communications. This is not the first instance of a social pattern's being functional for certain aspects of a social system and dysfunctional for certain individuals within that system. That, indeed, is a principal theme of classical tragedy (*21*).

Several laureates have sensed this social function of the Matthew effect. Speaking of the dilemma that confronts the famous man of science who directs the work of a junior associate, one of them observes:

> It raises the question of what you are to do. You have a student; should you put your name on that paper or not? You've contributed to it, but is it better that you shouldn't or should? There are two sides to it. If you don't [and here comes the decisive point on visibility], if you don't, there's the possibility that the paper may go quite unrecognized. Nobody reads it. If you do, it might be recognized, but then the student doesn't get enough credit.

Studies of the reading practices of scientists indicate that the suggested possibility—"Nobody reads it"—is something less than sheer hyperbole. It has been found, for example, that only about half of 1 percent of the articles published in journals of chemistry are read by any one chemist (*22*). And much the same pattern has been found to hold in psychology (*23*, p. 9):

> The data on current readership (i.e., within a couple [of] months after distribution of the journal) suggested that about one-half of the research reports in "core" journals will be read [or skimmed] by 1% or less of a random sample of psychologists. At the highest end of the current readership distribution, no research report is likely to be read by more than about 7% of such a sample.

Several of the Coles's findings (*24*) bear tangentially on the hypothesis about the communication function of the Matthew effect. The evidence is tangential rather than central to the hypothesis since their data deal with the degree of visibility of the entire corpus of each physicist's work in the national community of physicists rather than with the visibility of particular papers within it. Still, in gross terms, their findings are at least consistent with the hypothesis. The higher the rank of physicists (as measured by the prestige of the awards they have received for scientific work), the higher their visibility in the national community of physicists. Nobel laureates have a visibility score (*25*) of 85; other members of the National Academy of Sciences, a score of 72; recipients of awards having less prestige, a score of 38; and physicists who have received no awards, a visibility score of 17. The Coles also find (*24*) that the visibility of physicists producing work of high quality is heightened by their attaining honorific awards more prestigious than those they have previously received. Further investigation is needed to discover whether these same patterns hold for differences in the visibility (as measured by readership) of individual papers published by scientists of differing rank.

There is reason to assume that the communication function of the Matthew effect is increasing in frequency and intensity with the exponential increase (*20*, chaps. 1 and 2; *26*) in the volume of scientific publications, which makes it increasingly difficult for scientists to keep up with work in their field. Bentley Glass (*27*) is only one among many to conclude that "perhaps no problem facing the individual scientist today is more defeating than the effort to cope with the flood of published scientific research, even within one's own narrow specialty." Studies of the communication behavior of scientists (*28*) have shown that, confronted with the growing task of identifying significant work published in their field, scientists search for cues to what they should attend to. One such cue is the professional reputation of the authors. The problem of locating the pertinent research literature and the problem of authors' wanting their work to be noticed and used are symmetrical: the vastly increased bulk of publication stiffens the competition between papers for such notice. The American Psychological Association study (*23*, pp. 252, 254; *29*) found that from 15 to 23 percent of the psychologist-readers' behaviors in selecting articles were based on the identity of the authors.

The workings of the Matthew effect in the communication system require us to draw out and emphasize certain implications about the character of science. They remind us that science is not composed of a series of private experiences of discovery by many scientists, as sometimes seems to be assumed in inquiries centered exclusively on the psychological processes involved in discovery. Science is public, not private. True, the making of a discovery is a complex personal experience. And since the *making* of the discovery necessarily precedes its fate, the nature of the experience is the same whether the discovery temporarily fails to become part of the socially shared culture of science or quickly becomes a functionally significant part of that culture. But, for science to be advanced, it is not enough that fruitful ideas be originated or new experiments developed or new problems formulated or new methods instituted. The innovations must be effectively communicated to others. That, after all, is what we mean by a *contribution* to science—something given to the common fund of knowledge. In the end, then, science is a socially shared and socially validated body of knowledge. For the development of science, only work that is effectively perceived and utilized by other scientists, then and there, matters.

In investigating the processes that shape the development of science, it is therefore important to consider the social mechanisms that curb or facilitate the incorporation of would-be contributions into the domain of science. Looking at the Matthew effect from this perspective, we have noted the distinct possibility that contributions made by scientists of considerable standing are the most likely to enter promptly and widely into the communication networks of science, and so to accelerate its development.

The Matthew Effect and the Functions of Redundancy

Construed in this way, the Matthew effect links up with my previous studies of the functions of redundancy in science (*30*). When similar discoveries are made by two or more scientists working independently ("multiple discoveries"), the probability that they will be promply incorporated into the current body of scientific knowledge is increased. The more often a discovery has been made independently, the better are its prospects of being identified and used. If one published version of the discovery is obscured by "noise" in the communication system of science, then another version may become visible. This leaves us with an unresolved question: How can one estimate what amount of redundancy in independent efforts to solve a scientific problem will give maximum probability of solution without entailing so much replication of effort that the last increments will not appreciably increase the probability? (See *31*.)

In examining the functions of the Matthew effect for communication in science, we can now refine this conception further. It is not only the number of times a discovery has been independently made and published that affects its visibility but also the standing, within the stratification system of science, of the scientists who have made it. To put the matter with undue simplicity, a single discovery introduced by a scientist of established reputation may have as good a chance of achieving high visibility as a multiple discovery variously introduced by several scientists no one of whom has yet achieved a substantial reputation. Although the general idea is, at this writing, tentative, it does have the not inconsiderable virtue of lending itself to approximate test. One can examine citation indexes to find whether in multiple discoveries by scientists of markedly unequal rank it is indeed the case that work published by the scientists of higher rank is the more promptly and more widely cited (*32*). To the extent that it is, the findings will shed some light on the unplanned consequences of the stratification system for the development of science. Interviews with working scientists about their reading practices can also supply data bearing on the hypothesis.

So much for the link between the Matthew effect and the functions of multiple discoveries in increasing both the probability and the speed of diffusion of significant new contributions to science. The Matthew effect also links up with the finding, reported elsewhere (*33*), that great talents in science are typically involved in many multiple discoveries. This statement holds for Galileo and Newton: for Faraday and Clerk Maxwell; for Hooke, Cavendish, and Stensen; for Gauss and Laplace; for Lavoisier, Priestley, and Scheele; *and* for most Nobel laureates. It holds, in short, for all those whose place in the pantheon of science is largely assured, however much they may differ in the scale of their total accomplishment.

The greatness of these scientists rests in their having *individually* contributed a body of ideas, methods, and results which, in the case of multiple discoveries, has also been contributed by a sizable *aggregate* of less talented men. For example, we have found that Kelvin had a part in 32 or more multiple discoveries, and that it took 30 other men to contribute what Kelvin himself contributed.

By examining the interviews with the laureates, we can now detect some underlying psychosocial mechanisms that make for the greater visibility of contributions reported by scientists of established reputation. This greater visibility is not merely the result of a halo effect such that their personal prestige rubs off on their separate contributions. Rather, certain aspects of their socialization, their scheme of values, and their social character account in part for the visibility of their work.

Social and Psychological Bases of the Matthew Effect

Even when some of his contributions have been independently made by an aggregate of other scientists, the great man of science serves distinctive functions. It makes a difference, and often a decisive difference, for the advancement of science whether a composite of ideas and findings is heavily concentrated in the work of one man or one research group or is thinly dispersed among a great number of men and organizations. Such a composite tends to take on a structure sooner in the first instance than in the second. It required a Freud, for instance, to focus the attention of many psychologists upon a wide array of ideas which, as has been shown elsewhere (*30*), had in large part also been hit upon by various other scientists. Such focalizing may turn out to be a distinctive function of eminent men of science (*34*).

A Freud, a Fermi, and a Delbrück play a charismatic role in science. They excite intellectual enthusiasm among others who ascribe exceptional qualities to them. Not only do they themselves achieve excellence, they have the capacity for evoking excellence in others. In the compelling phrase of one laureate, they provide a "bright ambiance." It is not so much that these great men of science pass on their techniques, methods, information, and theory to novices working with them. More consequentially, they convey to their associates the norms and values that govern significant research. Often in their later years, or after their death, this personal influence becomes routinized, in the fashion described by Max Weber for other fields of human activity. Charisma becomes institutionalized, in the form of schools of thought and research establishments.

The role of outstanding men of science in influencing younger associates is repeatedly emphasized in the interviews with laureates. Almost to a man they lay great emphasis on the importance of problem-*finding*, not only problem-solving. They uniformly express the strong conviction that what matters most in their work is a developing sense of taste, of judgment, in seizing upon problems that are of fundamental importance. And, typically, they report that they acquired this sense for the significant problem during their years of training in evocative environments. Reflecting on his years as a novice in the laboratory of a chemist of the first rank, one laureate reports that he "led me to look for important things, whenever possible, rather than to work on endless detail or to work just to improve accuracy rather than making a basic new contribution." Another describes his socialization in a European laboratory as "my first real contact with first-rate creative minds at the high point of their power. I acquired a certain expansion of taste. It was a matter of taste and attitude and, to a certain extent, real self-confidence. I learned that it was just as difficult to do an unimportant experiment, often more difficult, than an important one."

There is one rough measure of the extent to which the laureates were trained and influenced in particularly creative research environments—the number of laureates each worked under in earlier years. Of 55 American laureates, 34 worked in some capacity, as young men, under a total of 46 Nobel prize winners (*35*). But apparently it is not only the experience of the laureates (and, presumably, other outstanding men of science) in these environments that accounts for their tendency to focus on significant problems and so to affect the communication function of the Matthew effect. Certain aspects of their character also play a part. With few exceptions, these are men of exceptional ego strength. Their self-assurance finds varied expression within the context of science as a social institution. That institution, as we know, includes a norm calling for autonomous and critical judgment about one's own work and the work of others. With their own tendencies reinforced by such norms, the laureates exhibit a distinct self-confidence (which, at the extreme, can be loosely described as attractive arrogance). They exhibit a great capacity to tolerate frustration in their work, absorbing repeated failures without manifest psychological damage. One laureate alluded to this capacity while taking note of the value of psychological support by colleagues:

> Research is a rough game. You may work for months, or even a few years, and seemingly you are getting nowhere. It gets pretty dark at times. Then, all of a sudden, you get a break. It's good to have somebody around to give a bit of encouragement when it's needed.

Though attentive to the cues provided by the work of others in their field, the Nobelists are self-directed men, moving confidently into new fields of inquiry once they are persuaded that a previous one has been substantially mined. In these activities they display a high degree of venturesome fortitude. They are prepared to tackle important though difficult problems rather than settle for easy and secure ones. Thus, a laureate recalls having been given, early in his career, "a problem about which there was no risk. All I had to do was to analyze [the chemical composition of certain materials]. You could not fail because the method was well established. But I knew I was going to work on the t—— instead and the whole thing would have to be created because nothing was known about it." He then went on to make one of his prime contributions in the more risky field of investigation (*36*).

This marked ego strength links up with these scientists' selection of important problems in at least two ways. Being convinced that they will recognize an important problem when they encounter it, they are willing to bide their time and not settle too soon for a prolonged commitment to a comparatively unimportant one. Their capacity for delayed gratification, coupled with self-assurance, leads to a conviction that an important problem will come along in due course and that, when it does, their acquired sense of taste will enable them to recognize it and handle it. As we have seen, this attitude has been reinforced by their early experience in creative environments. There, association with eminent scientists has demonstrated to the talented novice, as didactic teaching never could, that he can set his sights high and still cope with the problem he chooses. Emulation is reinforced by observing successful, though often delayed, outcomes. Indeed, the idiom of the laureates reflects this orientation. They like to speak of the big problems and the fundamental ones, the important problems and the beautiful ones. These they distinguish from the pedestrian work in which they engage while waiting for the next big problem to come their way. As a result of all this, their papers are apt to have the kind of scientific significance that makes an impact, and other scientists tend to single out their papers for special attention.

The character structure of these leading scientists may contribute to the communication aspect of the Matthew effect in still another way, which has to do with their mode of presenting their scientific work. Confident in their powers of discriminating judgment—a confidence that has been confirmed by the responses of others to their previous work—they tend, in their exposition, to emphasize and develop the central ideas and findings and to play down peripheral ones. This serves to highlight the significance of their contributions, raising them out of the stream of publications by scientists having less socially-validated self-esteem, who more often employ routine exposition.

Finally, this character structure and an acquired set of high standards often lead these outstanding scientists to discriminate between work that is worth publishing and that which, in their candid judgment, is best left unpublished though it could easily find its way into print. The laureates and other scientists of stature often report scrapping research papers that simply did not measure up to their own demanding standards or to those of their collegues (*37*). Seymour Benzer, for example, tells of how he was saved from going "down the biochemical drain": "Delbrück saved me, when he wrote to my wife to tell me to stop writing so many papers. And I did stop" (*38*). And a referee's incisive report on a manuscript sent to a journal of physics asserts a relevant consequence of a scientist's failure to exercise rigorous judgment in deciding whether to publish or not to publish: "If C—— would write fewer papers, more people would read them." Outstanding scientists tend to develop an immunity to *insanabile scribendi cacoethes* (the itch to publish) (*39*). Since they prefer their published work to be significant and fruitful rather than merely extensive, their contributions are apt to matter. This in turn reinforces the expectations of their fellow scientists that what these eminent scientists publish (at least during their most productive period) will be worth close attention (*40*). Once again this makes for operation of the Matthew effect, as scientists focus on the output of men whose outstanding positions in science have been socially validated by judgments of the average quality of their past work. And the more closely the other scientists attend to this work, the more they are likely to learn from it and the more discriminating their response is apt to be (*41*).

For all these reasons, cognitive material presented by an outstanding scientist may have greater stimulus value than roughly the same kind of material presented by an obscure one—a principle which provides a sociopsychological basis for the communication function of the Matthew effect. This principle represents a special application of the self-fulfilling prophecy (*42*), somewhat as follows: Fermi or Pauling or G. N. Lewis or Weisskopf see fit to report this in print and so it is apt to be important (since, with some consistency, they have made important contributions in the past); since it is probably important, it should be read with special care; and the more attention one gives it, the more one is apt to get out of it. This becomes a self-confirming process, making for the greater evocative effect of publications by eminent men of science (until that time, of course, when their image among their fellow scientists is one of men who have seen their best days—an image, incidentally, that corresponds with the self-image of certain laureates who find themselves outpaced by onrushing generations of new men).

Like other self-fulfilling prophecies, this one becomes dysfunctional under certain conditions. For although eminent scientists may be more *likely* to make significant contributions, they are obviously not alone in making them. After all, scientists do not begin by being eminent (though the careers of men such as Mössbauer and Watson may sometimes give us that mistaken impression). The history of science abounds in instances of basic papers' having been written by comparatively unknown scientists, only to be neglected for years. Consider the case of Waterston, whose classic paper on molecular velocity was rejected by the Royal Society as "nothing but nonsense"; or of Mendel, who, deeply disappointed by the lack of response to his historic papers on heredity, refused to publish the results of his further research; or of Fourier, whose classic paper on the propagation of heat had to wait 13 years before being finally published by the French Academy (*43*).

Barber (*44*) has noted how the slight professional standing of certain scientists has on occasion led to some of their work, later acknowledged as significant, being refused publication altogether. And, correlatively, an experience of Lord Rayleigh's (*45*) provides an example in which an appraisal of a paper was reversed once its eminent authorship became known. Rayleigh's name "was either omitted or accidentally detached [from a manuscript], and the Committee [of the British Association for the Advancement of Science] 'turned it down' as the work of one of those curious persons called paradoxers. However, when the authorship was discovered, the paper was found to have merits after all."

When the Matthew effect is thus transformed into an idol of authority, it violates the norm of universalism embodied in the institution of science and curbs the advancement of knowledge. But next to nothing is known about the frequency with which these practices are adopted by the editors and referees of scientific journals and by other gatekeepers of science. This aspect of the workings of the institution of science remains largely a matter of anecdote and heavily motivated gossip.

The Matthew Effect and Allocation of Scientific Resources

One institutional version of the Matthew effect, apart from its role in the reward and communication systems of science, requires at least short review. This is expressed in the principle of cumulative advantage that operates in many systems of social stratification to produce the same result: the rich get richer at a rate that makes the poor become relatively poorer (*46*). Thus, centers of demonstrated scientific excellence are allocated far larger resources for investigation than centers which have yet to make their mark (*47*). In turn, their prestige attracts a disproportionate share of the truly promising graduate students (*48*). This disparity is found to be especially marked at the extremes (*49*): six universities (Harvard, Berkeley, Columbia, Princeton, California Institute of Technology, and Chicago) which produced 22 percent of the doctorates in the physical and biological sciences produced fully 69 percent of the Ph.D.'s who later became Nobel laureates. Moreover, the 12 leading universities manage to identify early, and to retain on their faculties, these scientists of exceptional talent: they keep 70 percent of the future laureates in comparison with only 28 percent of the other Ph.D.'s they have trained. And finally, "the top twelve [universities] are much more apt to recruit future laureates who received degrees from other American universities than they are other recipients of the doctorate; half the laureates who were trained outside the top twelve and who worked in a university moved into the top twelve but only six percent of the sample of doctoral recipients did so."

These social processes of social selection that deepen the concentration of top scientific talent create extreme difficulties for any efforts to counteract the institutional consequences of the Matthew principle in order to produce new centers of scientific excellence.

Summary

This account of the Matthew effect is another small exercise in the psychosociological analysis of the workings of science as a social institution. The initial problem is transformed by a shift in theoretical perspective. As originally identified, the Matthew effect was construed in terms of enhancement of the position of already eminent scientists who are given disproportionate credit in cases of collaboration or of independent multiple discoveries. Its significance was thus confined to its implications for the reward system of science. By shifting the angle of vision, we note other possible kinds of consequences, this time for the communication system of science. The Matthew effect may serve to heighten the visibility of contributions to science by scientists of acknowledged standing and to reduce the visibility of contributions by authors who are less well known. We examine the psychosocial conditions and mechanisms underlying this effect and find a correlation between the redundancy function of multiple discoveries and the focalizing function of eminent men of science—a function which is reinforced by the great value these men place upon finding basic problems and by their self-assurance. This self-assurance, which is partly inherent, partly the result of experiences and associations in creative scientific environments, and partly a result of later social validation of their position, encourages them to search out risky but important problems and to highlight the results of their inquiry. A macrosocial version of the Matthew principle is apparently involved in those processes of social selection that currently lead to the concentration of scientific resources and talent (*50*).

References and Notes

1. The methods of obtaining these tape-recorded interviews and the character of their substance are described in H. A. Zuckerman, thesis, Columbia University, 1965.
2. R. K. Merton, *Amer. Sociol. Rev.* **22**, 635 (1957).
3. B. G. Glaser, *Organizational Scientists: Their Professional Careers* (Bobbs-Merrill, Indianapolis, 1964).
4. D. Crane, *Amer. Sociol. Rev.* **30**, 699 (1965).
5. W. O. Hagstrom, *The Scientific Community* (Basic Books, New York, 1965), chap. 1.
6. N. W. Storer, *The Social System of Science* (Holt, Rinehart and Winston, New York, 1966), p. 106; see also *ibid.*, pp. 20–26, 103–106.
7. H. A. Zuckerman, thesis, Columbia University, 1965.
8. S. Cole and J. R. Cole, *Amer. Sociol. Rev.* **32**, 377 (1967).
9. I have adopted this term for the general phenomenon from the monograph on the French Academy by Arsene Houssaye, *Histoire du 41^{me} Fauteuil de l'Académie Française* (Paris, 1886).
10. This partial list of men who have done

work of "prize-winning calibre" is derived from *Nobel: The Man and His Prizes* (Elsevier, London, 1962), an official publication of the Nobel prize-granting academy and institute, Nobelstiftelsen.

11. I am indebted to Marshall Childs for suggesting that this term, introduced into economics by James S. Duesenberry in quite another connection, could aptly refer to this pattern in the cumulation of prestige for successive accomplishments. For its use in economics, see Duesenberry, *Income, Savings, and the Theory of Consumer Behavior* (Harvard Univ. Press, Cambridge, Mass., 1949), pp. 114–16.
12. This process of a *socially reinforced* rise in aspirations, as distinct from Durkheim's concept of the "insatiability of wants," is examined by R. K. Merton in *Anomie and Deviant Behavior*, M. Clinard, Ed. (Free Press, New York, 1964), pp. 213–242.
13. T. Parsons, *The Social System* (Free Press, New York, 1951), p. 127.
14. Max Weber touches upon the convertibility of position in distinct systems of stratification in his classic essay "Class, Status, Party" [*From Max Weber: Essays in Sociology*, H. H. Gerth and C. Wright Mills, Eds. (Oxford Univ. Press, New York, 1946)].
15. The laureates are not alone in noting that prominent scientists tend to get the lion's share of credit; similar observations were made by less eminent scientists in the sample studied by Hagstrom (see *5*, pp. 24, 25).
16. A third case can be inferred from the protocols of interviews, in which the view is stated that, had a paper written by a comparatively unknown scientist been presented instead by an eminent scientist, it would have had a better chance of being published and of receiving respectful attention. Systematic information about such cases is too sparse for detailed study.
17. This compensatory pattern can only obtain, of course, among scientists who ultimately achieve recognition with its associated further rewards. But, as with all systems of social stratification involving differentials in life-chances, there remains the question of the extent to which talent among individuals in the deprived strata has gone unrecognized and undeveloped, and its fruits lost to society. More specifically, we have yet to discover whether or not the channels of mobility are equally open to talent in various institutional realms. Does contemporary science afford greater or less opportunity than art, politics, the practicing professions, or religion for the recognition of talent, whatever its social origins?
18. H. Zuckerman, "Patterns of name-ordering among authors of scientific papers: a study of social symbolism and its ambiguity," paper read before the American Sociological Association, August 1967. Dr. Zuckerman will not demean herself to give these practices their predestined tag, but I shall: plainly, these are instances of *Nobelesse oblige*.
19. B. Berelson, *Graduate Education in the United States* (McGraw-Hill, New York, 1960), p. 55.
20. D. J. deSolla Price, *Little Science, Big Science* (Columbia Univ. Press, New York, 1963).
21. This pattern of social functions and individual dysfunctions is at variance with the vigorous and untutored optimism unforgettably expressed by Adam Smith, who speaks of "a harmonious order of nature, under divine guidance, which promotes the welfare of man through the operation of his individual propensities." If only it were that simple. One of the prime problems for sociological theory is that of identifying the special conditions under which men's propensities and the requirements of the social system are in sufficient accord to be functional for both individuals and the social system.
22. R. L. Ackoff and M. H. Halbert, *An Operations Research Study of the Scientific Activity of Chemists* (Case Institute of Technology Operations Research Group, Cleveland, 1958).
23. *Project on Scientific Information Exchange in Psychology* (American Psychological Association, Washington, D.C., 1963), vol. 1.
24. S. Cole and J. R. Cole, "Visibility and the structural bases of observability in science," paper presented before the American Sociological Association, August 1967.
25. In the Coles's study (*24*), the term *visibility scores* refers to percentages in a sample of more than 1300 American physicists who indicated that they were familiar with the work of a designated list of 120 physicists. The study includes checks on the validity of these visibility scores.
26. D. J. deSolla Price has noted that "all crude measures, however arrived at, show to a first approximation that science increases exponentially, at a compound interest of about 7 per cent per annum, thus doubling in size every 10–15 years, growing by a factor of 10 every half-century, and by something like a factor of a million in the 300 years which separate us from the seventeenth-century invention of the scientific paper when the process began" [*Nature* **206**, 233 (1965), pp. 233–238].
27. B. Glass, *Science* **121**, 583 (1955).
28. See, for example, H. Menzel, in *Communication: Concepts and Perspectives*, L. Thayer, Ed. (Spartan Books, Washington, D.C., 1966), pp. 279–295; ———, *Amer. Psychologist* **21**, 999 (1966). See also S. Herner [*Science* **128**, 9 (1958)], who notes that "one of the greatest stimulants to the use of information is familiarity with its source"; S. Herner, *Ind. Eng. Chem.* **46**, 228 (1954).
29. Future investigations will require more detailed data on the actual processes of selecting scientific papers for varying kinds of "reading" and "skimming." But the data now available are at least suggestive.
30. On the concept of functional redundancy as distinct from "wasteful duplication" in scientific research, see R. K. Merton, *European J. Sociol.* **4**, 237 (1963).
31. One of the laureates questioned the ready assumption that redundancy of research effort necessarily means "wasteful duplication": "One often hears, especially when large amounts of money are involved, that duplication of effort should be avoided, that this is not an efficient way of doing things. I think that most of the time, in respect to research, duplication of effort is a good thing. I think that if there are different groups in different laboratories working on the same thing, their approach is sufficiently different [to increase the probability of a successful outcome]. On the whole, this is a good thing and not something that should be avoided for the sake of efficiency."
32. So far as I know, no investigation has yet been carried out on precisely this question. At best suggestive is the peripheral evidence that papers of Nobel laureates-to-be were cited 30 times more often in the 5 years before their authors were awarded the prize than were the papers of the average author appearing in the Citation Index during the same period. See I. H. Sher and E. Garfield, "New tools for improving the effectiveness of research," paper presented at the 2nd Conference on Research Program Effectiveness, Washington, D.C., July 1965; H. Zuckerman, *Sci. Amer.* **217**, 25 (1967).
33. R. K. Merton, *Proc. Amer. Phil. Soc.* **105**, 470 (1961).
34. Later in this discussion, I consider the dysfunctions associated with these functions of great men of science. Idols of the cave often continue to wield great influence even though the norms of science call for the systematic questioning of mere authority. Here, as in other institutional spheres, the problem is one of accounting for patterns of coincidence and discrepancy between social norms and actual behavior.
35. H. Zuckerman, *Amer. Sociol. Rev.* **32**, 391 (1967).
36. Germane results in experimental psychology show that preferences for riskier work but more significant outcomes are related both to high motivation for achievement and to a capacity for accepting delay in gratification. See, for example, W. Mischel, *J. Abnormal Soc. Psychol.* **62**, 543 (1961).
37. To this extent, they engage in the kind of behavior ascribed to physicists of the "perfectionist" type, who have been statistically identified by the Coles (*8*) as those who publish less than they might but whose publications nevertheless have a considerable impact on the field, as indicated by citations. It is significant that this type of physicist was accorded more recognition in the form of awards for scientific work than any other types (including the "prolific" and the "mass producer" types).
38. S. Benzer, in *Phage and the Origins of Molecular Biology*, J. Cairns, G. S. Stent, J. D. Watson, Eds. (Cold Spring Harbor Laboratory of Quantitative Biology, Cold Spring Harbor, N.Y., 1966), p. 165. This *Festschrift* clearly shows that Delbrück is one of those scientists who generally exercise this kind of demanding judgment on the publication of their own work and that of their associates.
39. For some observations on the prophylaxis for this disease, see R. K. Merton, *On the Shoulders of Giants* (Harcourt, Brace and World, New York, 1967), pp. 83–85.
40. It has been noted [G. Williams, *Virus Hunters* (Knopf, New York, 1959)] that the early confidence of scientists in the measles vaccine was a "paradoxical feedback of [Enders's] own scientific insistence, not on believing, but on doubting. His fellow scientists trust John Enders not to go overboard on anything."
41. This remains a moot conclusion. Hovland's experiments with laymen have shown that the *same* communications are considered less biased when attributed to sources of high rather than low credibility [C. I. Hovland, *Amer. Psychologist* **14**, 8 (1959)]. In an earlier study, Hovland and his associates found that, in the case of *factual* communications, there is "equally good learning of what was said regardless of the credibility of the communicator" [C. I. Hovland, I. L. Janis, H. H. Kelley, *Communication and Persuasion* (Yale Univ. Press, New Haven, Conn., 1953), p. 270].
42. For an analysis of the self-fulfilling prophecy, see R. K. Merton, *Antioch Rev.* **1948**, 596 (Summer 1948), reprinted in ———, *Social Theory and Social Structure* (Free Press, New York, 1957), pp. 421–436.
43. See R. K. Merton (*1*), who cites the following: R. H. Murray, *Science and Scientists in the Nineteenth Century* (Sheldon, London, 1925), pp. 346—348; D. L. Watson, *Scientists are Human* (Watts, London, 1938), pp. 58, 80; R. J. Strutt (Baron Rayleigh), *John William Strutt, Third Baron Rayleigh* (Arnold, London, 1924), pp. 169—171.
44. B. Barber, *Science* **134**, 596 (1961), reprinted in ——— and W. Hirsch, Eds., *The Sociology of Science* (Free Press, New York, 1962), pp. 539–556.
45. Quoted by Barber (*44*) from R. J. Strutt, *John William Strutt, Third Baron Rayleigh* (Arnold, London, 1924).
46. Derek Price perceived this implication of the Matthew principle [*Nature* **206**, 233 (1965)].
47. D. S. Greenberg, *Saturday Rev.* (4 November 1967), p. 62; R. B. Barber, in *The Politics of Research* (Public Affairs Press, Washington, D.C., 1966), p. 63, notes that "in 1962, 38 per cent of all federal support went to just ten institutions and 59 per cent to just 25." See also H. Orlans, *The Effects of Federal Programs on Higher Education* (Brookings Institution, Washington, D.C., 1962).
48. Thus, Allan M. Cartter reports that, in 1960—63, 86 percent of (regular) National Science Foundation Fellows and 82 percent of Woodrow Wilson Fellows free to choose their place of study elected to study in one or another of the 25 leading universities (as rated in terms of the quality of their graduate faculties) [A. M. Cartter, *An Assessment of Quality in Graduate Education* (American Council on Education, Washington, D.C., 1966), p. 108].
49. For this and other detailed information on the career patterns of laureates, see H. Zuckerman (*1, 32*).
50. Chancing to come upon the manuscript of this paper, Richard L. Russell, a molecular biologist of more than passing acquaintance, has informed me that a well-known textbook in organic chemistry [L. F. Fieser and M. Fieser, *Introduction to Organic Chemistry* (Heath, Boston, 1957)] refers to the "empirical rule due to Saytzeff (1875) that in dehydration of alcohols, hydrogen is eliminated preferentially from the adjacent carbon atom that is poorer in hydrogen." What makes the rule germane to this discussion is the accompanying footnote: "MATTHEW, XXV, 29, '. . . but from him that hath not shall be taken away even that which he hath.'" Evidently the Matthew effect transcends the world of human behavior and social process.
51. Earlier versions of this discussion were presented before NIH and AAAS. The work summarized was supported in part by NSF grant GS-960 to Columbia University's program in the sociology of science. This article is publication No. A-493 of the Bureau of Applied Social Research, Columbia University.