



Scientific Output and Recognition: A Study in the Operation of the Reward System in Science

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and their impingement upon social relations. The affectional closeness of mutually downwardly-mobile siblings and the affectional distance of occupationally disparate brothers fall outside the bounds of our basic explanatory device. It is here that the coalition theories of Simmel, Mills, Caplow and others might be of some usefulness. Theodore Caplow, for example, finds that "sibling coalitions appear to be based on similarity of sex, age, and interest rather than on the balance of strength in the triad."²⁵ This and other such concepts, while beyond the scope of the present paper, would be worth pursuing in attempting to add to our understanding of the subjective findings.

In addition, two general conclusions of considerable significance have emerged in the course of the investigation. We have noted (1) that generational kin ties tend to be stronger, both objectively and subjectively, than are lateral ties, at least among young

²⁵ Theodore Caplow, "Further Development of a Theory of Coalitions in the Triad," *American Journal of Sociology*, 64 (March, 1959), p. 493.

adults. Obligation and mutual concern apparently outweigh the age similarity of siblings in determining comparative involvement with parents and siblings.²⁶ Also obvious is (2) the fact that the social network is not spatially bound in this day of communications and high-speed transportation. The same technology which is responsible for widespread residential movement has produced the means for maintaining contact despite separation, with the result that contact with living parents is almost uniformly frequent.

The outlines of occupational position, mobility, and their relation to the kin of orientation are distinct; the inferences are in many cases tentative. Further research, particularly on downward mobility and on adult perceptions of parental roles in their socialization, is very much needed.

²⁶ On positive concern as a characteristic of the social relationship, see Bert N. Adams, "Interaction Theory and the Social Network," *Sociometry*, 30 (March, 1967), pp. 64-78.

SCIENTIFIC OUTPUT AND RECOGNITION: A STUDY IN THE OPERATION OF THE REWARD SYSTEM IN SCIENCE *

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The relationship between the quantity and quality of scientific output of 120 university physicists was studied. Although these two variables are highly correlated, some physicists produce many papers of little significance and others produce a few papers of great significance. The responses of the community of physicists to these distinct patterns of research publication were investigated. Quality of output is more significant than quantity in eliciting recognition through the receipt of awards, appointment to prestigious academic departments, and being widely known to one's colleagues. The reward system operates to encourage creative scientists to be highly productive, to divert the energies of less creative physicists into other channels, and to produce a higher correlation between quantity and quality of output in the top departments than in the weaker departments.

SOCIOLOGISTS of science have for some time investigated the relationship between the sheer quantity of scientists' published research and its quality. A high

or moderate correlation between the two has been found in several fields of science.¹

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want to thank Professor Merton for his helpful suggestions and Professor David Caplovitz for his criticism of an earlier draft of this paper. This may be identified as publication number A-460 of the Bureau of Applied Social Research, Columbia University.

¹ See among others: Kenneth E. Clark, *America's Psychologists: A Survey of a Growing Profession*,

But since the correlation is not perfect, this means, of course, that some scientists produce only a small number of papers which are judged to have contributed a great deal to their discipline while others have produced a long list of publications which have contributed relatively little.

This raises the question of how the community of scientists responds to these distinct patterns of research publication. Is it the case, as the "publish or perish" doctrine implies, that scientists publishing a long string of trivial papers will be rewarded while those who produce only a few papers, though these are of high quality, will be deprived of recognition? Or does the reward system operate somewhat more effectively so as to reward excellence where it is found.

This paper presents data, drawn from a larger study of university physicists, which illustrate a way of assessing the extent to which physicists receive recognition for the quantity of their published work and its quality. A methodological by-product of the paper may move us toward a better measure of the qualitative aspects of scientific output.

One set of data in this study consists of the scientific output of 120 university physicists, chosen from a sampling frame in which the population of university physicists was stratified along four dimensions: age, prestige rank of their university department, productivity, and number of honorific awards. The sample, selected for its suitability for several research problems, is not representative: it heavily overrepresents eminent scientists.² We have collected the

following information for the 120 physicists: the number of papers they have published; the number of citations to their work; the nationally assessed rank of their departments; and the number of their awards.³ A second set of data is designed to find out the relative prestige of awards received by members of the sample and the extent to which the 120 physicists are known within the national community of physicists. Questionnaires were sent to the 2,036 physicists who work in departments that have granted two or more Ph.D.'s in each of the last five years, and 1,281 usable ones were returned.⁴

Since we want first to investigate the relationship between the quantity and quality of research published by the 120 physicists, we must have reliable measures of both. As a measure of the quantity of scientific output, we take the total number of scientific papers by each physicist as listed in *Science Abstracts*.⁵ The problem of assessing the "quality" of scientific publications is more difficult and has long been a major impediment to progress in the sociology of science.

³ The number of papers published was based on *Science Abstracts*; the number of citations on the *Science Citation Index*; the rank of departments on the study by Allan M. Cartter, *An Assessment of Quality in Graduate Education*, Washington, D.C.: American Council on Education, 1966, which is briefly described below; the number of awards on listings in the tenth edition of *American Men of Science*. We are indebted to Dr. Cartter for making information available to us before publication of his book.

⁴ Of the total questionnaires sent to 2,036 physicists, 1,333 or 65.5 percent were returned. Twelve of these were incomplete or otherwise not useable; 40 were returned after the cut-off date for coding. This left 1,281 usable questionnaires. Physicists in highly-ranked departments responded to the questionnaire in the same proportions as those in lower-ranked departments; those with tenure rank to the same extent as those without tenure. So far as we can tell, the 1,281 physicists returning questionnaires are representative of physicists in American universities.

⁵ Two aspects of this decision should be noted. First, only papers and not books are included in research output, since physicists almost invariably publish their original research in papers, unlike the humanities and the social sciences. Second, we shall be using the total scientific output (the cumulative number of papers published by each physicist) rather than productivity-rates (average number of papers per year). We find that both measures exhibit the same patterns of relation to other variables examined in this paper. (See footnote 25).

Washington, D.C.: American Psychological Association, 1957, chapter 3; Wayne Dennis, "The Bibliographies of Eminent Scientists," *Scientific Monthly*, 79 (September, 1954), pp. 180-183; Bernard M. Meltzer, "The Productivity of Social Scientists," *American Journal of Sociology*, 55 (July, 1949), pp. 25-29; and Peggy Thomasson and Julian C. Stanley, "Exploratory Study of Productivity and 'Creativity' of Prominent Psychometricians," unpublished paper, University of Wisconsin, 1966.

² The distribution of the 120 physicists among the various specialties closely parallels the distribution of the population of university physicists. The only exception is found in solid state physics. Twenty-one percent of American university physicists compared to ten percent of our sample list solid state physics as their specialty.

Most of us have typically paid homage to the idea that quantity of output is not the equivalent of quality and have then gone ahead to use publication counts anyway.⁶ There seemed to be no practicable way to measure the quality of large numbers of papers or the total work of large numbers of scientists.⁷ The invention of the *Science Citation Index* (SCI) a few years ago provides a new tool which yields a reliable and valid measure of the significance of individual scientists' contributions in certain fields of science.⁸ Starting in 1961, SCI has listed all citations to scientific papers appearing in an increasingly large number of journals. Thus, it is possible to count the number of citations made in 1961 and certain later years to any paper or group of papers by physicists. The number of citations is taken to represent the relative scientific significance or "quality" of papers in each field.

There is some supporting evidence for this assumption and procedure. In what is perhaps the most thorough study of measures of scientific output, Kenneth E. Clark asked a panel of experts in psychology to list the psychologists who had made the most significant contributions to their field.⁹ He

then investigated the correlation between the number of choices received by psychologists and other indices of eminence. The measure most highly correlated with number of choices was the number of journal citations to the man's work ($r=.67$). Clark concludes that the citation count is the best available indicator of the "worth" of research work by psychologists.

Consider another kind of validating evidence for this measure. Recipients of the Nobel prize in the aggregate can be regarded as having contributed greatly to the advance of their fields in the physical and biological sciences, even though the great scarcity of Nobel prizes means that there are probably other like-sized aggregates of eminent scientists who may have contributed as much.¹⁰ Nevertheless, the laureates as a group can be safely assumed to have made outstanding contributions. The average number of citations in the SCI to the life-work of Nobel laureates (who won the prize in physics between 1955 and 1965) was 58,¹¹ compared to an average of 5.5 citations to the work of other scientists cited in 1961. Only 1.08 percent of the quarter of a million scientists who appear in the 1961 SCI received 58 or more cita-

⁶ See among many others: Derek Price, *Little Science, Big Science*, New York: Columbia University Press, 1963, p. 40; Myron B. Coler (ed.), *Essays on Creativity in the Sciences*, New York: New York University Press, 1963, p. xvi; Logan Wilson, *The Academic Man: A Study in the Sociology of a Profession*, New York: Octagon Press, 1964, p. 110; Diana Crane, "Scientists at Major and Minor Universities: A Study of Productivity and Recognition," *American Sociological Review*, 30 (October, 1965), pp. 699-714.

⁷ Researchers also have had difficulty in estimating the significance of even a small number of papers. Although a panel of judges is often used, problems of standardization of evaluation criteria and the individual biases of the evaluators have frequently been encountered. Cf. David Nasatir and David Elesh, "The Measurement of Quality in Educational Research: The Development and Validation of an Instrument," Bureau of Applied Social Research, Columbia University, 1965.

⁸ The *Science Citation Index* is compiled under the direction of Eugene Garfield. In 1961 the Index listed all citations made in 613 journals; 1962-63 have not yet been indexed. In 1964, 700 journals were covered and in 1965, 1,147 journals.

⁹ Kenneth E. Clark, *op. cit.*; for another study using number of citations as a measure of quality of scientific work, see Alan E. Bayer and John

Folger, "Some Correlates of a Citation Measure of Productivity in Science," *Sociology of Education*, 39 (Fall, 1966), pp. 381-390.

¹⁰ See: Harriet A. Zuckerman, "Nobel Laureates in Science: Patterns of Productivity, Collaboration and Authorship," in this issue of the *American Sociological Review*.

¹¹ We thought it possible that winning the prize might make a physicist more visible and lead to a greater number of post-prize citations than the quality of work warranted. We therefore divided the laureates into two groups: those who won the prize five or fewer years before 1961 and those who won the prize after. The pre-1961 laureates were cited an average of 42 times in the 1961 SCI; the post-1961 prize winners an average of 62 times. Since the prospective laureates were more often cited than the actual laureates, we conclude that the larger number of citations primarily reflects the high quality of the work rather than the visibility gained by winning the prize. These statistics are based upon the work of 24 of the 28 living laureates who won the prize in physics. The four living laureates who won the prize more than five years before 1961 were excluded so as not to introduce an age bias. Included in this computation are the non-American laureates; when they are excluded, the average number of citations to the work of American laureates is 68.

tions.¹² This evidence offers further support for the use of number of citations as an indicator of the scientific significance of published work.

Before analyzing the relationship between the number of published papers and the number of citations, we should consider two problems in the use of citations as a measure of quality. It is possible that the total number of citations to a man's work is not a completely independent indicator of quality, since scientists who publish a large number of papers each of which receives only a few citations might accumulate as many citations as those who have published only a few papers which are heavily cited.¹³ We therefore decided to take the number of citations to the three most heavily cited contributions by each physicist as an indicator of the impact of his best work. Since a contribution in physics does not typically take the form of a single paper but is usually presented in a series of papers, we have used citations to the year's output rather than the single paper as our unit of measure.¹⁴

¹² Irving H. Sher and Eugene Garfield, "New Tools for Improving and Evaluating the Effectiveness of Research," presented at the Second Conference on Research Program Effectiveness, Washington, D.C., July 27-29, 1965. We would like to thank Dr. Sher for making available some unpublished data. By way of emphasizing the difference between the number of citations to the work of laureates and to the work of the average scientist, we should also point out that many scientists do not appear in the SCI and that the modal number of citations to the work of men who do appear is one.

¹³ Although this is possible, our data suggest that it is not probable. The correlation between total number of papers and total number of citations is .60. The correlation between total number of papers and a measure of quality which is not influenced by quantity (described below) is .72. This is exactly the opposite of what we would find if the total number of citations were merely a function of the total number of papers. We conclude that the total number of citations could serve as an adequate indicator of quality, but we have used a refined measure because it seems substantively more suitable.

¹⁴ The year is also an arbitrary unit since physicists do not, of course, arrange their related papers to fit the calendar year. For a more exacting procedure, it would be necessary to identify the series of papers representing an integrated contribution, a requirement extremely difficult to meet in

Another problem in the use of citations as an indicator of quality is the extreme contemporaneity of science.¹⁵ Papers in physics now have a half-life of no more than five years; that is, at least half the citations in any year are to work published in the five preceding years. We must take this into account in comparing the work of physicists who made their most important contributions at different times. To facilitate this comparison, we have developed a technique of weighting citations. Since the *Physical Review* is the leading journal in physics, we have used the time-distribution of citations in this journal as the basis of the weighting technique.¹⁶ A study by M.

dealing with large numbers of working scientists. Without such detailed information, it would seem preferable to use a period of time as a unit rather than single papers. See also the recent study of scientific productivity by Diana Crane, *op. cit.*, which treats a series of four papers on the same topic as a "major" publication, and single papers as "minor" publications.

¹⁵ Still other problems in the use of the SCI as a measure of quality are: (1) Work of the highest significance often becomes common knowledge very quickly and is referred to in papers without being cited. (2) Citations may be critical rather than positive. (3) The various scientific fields differ in size. If we wish to compare the work of scientists in different fields, we must take into consideration the number of people actively working in these fields. (4) The significance of scientific work is not always recognized by contemporaries (e.g., Mendel). New ideas are sometimes ignored or resisted. When citations are used to measure quality, scientific work that is currently "resisted" or inadequately judged will be misclassified. For a discussion of resistance to scientific discovery, see Bernard Barber, "Resistance by Scientists to Scientific Discovery," in Barber and Hirsch (eds.), *op. cit.*, pp. 539-556.

For a further discussion of limitations of citations as a measure of quality and ways of dealing with these, see Leonard Ornstein, *et al.*, "Research, Scholarly Publication and Teaching: The Development of Some Objective Measures of a Scientist's Impact on Science," Mount Sinai School of Medicine, forthcoming.

¹⁶ The importance of the *Physical Review* is illustrated by the high percentage of citations to articles appearing in this journal. Forty-seven percent of citations in the *Physical Review* are to other articles in this journal; 28 percent of citations in *Physical Review Letters* are to articles in the *Physical Review*; and 34 percent of citations in the *Proceedings of Physical Society* (London) are to articles appearing in the *Physical Review*. M. M. Kessler, "The MIT Technical Information Project," *Physics Today*, 18 (March, 1965), pp. 28-36.

TABLE 1. PERCENTAGE DISTRIBUTION OF 120 PHYSICISTS BY QUANTITY AND QUALITY OF PUBLISHED RESEARCH

Quantity	High	Low
	Type I	Type II
High	33	12
	Type III	Type IV
Low	18	37

NOTE: Quantity refers to the number of published papers. "High" quantity is 30 or more; "low" quantity is less than 30. Quality refers to the average number of (weighted) citations to the three most cited years of the physicists' output. "High" quality is more than 60; "low" quality is less than 60. Sum of the percentages is 100.

M. Kessler found that 70 percent of all citations appearing in the *Physical Review* for 1957 are to work published within the five previous years; 17 percent to work published 6 to 10 years before; 9 percent, 11 to 20 years before; and only 4 percent to work published more than 20 years before.¹⁷ From this distribution, we can see the need for weighting citations if we are to compare research that is currently having its greatest impact with that which had its greatest impact at various times in the past. Thus, from Kessler we judge that research in physics published within the past five years is about 17 times more likely to be cited today than research of more than 20 years ago (i.e., 70/4). Following this model, we have assigned the following weights:

Citations to work 20 or more years before:
=17
Citations to work 11–19 years before: =8
Citations to work 6–10 years before: =4
Citations to work 0–5 years before: =1

Throughout this paper, the average number of weighted citations to a physicist's re-

search in his three most heavily cited years will be used as the measure of quality of his research.¹⁸

¹⁸ There is evidence that this procedure indexes the utilization or "significance" of the research by the 120 physicists. For one thing, the average number of weighted citations to the work of the Nobel laureates is 335. For another, every table in this paper was run using other citation measures with similar substantive results. There is a .96 correlation between the total of weighted citations and the average number of weighted citations in the three highest years, and a .80 correlation between totals of weighted and of unweighted citations. These high correlation coefficients once again illustrate the interchangeability of indices. See Paul F. Lazarsfeld, "Evidence and Inference in Social Research," *Daedalus*, 87 (Fall, 1958), pp. 91–130. Since the number of a physicist's weighted citations is correlated very highly with the number of his unweighted citations, our substantive conclusions would probably not be changed if we did not use the weighting technique. Although weighting is methodologically dispensable, we use it because substantively it seems to yield a more accurate indication of the impact of older works when these are compared with more recent publications. In deciding whether the weighting technique is valid we must consider whether the principle of weighting is logically sound and then the accuracy of the particular weights used. First, let us consider the logic of weighting. Jeffrey Reitz, Columbia University, has shown in a study of nineteenth century British scientists that the number of citations at one time period enables us to predict the number of citations at an earlier time. Reitz has shown, for example, that those scientists, who published papers in the 1830's and who received any citations in 1870, were four times more likely to have received citations in the ten years after their work was published.

Derek Price, *Little Science, Big Science*, *op. cit.*, has suggested that "although half the literature cited will in general be less than a decade old, it is clear that, roughly speaking, any paper once it is published will have a constant chance of being used at all subsequent dates." (p. 81) This means that papers published in 1930 which received an average of 10 citations in 1961 received 10 citations on the average in 1931. The weighting technique is in no way at odds with the model suggested by Price. The weighting technique is not meant to predict the number of citations received by papers in the past but to control for the increasing total number of citations. Due to the exponential growth in science, top papers today are receiving many more citations than top papers did in the past. A paper which receives 10 citations today is not among the most heavily cited. But a paper which received 10 citations in the nineteenth century would have been one with relatively great impact. Therefore, in comparing work written in different periods we must standardize for the total number of citations being made.

¹⁷ See M. M. Kessler, "Technical Information Flow Patterns," Massachusetts Institute of Technology, p. 253. Kessler found similar patterns for other major journals of physics. In the Russian JETP, 63 percent of the work cited in 1957 had been published within the preceding five years; 20 percent, 6–10 years before; 10 percent, 11–20 years before; and 7 percent more than 20 years before. Citations in *Physica* were slightly less contemporaneous; the corresponding figures were 55 percent, 19 percent, 8 percent and 18 percent. Similar patterns have been found in two leading journals of biology. Paul Weiss, "Knowledge: A Growth Process," *Science*, 131 (June 1960), pp. 1716–1719.

On the basis of these measures, Table 1 presents the relationship between the "quantity" and "quality" of the research papers produced by the 120 physicists. This table serves two purposes: first it indicates, as expected from earlier investigations, a correlation between the quantity and quality of the research published by these physicists ($r=.72$). Second, and of continuing interest to us throughout the rest of this paper, it generates four types of physicists, roughly described in terms of the production of scientific papers and their quality (as assessed by citations). Type I, comprising 33 percent of this selected sample, is the *prolific physicist*, in the dual sense of producing an abundance of papers which tend also to be fruitful (i.e., often used by others in the field). At the other extreme, Type IV, comprising 37 percent of the sample, is the relatively *silent physicist*: he produces comparatively few papers and, judging from the paucity of citations to them, they do not matter much to the field of physics. The other two types remind us that the sheer quantity of published papers is not always correlated with their quality. Type II is the indiscriminating *mass producer* in physics: the 12 percent in this sample who publish a relatively large number of papers of little consequence. As a type, these men seem geared to getting many papers into print without much regard for their scientific significance. And finally, there is Type III who might be described as the *perfectionist*: physicists who publish comparatively little but what they do publish has a considerable impact on the field. This type may include physicists who elect not

to publish work which, in their own (possibly mistaken) judgment, does not measure up to sufficiently high standards. As a result, they are not the prolific researchers of Type I.

It is obvious that these four types are only crudely approximated by the particular data used here. A much larger sample would be needed to identify the extreme types by more precise criteria, e.g., the silent physicists of Type IV being those who had published no more than, say, 2 or 3 papers all told and the prolific ones of Type I, say, 100 papers or more. The same can be said about the arbitrary cut-off points on the number of citations. Furthermore, a larger sample would enable us to identify intermediate and transitional types. All this is evident and is the basis for our saying that we are dealing here with only rough approximations to the four types. But, as we shall soon see, even these approximations permit us to get on with the main purpose of this paper: to analyze the recognition accorded the four types of physicists in the reward system of science.

THE REWARD SYSTEM OF SCIENCE

As Merton pointed out some time ago, the institution of science has developed a reward system designed to give "recognition and esteem to those (scientists) who have best fulfilled their roles, to those who have made genuinely original contributions to the common stock of knowledge."¹⁹ The graded forms of such recognition in science are many and, among these, we want to examine three kinds as these are distributed among the several types of research physicists. In this way, we move toward an

This raises the question of how this standardization should be accomplished or what weights to assign old papers. We have described one such technique in the text. Another would be to compute the total number of citations being made in a field at different times and multiply old citations by the factor which would be required to bring them up to the total of current citations. More research is needed to determine what the most suitable weights would be. However, we must point out that weighting is only an approximation since it is based on averages—with individual papers having citation patterns diverging from the average. Also, when we group years together we are inaccurate since the weights should be continuous. Finally, if the logic of weighting is accepted, it probably makes very little difference what weights are actually assigned.

¹⁹ Robert K. Merton, "Priorities in Scientific Discovery: A Chapter in the Sociology of Science," *American Sociological Review*, 22 (December, 1957), pp. 635-59, esp. 639-647. The significance of recognition in a social institution of science has been further examined by F. Reif, "The Competitive World of the Pure Scientist," *Science*, 134 (December, 1961), pp. 1957-1962; Barney G. Glaser, *Organizational Scientists: Their Professional Careers*, Indianapolis: Bobbs-Merrill Co., 1964, esp. chap. I; Crane, *op. cit.*; Warren O. Hagstrom, *The Scientific Community*, New York: Basic Books, 1965, esp. chap. II; Norman W. Storer, *The Social System of Science*, New York: Holt, Rinehart & Winston, 1966.

examination of how the reward system actually works.

The first form of recognition is the granting of honorific awards and memberships in honorific societies. Members of the 20 top-ranked physics departments listed more than 150 different awards after their names in *American Men of Science* (1960 edition). A total of 98 of these awards were ranked in prestige from a high of 5 to a low of 1 by a sample of nearly 1,300 physicists.²⁰ Two other options were included in the questionnaire: physicists could report that they had heard of the award but did not have enough information to rank its prestige or that they had never heard of the award. Prestige scores were computed by taking the mean of ranks assigned by the sample of physicists; the visibility of each award was indicated by the percentage of physicists who knew enough about it to rank it at all.

Most of the 98 awards are not highly visible to the national community of physi-

²⁰ The list of 98 awards was assembled from *American Men of Science* and *Physics Today*. Many of the awards listed in AMS were of such limited local significance that they could not be identified and were omitted from the study. All the awards listed in AMS which could be identified were included. Added to this list were awards which appeared frequently in *Physics Today*. Thus, our list of 98 represents a large sample of all awards granted to physicists.

Since it is not feasible to ask each physicist to rank 98 awards, we used five different forms of the questionnaire. Ten awards were included on all five forms. Since the difference between the scores of these awards which appeared on all five forms was statistically insignificant, we conclude that the score received by each award is representative of its prestige among academic physicists. As an example of the closeness of the ratings on the five forms, membership in the National Academy of Science received ratings of 4.28, 4.32, 4.03, 4.24, and 4.27 on the five forms. The wording of the question was:

"The following thirty awards represent a sample of several kinds of awards. For those which are known to you, we ask that you indicate your judgment of its prestige by circling one of the five rankings. You may not have heard of many of these awards, as most are not widely known. If you have heard of an award, but do not know enough about it to evaluate its prestige, please circle No. 6. If you have never heard of the award, circle No. 7. The circling of either 6 or 7 provides useful information as a ranking since it will indicate which awards are least known among physicists."

cists. Only 22 were ranked by as many as half and only 42 by as many as one-fifth of them. Evidently, a large number of the awards in which recipients take enough pride to list after their names are local honors which, though not a part of the national reward system, may nevertheless confer prestige in local environments. For this investigation, we adopted the convention that awards which were unknown to more than 80 percent of the physicists would be regarded as thoroughly parochial and were excluded from further analysis. Several aspects of the distribution of these honorific awards are evident. Of the 42 awards meeting the criterion of national visibility—familiar to at least 20 percent of the physicists—two stand out above all the rest. They are, of course, the Nobel prize (with a prestige score of 4.98 of a possible 5.0) and membership in the National Academy of Sciences (4.22). What is more, physicists at these highest levels of eminence monopolize all the other most prestigious awards (awards with scores of 4.01 or better). In our sample there was not a single recipient of the Fermi Award, Royal Astronomical Society Gold Medal, Albert Einstein Medal, Fritz London Award, and not a single member of the Académie Française or Royal Society who was not either a Nobel laureate or a member of the National Academy. However we interpret it, the fact is that the awards indicating topmost eminence in physics are closely confined to a comparatively small group of physicists.

As Table 2 shows, the prestige of the highest award received by physicists is correlated with the total number of their awards ($r=.70$). By way of anticipation, we note here that the total number of honorific awards correlates as highly as the prestige of highest award with almost every other indicator of recognition of scientific accomplishment.²¹ In due course, we shall examine the distribution of total number of honorific awards among the four types of research physicists.

Prestigious awards and membership in honorific societies are, almost by definition, in short supply and thus are inadequate in

²¹ See Table 3 and footnote 25.

TABLE 2. PERCENTAGE DISTRIBUTION OF PHYSICISTS BY PRESTIGE OF HIGHEST AWARD AND TOTAL NUMBER OF AWARDS

Prestige of Highest Award	Number of Awards					N
	4 or more	3	2	1	Total	
Nobel laureates	45	36	19	..	100	(11)
Member of National Academy of Sciences	27	23	36	14	100	(22)
Awards with scores of 3.00-3.99	..	12	55	33	100	(33)
Awards with scores of less than 3.00	17	83	100	(24)

NOTE: Of this sample of physicists, 30 had received no awards.

providing recognition of lesser degrees of scientific accomplishment. Even among the 632 physicists employed by the top 20 departments of physics in 1960 (at the rank of assistant professor or higher), only one-third listed any awards (in *American Men of Science*). A second form of recognition for scientific work, one which is more widely distributed, is the granting of positions at top-ranked departments. Physicists who would never dream of winning a Nobel prize or being elected to the National Academy, may nevertheless aspire to a tenure position in a major department. By drawing upon Allen Cartter's recent study²² which ranks 86 departments of physics, we can determine the extent to which the work of our four types of physicists has been rewarded by appointment to departments of varying rank.

The third kind of recognition we consider here is the most widespread and, in the view of Alan Waterman, operates as a greater incentive for scientists than more formal recognition, like awards and prizes. This is "the kind and degree of attention [one's] research receives from the scientific community."²³ Citations provide one feedback mechanism enabling scientists to gauge the extent to which their research is being used by others in the field. In this paper, we introduce an additional and perhaps more exacting measure. The sample of nearly 1,300 physicists was asked to describe the extent of their firsthand familiarity with the work of each of the 120 physicists in our more restricted sample, and if they had not read his work, to indicate whether or not

they had heard of him at all. We have adopted the percentage of the community of physicists who reported knowing any part of a physicist's work at firsthand as a measure of the "scope of his scientific reputation."²⁴ This provides us with our third measure of the extent of recognition of the four types of research physicists.²⁵

TYPES OF SCIENTIFIC ROLE-PERFORMANCE AND THE REWARD-SYSTEM

Having mapped certain components in the reward system of science, we are ready to examine the extent and kinds of recognition given the research of our sample of physicists. Table 3 indicates that the quantity and the quality of research are correlated with all three kinds of recognition. But there is a consistently higher correlation between quality of research and the three types of recognition than between quantity of research and recognition. Substantively, this

²⁴ The names of 24 different physicists appeared on each of the five forms of our questionnaire. Thus, the scope of the reputation of each physicist was determined by a random sample of approximately 250.

²⁵ The correlations among the three types of recognition are:

	Prestige of Highest Award	Number of Awards	Scope of Reputation
Number of awards	.70
Scope of reputation	.63	.63	...
Rank of department	.56	.50	.57

The "deviant" cases in which one form of recognition is not accompanied by another, should be studied for further clues to the working of the reward system.

²² Cartter, *op. cit.*

²³ Alan T. Waterman, *Science*, 151 (January, 1966), pp. 61-64.

TABLE 3. COEFFICIENTS OF CORRELATION BETWEEN QUANTITY AND QUALITY OF RESEARCH AND THREE MEASURES OF RECOGNITION

Quantity and Quality of Research	Measures of Recognition			
	Awards			Percent of Community of Physicists Familiar with Individuals' Research
	Prestige of highest award	Number of awards	Rank of Department	
1. Quantity	.35	.46	.24	.49
2. Number of papers per year	.28	.32	.19	.43
3. Quality	.41	.67	.33	.64

NOTE: For definitions of quantity and quality, see note to Table 1.

is the first specific indication that the reward system in physics operates to accord greater recognition to the quality of physicists' research than to its sheer bulk. Procedurally, Table 3 suggests that although the mere count of published papers as a measure of the recognized significance of scientists' research will not introduce gross error, the count of citations provides a better measure.

In Table 4, we turn to the extent of recognition through formal awards accorded the four types of research physicists. We should begin by noting that the *absolute size* of the percentages in this table is in part an artifact of the sampling frame by which the 120 physicists were chosen and of the arbitrary cut-off points for classifying their research. Some physicists classified here as relatively "low" on both quantity and quality have probably produced more and better papers than the average physicist. The significant finding is not in the absolute percentages indicating that most of this partic-

ular sample of physicists received some recognition through awards. Rather, it is in the pattern of *differences* between the percentages. And in this light the story is reasonably clear. It is the quality of research rather than its sheer amount that is most often recognized through honorific awards.²⁶ Although they have published fewer papers than the prolific Type I physicists, the Type III ("perfectionist") physicists are just as apt to be accorded recognition, and both these quality-producers are far more likely to receive awards than the Type II mass producers who publish large numbers of papers indiscriminately.²⁷

The conclusion drawn on the basis of the data in Table 4 is supported by the results

²⁶ The results were the same when the average number of papers published per year, rather than total number of papers published, were used.

²⁷ We were interested in seeing what results would be obtained by weighting the stratified sample so that it would be representative of the *top twenty departments*. There are not enough cases in the sample of 120 which can be weighted so as to make it representative of the entire population of university physicists. We therefore converted the sample back to a simulated random sample of the top 20 departments of physics by weighting each man according to the percentage of the population he represented in the stratified sample. Thus, a man drawn from a group representing 20 percent of the population was weighted twice as heavily as a man drawn from a group representing only 10 percent. As can be seen in the table below, this weighting procedure gives a pattern of results similar to those of Table 4. This suggests that the finding—quality is a far more frequent correlate of eminence than is quantity—may be generalized at least to the population of physicists in the 20 most prestigious departments. The absolute percentages here are also closer to the distribution which we suppose would be

TABLE 4. PERCENTAGE OF PHYSICISTS HAVING AT LEAST ONE AWARD BY QUANTITY AND QUALITY OF PUBLISHED RESEARCH

Quantity	Quality	
	High	Low
	Type I	Type II
High	90(40)	64(14)
	Type III	Type IV
Low	91(22)	57(44)

NOTE: For definitions of quantity and quality, see note to Table 1. Base of percentage is shown in parentheses.

of a multiple regression analysis.²⁸ As we have noted, the zero-order correlation coefficient between number of awards and production of papers is .46. Thus, the sheer number of papers explains 19 percent of the variance in number of awards. When quality of work, as measured by citations, is introduced into the equation, the percentage of variance explained increases to 45 percent. If we reverse the order of introducing the independent variables, 44 percent of the variance is explained by quality alone. When we then introduce quantity, the amount of variance explained does not increase. Thus, once we know the quality of physicists' research, we need not know how much research they have produced to predict their "eminence." However, if we know only the quantity of their research, we will be greatly helped in predicting their eminence by knowing the quality of this work.²⁹ (Incidentally, it should also be noted that the regression analysis provides more reliable findings than those based on cross-tabulations since they are based upon ungrouped continuous data while the categories in the cross-tabulations are dependent upon arbitrary cutting points.)

The second kind of recognition for scientific work is appointment to a major academic department. Believers in the prevalence of the "publish or perish" policy hold that the mere number of publications determines appointments and that even in the top-ranking universities, the academic man who has published only a few papers, albeit significant ones, will typically be passed over in favor of the mass producer of trivia. Our data, presented in Table 5, do not support this belief: Type III physicists (publishing relatively few papers which are

TABLE 5. PERCENTAGE OF PHYSICISTS IN TOP TEN DEPARTMENTS OF PHYSICS BY QUANTITY AND QUALITY OF PUBLISHED RESEARCH

Quantity	Quality	
	High	Low
High	Type I 58(40)	Type II 29(14)
	Type III 77(22)	Type IV 27(44)

NOTE: For definitions of quantity and quality, see note to Table 1. Base of percentage is shown in parentheses.

widely cited in the field) are the most likely to be in the "distinguished" (top ten) departments, even more so than the prolific Type I physicists.³⁰ But the mass producer Type II physicists fare no better than those who produce fewer relatively undistinguished papers. The top departments, at least, prefer to choose their physicists on the basis of quality of research rather than on mere quantity.

Data bearing on our third form of recognition—familiarity with one's research among fellow scientists—were obtained by asking subsets of the nearly 1,300 physicists to indicate the extent of their firsthand acquaintance with the work of our prime sample of 120 physicists. The extent of such direct knowledge we call the "scope of reputation." As Table 6 shows, the perfectionist Type III physicists are more likely to be known to their colleagues than the mass producer Type II physicists. Of all three forms of recognition, scope of reputation is the most influenced by quantity. Type I physicists are still more likely to be known than Type III, and Type II physicists are much more likely to be known than Type IV. This is exactly what we would expect. The other two forms of recognition—receiving awards and holding positions at top departments—are tied more closely to a *positive* evaluation of the scientist's work

found if we used more precise criteria of quantity and quality.

Type of Physicist	Percent with One or More Awards
Type I	80 (129)
Type II	53 (63)
Type III	91 (91)
Type IV	29 (401)

²⁸ We want to thank John Shelton Reed, Jr., for aid in using the Coregn regression program.

²⁹ The same kind of regression analysis applied to several different measures of citations and also to measures of productivity (average papers per year) yields similar results in every case.

³⁰ A possible explanation of this finding is that those physicists who make a few important contributions along with many run-of-the-mill contributions may dilute their reputations. It may also be that in the top departments, papers are more often circulated among colleagues with the result that they are sometimes not published at all.

TABLE 6. PERCENTAGE OF PHYSICISTS HAVING AT LEAST FIFTY PERCENT OF FELLOW PHYSICISTS FAMILIAR WITH THEIR WORK, BY QUANTITY AND QUALITY OF PUBLISHED RESEARCH

Quantity	Quality	
	High	Low
	Type I	Type II
High	68(40)	29(14)
	Type III	Type IV
Low	55(22)	5(44)

NOTE: For definitions of quantity and quality, see note to Table 1. Base of percentage is shown in parentheses.

than is scope of reputation. Although scope of reputation is highly correlated with quality ($r=.63$), it is easier for the mass producer to make himself known through persistent exposure, than it is for him to have his work positively evaluated.

But the high producers, although somewhat more likely to have at least part of their work read, are no more likely to have their reputation spread beyond the circle of those who have actually read their work. We can divide physicists who have not read any of the work of a particular physicist into two groups: those who have heard of him and those who have not. The number of those who have heard of a physicist taken as a percentage of all those who are not directly familiar with his work is described as his "reputational visibility." Thus, if 100 physicists have never read a particular physicist but 50 of them have at least

heard of him, his reputational visibility is 50 percent. As we see in Table 7, quality exceeds quantity in making for reputational visibility. The reputation of physicists who have produced research of consequence travels far within the national community of physicists regardless of the number of publications they have produced. Table 7 also suggests that the mass producer (Type II) is likely to be visible only to physicists who have had to look at some of his work because of its subject matter. Finally, the comparatively "silent physicists" of Type IV are practically unknown to the national community of physicists.

From the data presented in Tables 4-7, it appears that the reward system in physics operates to give all three kinds of recognition primarily to *significant* research, whether this is found in the work of high producers or low. Mere quantity of published research seldom makes for equivalent recognition. To this extent, the reward system of physics approximates the often expressed norm that excellence of research is what truly matters.

The data also testify that, by and large, the high producers *tend* to publish the more consequential research. There are at least two basic factors creating the high correlation between quantity and quality of work. The first is that engaging in a lot of research is in one sense a "necessary" condition for the production of high quality work. As scientists of the first rank remind us,³¹ producing significant science is a risky business, full of uncertainties. There is seldom a guarantee that a program of research will produce important results, and do so in short order. A physicist will try an idea out and sometimes it will work but more often it will not. It is the rare scientist whose eye for crucial problems is so keen that he limits his energies solely to an investigation of these problems. Even the average top scientist must make many experiments be-

TABLE 7. PERCENTAGE OF PHYSICISTS HAVING REPUTATIONAL VISIBILITY (SCORES OF 50 PERCENT OR MORE) BY QUANTITY AND QUALITY OF PUBLISHED RESEARCH

Quantity	Quality	
	High	Low
	Type I	Type II
High	42(40)	21(14)
	Type III	Type IV
Low	41(22)	7(44)

NOTE: For definitions of quantity and quality, see note to Table 1; for "reputational visibility," see text. Base of percentage is shown in parentheses.

³¹ H. A. Zuckerman, "Nobel Laureates in the United States," unpublished Ph.D. dissertation, Columbia University, 1965, for the reports of laureates on the "pedestrian research" that often precedes scientific results of significance; see also Michael Polanyi, *Personal Knowledge*, London: Routledge & Kegan Paul, 1958.

fore he gets an exciting result. We believe that unless a physicist makes a large number of attempts (i.e., has high productivity) the probability of his making a significant discovery will be low.

A second reason why quality and quantity of work are so highly correlated is that the reward system operates in such a way as to encourage the creative scientists to be productive and to divert the energies of the less creative scientists into other channels. In the last part of this paper we shall analyze the processes through which the reward system works to create and then reinforce the correlation between quantity and quality of work.

REINFORCEMENT OF RESEARCH ACTIVITY BY THE REWARD-SYSTEM

Most physicists have been trained in the major departments. Fifty-six percent of the physicists among the nearly 1,300 we queried received their doctorates from the top-ranked fifteen departments; 44 percent from the top ten. We assume that, as students, they internalized the norm prevailing in these departments of doing research. Soon after they receive their degrees (and sometimes before), these young physicists begin to publish their research, whether solo or as part of a research team. Their papers must first pass through the evaluation system. The first screening of this work is by the referees associated with the journals; the prime journal in the field, the *Physical Review*, for example, has an especially elaborate system of refereeing papers. Standards are high and the manuscripts even of eminent scientists are sometimes rejected. Once the paper passes through this screening and is published, it is then informally evaluated by the national and international community of physicists. Sometimes it is largely ignored, with few citations to it, or it may be identified as a significant contribution and put to use in many other published researches. If the reward system, in the form of recognition by citation, does affect research productivity, we assume that the greater such collegial recognition of these early researches by physicists, the greater the probability that they will continue to be productive. We hypothesize that few scientists will continue

to engage in research if they are not rewarded for it.³²

To test this hypothesis, we have traced the sequence of publication patterns among the sample of 120 physicists. We divided them into two broad categories: the early producers who published three or more papers within five years after the Ph.D., and the others who published fewer than three. We then examined the collective responses to these early publications as measured by the number of citations they received within the same five-year span.³³ Finally, we compared the later productivity of the physicists receiving differing amount of recognition in these early years. The results are presented in Table 8. Three-quarters of these physicists began their professional careers by publishing at least three papers soon after their doctorates. There are few "late bloomers":³⁴ only five of the 30 physicists who started off slowly ever became highly productive (averaging 1.5 or more papers a year).

Consider, next, the sequence of publication patterns among physicists who started their research careers by being productive. The more citations their early work received, the more often they continued being productive. Only 30 percent of those who received 0-25

³² Storer suggests the importance of recognition in reinforcing motivation for scientific research. ". . . recognition is frequently interpreted by him [the scientist] . . . not only as confirming the validity and significance of his work but more generally as an affirmation of his own personal worth, and it thereby gains meaning as an intrinsically satisfying reward. Further, . . . the act of creativity does not seem to be complete without some feedback from others, and in science, this feedback takes the form of recognition. Recognition is thus the appropriate response to creativity and is of *significant importance in the desire to engage in research.*" (Italics added). Norman W. Storer, "Institutional Norms and Personal Motives in Science," presented at the annual meetings of the Eastern Sociological Society, April, 1963.

³³ Citations are being used in a slightly different sense here. From the standpoint of the system of science, citations indicate the impact of a piece of research; from the standpoint of the individual scientist, citations to his work provide a type of recognition.

³⁴ See Robert K. Merton, "The Recognition of Excellence," in Adam Yarmolinsky (ed.), *The Recognition of Excellence*, Glencoe, Ill.: The Free Press, 1960, pp. 297-328.

TABLE 8. PERCENTAGE OF PHYSICISTS WHO CONTINUE TO BE PRODUCTIVE, BY EARLY RECOGNITION AND EARLY PRODUCTIVITY

Early Recognition (Number of weighted citations received in first five years after Ph.D.)	Early Productivity	
	Wrote 3 or more papers in five years after Ph.D.	Wrote less than 3 papers in five years after Ph.D.
0- 25	30 (27)	15 (27)
26-100	48 (38)	33 (3)
101 or more	76 (25)

NOTE: Base of percentage is in parentheses. Since the percentages are based upon relatively small numbers, we computed chi square for the first column. $\chi^2=11.35$, d.f. 2, $P>.01$. Continued productivity was defined as producing 1.5 or more papers per year after the immediate post-doctoral years.

weighted citations went on to continued high productivity, in contrast to the 76 percent with more than 100 citations. These findings suggest that when a scientist's work is used by his colleagues he is encouraged to continue doing research and that when a scientist's work is ignored, his productivity will tail off.³⁵ Of course, it may be that the early starters who were also doing quality work as indicated by the frequent use of their research, would have continued to produce even if they had not received recognition.³⁶

These findings also suggest that the Type II researchers—those who publish many papers which are largely ignored—probably get their rewards from other parts of

TABLE 9. PERCENTAGE OF PHYSICISTS PRODUCING HIGH QUALITY RESEARCH, BY QUANTITY AND PRESTIGE OF DEPARTMENT

Prestige of Department	Quantity	
	High	Low
High	85 (27)	59 (29)
Intermediate	62 (16)	14 (21)
Low	45 (11)	13 (16)

NOTE: Base of percentage is shown in parentheses. "High quality" research is defined in note to Table 1.

the system. For, as we have suggested before and now see more definitely, the criteria utilized in that system are not evenly distributed through every sector of academia. Table 9 indicates that the correlation between the quantity and quality of research output is higher in the better departments of physics than in the weaker ones. Among the high producers in the top ten departments, 85 percent had written high quality papers in comparison with 62 percent and 45 percent in the weaker departments.

The results of Table 9 can be interpreted in at least two different ways. The reported distribution may result from a process of social selection whereby the outstanding departments recruit abler researchers with a better sense for the significant research problem. On the average, then, their research has a greater impact on the field. The weaker departments, further removed from the springs of scientific advance, tend to recruit less able investigators who gradually lose contact with the rapidly advancing frontiers of physics and produce work of less significance. Some of the physicists in these departments continue to publish and it is in these departments, which find it difficult to recruit faculty who do research at all that the sheer number of publications is more apt to be an important criterion for promotion. Thus, the reward system of the weaker departments more often makes for that displacement of goals which is expressed in the policy of "publish or perish."

The findings from Table 9 might also be interpreted to result from an imperfect communication network in science. This interpretation starts from the assumption that

³⁵ Zuckerman, "Nobel Laureates in the United States," *op. cit.*, chapter X, provides qualitative evidence for the reinforcing character of recognition in the early years of research.

³⁶ Table 8 shows how the correlation between quantity and quality is reinforced by the least institutionalized part of the reward system—the use of one's work by colleagues. We believe that the more institutionalized parts of the reward system work in the same way. Thus, we would hypothesize that those scientists who have received some award in the early part of their careers are more likely to continue being productive than their colleagues who have not received awards. Also, those young scientists whose doctoral and immediate post-doctoral work is recognized by appointment as an assistant professor at a top university are more likely to continue producing than their colleagues who become assistant professors at the lesser departments.

the flow of scientific information moves principally in a one-directional path from the major to the minor centers of scientific inquiry. Physicists in the weaker departments are apt to know more of the work of men in the stronger departments while men in the stronger departments less often monitor the work of physicists in the weaker departments. Thus, work of "equal quality" produced in departments of differing rank will be differentially recognized and cited in the field. An extension of this view of the communication network maintains that the leading journals of physics are "controlled" by the same group of men who control the top-rated departments. The journals more readily publish papers by members of the in-group and their students who tend to cite the work of others in that group. This results in differing citation rates for research of comparable significance published by physicists located in departments of differing prestige.

To study these tentative interpretations, we follow two lines of investigation. We first examine differences in the visibility of scientific work produced by scientists of differing location in the social structure of science. For example, are physicists at the higher ranked departments (e.g., Berkeley or Harvard) as familiar with "high quality" research in their field carried out at less prestigious departments (e.g., Georgetown or the University of Kentucky) as are physicists in the lower ranked departments with comparable work in the higher ranked ones? Second, does the reward system uniformly operate to reward excellence of research by physicists, irrespective of the rank of their departments?³⁷

On this latter point, some preliminary evidence suggests rank-of-department differentials in the working of the reward system. When we take honorific awards as the dependent variable and introduce quality of research (weighted citations) into the regression equation, we account for 44 percent of the variance (as we have previously

noted). When we introduce rank of department into the equation, we increase the percent of variance explained to 53. This indicates that high quality research by men in the higher ranked departments is more often recognized in the form of awards than comparable work in the lower ranked departments. (The partial correlation between rank of department and number of awards when quality is held constant is .41.) That this results at least in part from the wider reputation of the physicists in the top departments is suggested by the fact that when scope of reputation is introduced into the regression equation prior to rank of department, the latter variable explains only an additional four percent of the variance in number of awards. The implications of these preliminary findings need to be pursued.

SUMMARY

The quantity and quality of research by physicists tend to be related (generating the types we have described as the "prolific" and the "silent" physicists). When there is an inconsistency between quantity and quality of work—as in the cases of the "mass producers" and the "perfectionists"—quality proves to be a more significant correlate of the amount of recognition accorded research physicists. This is the case for three forms of recognition: honorific awards, appointments to top-ranking departments, and having one's research known in the national community of physicists. To a degree, the reward system operates to reinforce the patterns of work by the more creative scientists and tends to confirm the observed relation between quantity and quality of research. Some preliminary evidence suggests, however, that the reward system does not operate uniformly among all academic departments of physics. There are indications that the sheer quantity of publications is more likely to be used as a criterion of promotion in the less prestigious departments, and that quality research is more often rewarded when it is produced by physicists in high ranking departments.

³⁷ Crane, *op. cit.*