magnitude less than by conventional spectroscopic methods. This reduction in time permits the study of transient species, or by "time-averaging" procedures S/N may be improved without the expenditure of inordinate amounts of time. The FT method finds especially important application in the study of NMR spectra of nuclei of low sensitivity and low abundance, such as ^{13}C.

References and Notes

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21. We thank Dr. E. Gross, National Institutes of Health, for supplying the sample of nisin used to obtain the spectrum of Fig. 11. We thank Dr. V. Mockel, Firestone Tire and Rubber Company, for permission to use the spectrum shown in Fig. 10 and Dr. G. Levy, General Electric Company, for permission to use the spectrum shown in Fig. 12.

The Ortega Hypothesis

Citation analysis suggests that only a few scientists contribute to scientific progress.

Jonathan R. Cole and Stephen Cole

Most scientists are aware that science is a highly stratified institution. Power and resources are concentrated in the hands of a relatively small minority. For the past several years we have been studying the social stratification system of science (*1–3*). Most of our research has concentrated on the social processes through which individual scientists are evaluated, to discover why some scientists rise quickly to positions of eminence and others remain relatively obscure. Two conflicting theories explain social mobility in science. According to one theory the stratification system of science operates on strictly universalistic criteria: the scientists who publish the most significant work receive the ample recognition they deserve; those not publishing significant work are ignored. According to the other theory, a small elite at a handful of universities and government-supported laboratories control the social institutions of science in such a way as to perpetuate their own ideas and assure the social mobility of their intellectual children. The results of our research have for the most part supported the former theory. We have found that quality of published research explains more variance than any other variable on several types of recognition.

Contributions from Scientific Strata to Progress in Science

Whereas most of our previous research has dealt with the processes through which individuals find their level in the stratification system, in this article we analyze another problem. We present data evaluating the comparative contributions of the various scientific strata to scientific progress, indicating whether progress is built on the labor of all "social classes" or is primarily dependent on the work of an "elite." In the past, historians and philosophers of science have attributed much of the growth of science to the work of the average scientist, who, it is suggested, has paved the way with his "small" discoveries for the men of genius—the great discoverers. This hypothesis is asserted in many sources, but perhaps no more clearly than in the words of Jose Ortega y Gasset (*4*):

> For it is necessary to insist upon this extraordinary but undeniable fact: experimental science has progressed thanks in great part to the work of men astoundingly mediocre, and even less than mediocre. That is to say, modern science, the root and symbol of our actual civilization, finds a place for the intellectually commonplace man and allows him to work therein with success. In this way the majority of scientists help the general advance of science while shut up in the narrow cell of their laboratory, like the bee in the cell of its hive, or the turnspit of its wheel.

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Ortega seems to be suggesting that average scientists, working on relatively unambitious projects, make minor contributions, but that, without these minor discoveries by a mass of scientists, the breakthroughs of the truly inspired scientist would not be possible. Thus the work of the great scientist is built upon a pyramid of small discoveries made by average scientists. This view of science is widespread. Some even go so far as to maintain that scientific advance is more dependent upon the small discoveries of the many average scientists than upon the breakthroughs of the great scientists. Lord Florey, a recent president of the Royal Society, expressed this point of view (*5*):

> . . . Science is rarely advanced by what is known in current jargon as a "breakthrough"; rather does our increasing knowledge depend on the activity of thousands of our colleagues throughout the world who add small points to what will eventually become a splendid picture much in the same way the Pointillistes built up their extremely beautiful canvasses. . . .

In the view of science, of course, a number of assumptions are made. Consider two: it is assumed (i) that the ideas of the average scientist are both visible and used by the outstanding scientist; and (ii) that the minor work is necessary for the production of major contributions. In short, it is proposed that the work of the average scientist is indispensable if science is to advance. Little empirical evidence exists to substantiate these widely held beliefs. We shall examine data bearing upon the validity of this view of scientific progress. To make an empirical test of this conception manageable, we confine ourselves to one of its several aspects and to only one field of science. We examine the work of several groups (samples) of physicists and analyze what work these men used in making their discoveries.

We do not intend to suggest that great discoveries in science by an Einstein or a Lee and Yang are not preceded by numerous "smaller" discoveries, or that great discoveries do not in turn stimulate a multitude of lesser ones (*6*). We will suggest that even the scientists who make these "smaller" discoveries come principally from the top strata of the scientific community. In the proper perspective of the history of science, "normal science" as Kuhn (*7*) defines it, is not done by the average scientist but by the elite scientists (*8*). Indeed, in the longer perspective, the work of many of today's outstanding scientists, such as Nobel laureates and members of the National Academy of Sciences, may turn out to be minor footnotes in the history of science.

The question that we consider is how many scientists are contributing through their published research to the movement of science, and how many are not. There are, of course, many ways to contribute to scientific progress other than through published research. The scientists who are primarily teachers, administrators, or technicians may play crucial roles in scientific development. We do not intend to downgrade the importance of these roles. Nevertheless, it is still valid to ask how many scientists contribute to scientific progress through their published work if we keep in mind that to the list of contributors of this type we must add the names of contributors of other types.

Price, following Lotka, has estimated that the number of scientists producing n papers is approximately proportional to $1/n^2$ (*9*). This inverse-square law of productivity estimates that, for every 100 authors producing one scientific paper, there are only 25 who produce two, 11 who produce three, and so on. Using Price's model we can estimate that roughly 50 percent of all scientific papers are produced by approximately 10 percent of the scientists. What remains problematic is the extent to which the 10 percent of the scientists who produce 50 percent of the research publications are dependent on the other 90 percent of research scientists and the 50 percent of the total research they produce. If the bulk of the scientific community produced work that is rarely used—that is, is infrequently cited in the work of outstanding scientists—the indication may be that their work does not materially advance the development of science. The basic question to ask is what the intellectual sources of influence on the production of scientific research of varying quality may be. If Ortega is correct, the work of scientific frontiersmen will to some extent be dependent upon the work of the vast majority of physicists.

Citation Practices of Academic Physicists

We collected data to illustrate the citation practices of academic physicists. One set of data consists of the citations made by 84 university physicists in their paper most heavily cited in the 1965 *Science Citation Index* (SCI) (*10*). We consider this to be the physicist's outstanding piece of work as gauged in 1965 (*11*). The 84 physicists are, in fact, a subsample of a sample 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 the number of honorific awards received. A second set of data consists of information on a one-third random sample of the scientists cited in the best paper of each of the 84 physicists. For the sample of 385 cited authors we collected data that enabled us to locate them in the stratification system (*12*).

A basic assumption in this analysis is that the research cited by scientists in their own papers represents a roughly valid indicator of influence on their work. Of course, not all citations represent direct and specific influence. Everyone knows that scientists occasionally make ceremonial citations to friends, colleagues, or eminent people in the field. Sometimes a citation to an expert in the field serves the function of legitimating the new paper. Even when we cite work that has influenced us, it is difficult for the reader to know when it represents a significant, even necessary, antecedent to our work as opposed to a tangentially relevant piece of work, in which we are merely demonstrating our "knowledge of the literature." Furthermore, relevant and influential material is passed from one scientist to another through private communication, which, though often mentioned in today's age of Big Science, sometimes do not show up as citations. However, a reasonable case can be made that citations generally represent an authentic indicator of influence (*13*).

Let us consider the process through which we decide what to cite in our papers. Some part of our citations will be very clear-cut. We will, of course, cite papers that contributed directly to the current state of knowledge in our problem area. In this paper, for example, such a citation would be to the work of Derek de Solla Price. Another group of references, however, would be more questionable, namely, those to people who have done work in the area but have not had a direct influence on the paper. The reason for citing these rather than others may be that we tend to cite scientists having the

Table 1. Marginal distributions of the social and individual characteristics of the authors cited in 84 papers and comparative figures for the entire field of physics [see (*25*)].

Characteristics of cited authors: Category	Characteristics of cited authors: Percent	Comparative "population" statistics (%)
Current affiliation		
University	72	
College or nonacademic research laboratories	10	43
Industry	10	34
Government	8	11
N	(385)	(26,698)
Rank of department of those in academic departments		
Distinguished (top nine)	60	21
Strong and good	23	42
Lesser universities and colleges	17	37
N	(299)	(1,308)*
Number of honorific awards		
Zero	32	73
One	18	15
Two or three	23	9
Four or more	27	3
N	(385)	(1,308)*
Quality of scientific output (number of citations: 1965 SCI)		
Under 15	25	67
15 to 29	33	25
60 or more	43	8
N	(385)	(1,308)*

* These figures are drawn from the sample of 1,308 university physicists.

highest visibility. Scientists gain visibility originally by publishing significant research. After such visibility is gained, they enjoy a halo effect as their research gains additional attention because of their visibility. Thus, if we consider the sum of a scientist's citations, some part will be due to the halo effect. But the size of the effect will probably be directly related to the significance of the scientist's research. The processes of objective evaluation of contributions and the subjective working of the halo effect combine to create substantial gaps between the number of citations received by members of the elite and the average scientist.

The halo effect would cause us to cite a scientist whose work was not directly influential. But we are primarily interested in situations in which work that is directly influential is not cited. The norms of science require scientists to cite the work that they have found useful in pursuing their own research, and for the most part they abide by these norms. Moreover, the audience of the work generally takes citations as an indicator of influence. We only have to think of the number of times we have taken a quick glance at the acknowledgments and references in books and papers with the intent of noting the influence on a piece of work to realize that, at the very least, citations do indicate intellectual connections.

Sometimes, however, a crucial intellectual forebear to a paper is not cited. The omission is rarely due to direct malice on the part of the author but more often to oversight or lack of awareness. It occurs most frequently when a scientist's work has had such a deep impact on the field that the ideas have become part of the accepted paradigm and explicit citation is not considered necessary. Only the work of a handful of scientists ever achieves this status, and they generally receive very heavy citation anyway. (The work of Einstein, for example, was cited 281 times in the 1970 edition of SCI.) We can assume that omitted citations to less influential work are random in nature, and that even if we may fail to cite the important work of a particular scientist, others will not be likely to make the same error. In general, the procedure of using citations as an indicator of influence probably errs on the side of overinclusion rather than exclusion of significant influences.

Characteristics of Cited Authors

The characteristics of the sample of 385 authors cited in the best papers of 84 university physicists are presented in Table 1. We wish to compare the characteristics of these cited authors with those of the population of physicists. This is difficult because many of the population parameters are unknown. We have, therefore, chosen a sample of 1308 university physicists as the comparison group; for a full description of this data set see (*2*). This sample, of course, is itself an elite group and far from representative of the more than 25,000 American physicists. As the data of Table 1 indicate, physicists in the top strata are far more likely to be cited than those below the top. Whereas 73 percent of the sample of 1308 university physicists had no awards listed after their names in AMS, only 33 percent of the cited authors had no awards (*14*). The same results are found when we examine citations to the work of the cited authors and the university sample. On the average, the cited authors received 119 citations to their life's work in 1965; all the authors listed in the 1965 SCI received a mean of 6 citations. Further, although only 8 percent of the 1308 university physicists averaged 60 or more citations, 43 percent of the cited authors exceeded this lofty number (*15*).

The data in Table 1 lead to the conclusion that most of the work used by university physicists in their best papers is produced by only a small proportion of those who are active in the field. It is equally important, however, to note that a significant *minority* of cited work is being produced by non-elite physicists. So far, we have not made any distinctions among the citing papers. We have merely considered the references in the "best" papers of a stratified random sample of university physicists. Many of these best papers may have been of relatively little significance. If the Ortega view of science is correct, we should find the top papers making just as much use of the work of little-known physicists as the less significant papers. We shall present three sets of data to test this hypothesis. Obviously, the number of citations to these best papers varied greatly. Some papers received only 1 or 2 citations; others received more than 20 or 30. We shall now see the extent to which authors of papers of varying quality depend upon the work of elites and non-elites.

As the data of Table 2 indicate, highly cited papers, more often than those receiving few citations, make use of high-quality work produced predominantly at the nine most distinguished departments (*16*). We see that a mere 7 percent of the citations in the most highly cited discoveries go to scientists working in the lower-prestige university departments and colleges, whereas 60 percent are to scientists at distinguished departments. Even in the papers receiving less than 10 citations, the work of scientists at top universities is cited much more frequently than that of those in departments of lower prestige. The best papers cite other significant papers predominantly. Fifty-four percent of the citations in papers receiving 20 or more citations and 33 percent in those receiving less than 10 citations go to the work of scientists who have received 60 or more citations. Finally, note the extent to which papers of high quality rely on the work of Nobel laureates and National Academy members. Forty-five percent of the citations in these papers go to the work of no more than 200 scientists and their collaborators. Although the papers of lower quality do not cite these "elites" to the same extent, the work of the elites receives proportionately greater use in these papers as well.

Table 2. The distribution of citations in individual papers of varying quality according to the characteristics of the cited scientists. High, 20 or more citations; medium, 10 to 19 citations; low, 0 to 9 citations.

Characteristics	Quality of citer's "best" paper		
	High (%)	Medium (%)	Low (%)
Rank of department			
Distinguished	60	50	36
Strong-good	14	19	19
Lesser	7	12	18
No academic affiliation	19	19	27
N	(95)	(139)	(151)
Quality of scientific output (number of citations)			
High (60 or more)	54	48	33
Medium (15 to 29)	28	30	36
Low (less than 15)	18	22	31
N	(95)	(139)	(151)
Prestige of highest award			
Nobel prize, NAS member	45	32	25
Other honorific awards	15	8	12
Only fellowship,* no awards	40	60	63
N	(95)	(139)	(151)

* Fellowships such as the Guggenheim, Sloan, Rockefeller, and Fulbright were here considered as honorific awards as distinct from other postdoctoral fellowships.

Throughout this article we have defined important discoveries simply by the number of citations they have received. As a further test of the Ortega hypothesis, we asked a well-known physicist to list the five most important contributions to elementary particle physics in the last 10 years. We admit that in many ways this procedure falls short of the rigorous study needed to test the hypothesis further. It would be useful, for example, to have a broad, stratified panel of judges evaluate the merits of various pieces of research and then look at the citation patterns in papers judged to be of highest impact. It is noteworthy, however, that the five papers chosen by our informant received a mean of 67 citations in the 1965 SCI (*17*). We examined all the journals and private communication citations in these five papers, and then located the cited authors in the stratification system of physics. The five papers cited a total of 51 articles (not counting self citations) involving 126 authors, of whom 19 were located at foreign universities and foreign research laboratories.

The data corroborate the earlier findings. Of the 107 American scientists cited in these five "pathfinding" papers, all but one were located at one of the top nine physics departments in the United States or at such distinguished laboratories as Brookhaven or the Lawrence Radiation Laboratory. All 51 articles were produced at one of these top nine departments or laboratories. The average number of citations to the cited authors is equally impressive. This group had a mean number of citations of 69 in the 1965 SCI; 74 percent of these authors had more than 60 citations to their work in 1965 (*18*). Among the 107 cited authors were a number of younger and not yet widely recognized scientists who were coauthors of more eminent colleagues. The mean number of citations to either a single author or the most highly cited author of collaborative papers is 134.

Additional Test of the Ortega Hypothesis

Since this type of subjective sampling procedure may indeed be methodologically suspect we decided to perform one final test of the Ortega hypothesis. We replicated the essential aspects of the study design using a set of independent data. We had a complete list of all papers cited three or more times in *Physical Review* in 1965; it contained more than 3000 scientific articles and substantive letters. A few of these papers were cited often; most received less than five citations. Since we are primarily concerned with the pattern of citation in influential papers, we initially examined the ten papers that were most often cited in *Physical Review*. After identifying these "super" papers, we listed the scientific articles that the authors of these influential papers cited. Finally, we counted the number of citations received in 1965 by authors of papers cited in the "super" articles.

This procedure can be clarified by reference to a specific example. Murray Gell-Mann produced the most heavily cited article on the list, receiving a total of 49 citations in *Physical Review* in 1965. We listed the references in Gell-Mann's paper. A total of 33 publications, or 55 scientists, were cited in the paper. We then noted the number of citations that the life's work of each of these 55 scientists had received in the 1965 SCI. The same process was followed for all the scientists cited in the ten super papers. Thus, even though we were examining only the ten most highly cited papers, we studied a total of 299 authors.

The results obtained from this replication offer further evidence in support of the earlier findings. It turns out that authors cited in these ten papers were scientists who, on the average, had produced truly outstanding scientific work. In 1965 the 299 cited scientists produced research that received an average of 135 citations. Since this average includes citations received by beginning scientists yet to make their mark, who are collaborating with their more eminent colleagues, the statistic is actually lower than it would be otherwise. In fact, if we take only single-authored papers and the most-cited author in each collaborative paper and compute the average number of citations to the author's life's work, the mean is increased to 175 citations. Clearly these data lend added weight to the counter hypothesis that work used by the producers of outstanding research is itself produced by a small minority of scientists. The work of the average researcher is rarely the work that is influential in the production of high-impact scientific research.

A question remains to be answered. What is the quality of the research cited in the work of the physicists whose papers receive fewer citations than those of the ten "super" papers? From the same *Physical Review* list we drew a small random sample of papers that had received from 23 to 3 citations in 1965. Papers that received 23 citations were of approximately the same impact as some of the top ten, since the citations to the "super" papers ranged from 49 to 24. This small sample consisted of 36 papers, within which references were made to 492 communications. We computed the number of citations in 1965 to the 837 physicists who authored these 492 papers. We examined citation rates to cited authors who produced single-authored papers and those in collaborative teams whose work was most often cited. The data presented in Table 3 suggest that even authors of less than super quality papers were predominantly influenced by work of elites. Whereas the top ten papers made use of work produced by physicists who received an average of 175 citations to their life's work, the average quality of cited work found in papers receiving 5 to 9 citations is not appreciably lower. Only when we examine the citation patterns in papers receiving 3 or 4 citations is the average quality of work cited significantly lower. But, even in this group, the scientists cited are among the elite insofar as the quality of their work goes.

These high averages are not due to a handful of extreme individuals. Forty-one percent of the 837 cited physicists received more than 100 citations; another 13 percent, from 60 to 69. Thus a total of 54 percent received more than 60 citations, a figure similar to those presented in Table 2. Only 11 percent of the cited authors received less than 5 citations to their life's work, and 90 percent of the scientists comprising this 11 percent were coauthors on papers for which one of the other authors was more heavily cited. In short, there were virtually no cited authors whose work was not of above average quality.

Consider once again a set of comparative statistics: (i) about one-half of all the papers published in the more than 2100 source journals abstracted in the SCI do not receive a single citation during the year after it is published; (ii) the average cited author in the 1965 SCI received a mean of 6.08 citations to his life's work. These data offer further support for the hypothesis that even the producers of research of limited impact depend predominantly on the work produced by a relatively small elite.

Table 3. Citation patterns found in papers cited in the 1965 *Physical Review*.

Citations to paper in the 1965 *Physical Review* (No.)	Citations in 1965 to the life's work of major authors* cited in the papers	
	Mean No.	No. of major authors
24–49	175	(174)
20–23	169	(88)
10–17†	158	(215)
5–9	149	(124)
3–4	85	(65)

* By major authors is meant all single authors and for collaborative papers the author whose life work has received the highest number of citations in the 1965 SCI. † No papers were cited 18 to 19 times in *Physical Review* in 1965.

Conclusions

Let us consider, then, some general conclusions that may be drawn from the findings reported in this study. The data allow us to question the view stated by Ortega, Florey, and others that large numbers of average scientists contribute substantially to the advance of science *through their research.* It seems, rather, that a relatively small number of physicists produce work that becomes the base for future discoveries in physics. We have found that even papers of relatively minor significance have used to a disproportionate degree the work of the eminent scientists. Although the conclusions of this paper may be reasonably clear, the implications of these data for the structure of scientific activity, at least in physics, need careful consideration.

Consider only one problem emerging out of the findings that needs a great deal of further research: the size of the research establishment of modern science. If future research on other fields of science corroborates our results, we may inquire what it implies about the relationship between the number of scientists and the rate of advance in science, and whether it is possible that the number of scientists could be reduced without affecting the rate of advance. The data would seem to suggest that most research is rarely cited by the bulk of the physics community, and even more sparingly cited by the most eminent scientists who produce the most significant discoveries. Most articles published in even the leading journals receive few citations. In a study of citations to articles published in *Physical Review*, we found that 80 percent of all the articles published in the *Review* in 1963 were cited four times or less; 47 percent, once or never in the 1966 SCI. Clearly most of the published work in even such an outstanding journal makes little impact on the development of science. Thus the basic question emerges, whether the same rate of advance in physics could be maintained if the number of active research physicists were to be sharply reduced.

Several criticisms of our position are possible.

1) The data indicate that about 15 to 20 percent of the work cited in significant discoveries is produced by "average" scientists. We do not know whether the important discoveries could or could not have been made if only the work of eminent scientists had been considered. It might be maintained that the 20 percent of references produced by, let us say, 80 percent of researchers are just as crucial for scientific advance as the 80 percent of references produced by 20 percent of researchers. To suggest a reply to this first criticism we must make explicit an idea implicit in much work done in the sociology of science. Our entire analysis is dependent upon the assumption that no single scientist, elite or non-elite, is crucial for scientific advance. The study of independent multiple discoveries leads to the conclusion that, if a particular scientist had not made a particular discovery, another would have (*19*). If the scientist who makes a discovery had not made it, it would be only a matter of time—probably a relatively short period—before the discovery would be made by another scientist.

The history of science is replete with examples of discoveries made independently by two or more scientists within a short period. Merton has suggested that multiple discoveries are the norm rather than a rare occurrence. Furthermore, many discoveries that are not multiples are forestalled multiples, for most scientists will stop working on a problem when they learn of the success of a competitor. As we learned from *The Double Helix*, if Watson and Crick had not made their historic breakthrough it probably would have been made in short order by Pauling. Most scientists working on important problems realize that many others are working on the same problems. Indeed, chance often plays its part in determining who makes a discovery first.

If the work done by any scientist, elite or non-elite, can be replaced by work done by other scientists, how then do we evaluate the extent to which a particular scientist is necessary for scientific advance? Merton (*19*) defines the scientific genius as a man who is involved in multiple multiples—the functional equivalent of many other scientists (*19*). Although no one citation or one man is crucial for any scientific discovery, the scientist who writes one paper that is cited once in an important discovery is less crucial than the scientist who writes many papers that are cited many times in many important discoveries.

Even though it might be maintained that all the work referred to in a paper is necessary for the production of that discovery, it does not therefore follow that all the individuals cited were essential for the discovery. Although all scientists are replaceable in the sense that other scientists would eventually duplicate their discoveries, some scientists have many more functional equivalents than others. For example, it would be relatively difficult to replace the work of a Murray Gell-Mann, but not so difficult to replace that of a scientist who is cited once in one of Gell-Mann's papers. If the less distinguished scientists have many functional equivalents, so do the many laboratory technicians and staff workers who often perform vital tasks in the making of scientific discoveries. We are not saying that the tasks are unnecessary, but that there are many people who could perform them.

We draw an analogy that may crystallize the point. Sanitation men perform socially useful and necessary functions. Without them a complex industrial society would not function very smoothly. A prolonged strike of sanitation men would probably create more chaos than a strike of teachers, social workers, or even perhaps nurses and doctors. Yet the job that sanitation men do could be performed by the National Guard, whereas the jobs performed by professionals could not be handled by untrained people. The sanitation man is given little prestige in the hierarchy of occupations not only because of the lower salary and poorer working conditions he has relative to a doctor, a lawyer, or a scientist, but also because he has many more functional equivalents in the social system than a professional man (*20*). It is far easier to find replacements for the individual sanitation man than for the individual scientist or doctor. The same principle operates within a single occupation. Within science some men are more easily replaced than others. We suggest that it may not be necessary to have 80 percent of the scientific community occupied in producing 15 or 20 percent of the work that is used in significant scientific discoveries, if perhaps only half their number could produce the same work.

2) A second possible criticism of our analysis is that we have dealt with only one generation of influence. Untested in this paper is the possible "filtration" of ideas from the lower to higher levels of the stratification system. The filtering process may take a number of "generations" of papers before the low-impact papers have an influence on important discoveries. Further, in the process of filtration a minor contribution may be entirely absorbed by the next generation paper that makes use of it. Thus only a single citation might be necessary for a piece of work to become part of the stockpile of knowledge. A minor contribution then might ultimately have an effect on the production of a great idea through a "great chain" of papers. The first links in the chain would be concealed from our vision because they were not cited by later generations. What is clearly called for is a study of the sociometrics of multiple generations of papers, in which we would examine the number of scientists added to the list of those who influence discoveries as we add new generations of papers.

We are now tracing patterns of influence. As we go back we add new names to the matrix. But, in line with the assumption of the "replaceability" of scientists, we would argue that the crucial question is not how many new scientists, but how many central names, are added to the matrix. We might define a scientist that appears three times or more as "central." We would guess that as new scientists are added to the matrix the proportion of central scientists will drop off sharply and soon hit zero. We hypothesize that we will not have to look at many generations of influence before we find that all new names added to the matrix are appearing only once. Kessler found the same pattern in his study of citations in *Physical Review*: 95 percent of the references were to articles published in the *Review* itself plus 55 other journals. He suggests (*21*):

> . . . The same list of 55 journals . . . will account for the majority of references year after year. The remaining 5 percent of the references is to a large and ever-growing list of rarely used sources. . . . This list has no stability in time; each new volume examined is destined to carry 96 percent of the references in the subsequent 35 volumes. As we examine those volumes, 78–96, it is clear that, although the list of new titles never ends, their contribution to the total reference literature is comparatively small.

3) An additional criticism of this article could be that we have considered only the research function of scientists. As we pointed out above, scientists can, of course, make important contributions to the advance of science through excellent performance in other roles, like teaching and administration. However, just as it would be incorrect to ignore these important roles, it would also be an error to assume that separation of these roles is necessary. Possibly the same scientists who produce the most significant research are also doing the most significant teaching and administration.

Let us look at the teaching function performed by scientists. If the assumption is correct that it is primarily elite scientists who contribute to scientific progress through their research, we should be primarily concerned with the teachers of future members of the elite. We know from qualitative sources and statistical studies of Nobel laureates, National Academy members, and other eminent scientists that the great majority of scientists who end up in the elite strata are trained by other members of the elite (*22*). In fact, 69 percent of current members of the Academy and 80 percent of American Nobelists received their doctorates from only nine universities. It might be facetiously asserted that the best way to win a Nobel prize is to study with a past laureate. Analysis of the graduate schools attended by physicists whose work is heavily cited indicates that a large majority of scientists who turn out to be productive get their doctorates at the top 20 graduate departments. We would not claim that unproductive scientists teaching at low-prestige institutions serve no function: they may, for instance, serve the truly important function of educating nonscientists to the objectives and methods of science. Nevertheless, in fact little evidence exists that they contribute to the progress of scientific research through their teaching.

4) Another possible criticism could be that, even if all our hypotheses were supported by the necessary extensive future inquiries, we would still be left with a critical and difficult problem before any policy implications that may be found in these data could possibly be acted upon. We would still have to identify correctly the scientists who would go on to produce important scientific discoveries. We would need a set of accurate predictive measures that could identify at an early age the students who would produce truly significant discoveries. Although the problem lies beyond our current capabilities, we believe that it would not be as difficult to solve as it seems at first.

We pointed out above that the majority of scientists who contribute to scientific progress are educated at a small number of graduate institutions. Probably most of the exceptions chose to attend institutions of lower prestige for idiosyncratic personal reasons rather than because they were not admitted to one of the leading departments. If each field had only about 20 graduate departments, all individuals showing any talent and interest in it would by necessity have to apply to one of these institutions. It is unlikely that, if 20 departments each admitted between 25 and 50 new graduate students each year, thus potentially reducing by a factor of 2 the number of doctorates granted, many students who in fact had the potential to make important scientific contributions would be denied access to graduate education. For example, if 20 graduate departments of physics admitted only 50 students a year, 40 of whom were to receive their doctorates in due course, these 20 departments would produce 800 Ph.D.'s each year, or about half the total number of American physics doctorates awarded in 1970 (*23*). A reduction in the absolute number of training centers would not imply a reduction in the competition between these universities for talented researchers or students.

It is a fact all too well known that new Ph.D.'s in science, especially in physics, are having a difficult time finding jobs. Most projections of supply and demand for scientists are not optimistic (*24*). One way to handle this inequity in supply and demand is to cut back sharply the number of Ph.D.'s we are producing. The data we have reported lead to the tentative conclusion that reducing the number of scientists might not slow down the rate of scientific progress. One crucial question remains to be answered: whether, if the number of new Ph.D. candidates is sharply reduced, will there be a reduction in the number of truly outstanding applicants or will the reduction in applicants come from those whom we would now consider borderline cases. This is not a matter of social selection, for we believe it possible for academic departments to distinguish applicants with high potential. It is a matter of self-selection. A reduction in the size of science might motivate some very bright future scientists to turn to other careers.

The ability of an occupation to attract high-level recruits depends to a great extent on the prestige of the occupation, working conditions, and perceived opportunities in the occupation. We, of course, do not intend to suggest the advisability of any policy that would either reduce the prestige of science or the resources available to scientists. What we are suggesting is that science would probably not suffer from a reduction in the number of new recruits and an increase in the resources available to the resulting smaller number of scientists. Perhaps the most serious problem that science faces today in recruiting is the perceived reality that there are few jobs available to new Ph.D.'s. Reducing the size of science so that supply would be in better balance with demand might ultimately increase the attractiveness of science as a career.

References and Notes

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7. T. S. Kuhn, *The Structure of Scientific Revolutions* (Univ. of Chicago Press, Chicago, 1962).
8. We use the term "elite" here and throughout in a statistical sense to refer to the small group of eminent scientists who publish the most, are most frequently cited, and occupy the most prestigious positions. In fact, Zuckerman notes that this statistical elite does form a fairly cohesive social group. See H. Zuckerman, in "Stratification in American science." *Social Stratification: Research and Theory for the 1970s*, E. Laumann, Ed. (Bobbs-Merrill, Indianapolis, 1970), p. 239.
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10. The *Science Citation Index* (SCI), published in Philadelphia by the Institute for Scientific Information under the direction of Eugene Garfield, lists all references made each year in more than 2000 journals. Much research has been done showing that the number of citations a scientist's work receives is a roughly valid indicator of its quality. Let us consider just one piece of validating evidence for this measure. Recipients of the Nobel prize are generally regarded as having contributed greatly to advances in physical and biological sciences. Since the number of Nobel prizes is limited, however, there may be other like-sized aggregates of eminent scientists who have contributed as much. Nevertheless, the laureates as a group can be safely assumed to have made outstanding contributions. The average number of citations in the 1961 SCI to the work of Nobel laureates (who won the prize in physics between 1955 and 1965) was 58, as compared with an average of 5.5 citations for other scientists. Only 1.08 percent of the quarter of a million scientists who appear in the 1961 SCI received 58 or more citations. We thought it possible that winning the prize might make a scientist more visible and lead to a greater number of postprize citations than the quality of his 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 it after that year. The 1957–1961 laureates were cited an average of 42 times in the 1961 SCI; the future prize winners (those winning the prize between 1961 and 1965), an average of 62 times. Since the prospective laureates were more often cited than the actual laureates, we concluded that the larger number of citations reflects the high quality of work rather than the visibility gained by winning the prize.
11. Since we were primarily interested in the influences upon the pure research of these scientists, and were using citations to measure that influence, we decided to omit any "review article" from consideration, for we were not interested in a review of the literature and thought that citations to an enormous amount of literature of this type would distort our results. Therefore, if a review article was the most heavily cited, we chose the next most heavily cited paper to include in our sample. In looking up the cited authors within these individual papers we were faced with the problem of what to do with collaborating authors. We decided to treat a paper as a single unit and locate information on all collaborators in the research team. To some extent, junior collaborators, many of whom were students of the senior authors, dropped out of our sample because no information could be found for them in *American Men of Science* (Cattell Press, New York). But it can be seen that the average level of eminence of cited authors is weighted against our hypothesis to some extent, since our sample included junior men of distinctly less eminence than their senior collaborators.
12. One of the limiting features of this study has to do with the collection of information on the scientists who are being studied. Perhaps the best source for independent information short of a questionnaire sent to a sample of physicists is the *American Men of Science* (AMS). The sample includes only men and women listed in AMS. Only about one-half of all cited authors appeared in these volumes. We wanted to see whether there were systematic differences in the types of scientists who appear in AMS and those that do not. We found, of course, that a large proportion of the men who were not found in AMS were foreign scientists. The second largest group of individuals who could not be found turned out to be students at the institution where the research was being done. It is not infrequent that older, more eminent scientists collaborate with their doctoral students. It should be added that AMS tends to include more academic scientists than scientists in industrial

concerns. We did find that the average number of citations to men in AMS was approximately 1.5 times that of those not found in it. We also found, as was to be expected, that the cited authors in the best work were more often found in AMS than those cited in work of lesser quality. To the extent that the AMS does not include the less eminent members of the scientific community our sample of cited authors overrepresents eminent scientists.
13. The extent to which unpublished work is being cited in leading journals is increasing rapidly, at least in physics. Second to articles published in *Physical Review* (American Institute of Physics, New York), private communications are the most-cited source of information in contemporary physics.
14. It would probably be safe to assume that more than 90 percent of the population of physicists have no awards.
15. Inclusion in the scientific elite could be a function of longevity if the bulk of citations went to older scientists. The data do not support this possibility. In fact, the pattern of citations by scientists in various age groups suggests that older scientists tend to cite work by older scientists; younger scientists tend to cite most often the work of other young scientists. More than 50 percent of the cited authors, however, were under 50 years old.
16. The so-called "Cartter" rankings of leading departments of physics were based on evaluations of 86 institutions "that reported the award of one or more doctorates in physics from July 1952 through June 1962." The ratings were based on a scale ranging from 5 (highest) to 1 (lowest). All institutions with a mean ranking greater than 4.0 were called "distinguished." There were nine such physics departments. A. M. Cartter, *An Assessment of Quality in Graduate Education* (American Council on Education, Washington, D.C., 1965).
17. These five articles included, for example, Lee and Yang's now famous paper on parity conservation. Three of the authors turned out to be Nobel prize winners; the others, members of the National Academy of Sciences.
18. Nobel laureates in physics who received their prize between 1950 and 1964 averaged 130 citations to their life's work in the 1965 SCI.
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NEWS AND COMMENT

Scientists in Politics: A Late Entry for Nixon's Group

Just as Scientists for George McGovern moved into the final phase of a year's vigorous, if sporadic, campaign activity last week, President Nixon's reelection committee came up with a counterpart group. In a brief announcement, the Nixon campaign headquarters said that a newly formed, 29-member Science and Engineering Council would work in support of the President, and might even live on after the election "to serve as another link" between the scientific community and the Nixon Administration.

The announcement, and a simultaneous press conference, left unclear precisely what the Nixon group might do to advance the cause of its candidate with only 3 weeks left before election day. There were indications that some members of the committee wondered the same thing themselves, but, even as window dressing for the President's candidacy, the group at least serves to round out the pattern of partisan activity by scientists and engineers in behalf of the presidential candidates that began with the Johnson-Goldwater campaign of 1964.

The Nixon group was put together, with a little prompting from the White House, by William O. Baker, the vice president for research of Bell Telephone Laboratories and a man who has emerged lately as something of a shadow science adviser in the Nixon Administration, and by California industrialist Simon Ramo. Baker, who, with Ramo, is cochairman of the committee, said they began organizing the group several weeks ago and that a small number of its members held an initial meeting on 5 October.

"I could tell by the gleam in his eye that Bill Baker was up to something," observed George Kistiakowsky, Eisenhower's science adviser and an active partisan in the McGovern camp. "He is a very influential man in Washington and perhaps even in the White House," Kistiakowsky needled.

During a news conference at Republican campaign headquarters, Baker emphasized that the advisory council was a purely spontaneous, grass-roots organization. "Independent, self-generated, not an instrumentality of government," was the way he put it.

As it happened, though, a minor gaffe by a campaign worker lent substance to the cynic's view that the council was closely tied to, if not conceived by, White House staff. Stapled to the back of a press release from the campaign staff was an internal memorandum adding the name of Lawrence A. Goldmuntz to the list of council members "per Pagnotta instruction." Frank R. Pagnotta is an administrative aide to Edward E. David, Jr. Until recently, Goldmuntz was the staff man in the White House Office of Science and Technology (which David heads) in charge of civilian technology.

Evidently an ardent Nixon fan, Goldmuntz appeared at the press conference and averred that Nixon was "the first President since Jefferson with a genuine interest in technology." For his part, Pagnotta said he had merely passed some information about Goldmuntz to Baker in response to an inquiry. "We get lots of inquiries," Pagnotta said. "But I don't know anything about any instructions."

The Baker-Ramo committee bears little resemblance to the science advisory group mobilized for Nixon in 1968 by Rear Admiral Lewis L. Strauss, who rounded up a hawkish assemblage heavily weighted with retired military men and conservative alumni of the Manhattan Project. This year's group contains a preponderance of industrial research administrators (17 of 29) mostly from aerospace and electronics corporations along the West Coast and in Texas—all of which only suggests that the makeup of campaign advisory groups probably is determined as much by the social orbits of the chairmen as by the candidates' attitudes toward science and technology.

As in years past, the Republicans