MEASURING THE QUALITY OF SOCIOLOGICAL RESEARCH: PROBLEMS IN THE USE OF THE *SCIENCE CITATION INDEX*

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The problem of assessing the "quality" of scientific publications has long been a major impediment to progress in the sociology of science. Most researchers have typically paid homage to the belief that quantity of output is not the equivalent of quality and have then gone ahead and used publication counts anyway (Coler, 1963; Crane, 1965; Price, 1963; Wilson, 1964).[1] There seemed to be no practicable way to measure the quality of large numbers of papers or the life's work of large numbers of scientists. The invention of the ***Science Citation Index*** (SCI) a few years ago provides a new and reliable tool to measure the significance of individual scientists' contributions. Starting in 1961, the SCI has listed **all** bibliographic references appearing in an increasingly large number of journals.[2] The number of citations an individual receives may be tabulated and used as an indicator of the relative scientific significance or "quality" of that individual's publications.

Until now the SCI has not included sociology journals in its files, but director Eugene Garfield informs us that in 1970 the SCI file will include major sociology journals. References made in these journals will also be added to the 1961 and the 1964—1969 files. Thus, we will be able to count the number of references made in recent years to any particular article, book, **or** sociologist, and we will be able to quickly generate lists of these facts.[3] This should lead to **major** advances in the sociology of sociology. In practically all studies of the social organization of sociology the quality of work of individual sociologists and of particular institutions is an important variable. With the impending addition of sociological jour. nals to the SCI file, this might be a propitious time to examine certain problems involved in the use of the SCI to measure the quality of work. We have been using this tool in our research in the sociology of science (basically physics) for several years, and we present below a discussion of several problems we have encountered and the solutions we have found for them.

There is some supporting evidence for the assumption that the number of citations a person receives is a roughly valid indicator of the significance or "quality" **of** his publications. In a thorough study of measures **of** scientific output, Kenneth E. Clark (1957:chapter **3)** asked a panel of experts in psychology to list the psychologists who had made the most significant contributions in their field.[4] 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 concluded that the citation count was 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 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,*** compared to an average of 5.5 citations for other scientists. Only 1.08 per cent of the quarter of a million scientists who appear in the 1961 SCI received 58 or more citations (Sher and Garfield, 1965).[5]

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[1] Researchers 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 individual biases of evaluators are frequently encountered.

[2] Compiled under the direction of Eugene Garfield, the SCI in 1961 listed all citations made in 613 journals; the 1962–63 journals have not been indexed. In **1964,** 700 journals were covered, and in **1965, 1,147** journals. Virtually all important journals in the natural sciences are included.

[3] Even though the SCI files include only references made in journals, they include citations ***to*** all books or other publications cited in the journals. It is doubtful that the citation patterns in journal articles will differ substantially from the citation patterns in books. We would guess that there would be a high correlation between the number of an author's journal publications and the number of his books.

[4] For another study using number of citations as a measure of quality of scientific work, see Bayer and Folger (1966).

[5] We thank Dr. Sher for making available some of his unpublished data. By way of emphasizing the difference in the numbers **of** citations received by laureates and average scientists, we would also point out that many scientists do not appear in the SCI, and

We thought it possible that winning the prize might make a scientist more visible and lead to a greater number of post-prize 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 the prize after. The 1957–1961 laureates were cited an average of forty-two times in the 1961 SCI; the future prize winners (those winning the prize between 1961 and 1965), an average of sixty-two times. Since the prospective laureates were more often cited than the actual laureates, we concluded that the larger number of citations primarily reflects the high quality of work rather than the visibility gained by winning the prize.[6] Here, we have used receipt of the Nobel prize as an independent measure of the quality of a scientist's work. In recent studies we have found measures of quality based upon citations to be highly correlated with other measures of eminence. For example, we found the quality of work, measured by SCI data, of 120 university physicists to be correlated ($r = .64$) with the number of awards they had received (Cole and Cole, 1967). These data offer further support for the use of number of citations as an indicator of the scientific significance of published work.

"If a paper presents an error that is important enough to elicit frequent criticism, the paper, though erroneous, is probably a significant contribution."

Citation counts enable us to distinguish the extent of contributions by various types of scientists. Consider, for example, the citations to one relatively productive and eminent group of scientists: members of university departments of physics in the United States. Among a representative sample of academic physicists totaling 1,308, only 2 per cent had sixty or more references to their work in the 1961 SCI, 12 per cent had between fifteen and fifty-nine citations, and *86* per cent had fewer than fifteen citations. Thus only a small fraction of university physicists received as many citations as did the average laureate, who received fifty-eight. In short, very few scientists are heavily cited, and there are distinct differences in the number of references to physicists whose quality of research has been validated by other measures of eminence.

Clearly, increasing corroborative evidence suggests that citations provide a good, if rough, indicator of the quality of research output in the natural sciences. With the inclusion of sociological journals in the SCI file we will be able to investigate a number of areas of interest to the sociologist of sociology. For example, we can test whether the correlations between citations and various independent measures of quality of research are comparable to those found in physics, psychology, and other disciplines. Whether stronger or weaker, the correlations should be interesting since they should give clues to the different structures of the fields.

We can also examine the relationship between quality and quantity of research output and determine whether sociologists are rewarded for the bulk or the quality of their publications. Next, we can estimate the extent to which the quality of a sociologist's research affects his visibility to the sociological community. Finally, we can investigate patterns of communication and identify intellectual linkages within sociology. Through an examination of citation patterns we may begin to answer questions about shifting foci of attention and cross-fertilization between subfields of our discipline. With the measure of quality of output obtainable from the SCI, a series of comparative studies will become researchable for the sociologist of sociology. We will be able to make meaningful empirical comparisons of the organization and intellectual structure of different disciplines.

Although citations provide us with an improved method of assessing the quality or impact of a scientist's research, there are, of course, a number of definite problems in the use of this measure. Most of these are basically substantive as opposed to technical and we shall consider the substantive problems first. In doing *so,* we shall draw upon data from a number of studies in the sociology of science.

Errors in Evaluation

There may be occasional "errors" in the evaluation of scientific works. The significance of work done by a scientist is not always recognized immediately, for new ideas, especially those that lead to changes in basic scientific paradigms, are sometimes resisted or ignored (Barber, 1962). Some great scientific innovators remain obscure in their own time only to be accorded posthumous recognition. Mendel is a classic example of a scientist whose work was unappreciated by his contemporaries but greatly honored by scientists of later generations. Using citations to measure quality, we may sometimes misclassify work that is currently being "resisted" or that has been judged inadequately. Since history alone will reveal which work has been resisted or misjudged, this flaw in the procedure is inevitable. However, the problem of resistance to significant contributions may be less important in contemporary science than it was in the past. In a recent study of delayed recognition of scientific discoveries we examined citation patterns over time (Cole, 1970). For a sample of 177 papers published in *The Physical Review* we found a strong correlation between the number of citations they received one year after publication and the number they received after three years ($r = .72$). When we examined citation patterns to papers published between 1950 and 1961 in different scientific fields we found a similarly high correlation between the number of citations received by the papers in the 1961 SCI and the number received in the 1966 SCI. Although an ideal study of delayed recognition of scientific dis-

that the modal number of citations to the work of men who do appear is one.

[6] These statistics are based upon the works of twenty-four of thr twenty-eight living laureates—those who won the prize in physics as of 1965. The four living laureates who won the prize before 1957 were excluded so as not to introduce an age bias. These computations include non-American laureates; when they are excluded, the average number of citations to the works of United States Nobelists is 68.

coveries would require citation data for a longer period, these data at least suggest that the communication and evaluation systems of modem science work effectively and that there are relatively few cases of research that go unrecognized at the time of publication and then turn out later to be significant.

It is quite possible that our conclusions on the relative unimportance of errors in evaluation will not apply to sociology, for there is probably less consensus in sociology than in physics on the criteria to be used in evaluating work. Nevertheless, with the inclusion of sociology journals in the SCI file we shall be able to compare citation patterns over time in sociology with those in other fields. Are good papers more likely to be ignored in sociology than in the natural sciences?

Critical Citations

Citations may refer to papers that are being criticized and rejected rather than utilized. It is unlikely, however, that work of little value will be deemed significant enough to merit extensive criticism. If a paper presents an error that is important enough to elicit frequent criticism, the paper, though erroneous, is probably a significant contribution. The significance of a paper is not necessarily determined by its correctness. Much work done by the great historical figures of science was in some sense "wrong" or mistaken (Kuhn, 1962). It is unlikely that any work which is wrong without being a "fruitful error" will ever accumulate very many citations. Let us examine this problem more closely. Suppose we had a total of one thousand citations to scientific work, and as many as one hundred of these were to work being criticized or rejected. The majority of these "critical" citations are dispersed among a large number of papers, such that most papers cited critically would receive no more than one or two citations. The same dispersion is found for "positive" citations. Let us say that one paper actually receives as many as twenty-five "critical" citations. We suggest that these few pieces of research that stimulate wide criticism have, in fact, stimulated other research. Consequently, it must be considered mistaken but significant; it must be seen as work which has had an impact on future scientific research. In sociology most of us would certainly agree that a paper such as Davis and Moore's "Principles of Stratification" (1945) was a significant contribution even if it has elicited many critical responses.

Treating All Citations as Equal Units

If each citation to a paper is given equal weight, errors in assessing the impact of research may follow. A paper that is cited widely by first-rank scientists should not be equated with a paper cited predominantly by scientists who have made only minor contributions. Since citations do not have equal meaning, should we consider classifying them by the characteristics of the citer? In effect, would a citation count weighted for the quality of research produced by the citer be a superior measure of the quality of scientific papers?

To answer this question, we did a detailed study of the citers of 171 of our 1,308 university physicists. For each physicist in the subsample, we collected data on a random sample of his citers. This enabled us to classify each physicist by the characteristics of his citers. By giving each citer a score that depended upon the number of citations his own works had received, we were able to measure the quality of work of each *subject* physicist by the quality of work of his citers.[7] The correlation between this index and the total number of citations received by the subject physicist was $r = .40$. We believe that this correlation is not higher because a disproportionate number of citations are made by a small group of scientists who publish heavily and are themselves generally highly cited. Consequently, the same men are likely to be found among the citers of high-quality work and low-quality work. Thus, 62 per cent of the citers of relatively low-quality work and 70 per cent of the citers of relatively high-quality work received ten or more citations to their own work (J. Cole, 1969). Although the two indices of quality—the total number of citations received and the index based upon the quality of the citers—are not highly correlated, they yield similar correlations with other variables (see table 1). On the basis of the correlation

TABLE 1. CORRELATION COEFFICIENTS FOR TWO INDICES OF QUALITY OF SCIENTIFIC RESEARCH AND SEVERAL CHARACTERISTICS OF PHYSICISTS

Characteristics of Phvsicists	Number of Citations to Phvsicist	Index Based on the Number of Citations to the Citers of the Phvsicist
Age	.11	.10
Academic rank	.12	.10
Number of awards received	.29	.20
Prestige of highest award	.41	.20
Prestige of Ph.D. department	.11	.13
Rank of present academic department	.19	.22

Note: Statistics based on a sample of 861 citers.

coefficients presented in table 1 we conclude that a citation index that includes the characteristics of the citing authors would probably yield substantive conclusions similar to those of an index that treats all citations as equal in value.

Quantity and Quality of Research Output

The number of citations a scientist receives may in part depend upon the quantity of his output. A scientist who publishes a large number of papers and receives only a few citations for each may accumulate as many citations as the scientist who publishes only a few papers that are heavily cited. In our sample of 120 university physicists,

[7] Scores were assigned in the following way. Citers whose works had received one hundred or more citations in the 1965 SCI were given a score of 5, citers with fifty to ninety-nine citations were given a score of 4, those with twenty-five to forty-nine citations received a score of 3, those with ten to twenty-four citations received a score of 2, those with one to nine citations received a score of 1, and those whose works were not cited received a score of 0. These scores were then totaled for all citers, and each subject physicist was classified by the total index score.

we found a correlation of .60 between the number of papers a physicist published and the number of citations listed after his name in SCI. However, the correlation between number of papers and number of citations to the three most frequently cited contributions of the scientists (a measure that could not be an artifact of the quantity of publications) is .72 (Cole and Cole, 1967). This is the opposite of what we would find if the total number of citations were primarily a function of the total number of papers. We conclude that the total number of citations could serve as an adequate indicator of quality.

In several of our studies we chose to use more refined measures of citations because they seemed substantively more suitable. For example, in a paper analyzing the relationship between the quantity and quality of the output of physicists, we used the number of citations to the three most frequently cited contributions by each physicist as a measure of quality. This was done in order to eliminate any possible effects of sheer productivity on total citations. Moreover, since a contribution in physics does not typically appear in a single paper but is usually presented in a series of papers, we used citations to the year's output rather than to single papers as the unit of measurement (Cole and Cole, 1967).[8]

"A citation index that includes the characteristics of the citing authors would probably yield substantive conclusions similar to those of an index that treats all citations as equal in value."

A further word is necessary on the relationship between the quantity and quality of a scientist's research. Since in physics these two variables are highly correlated, we may conclude that where citation counts are not readily available (as in historical research) publication counts are roughly adequate indicators of the significance of a scientist's work. As noted above, with the inclusion of sociology journals in the SCI files we shall be able to discuss the extent to which the quantity and quality of research output are correlated in sociology. Where citation counts are available, they should be used, for they have definite advantages over counts of publications. In a recent paper we presented data to show that physicists who were frequently cited, yet had not published numerous papers, received as much institutional recognition as did physicists who were both prolific and widely cited. These "perfectionist" physicists turned out, in fact, to be the most highly recognized scientists in our sample (Cole and Cole, 1967). We hypothesize that quantity of output may be more heavily rewarded in sociology than in physics. When scientists cannot agree upon what high quality is, their concern is likely to be with quantity of output.

Size of Scientific Fields

Differences in the size of various scientific disciplines pose another potential problem in the use of citations as a measure of quality. Comparisons of the works of scientists in different fields must take into account the number of people actively working in those fields. With two writings of equal importance, the one in physics, the other in chemistry, the latter might be expected to receive more citations merely because the field of chemistry is larger than that of physics. The number of citations might be an artifact of the number of chemists and physicists, the number of journals in each field, and the amount of work that is being published. The same applies to specialties within a field. If there are more publications in solid-state physics than in elementary particles, we might expect that of two papers of equal impact in the two fields, the one in solid-state physics might receive the greater number of citations.

Though at first this position seems plausible, under closer scrutiny it does not appear logically sound. While there are more citations being produced in solid-state physics than in elementary particles, there is also more work being done in solid-state physics and, consequently, more literature that is potentially citable. Therefore, the likelihood that a work will receive more citations simply because its field is larger does not logically hold. Furthermore, if size of field were related to number of citations, we would find a positive correlation between these two variables. This would be similarly true for specialties within a given field. Men working in larger specialties would receive more citations than men working in smaller specialties. We have evidence that, at least in physics, there is no relationship between the size of a specialty and the number of citations to the work of men in that specialty. According to the National Science Foundation (American Science Manpower, 1966:183) in 1966 there were 4,593 solid-state physicists and 1,833 elementary-particle physicists. Our data on 1,308 university physicists show no significant differences in the rate of citation to men active in these two specialties. In the 1961, 1964, and 1965 editions of SCI, solid-state physicists had a mean of seventeen citations while elementary-particle physicists had a mean of nineteen citations.

In exceptional cases, of course, there may be a relationship between the size of the field and the number of citations to men working in that field. For example, in a specialty with only a few scientists working in it the number of citations to their works would necessarily be limited by the small total number of citations to work in that specialty.

Contemporaneity of Science

Papers in physics now have a half-life of no more than five years; that is, at least half the citations that appear in a given year are to works published in the five pre-

[8] 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 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 Crane (1965), which treats a series of four papers on the same topic as a "major" publication and single papers as "minor" publications.

ceding years. The half-life of sociological papers is only slightly longer. We must take this into account in comparing the publications of scientists who made their most important contributions at different times. Two papers which were originally of equal quality may have a different number of citations in the 1961 SCI if one paper was published in 1941 and the other in 1959. This would not matter if the researcher were interested in the *current* significance of both papers. However, he might want a measure of quality that is not time-bound. To handle this problem we developed a technique of weighting citations (Cole and Cole, 1967) wherein older citations were given greater weight than recent citations. For example, since 70 per cent of the citations in a particular field (physics) are to works published within the preceding five years and 4 per cent are to works published more than twenty years before, we gave a weight of 17 (70 per cent divided by 4 per cent) to works published twenty or more years before the date of citation. Although weighting citations for their age seems to be substantively necessary, we found a high correlation ($r = .80$) between the total of weighted and of unweighted citations. When we compare the number of weighted and unweighted citations to physicists' publications in their three "best" years, we get an even higher correlation ($r = .96$). These high correlation coefficients once again illustrate the interchangeability of indices (Lazarsfeld, 1958). Since the number of a physicist's weighted citations is correlated so highly with the number of his unweighted citations, substantive conclusions would probably not be affected if the weighting technique were not used.

If weighting is used, one more problem must be considered. Derek Price (1963:81) 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." In the study of delayed recognition we found that it was in fact empirically correct that papers that were cited in 1961 had on the average roughly the same number of citations in 1966 (Cole 1970). Thus, at least for short periods, it is likely that Price was correct. The weighting technique discussed above is not at odds with the model suggested by Price—it is not meant to predict the *number* of citations received by papers in the past but to control for the increasing number of citations. Due to the exponential growth in science, papers presenting important scientific contributions today are receiving many more citations than similar papers did in the past. A paper that receives five citations today is not among the most heavily cited; but a paper published in the nineteenth century that received five citations would probably have been among the most heavily cited of papers. Thus, in comparing works published in different periods we must standardize for the total number of citations made.

Integration of Basic Ideas

Widespread basic ideas are often utilized in papers without explicit citation to their well-known source. Who today cites that paper in the *Annalen der Physik* as the source of $E = MC^2$? For scientists who have achieved eponymy, there may be a decline in the number of formal citations to their work. The "Mossbauer Effect" is an example of a recent contribution to science that has been thoroughly integrated into the common body of knowledge and is infrequently given formal citation. Let us examine such cases in terms of our measure. A scientist who makes discoveries that lead to eponymous recognition probably will also produce other research of the first rank that will be heavily cited by the scientific community. Thus most Mossbauers will be classified as "high quality" despite the fact that one of their outstanding achievements receives relatively few formal citations.[9] It is true, however, that integration of a discovery into the body of scientific knowledge may lead to errors in assessing the quality of that discovery through citations. The use of citations as a measure of quality does involve a degree of error. However, evidence presented above and below leads to the conclusion that such error, which may be substantial in individual cases, will not be significant in considering the publications of any fair-sized sample of authors.

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The problems in using citation counts as measures of the quality of scientific output discussed above were primarily substantive. We now turn to a consideration of two problems that are primarily technical in nature but nonetheless important to those who might use citation indexes.

Citations to Collaborative Papers

Citations to all single-authored papers are recorded in the SCI. Citations to collaborative works are listed only after the name of the first-named author. Since many collaborative papers list authors alphabetically, one might think that collaborators whose names begin with letters late in the alphabet would be misclassified if we counted only citations appearing after their names in the SCI. Data from our research suggest that omission from the SCI of citations to collaborative work does not present a formidable problem. For the sample of 120 physicists we have a full range of citation data for each author: citations to works he produced alone, citations to collaborative works on which his name appeared first, and citations to jointly authored writings where he was not the first-named author. The latter information was obtained by looking up the author's collaborative papers in *Science Abstracts* and then looking up those papers on which he was not first author in the SCI. The correlation is .96 be-

[9] Although the Mossbauer Effect is not as heavily cited as some other major discoveries because of the degree to which it has been integrated into the general fund of knowledge, Mossbauer's other publications received a total of fifty-three citations in the 1965 SCI. Thus, in terms of our measure, his work is rated about equal to the average laureate and National Academy member.

tween a straight citation count and total citations (including citations to collaborative work on which the physicist was not first-named author).

We also arrayed the **120** physicists in two ranked lists, one according to a straight citation count and the other according to total citations; the Spearman rank-order correlation coefficient is .85. Although the outcome was to some extent predetermined by the size of the zero-order correlations, we decided to make a final test of the relationship between straight counts and counts that included citations to collaborative work on which the physicist was not the first-named author. We divided our sample of **120** physicists into two groups: the first group included physicists whose last names began with A to M; the second group was composed of those whose names started with N to Z. For each group we calculated the percentage of total citations both for single-authored papers and for first-author collaborative papers. By this technique we could estimate the extent to which scientists whose names begin with letters late in the alphabet were "deprived" of their due in terms of citations to work they had actually helped produce. The data indicate little difference between the groups. Sixty-seven per cent of the citations in the A to M group and **71** per cent of the citations in the N to Z group were to single-authored or first-author collaborative papers. The small difference in the direction opposite from that expected suggests that the omission of citations to collaborative papers on which the author was not listed first does not affect substantive conclusions. For the most part, differences that we did find were among scientists whose work was of the first rank. For example, Murray Gell-Mann had almost six hundred citations to his publications in one volume of the index. When we looked for citations to his collaborative research where he was not first-named author, we found over one hundred additional citations. While these add substantially to Gell-Mann's total of six hundred, they do not affect our classification of the quality of his work. However, when we want to study the quality of specific papers, we must look up collaborative papers under the name of the first author. Also, the researcher must be aware that because of the procedure adopted by the SCI, he may make errors in measuring the quality of work of a particular scientist.

Clerical Problems

Warren Hagstrom **(1968)** has recently pointed out other technical problems in the use of citation counts. First, he notes that there are clerical errors in the list of citations. Second, the works of two authors may appear under one name. If, for instance, there were two E. McMillan's, one the Nobel physicist and the other a relatively unknown sociologist, the citations of both men would appear together under the same name. Although these two problems make for inefficiencyin collecting citation data, both can be handled. Clerical errors probably occur randomly throughout the index. Therefore, while the counts may be off slightly, there is no reason to believe that there are patterned errors in the listings. The second problem is more vexing, but can be handled by careful compilation of the citation data. The index lists along with the cited author the names of his citers and the title, volume, and page of two journals: the journal that published the article and the one that cited it. Consequently, it is possible to identify the articles produced by the scientists that one is interested in. It is relatively easy to distinguish between scientists working in different fields; it takes more effort to distinguish between two physicists who happen to have exactly the same name.[10] Thus neither problem materially detracts from the value of the index as a measure of the quality of scientific output.

Conclusion

The data available indicate that straight citation counts are highly correlated with virtually every refined measure of quality. Correlations between straight counts and weighted counts and between straight counts and those that take into account citations to collaborative research in which the author is not the first-named author, are all greater than .80. Consequently, it is possible to use straight counts of citations with reasonable confidence. In some research situations it may be substantively more appropriate to use weighted counts or to take into account collaborative work, but the use of these refinements is not of methodological necessity.

It is clear that there are problems in using citation counts as indicators of the quality of scientific output. Nevertheless, the value of using them as rough indicators of the quality of a scientist's work should not be overlooked. To interpret small differences in the number of citations as meaningful, however, would be unwise. It would not be accurate, for example, to say that scientists who received six or seven citations to their publications in the **1961** SCI did better work than those who received four or five citations. In other words, citations should not be used as a fine measure of quality. Nevertheless, large differences in the number of citations received by scientists do adquately reflect differences in the quality of their work.

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