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35. I thank J. Schubart, Astronomisches Rechen-Institut, Heidelberg, for providing a copy of the *n*-body integration computer program written by P. Stumpff and himself; and K. Aksnes, Smithsonian Astrophysical Observatory, for writing programs for converting position and velocity components into Keplerian orbital elements, and vice versa. I also thank Elizabeth Roemer and Fred L. Whipple for commenting on the manuscript.

Citation Indexing and Evaluation of Scientific Papers

The spread of influence in populations of scientific papers may become a subject for quantitative analysis.

J. Margolis

As a result of the recent expansion of scientific literature, more time and effort are being devoted to the selection of what is to be read than to the actual reading. To cope with the demand there has been a corresponding growth of abstracting and indexing services as well as of sophisticated computer-based systems for information storage and retrieval such as the "Medlars" development (*1*). Against this rapidly shifting background it is almost impossible to say what is the prevalent attitude of users toward scientific publications, but presumably most readers still try first to ascertain the nature of an article's contents by reference to its author, title, or other subject descriptors.

The use of the bibliography as a point of departure is a relatively new approach, which became practicable only with the compilation of citation indexes (*2*). It is self-evident that the contents of an article determine to what papers the article will refer. Perhaps less obvious is the fact that in some respects the bibliography appended to an article specifies uniquely, if indirectly, its subject (*3*). The practice of appraising a paper by noting the references it cites is probably quite common. When a busy research worker scans the current periodicals he may be able to decide at once from the list of references whether an article with an interesting title is worth reading. He may, for example, be inclined to reject a paper that does not mention some important contributions on the subject. More generally, each item on the list provides a clue, and the total "spectrum" of such clues will often identify the theme. For those who can read the code, this identification is an act of instant and effortless recognition—effortless, that is, compared with evaluation of any part of the contents. However, this approach can be useful only to the reader who is already familiar with the literature, and, in any case, it depends on finding the article first, either by chance or by way of the existing subject-oriented information channels.

The appearance of a comprehensive *Science Citation Index* (*4*) has made it possible for the first time to systematize this procedure for general use. The structure and operation of the *Index* have been described in detail elsewhere (*2, 5–7*). In essence, it is produced by listing all the items cited in papers (sources) in a multidisciplinary selection of scientific, technical, and medical periodicals (613 journals in 1961 and more than 1500 in 1966). The items are in the form of line entries, arranged alphabetically by the name of the first author, followed by the year, name of the journal, volume, page, and certain other coded information. Under each citation are listed all the citing (source) articles, identified in a similar manner. The *Index* is produced quarterly (with a cumulative issue at the end of each year) and lists only the source papers published in the journals being processed at the time. No such restrictions apply to the cited items. Anything that may appear in the list of references, from "personal communications" to citations of Lewis Carroll or Confucius, is a legitimate entry.

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For the purpose of retrieving the most up-to-date information, the search of the current *Index* usually starts with one or more specific items, which, one believes, are bound to be cited by any article that is worth reading. Since publications belonging to the recent "research front" (*8*) form a tightly interwoven network and an average paper carries approximately 15 references, the user need not be particularly careful, for a generic search, in his selection of entry points. A search based on, say, five respectable citations will, almost inevitably, lead to all the important papers in the source journals but, probably, also to a number of irrelevant papers. The latter must be treated as "noise" (*2*), which can, however, be reduced by various screening procedures. First of all, the author's name and the source journal may already be a sufficient reason for immediate exclusion. Next, by reference to full titles listed in the *Source Index* that accompanies the *Citation Index,* a large proportion of the irrelevant items can be eliminated. Using a process of "bibliographic coupling" (*6*), one may narrow the search at will by accepting only those items which cite two or more of the starting references. Lastly, with the aid of the existing subject indexes, one may use any combination of citation and subject descriptors as a basis for an automatic search program (*3*).

One of the most attractive features of citation indexing is the fact that it is essentially an algorithmic process (*9*). Once the panel of source publications and the computer system have been specified, the rest does not require specialized scientific knowledge. However, this process cannot eliminate all errors in the original material, so the *Index* contains many inaccuracies, mostly in the volume or page number, and variations in initials, which may sometimes cause the same item to be listed as two or more nonconsecutive entries. By and large, these are only a minor nuisance and do not cause failure in retrieval for the user who starts with a few definite references and hopes to be guided to the most recent papers that have cited these. All the same, this means that the *Index* cannot be used without some intellectual effort, as it could be were it not for these defects.

Errors, variations in spelling, and homographs—that is, multiplicity of authors with identical names and initials—are a more serious drawback in statistical studies. This can be corrected for or tolerated when one is dealing with individual items, but at present there appears to be no way of reducing a survey of author citations to clerical work, let alone to computer processing.

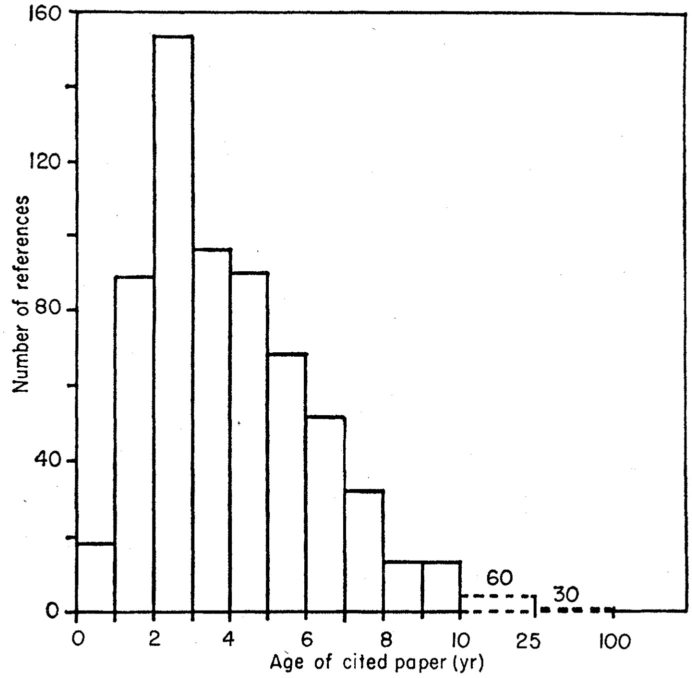
Another remarkable aspect of a citation index is its self-organizing nature, which stems from the "ancestor"-versus-"descendant" relationship between the cited and citing publications, respectively. Every time an author refers to other papers he becomes an unwitting contributor not only to the size of the system but also to its integration. What is more, the very existence of the *Science Citation Index* will almost certainly have various feedback influences on the writing and citing habits of future authors. This would, in turn, be reflected in the contents of the *Index* and could progressively increase its usefulness. While it is very difficult to anticipate all that can happen to such a self-regulating system, some of the possibilities are examined below.

Citation Habits

It is self-evident that the healthy operation of citation indexing depends on citation habits. These must vary enormously from article to article, but statistical regularities discovered in the *Index* point to the existence of certain vague norms of citation behavior (*10*). Authors of original contributions are probably the best-qualified critics of the literature in their field, but, since reviewing is not their primary aim, their selection of references is apt to be somewhat casual. Granted that really important papers are usually cited and trivial ones usually ignored, there remains a wide area in between which is influenced by familiarity, language, loyalty, and self-interest, all of which contribute either to the general level of semantic noise or to retrieval loss. Extended use of citation indexing may well lead to general improvement of standards, as a result of editorial policy and recognition of moral obligations on the part of the authors. But a more realistic motive, and one that enhances the self-organizing features of the system, could be the author's self-interest in feeding the *Index* with relevant information so that he may reach his readers. He knows that his article may be overlooked if it does not cite pertinent literature. Haphazard citation of respectable publications of a general nature, such as textbooks or monographs, may also be against the author's interest because many readers may be discouraged, by the large numbers of entries for such frequently cited works, from using them as a point of departure. Thus, from the writer's point of view it pays to exercise care in the choice of bibliography.

On the debit side, such a user-oriented outlook might promote an unjustifiable increase in the number of references, since this number, of course, determines the number of times the source paper is listed in the *Index*. However, even now many leading journals discourage the inclusion of exhaustive bibliographies, and an extension of this policy may be sufficient to prevent such a practice, especially because it would be in the interest of "evaluation-oriented" writers not to be overgenerous in acknowledging a debt to others.

In his book *Little Science, Big Science* (*11*), de Solla Price remarks that most scientists "have a secret hope that some standard will be found for the objective judgment of their own caliber and reputation. . . ." The value of a scientific paper can be measured by the influence it has on others, and citation indexing provides, as a by-product, a measure of the impact of articles, authors, and journals (*12, 13*). For example, of all cited papers listed in the *Science Citation Index* for 1961, 75 percent were cited once only, 12 percent twice, 6 percent three times, and only about 1 percent six times or more (*8*). It is reasonable to expect that the best contributions would have been among those cited most, while relatively unimportant papers would have attracted few, if any, citations. Among papers cited only a few times, however, many may have failed to be noticed because of the language or journal in which they appeared, or for other reasons which have nothing to do with their quality; indeed, some may even be too far ahead of their time (*6*). Moreover, a high score cannot be taken at face value in every case. A paper may be heavily cited because it provoked criticism or described a minor improvement of an established method, or simply because the author himself cited it frequently in subsequent publications (such citation of one's own work may, of course, be justified). Thus, impact and quality do not always



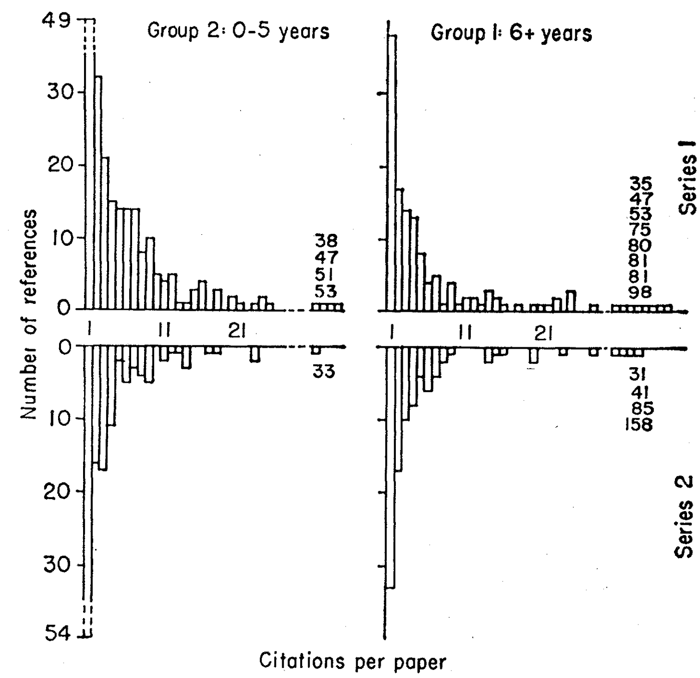


Fig. 1 (left). Age distribution for references cited in a sample of biomedical papers published between 1961 and 1965. Fig. 2 (right). Frequency distribution of citations to papers in series 1 and 2 of Table 1. Papers cited more than 30 times are grouped together, and indicated by individual blocks and figures opposite.

go together, and it would be preposterous to suggest that the most cited author would be automatically entitled to a Nobel prize (*14*).

Quite apart from accidental or deliberate irregularities, a yearly "popular poll" of a mere 0 to 6 "votes," the range of "votes" received by about 99 percent of publications, would suffer from too much random variation to be reliable. This sampling error might be reduced, by fields, in one or the other of two ways: through a wider coverage of sources or through a cumulative citation count over a number of years. The latter procedure, however, introduces another factor—the survival value, which, on its own, may be more telling than the immediate impact.

Survival Value

According to results of the present pilot survey of recent papers (Fig. 1), which agree with the earlier data from the *Science Citation Index* for 1961 (*8*), the average citation rate tends to reach a peak during the 3rd year after publication and to fall off by 50 percent every 3 to 5 years thereafter. This, however, is the pattern for the entire population of cited papers and not necessarily for the top 1 to 2 percent, which could conceivably have quite a different survival profile. An examination of 200 articles published in 1958 and cited a total of 1600 times in the two issues of the *Index* (1961 and 1964) showed that papers cited, on the average, 1, 5, 13.4, or 23 times in 1961 were likely to be cited, in 1964, 0.4, 2.5, 9.5, or 16 times, respectively (the coefficient of variation of these estimates being in the region of 30 percent). Only 12 percent of the 200 articles were cited more often in their 7th than in their 4th year. It appears, therefore, that most articles which are heavily cited in any one year continue to be cited a proportionately large number of times in subsequent years but that their survival profile is not very different from the average. On the other hand, a small proportion of papers have survival profiles showing unusual longevity. Future studies of this kind may show whether there is anything special in these contributions which could qualify them for some "Journal of Really Important Papers" (*8*).

Indirect Influence

The total influence of a scientific paper extends beyond its direct impact, for its influence may continue, and possibly grow, through successive generations of other publications. When comprehensive citation indexing is a few years older, it will be interesting to follow these relationships quantitatively and determine whether a high impact value tends to run through bibliographic chains. Since only a small percentage of papers are heavily cited, one would imagine that "clones" of such papers would be quite conspicuous against the general background (*15*). In the present context it is convenient to speak of the references cited in an article as the article's "ancestors" and of the subsequent source papers which cite the article as its "descendants."

On the basis of the two complete issues of the *Index* (for 1961 and 1964) available at the time of writing (*4*), only a limited survey of the "ancestry" and "progeny" of highly cited papers was possible. First, a random selection, from the *Index* for 1964, of "research articles" (as distinguished from reviews and monographs) published in 1961 which the *Index* showed as having received 10 or more citations in 1964 was retrieved. Each of these "series-1" articles was matched by an article which had appeared in the same journal and volume and which was cited once only in 1964 ("series 2"). Next, all the papers cited by articles in the two series were located in the *Index* for 1961, and a count was made to determine their citation scores in 1961. The neighboring entries in the *Index* which

Table 1. Data, from the *Science Citation Index* for 1961, for papers cited in two series of papers published in 1961 which were, in turn, cited in subsequent papers, as noted in the *Index* for 1964. For the meaning of series 1, series 2, group 1, and group 2, see text. (For frequency distributions, see Fig. 2.)

1	2	3	4	5		6		7	
Series No.	No. of papers in series (all published in 1961)	No. of citations, in *Index* for 1964, of papers of col. 2	Average, per paper, for the citations of col. 3	No. of papers cited by papers of col. 2		No. of times papers of col. 5 were cited in *Index* for 1961		Means, per paper, for citations of col. 6	
				Group 2	Group 1	Group 2	Group 1	Group 2	Group 1
1	17	238	14 (10–24)	214	134	1331	1230	6.2	9.2
2	17	17	1	174	97	522	705	3.0	7.3

were discovered to be multiple listings of a single item resulting from inaccuracies in volume or page number as given by the citing article were also included in the count. All the same, as noted above, a survey of this type is subject to error because of inaccurate entries which cannot be easily traced.

When the citation scores were tabulated according to the age of the cited papers in 1961, the papers fell into two groups: (group 1) those published before 1955 and (group 2) the more recent ones (0 to 5 years old). In group 2, the mean citation score for items referred to in the articles of series 1 was twice as high as the mean citation score for items referred to by papers of the control series (series 2) (see Table 1 and Fig. 2). This might possibly mean that the authors of these more-cited papers were more selective in the choice of references, especially of references to recent papers which had not had time to become widely known through textbooks and reviews. The possible argument that the *Index* could have influenced the reading habits of the authors may be discounted, because, unlike the later *Indexes*, which are completely up to date, the *Index* for 1961 was based on experimental files and was not generally available till 1964.

In the second part of the survey, these relationships were traced forward. A sample of papers published in 1958 and cited 10 or more times (mean number of citations, 11.6) in 1961 was compared with papers cited once only in 1961. The citing (source) articles were followed into the *Index* for 1964. The mean citation score for a descendant paper of the first series was 2.5 citations; that for a descendant paper of the second series was 1.0.

When all the data are combined and three generations of papers separated by 3-year intervals are considered, the relationship between the citation score of a paper and the scores of its ancestors or references (that is, the papers it cites) and its descendants or citations (the papers that cite it) would appear to be roughly as shown in Table 2. The tendency for the ancestors to have higher citation scores than the descendants may at first appear surprising, since the *Index* covers only a very select fraction of the citing journals while the population of ancestors is quite unrestricted. However, in this system, selection operates backward, because it is the descendants who choose their ancestors.

It must be recognized that the figures based on a 3-year span between generations are not optimal but are dictated by the fact that the *Indexes* for 1961 and 1964 were the only ones that had been published at the time of writing. For instance, a paper written in 1958 and cited 12 times in 1961 is likely to have been cited 15 to 18 times in 1960 and 60 times in the period 1958–64 (see Fig. 1). Second, the arithmetical mean values do not convey the whole picture, because the frequency of citations does not follow a normal distribution but follows a reciprocal cube curve (see Fig. 2), the number of papers cited n times decreasing approximately as $1/n^3$ (*8*). When this is taken into account, we find that at least 50 percent of "fertile" articles are directly linked with a small "elite" group of equally fertile ancestors and successors.

Networks of Publications

Although it is quite reasonable to expect general statistical trends in populations of papers, the place of an individual contribution can be determined only from its unique relationship to the whole body of scientific literature, which resembles a self-organizing growing network, where every paper is linked with its bibliographic ancestors and descendants. The texture of this network is not homogenous but shows a hierarchy of structures and substructures depending on the scale of observation. If we had a plot of these interrelationships for all papers published in the past century, during which time the present form of scientific journals became established, we could expect to see a reasonably faithful map of the history of science outlined by bibliographic connections (*15*). It would reflect the development, divergence, and confluence of major disciplines, fields, and ideas; classical contributions would be distinguished by thick bundles of connections with the later literature, while the less important elements would be lost in the background. Further, we would note that the network doubles in size every 10 to 20 years, most of the growth taking place along the leading edge of the "research front" (*8*) characterized by increased tightness of the mesh due to relatively more frequent reference to recent papers.

At present we can only study theoretically general properties of such networks or examine in detail limited areas in a specific field or journal (*2, 8, 15*). More comprehensive investigations of this nature will become possible as further volumes of the *Index* become available.

Estimation of Indirect Impact

However much we may deplore it, a count (or, more cynically, weight) of publications is commonly used as a measure of scientific productivity. Against this background the introduction of an independent measure, such as may be provided by citation data, can only improve matters. Granted that a short-term citation count is unreliable and that historical evaluation takes too long, there must be an intermediate period when citation networks begin to show sufficient organization to be susceptible of meaningful analysis. One can only make an educated guess as to how long this process will take. A great deal depends on the interval of latency between successive generations of papers. As was noted earlier, papers 2 to 3 years old have the greatest chance of being cited. At this rate, by 1970 we shall have citation data pertaining to at least five well-stocked generations of papers. It is hard to believe that this information will not be used for the evaluation of journals, institutions, and individual workers. One can only hope that it will be used with discretion.

A reasonable objection to a simple citation count is the argument that it may tend to favor applied research papers at the expense of the fundamental contributions on which they are based. It is also likely that publications in a new field, with only a few workers, will be cited less often than those in a crowded field. This bias could be, at least in part, corrected by use of the indirect method of scoring discussed below. By analogy to more familiar systems, this would be like estimating the distribution of a particular genetic character in a population of descendants, or, more graphically, like calculating the stress on a given node of a suspended network, assuming that all the nodes are of equal weight and that stresses are evenly distributed along weightless connections. Quantitatively, these models are equivalent and could be dealt with formally by application of the theory of graphs (*16*), but for the present purpose a more elementary treatment will suffice.

First, let us consider a simple case in which each node has two ancestors and two descendants (Fig. 3): the total contribution, or influence, of a given node would then increase at each level by one unit, because with each duplication only one half would be transmitted downward. An interesting feature of these models is the fact that the above description applies equally well to open-ended, branching structures (Fig. 3A) and to networks proper, where some of the nodes have ancestors related to each other by common descent, thereby completing a loop (Fig. 3B). In the latter case the original influence would be diffused among fewer members, but the total would remain the same as in an open branching system. This greatly simplifies mathematical treatment of complex networks.

If, at any level, there are more descendants than ancestors per node, the influence will be proportionally increased. The general expression for calculating the total contribution (C) of a given node after n generations would, therefore, be

$$C = \sum_{i=1}^{n} \frac{p_1 p_2 p_3 \cdots p_i}{q_1 q_2 q_3 \cdots q_i} \cdots \qquad (1)$$

where p_1, p_2, p_3, . . . are the arithmetical mean numbers of descendants and q_1, q_2, q_3, . . . are the harmonic means of the numbers of ancestors per node in the 1st, 2nd, 3rd, . . . nth generation.

When one applies Eq. 1 to bibliographic networks reconstructed with the aid of the *Science Citation Index*, instead of considering the total number of references (q) in a citing paper, it would be proper to consider only the references to journals listed as sources in the *Index* (that is, to exclude references to monographs, private communications, or periodicals not covered by the *Index*). In Table 3, the values q are based on these "eligible" items. In the papers surveyed in Table 1, 80 percent of the references, in both groups, were eligible for inclusion, and the data in Table 3 were calculated on this assumption.

For example, in order to estimate the total indirect impact of a paper A_0 in the period 1958–64, we would first list all the direct citations A_1, B_1, C_1 . . . of A_0; then the second-generation—A_2, B_2, C_2, . . . of A_1; next, D_2, E_2, F_2 . . . of B_1; and so on. Each count is independent of the others, so that C_1, A_2, and D_2 could well refer to a single item, counted three times as a descendant of A_0, A_1, and B_1, respectively. After determining the mean number of citations per paper and the harmonic means of eligible references (q) in each generation, one can calculate the total score, as described. In dealing with large populations, average values obtained from sample counts at each level could be

Table 2. Citation frequency (in number of citations during the 3rd year after publication) of three generations of papers, 3 years apart. "Middle generation" is a hypothetical test sample, reconstructed from data on papers published in 1958 and in 1961, for computing the number of citations to descendant (citing) and ancestor (cited) papers (see text).

Middle generation	Ancestors	Descendants
12	6	2.5
1	3	1.0

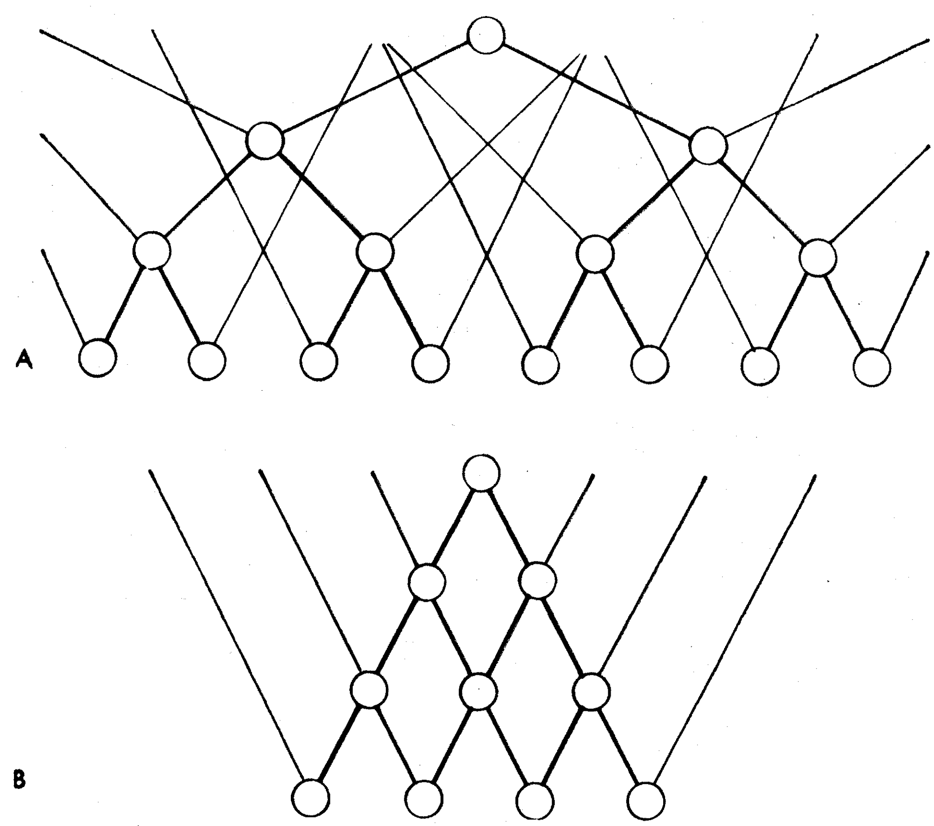


Fig. 3. A system of nodes connected into an open-ended (A) or looped (B) network. In the two cases the same downward stress is transferred to the apical node.

Table 3. Calculation of indirect-impact score for the period 1958–64. These scores are estimates based on average values for a sample of papers published in 1958 and cited 10 times in 1961. The probable citation figures for the years 1958, 1959, 1960, 1962, and 1963, not covered by the *Index*, were inferred from the data of Fig. 1.

Generation* of descendants (i)	Mean number of citations from i to $i-1$ (p_i)	Mean of reciprocals of references per paper† ($1/q_i$)	Indirect-impact score† for each generation: Extent of the network ($p_1 p_2 p_3 \ldots p_i$)	Fractional impact $\left(\frac{p_1 p_2 p_3 \ldots p_i}{q_1 q_2 q_3 \ldots q_i}\right)$
1	55	0.10	55	5.50
2	11	.15	605	9.07
3	2.2	.20	1331	4.00
4	0.7	.20	932	0.56
5	.1	.20	93	.01
6, 7	Negligible			
Totals	†		3016	19.14

* Note that *generation* does not refer to the number of years since publication of the first paper. † See text.

used. In any case, citation data are already stored in a form suitable for automatic processing, so that dealing with large samples does not present insuperable technical difficulties.

Actual trials of this method of scoring cannot be attempted until two or three more volumes of the *Index* become available, but, as an illustration, the following hypothetical example could be reconstructed from partial data based on the *Indexes* for 1961 and 1964. To fill the gaps, citation frequency was assumed to vary with the age of a paper roughly as shown in Fig. 1. Thus, an article published in 1958 with 10 citations listed in the *Index* for 1961 would, typically, have collected about 55 citations by the end of 1964. Of the citing articles, each could be expected to have a mean of 11 citations, and so on (Table 3). The total score applies to the period of 6 to 7 years (depending on the exact date of publication), so that the mean yearly score would be 430 to 500, expressed as the extent of the network, or approximately 3.0 in terms of fractional transfer of impact. By comparison, an average 1958 paper cited twice in 1961 is likely to have a yearly score of less than 10 and 0.3 on the respective scales, but the individual scores would be subject to a very large variation: the papers whose descendants had high citation rates would have high indirect citation scores. Among them could well be found important fundamental contributions which had a small initial impact but became cornerstones of large fields of knowledge.

Even though the above methods of indirect scoring may lessen the bias against theoretical papers or papers in a small emerging field, the score could serve only as an index for matching comparable papers or groups of papers. Neither scale has any objective meaning as a measure of the actual transfer of information. First, the influence passed on by a scientific paper (unless it be purely a review) consists not only of the paper's "genotype" but includes the author's original contribution. In terms of the genetic model this would be equivalent to recurrent mutations which would progressively dilute the pool of the original genes. If we could objectively define the fraction of a contribution $1/r$ which represents the debt to its predecessors, then the transferable impact could be expressed as p/qr instead of p/q. Second, not every cited reference has an equal share in this debt. To correct for this, a relative numerical value would have to be allotted each reference according to some scheme of citation relationship indicators (*12, 17*) and converted into a fraction ($1/s$) of the total bibliographic debt ($1/r$) of the paper.

When the terms rs are substituted for q in Eq. 1, the general expression for assessing the influence of papers becomes

$$\sum_{i=1}^{n} \frac{p_1 p_2 p_3 \cdots p_i}{r_1 s_1 r_2 s_2 r_3 s_3 \cdots r_i s_i}$$

Until we can determine objectively what part of the paper represents truly original work and what is the relative degree of indebtedness to each cited reference we cannot speak of quantitating the influence of published papers. It may, however, be interesting to assign some arbitrary values to r and s as defined above, in order to test the applications of this approach.

Conclusions

Evaluation by means of citation patterns can be successful only insofar as published papers and their bibliographies reflect scientific activity and nothing else. Such an innocent description is becoming less and less tenable. The present scientific explosion gave rise to more than a proportional publication explosion, which not only reflects the scientific explosion but has its own dynamics and vicious circles. Publication of results is probably the main means of accomplishing the almost impossible task of accounting for time and money spent on research. Inevitably, this puts a premium on quantity at the expense of quality, and, as with any other type of inflation, the problem worsens: the more papers are written, the less they count for and the greater is the pressure to publish more. What makes matters worse is the fact that the sheer volume of the "literature" makes it increasingly difficult to separate what is worthwhile from the rest. Critical reviews have become somewhat of a rarity, and editorial judgment is usually relegated to referees, who are contemporaries and, perhaps, competitors of the authors—a situation which has its own undesirable implications (*11, 18*). It requires little imagination to discover other vicious circles, all arising from distortion of the primary reasons for publishing the results of scientific inquiry.

There are, it is true, signs of adjustment to this crisis, partly due to some easing of the pressure to publish at all costs, and partly due to the readers' changing attitudes toward the flood of publications. An increasing amount of research is now being carried out in the form of collective projects in large institutions where publication is no longer the standard method of accounting for individual work. At the same time there is apparent an increasing tendency for scientific journals to polarize into the relatively few leading ones which carry important information and the many subsidiary journals which serve as vehicles for interim local accounting and, in a way, substitute for detailed intradepartmental reports. This division is a result not of some arbitrary decree but of normal competition between journals, as a result of which, however, the strong usually get stronger and the weak get weaker. Were it not for these changes and also for a striking improvement in abstracting, indexing, and alerting services, most research workers would have found long ago that, even in their own specialized fields, new information is accumulating faster than it can be sorted out. These developments can provide only a temporary reprieve, so long as there remains a strong incentive to publish the greatest possible number of papers. A new scale of values based on citations is by no means infallible or, in many cases, even fair, but at least it provides an alternative to the existing one, which is at the root of the crisis.

It might, of course, be asked whether wide acceptance of such new standards would not lead to deliberate abuses. A little reflection shows that the system is less open to manipulation than might appear. First, the referees are expected to see to it that the submitted papers cite work which is pertinent to the subject. An increased awareness of the usefulness of citation indexing as a tool for retrieval and evaluation will make this aspect of refereeing more important, and what now passes for minor carelessness or discourtesy could easily come to be regarded as serious malpractice. Second, as noted above, careful selection of references is in the author's own interest, because it helps him to reach his readers. There is, therefore, some room for hope that healthy feedback in the system will tend to keep it viable. At the basis of this hope lies the supposition that, in the long run, only good work can ensure recognition.

As Martyn (*2*) has pointed out, as an information-retrieval method, citation indexing is rather "noisy." The word *noisy* may apply even more to the problem of evaluation. Whereas in information retrieval much of the unwanted information can be filtered out by suitable search strategy (*2, 6*), this is not so easy to do for the purpose of evaluation, because a simple descendence relationship between papers is still an ideal far removed from actuality (*7*). The situation would be much better if we could at will exclude all citations which do not indicate real indebtedness. A scheme of citation relationship indicators, first mentioned by Garfield (*12*) and elaborated by Lipetz (*17*), would be a help, but, even if it were technically feasible, to provide such indicators would greatly add to the production costs of the *Index*.

Another possible way to minimize the effects of "noise" is to increase the size of the samples on which the reckoning is based. Now that research has become a rather popular occupation, it seems that a kind of public vote may have to be accepted as a factor in evaluation. Since this is the case, there is something to be said for extending the "franchise" to minimize accidental effects. An index which attempted to process all scientific publications would be several times the size of the present *Index*, and, what is more, it would not necessarily be an improvement as a tool for information retrieval because most of the significant work is already concentrated in the present *Index*. Whether this attempt will ever be considered worthwhile remains primarily a matter of policy and economics. In the meantime there is an urgent need for more experience with the existing services.

It is not the purpose of this article to advocate evaluation of scientific work by some kind of public opinion poll; its purpose is to recognize a possible trend in this direction. Any judgment by public acclaim is subject to obvious fallacies, but we must not be carried away by the analogy to the Stock Exchange or to electoral practices. The fact that, in this case, the "public" consists of authors whose contributions are generally linked creates quite a new pattern of organization. In this discussion some of the aspects of this pattern have been explored through analogy to idealized genetic or mechanical network models, but the very uniqueness of the system, with its many self-organizing ramifications, makes it a new field which deserves close study, since these developments may have profound effects on the future of scientific communication.

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