The citation culture

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Chapter 2

The creation of the Science Citation Index

2.1 Mixed reception

For Immediate Release

$300,000 GRANT TO PROBE INFORMATION RETRIEVAL AWARDED TO INSTITUTE FOR SCIENTIFIC INFORMATION BY NATIONAL INSTITUTES OF HEALTH AND NATIONAL SCIENCE FOUNDATION...

THREE YEAR PROJECT TACKLES CITATION INDEX TECHNIQUES FOR SCIENCE

Research scientists will soon be consulting a more precise and specific literature index that links together subject material that would never be collated by usual indexing systems. Concerned with new starting points for scientific literature searches, the unique concept uncovers sometime-buried associations, relating important works and authors, yet keeps the researcher abreast of the masses of current published scientific information. This new approach to information retrieval is called the Citation Index.

A $300,000 grant extending over a three-year period has been awarded to the Institute for Scientific Information, Philadelphia, Pennsylvania, to study the practicability of citation indexes and to test their techniques of preparation. The project, under joint sponsorship of the National Institutes of Health and the National Science Foundation, is aimed at producing a unified citation index for science including the publication of a genetics index.
Two years after this press release\(^1\), the Genetics Citation Index was published (Garfield & Sher 1963). It was quickly followed by the first volume of the Science Citation Index proper (Garfield 1963). Since then, the SCI has been published four times a year. Nobel Prize winner and Stanford University geneticist Joshua Lederberg\(^2\) wrote the preface to the Genetics Citation Index. He emphasized, as the press release in 1961 had done, the potential of the SCI as a tool for the scientist: “Citation indexing can uncover unexpected correlation of scientific work that no other method could hope to find, and a successful match can often be located with great speed and assurance”\(^3\). Yet, the new tool was not generally applauded.

“Citation indexes have had a mixed reception”, judged library scientist John Martyn on 31 March 1965 at the evening meeting of Aslib in London. According to his estimation, based on personal contacts, scientists were on the whole in favour of citation indexes, while librarians were much more cautious (Martyn 1965, 188). The latter tended to compare the citation index with the bibliographic tools they were used to, and did not find the new method quite so advantageous. As one library scientist noted somewhat regretfully: “Citation indexing (...) has been imposed upon us” (Shank 1965). According to Martyn this divergence can be explained by the differing information need: “The librarian is concerned with information retrieval, whereas the scientist is more interested in information access; regarded as a retrieval tool, the Citation Index is not as efficient as some more conventional approaches to the literature, but as an access tool it functions very well” (Martyn 1965, 189).

A number of reviews in scientific journals were however positive. “This Genetics Citation Index represents a landmark in literature-searching in genetics” wrote the Eugenetics Quarterly (Anonymous 1964\(^b\)). A colleague agreed:

> At $ 100, the price may seem high—but to those who can use this index, it could pay for itself in time alone, to say nothing of its value in supplying new leads. This latter will probably be its chief value, for, unlike other indexes which supply only the information specifically sought (if that), citation indexing can lead in unforeseen directions and to unforeseen relationships. (Anonymous 1964\(^a\))

Geneticist J. A. Beardmore saw the SCI as “a real effort to help those people endeavouring to follow specific lines of thought and endeavour through the rapidly increasing bulk of articles concerned with genetics and related fields” (Beardmore 1964)\(^4\). Not every scientist shared this appreciative outlook, though.

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\(^1\)For the complete text see the appendix.

\(^2\)Lederberg received the Nobel Prize in 1958 for his work on the bacterial genome.

\(^3\)Lederberg also praised the inventor of the SCI, Eugene Garfield: “My own contribution to the project has been too limited to inhibit me from commending Dr. Garfield and his associates for organizing and implementing a project which has required an unimaginable attention to detail, technical skill, enthusiasm, and above all, an irrepressible concern for meeting the real need of scientists. To flourish, science has many needs but none are more vital than responsible communication with history, society, and posterity embodied in what we casually call the scientific literature.” (Garfield & Sher 1963, iii) This chapter will show that Lederberg’s role in the citation indexing project was anything but limited.

\(^4\)The enormous amount of work involved in the building of the citation index was commonly praised. Heinisch spoke of “bewundernswertem Fleiß” (Heinisch 1965).
Nature’s reviewer was more critical of the SCI:

As it is, for the physicist, this index covers only about 5 per cent of the 800 journals included in the Physics Abstracts for 1961. While it would be unwise to underestimate the possible value of this method of indexing, I cannot visualize many situations where these volumes could be used more effectively than other indexes. (Cleverdon 1964)

One year later, the author judged the cumulative SCI for the year 1964 (Garfield 1965) “an improvement in many ways” (Cleverdon 1965). But he still found the future potentialities of the SCI “difficult to assess”. In 1966, Nature was even more critical:

Have the bad old days when it was only possible to get on by knowing the right people gone for good? Alas, no. An instrument recently introduced requires just this kind of intellectual nepotism, yet without the necessary personal acquaintance. (Anonymous 1966)5

Some readers could not agree less. A “regular user” of the SCI reacted:

I suspect that you have not used the Index for its proper purpose. (...) S.C.I. permits the questioner to exploit fully his own intelligence and experience in his analysis of the literature without having to rely on the arbitrary choice and use of key words by abstracters. (Davies 1966)

Physicist John Ziman attacked the lack of the physics journal Philosophical Transactions, the extremely small print of the index and the fact that the first SCI did not give the titles of the citing articles. The first regular issue in 1964 was improved in several of these respects (including the citing titles) and ISI promised a rapid increase in journal coverage.

Whereas Nature’s Cleverdon reasoned partly along the lines of the librarian, Science had the SCI reviewed by a zoologist, in an experimental way. Steinbach6 reviewed the SCI “as a novel and interesting example of a device to cope with the scientific literature explosion” (Steinbach 1964). He came to a moderately positive judgement7. The Genetics Citation Index did not yet include titles, it was mainly a list of author’s names (cited and citing authors). A trial run which he conducted with some graduate students resulted “in a split vote” on the question “whether the virtue of completeness compensates, to the scientist, for the work necessary to relate authors’ names to scientific content” (Steinbach 1964). The overall judgement was positive: “Reading about and trying to use the Index has convinced me that it will be of value and that all scientists should indeed examine it and consider its potentialities” (Steinbach 1964, 142). Nevertheless, the author

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5This publication was a short note about the first quarterly volumes of the SCI for 1966.
6The reviewer was professor of zoology and chairman of the department of zoology at the University of Chicago; he served as chairman of the Division of Biology and Agriculture of the National Academy of Sciences-National Research Council from 1958 to 1962 (Steinbach 1964).
7“Any real evaluation of Science Citation Index must be based on an extensive use test, and there has not been time for that” (Steinbach 1964).
“could not agree completely” with Garfield’s statement that both the usefulness and the desirability of citation indexes was already proven (Garfield 1964): “This statement may be correct with respect to its use by administrative personnel and librarians; its usefulness in advancing the wisdom of science must be judged in the future by scientists.”

The difference of opinion between Steinbach and Garfield relates directly to the role attributed to scientific literature. Steinbach was critical of Garfield’s enthusiasm for collecting all information in one system. “There is something wistfully comforting in the thought of knowing all facts. It is an old idea that, if the scientific facts are known, wisdom follows” (Steinbach 1964). He did not share this idea. In his opinion, the more basic the research and the more important the idea, the less important the literature. Hence, he found it “hard to think that Newton, Lavoisier, or Loewi” would have used a citation index (Steinbach 1964). The main point of contention was, however, the broad range of possible uses of the SCI advertised by ISI (Institute for Scientific Information 1964). These concerned, amongst others, the evaluation of the impact of research, sociological research, and studies into the literature use by scientists. Steinbach warned against these uses:

Misused, some of these “other applications” could cause some important difficulties, tending to foster the idea that what has been good in the past is best for the future. (A “high impact factor” means more support?)

(Steinbach 1964)

Two years later, Science expressed a much more favourable opinion, notably on the policy aspects in its lead article by Philip Abelson, the journal’s editor:

A particularly useful tool in a search for significant articles in a particular field is the Citation Index. (...) An interesting by-product of the Citation Index is a new method of evaluating scientific productivity. Instead of counting a man’s reprints, one counts citations of his work by others. Already sociologists are examining the value of this new analytical tool. They note some limitations but find that a citation index is a valuable aid to management.

(Abelson 1966)

In summary, the record of initial reactions to the first publications of the SCI confirms Martyn’s (1965) conclusion that librarians were sceptical (Martyn 1965, Martyn 1966). Scientists seem, however, to have been more divided than Martyn acknowledged at the time. Some were quickly sold on the new possibilities of roaming the literature of their own accord. Scientists who focused less on keeping abreast with the literature were probably indifferent. “Not every scientist greets the appearance of a Citation Index with enthusiasm”, Martyn also noted in 1965 (Martyn 1965). Scientists as well as librarians who gave the SCI some consideration, seem to have been especially wary of the possible use science policy.

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8Note that Steinbach, in contrast with Martyn, thought that librarians would be more appreciative than scientists!

This was explicitly mentioned in the leaflets and brochures of the Institute for Scientific Information, the publisher of SCI (Institute for Scientific Information 1964), as well as in Garfield’s publications (Garfield & Sher 1966) from the very beginning\textsuperscript{10}.

These mixed feelings were, nevertheless, far more positive than the reactions Eugene Garfield had experienced in the preceding years. He had been actively propagating the idea of a citation index for science since he had become with it in 1953. Few had responded. Even a few years before getting the grant from NIH and NSF, referees of his proposal were quite critical, some even hostile\textsuperscript{11} (Anonymous 1959 or 1960):

- It is my firm conviction that what is needed most urgently at this time is not more components research (e.g., citation index) but a prototype system of scientific communication that can serve as a test bed for existing and proposed components and that will guide their development. .... To the extent that Mr. Garfield’s proposal does not promote the growth of an experimental test environment, to that extent I consider it unwise and wasteful.
- As a necessary step, presumably, the applicant will use non-scientists to scan the literature. This step can be very harmful. A citation index must be overcautious about errors.
- I fail to see how this sort of publication (i.e., through journals) even as an ‘intermediate mechanism’ could be of much value to geneticists.
- The problem of avoiding a flood of citations to routine, expected, ordinary, usual, humdrum references, which nobody in the world would ever want to consult, is one that should be solved before a grant is made favorable to this proposal\textsuperscript{12}.

This resistance to the idea of investing in citation indexes of scientific literature may come as a surprise to the present-day user of the SCI and SSCI. After all, scientists must acknowledge their peers and must share their ideas and resources with their colleagues (chapter 1). Therefore, it seems rather obvious to use the footnotes of a scientific article, or the bibliography of a book, as an entry in a literature searching procedure. The use of citation frequencies, the number of times an article is quoted, may seem rather straightforward too. The citation score of an article may be seen as a measure of its use by other researchers and therefore of its impact, importance or quality. Not coincidentally, Eugene Garfield considers citation scores to be the condensed peer review of the entire scientific community (Garfield 1979).

\textsuperscript{10}I will come back to the relevant policy debates later in this chapter as well as in chapter 6.

\textsuperscript{11}An undated and anonymous overview of the referees comments (Anonymous 1959 or 1960) can be found in Garfield’s Personal Archive, Philadelphia. I have not been able to locate these documents in NSF’s Historian’s Archive, most documents having been destroyed. November 9, 1964 Garfield sent this document to his collaborator Irving Sher with the following note: “Did you ever see this? Must have come to me when I first applied to NSF for support five years ago.” To which Sher replied: “Oh gosh! The problems you had to overcome!!”

\textsuperscript{12}Some comments were positive, while most dealt with technical details (Anonymous 1959 or 1960).
The fact of the matter is, however, that the concept of the citation index did not come to science as naturally as this interpretation would suggest\textsuperscript{13}. Without Garfield’s perseverance and his strong belief in the usefulness of a citation index, we would probably live in a world without ISI’s bulky volumes and, of late, shining disks. The creation of the SCI is less the outcome of some inevitable process in science than historically contingent. To start with, the SCI has its roots not in science but in law.

2.2 Enthusiasm for citation

2.2.1 Shepard’s

Citation indexes were already old hat for American lawyers at the time this history of SCI starts. In the second half of the nineteenth century, one Frank Shepard in Illinois deemed it useful to know whether a legal proceeding was still valid. He produced gummed paper with lists of cases which cited the case in hand. Lawyers in Illinois glued them into their dossiers so enthusiastically that in 1873 Shepard set up a commercial business. His company, Shepard’s Citations Inc.\textsuperscript{14}, had the monopoly on producing the one and only citation index “To serve the Bench and Bar”. First in Chicago, later in New York and then in the 1950s in Colorado Springs, a staff of highly qualified lawyers produced the Shepard’s Citator by hand, covering all judicial decisions in the United States. Shepard’s was a respectable firm, proud of its supreme reliability\textsuperscript{15}. Its product was grounded in the norms and procedures of the legal system. As W. C. Adair, former vice-president of the company, explained to the readers of American Documentation in 1955:

The lawyer briefing a case must cite authorities to back up his arguments. So must the court in writing its opinions. This is because of the doctrine of “Stare Decisis” which means that all courts must follow precedents laid down by higher courts and each court generally also follows its own precedents. (...) The lawyer, however, must make sure that his authorities are still good law, that is, that the case has not been overruled, reversed, limited or distinguished in some way that makes it no longer useful as a valid authority. Here is where the use of Shepard’s Citations comes in. (Adair 1955)

The searching procedure was simple. First, the lawyer located a case similar to his own, then looked up Shepard’s citator to see whether later cases had cited it. He would immediately see whether the precedent was still valid\textsuperscript{16} and which other cases had made use of it. Adair told his audience that important law suits were won “on the strength of a case located by the use of Shepard which no other method of research disclosed” (Adair 1955). In summary, “Shepardizing” legal literature has since 1873 been based on the authority-centred norms of the United

\textsuperscript{13}As will be shown in this study, the SCI has profoundly changed its meaning and function throughout its history.

\textsuperscript{14}Shepard’s was taken over by Reed Elsevier and The Times Mirror Company on July 3, 1996.

\textsuperscript{15}“To Serve You Better” was the slogan of an advertisement of Shepard’s Citation Index in 1954.

\textsuperscript{16}A small r in front of the case meant for example that it was reversed.
States’ legal system: the most recent decision of the highest court is valid\textsuperscript{17}. The way of indexing by citation perfectly tied in with this value system. One can hardly think of a sharper contrast with supposedly ruthless scientific criticism. This hierarchical indexing style served nevertheless as the model for ISI’s Science Citation Index.

2.2.2 Adair

Retired from Shepard’s, running a cattle ranch in Colorado Springs, but still eager to work, William Adair read in the local newspaper sometime in 1953 an article stating that the scientific world “was being swamped in a sea of literature”. It was a report on The First Symposium on Machine Methods in Scientific Documentation organized by the Welch Medical Indexing Project at John Hopkins University in Baltimore. This project had been sponsored by the Army Medical Library since 1948 (Miller 1961, Larkey 1949). The main task of the project was to find out whether machines could be used to improve the efficiency of indexing and retrieving medical literature, and if so how. The indexing itself was supposed to be the tried and trusted subject indexing. In this respect the Welch Medical Library was not very innovative. Within these boundaries, the staff had to devise new systems of indexing, subject-heading and ways of using machines to solve “the literature problem” and was organizing meetings in the country. Adair decided to write a letter to the supervisor of the project, Sanford Larkey. He told Larkey about the citation indexing system, informing him of his opinion that “if the whole body of American Law can be classified so that a knowledge of one case can be used as a key to locate all other cases in point, the same thing can be done with medical articles”\textsuperscript{18}. Adair offered his expertise: “I have retired from Shepard’s and am now free to undertake and organize such a project”. He got a reply of a twentyfive-year old junior member of the staff, named Eugene Garfield. Garfield did not know anything about citation indexing. He wrote Adair that his suggestion would be investigated, but kept him at a distance. “We do not have any positions open for staff members”, Adair was told\textsuperscript{19}. Nothing happened. Adair’s initiative had no impact on the Medical Indexing Project.

Only more than a year later, after he had been fired by Larkey\textsuperscript{20}\textsuperscript{21}, did Garfield resume contact with Adair “with the idea of writing a paper to be published in one of the learned society journals”\textsuperscript{22}. Having browsed through Shepard’s Citations at the public library, the idea had begun to appeal to him. Garfield had even written a paper on “Shepardizing the scientific literature” for his professor while he was a fellow at Columbia University (Garfield 1954\textsuperscript{b})\textsuperscript{23}. At first, Garfield was

\textsuperscript{17}This is true for every juridical system in which all citizens are equal before the law.

\textsuperscript{18}Adair to Larkey, March 10, 1953.

\textsuperscript{19}Garfield to Adair, March 16, 1953.


\textsuperscript{21}In the first half of 1954, Garfield joined the literature section of the pharmaceutical company Smith, Kline & French as “consultant in machine documentation” (Garfield to Adair, June 11, 1954; Garfield to Adair, October 7, 1954, Garfield to Adair, August 24, 1954).

\textsuperscript{22}Garfield to Adair, June 11, 1954.

\textsuperscript{23}Garfield wrote Garfield (1954\textsuperscript{b}) for professor Fleming while he was the Grolier Society Fellow
not sure if citation indexes could be indeed applied to science:

Without knowing exactly what you had in mind I do not feel it is fair for me to be discouraging at the outset. But the one thing that must be kept in mind when comparing the field of science with that of law, is that there are anywhere from one to three million articles each year appearing in the scientific journals.\textsuperscript{24}

At that time, Garfield was not yet thinking of building a citation index. Working as a consultant in automation, he focused on possible uses of computers\textsuperscript{25}. He perceived a possible opportunity in automating the production of citation indexes. Adair “was very glad” to receive Garfield’s letter\textsuperscript{26}. He did not think the “vastness of the field” affected the problem at hand “except insofar as it bears on the expense and labor required”. Whether a citation index of medical literature would be effective, depended in his view on “how much a certain article cites other authorities”: “In the legal field it is rare that one finds a case on statute no matter how old that has never been cited”\textsuperscript{27}. A citation index would provide “a chain which would automatically string together authorities alike probably not all”. Adair was less certain about potential investors:

There is no lawbook publisher of any size, and I know them all, who would be in a position to make the enormous investment necessary and would have the extra plant and equipment. To form a new company to undertake the

\textsuperscript{24} Garfield to Adair, June 11, 1954.

\textsuperscript{25} “My interest in writing you is merely as an individual who is concerned with the difficulty of managing our scientific research record. My own specialty is the utilization of machines in facilitating the compilation of compendia such as Shepards. It is not unlikely that the Shepard Company already has mechanized many of its procedures, but my first reaction would be that an enterprise such as the Shepard citation system would find many uses for punched-card machines or large scale computers, commonly referred to as “electronic brains”, an appellation which has probably been detrimental in their quick utilization by such companies as Shepards. (Garfield to Adair, June 11, 1954). With the term “electronic brains”, Garfield probably refers to Shaw (1949), in which the librarian of the US Department of Agriculture Ralph Shaw discusses the “rapid selector”. This was an electronic device using a photocell which enabled rapid selection and printing of documents stored on microfilm. Originally a German invention, it was further developed by Vannevar Bush at the Massachusetts Institute of Technology. Bush and later Shaw devoted much attention to this device because it promised to improve “the quality of organization of knowledge both for administrative routines and for communication among scientists” (Shaw 1949), apart from the reduction in storage space needed. Shaw (1949, 169) also discusses whether this microfilm-reader was a thinking machine: “The selector has been termed a ‘thinking’ machine or ‘electronic brain’. Without more knowledge of what ‘thinking’ consists of, it is difficult to say whether the selector thinks or does not think. Certainly in the common sense of the term the selector is not a thinking machine. It merely stores vast amounts of data and sorts and reproduces them in accordance with instructions given to the machine both in the coding and in the selection. All the machine ever does is to match black dots (or, if you prefer, light dots) with complementary dots in an interrogating card.”

\textsuperscript{26} Adair to Garfield, June 21, 1954.

\textsuperscript{27} Adair to Garfield, June 21, 1954.
risk involved would not be feasible. To my mind it could be done only in one way. That would be to interest one of the large foundations on the ground of increase in the dissemination of scientific knowledge.  

He was willing to write an article but was not sure whether it would be understood by scientists. After some encouragement, Adair sent Garfield a rough draft which Garfield forwarded to Jesse Shera, editor of *American Documentation*, who readily accepted the paper. Only now did Garfield admit that he had submitted a paper (Garfield 1954b) to *Science* in April. Adair was enthusiastic about Garfield's text: “I have read your article and think it’s ideal to follow mine. (...) I was quite surprised at your grasp of how the work might be done.” Adair’s article appeared in the January 1955 issue of *American Documentation*.

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28 Adair to Garfield, June 21, 1954.

29 “I could write an article and would be glad to but since my experience has been in the legal field, I’m afraid an article on the citation phase of legal research wouldn’t be too understandable to those outside the legal profession.” (Adair to Garfield, June 21, 1954.)

30 In his reply, Garfield proposed they write an article together: “In any case, my original thought was that possibly you and I could join forces to compose an interesting article that would be comprehensible to scientists, de-emphasize the legal citation phase, except by way of reference and illustration” (Garfield to Adair, June 26, 1954). Again, Adair responded positively: “I do have time to write an article on the citation phase of research and would be glad to do so if you think it would be worthwhile. I agree that the legal phase should be emphasized only for purposes of illustration and to suggest how citations might be applied in other fields. I would think that you might add whatever you thought might be appropriate from the index angle. At any rate I should be glad to have you make whatever changes you thought might improve the article since you are closer to the field of scientific research than I. If you approve, let me know and I’ll get busy on it at once” (Adair to Garfield, July 22, 1954). Garfield encouraged him and ensured him of its usefulness: “An article on the citation phase of research could be a great contribution. As an associate editor of the journal *American Documentation*, I am sure it would be published. I would be glad to read it over or help in writing it. Perhaps the best way to start is for you to write a first draft (let me handle the typing, i.e. my secretary) and on the basis of this I can get a better idea of what you have in mind and let you know if we have the same basic problem in mind” (Garfield to Adair, August 24, 1954), and promised to send him a copy of his earlier paper: “Professor Fleming at Columbia University has still not returned to me the paper I did for him on “Shepardizing the Scientific Literature”, but I am writing him again so that I can send it on to you.” (Garfield to Adair, August 24, 1954)

31 Adair to Garfield, September 18, 1954.

32 “I have heard from Dr. Shera and as I expected he is delighted to have your paper for publication in *American Documentation*.” (Garfield to Adair, October 7, 1954)

33 “Now that you have submitted your paper I am sending you a paper I wrote while I was on a fellowship at Columbia University as I wrote you in my letter of June 11, 1954. A while ago I sent this paper to a colleague at Johns Hopkins University, Professor Bentley Glass, who is a member of the editorial board of *SCIENCE* the publication of the American Association for the Advancement of Science (AAAS). Should it be accepted for publication it is possible that some foundation may see the value of a citation system in disseminating scientific knowledge—as you mentioned in your letter of June 21, 1954. Ther (sic) is small chance that my article could be published before next year sometime. That is why I feel your article will make an excellent start, especially since *American Documentation* is widely read in scientific circles. When my article is revised I will then be able to make reference to your paper. Any suggestions you may have that will improve my paper will be greatly appreciated—just indicate the comments on the manuscript in your wonderful green ink.” (Garfield to Adair, October 7, 1954)

34 Adair to Garfield, October 11, 1954.

35 Its title was proposed by Garfield. Originally, Adair had titled it “A Citation System for
The author stressed the potential of the citation index as a novel way to unlock the body of scientific literature, a point which would be raised again and again by Eugene Garfield in the years to come:

The amazing efficiency of the citation method is such that once the starting case or statute is found it becomes a key that unlocks the entire store of law on a given point. It is this function which it appears would be of great value in other fields. An article on any scientific subject would be the key to all others. It may be objected that a comprehensive index would do the same thing. Even then the vast number of titles, sub-titles, cross references, etc. make the most skilfully compiled index difficult to use for the purpose of exhausting a subject. (…) The index represents the opinion of the compiler as to where a given subject should be pigeonholed. The list of citations is essentially determined by the authors, i.e., the courts. (Adair 1954)

He also anticipated objections “that whereas legal cases and statutes have standard references, scientific articles do not”, and the problems stemming from the “enormous amount of scientific literature”. Adair closely followed the practice in law in dealing with these potential problems. The references could be standardized36 and the vastness of the task could be made smaller by “splitting up the field of science broad by subject matter such as chemistry or medicine and by restricting the number of years covered”. The main point, according to Adair, was that of the citing culture in science: “More important than these is the question as to how much do writers on scientific subject cite other writers and articles. The writer must assume that they do this to a considerable extent”. (Adair 1954).

2.2.3 Computers

By being made privy to Adair’s personal experience with citation indexing, Garfield became more enthusiastic “with each passing day”38. The use of computers was the crucial perspective Garfield added to Adair’s way of thinking about citation indexing. They discussed the possible applications of punched card systems and computers in considerable detail. Adair did not think much of it, though. He told Garfield that it would not be very easy to use computers:

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36“Shepard’s covers many law reviews and journals and some special publications such as the Journal of the American Patent Society. These are given abbreviations and an abbreviation table is shown in the front of the books.” (Adair 1954)

37Adair refers here to the introduction of his article: “In order to clarify the scope and purpose of this article it is perhaps well to explain that the writer was for many years the executive Vice President of the Frank Shepard Company, publishers of Shepard’s Citations, a system of legal research used with great success by lawyers and jurists for over three quarters of a century. In the course of his incumbency he has seen occasional requests from members of the medical and engineering professions for information and advice as to whether such a system might not be used in their special fields. Unfortunately no one connected with the Shepard Company had the time to go into these questions thoroughly”.

38Garfield to Adair, October 18, 1954.
Concerning your idea of the possibility of interesting the Shepard people in mechanical processes I might say that we went into this very thoroughly back in New York. The Hollerith people put their best man on the job and I worked with him for over a month. We finally had to give up for two reasons. First the cards didn’t have enough spaces to cover the vast bibliography involved. Second and more important each citation would require a card and since citation slips are handled by the millions the punching machines and trained operators required was prohibitive. In addition to this very detailed attempt we twice had very good industrial engineering firms make surveys and their results also were mil [nil] on the production side.

This did not deter Garfield:

To answer your comments concerning your previous experience with the Hollerith (IBM) people — I am sure that the problem here is by no means a simple one, but on the other hand my own experience with IBM has time and again taught me that they do not have the best talent available for every type of problem, and indeed are often beat at their own game so to speak.39

Adair insisted. In his reply he informed Garfield about the way Shepard’s produced its citator. An army of highly competent lawyers and clerks checked and double-checked the validity of every citation. They did everything by hand. It was necessary for the slips on which the citations were written to be legible. Adair was of the opinion that punched cards, incomprehensible by nature, would introduce too many errors into the process40. Garfield immediately sent him a sample of a medical index page prepared with IBM cards, proving that legible information could be printed on them41. He asked Adair whether the latter would not recommend him to Shepard’s, and explained how he would proceed in advising them as a documentation consultant. Adair did not, however, wish to be explicitly involved: “If you feel like writing to the Company, do so. But please do not refer to me. Mr. W. G. Packard who is still the President knows that I worked on this problem for years and that I found it unsolvable. He would wonder a great deal if I now recommended a further survey”42. Adair was con-
vinced by the punched cards Garfield had sent him, but insisted that automation was still highly questionable. His main argument was the editorial work needed. Garfield nevertheless sent the Shepard firm his draft article for *Science* (Garfield 1954a), as well as a series of detailed questions about the way the citator was produced. L. A. de Bow, executive vice-president of Shepard replied politely without answering all questions. He objected to Garfield using the word “Shepardizing” in his draft article, and informed Garfield that “the basic idea of a citator for scientific literature is, as you of course know, not a new one”: “We have ourselves often considered such a publication and have, on numerous occasions over the past 25 years, discussed with doctors the possibility of a citator covering medical literature”. By then, Garfield was already in possession of this information. As early as 1947, Shepard’s had been approached by an officer of the American Medical Association requesting the company to undertake a citation system covering medical literature. But the Shepard management did not feel it had the resources to produce any system other than in its own legal field. Somewhat later the company produced a sample system for the American Society of Electronic Engineers. However, as Adair later recalled, this society did not have the money to go on with it. In 1954, as before, Shepard’s did not venture beyond its juridical domain, Garfield’s light prodding notwithstanding.

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43 “Would the machines save enough time in the assorting, which is small in the current work, to show a reduction in cost on this one operation? This is the point on which I have doubts. The big cost factor in the Shepard work is the editorial work which must be done by lawyers. (...) there is, of course, no mechanical means for doing this mental work. This cost has been the impasse for all engineers the company has employed” (Adair to Garfield, “Tuesday” See note 42.)

44 Adair did not expect much from this: “If you feel like making a proposal to them I suggest that you do so by all means. The last survey was by a firm of young industrial engineers who thought they could apply the same methods they would use in a steel plant. It was a fiasco which cost the company a large amount with no return.” (Adair to Garfield, December 2, 1954.)

45 “the time required to do so would be much greater than we can devote to such a tabulation” (De Bow to Garfield, January 4, 1955).

46 “We are certain you will understand our position that “Shepardizing” is a trade term relating to the use of Shepard’s Citations only” (De Bow to Garfield, January 4, 1955)

47 Adair to Larkey, March 10, 1953; Adair to Garfield, March 24, 1953.

48 Adair to Larkey, March 10, 1953; Adair to Garfield, June 21, 1954.

49 To Garfield’s question of whether Shepard’s had lost interest in citators outside of the field of legal literature (Garfield to De Bow, January 17, 1955), the answer was no: “The fact that we have, as I told you, several times in prior years considered citators to scientific literature, particularly to publications in the medical field, does not mean that we are no longer interested in extending our activities to those fields. On the contrary we have that possibility very much in mind but to date have been sufficiently occupied in completing citators for all the juridical jurisdictions and simply have not come to any conclusion on any other subject” (De Bow to Garfield, January 24, 1955). De Bow was curious about Garfield’s punched cards but in the end concluded that “As of this time, we do not feel that punched-cards alone offer any advantage over the methods we use” (De Bow to Garfield, May 18, 1955). They exchanged a couple of more letters, but nothing came of it.
2.2.4 Patents

In the mean time, Garfield had been looking for possible applications of a citation index\(^{50}\). His first idea was to look into the potential of a citation index for patents\(^{51}\). His idea was to submit a formal proposal to the Patent Office. He asked Adair to be one of his consultants. Adair was very interested in this\(^{52}\). He also informed Garfield that, again, he was not the first to think of a patent citation index\(^{53}\). Patent attorney Harry Hart of Bell Telephone Laboratories had spoken with Adair in 1945 about “Shepardizing the Proceedings of the Institute of Radio Engineers”\(^{54}\). The firm had at the time been “too busy moving out of New York” to do much about it\(^{55}\). In 1947, Hart suggested US Patent Office Commissioner Ooms that he adopt the same system as the lawyers (Hart 1949)\(^{56}\). Two years later, patent lawyer Arthur Seidel of Gulf Oil Company published the same idea in the *Journal of the Patent Office Society* (Seidel 1949), explaining that a citation index “would not require much effort upon the part of the Patent Office” whereas it would accelerate the search of comparable patents. Hart immediately took up the issue once again, for the first time hinting at the notion of a citation network: “It furnishes a network of paths which cut across the major highways

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\(^{50}\) “After giving a great deal of thought to the possible use of Citation Indexes as outlined in my paper, I pondered where such a system would be particularly useful. To make a long story short, I quickly realized that a wonderfully fertile application is in the area of patents. I immediately checked to see the type of patent coverage Shepard’s already has and found that it concerns only the legal phase, albeit a very important one. (...) Since patents form a very substantial and important part of the scientific literature, I could not help but be impressed by the possibility of Shepardizing the patent literature. I immediately did a sample of the work necessary and find that the practices of patent examiners make the application of a Citation Index an extremely pertinent one because there appears at the end of every printed patent specification a list of patents “cited” as well as any other pertinent literature references. These cited patents are provided by the patent examiner and not the patentee, making them extremely useful and pertinent to any future patent search (Garfield to Adair, October 18, 1954).

\(^{51}\) Garfield made an appointment with Mr. Lanham of the U.S. Patent Office, with whom he was “extremely well acquainted”.

\(^{52}\)”In regard to your using my name as a consultant on anything pertaining to citations, feel free to do so. I could devote almost unlimited time right now. (...) Your technical knowledge on scientific literature and indexing is far beyond mine. We ought to supplement each other nicely. (Adair to Garfield, “Thursday”. This letter is undated but is written on October 21, 1954, as is clear from Garfield to Adair, October 27, 1954.)

\(^{53}\)”As regards your idea for patents, I know that patent lawyers would welcome it. We used to get many requests from patent firms for such citations. At the time we revised the Federal Reporter Citations I spent considerable time both by correspondence and conferences with patent firms and officials of the patent office. I found that such citations would relieve patent lawyers of the necessity of a trip to Washington to examine the file wrapper or of obtaining the information through a correspondent Washington firm. Here is the reason we didn’t go into it. If we put such a section in the Federal it would bulk the book tremendously and the majority of subscribers not being patent lawyers would feel they were paying for something they didn’t need. Why not a separate publication? We took a list of all the patent lawyers in the U.S. From this we estimated the number of firms, then added the big law libraries. The total subscribers we could hope to get wasn’t sufficient to cover the cost” (Adair to Garfield, “Thursday” See note 52.).

\(^{54}\) Garfield to Adair, October 27, 1954.

\(^{55}\) Adair to Garfield, October 30, 1954.

\(^{56}\) The suggestion was forwarded to the Shepard firm.
marked by the (inevitably artificial) boundaries of Classification” (Hart 1949). Yet, nothing came of it. Garfield was not discouraged by these failed precedents. Together with the Atlantic Research Company he submitted a proposal to the Patent Office, after having attended a meeting of the Advisory Committee on Patent Office Mechanization. By May 1955, however, nothing had happened yet. The Patent Office had set up a new Research and Survey group which, Garfield thought, “is considering the proposal that was submitted.”

2.3 The citation introduced to science

2.3.1 Science

The energetic Garfield, who was by now firmly sold on the concept of citation indexing, hoped that the articles Adair and he were going to publish would attract attention. The reactions to the publications in 1955 in Science and American Documentation were, however, disappointing. True, Garfield received several letters endorsing the idea and encouraging him to develop it further. Generally, however, resistance seemed to prevail. Science published two letters from experts in scientific documentation, both unfavourable to Garfield’s plea. “Amid today’s overwhelming difficulties in scientific communication (...) this index would solve too few problems to justify its surely great cost at this time” (Schoenbach 1956). This counter plea for more inclusive subject indexing was strongly supported by a second letter (Welt 1956). In Science, Garfield’s proposal was not discussed any further for the next few years.

This is remarkable, because in his article Garfield tried to tie in his proposal, as directly as possible, to the running debate about scientific documentation. He opened by referring to earlier articles on the “uncritical citation of criticized data” (Thomassen & Stanley 1955, Zirkle 1954):

In this paper I propose a bibliographic system for science literature that can eliminate the uncritical citation of fraudulent, incomplete, or obsolete...
data by making it possible for the conscientious scholar to be aware of criticisms of earlier papers. It is too much to expect a research worker to spend an inordinate amount of time searching for the bibliographic descendants of antecedent papers. It would not be excessive to demand that the thorough scholar check all papers that have cited or criticized such paper, if they could be located quickly. The citation index makes this check practicable. (Garfield 1955, 108)

The index would be very handy for the working scientist: “It is best described as an association-of-ideas index, and it gives the reader as much leeway as he requires”. In this respect, the citation index would be, Garfield stressed, far superior to the traditional subject indexes which by nature restrict the interpretation of the article to a predefined number of topics:

One of the basic difficulties is to build subject indexes that can anticipate the infinite number of possible approaches the scientist may require. (...) What seems needed, then, in addition to better and more comprehensive indexes, alphabetical and classified, are new types of bibliographic tools that can help span the gap between the subject approach of those who create documents—that is, the authors—and the subject approach of the scientist who seeks information. (Garfield 1955)

Not only did Garfield focus on the information needs of the scientist, he also translated the concept of the citation index in terms of the subject indexes with which both scientists and librarians were more familiar.

Over the years changes in terminology take place, that vitiate the usefulness of a standard subject index. To a certain extent, this is overcome through the citation approach, for the author who has made reference to a paper 40 or 50 years old has interpreted the terminology for us. By using author’s references in compiling the citation index, we are in reality utilizing an army of indexers, for every time an author makes a reference he is in effect indexing that work from his point of view. This is especially true of review articles where each statement, with the following reference, resembles an index entry, superimposed upon which is the function of critical appraisal and interpretation.

His experiences at the Welch Medical Indexing Project, as well as his exchanges with Adair were utilized in the discussion of the coding of entries and the way the citation index would look65. Although he hinted at the possibility of giving the index the appearance of a bibliography66, Garfield thought of the index as an ordered array of numbers. Each article would be represented by a two-part code (the first part referring to the journal, the second to the article). Under each cited article, the citing articles would be printed, with a one-letter classification

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65These experiences are also reflected in the mentioning of the review articles about which a great deal of discussion had taken place at the Welch Medical Indexing Project.

66“Thus, it would be possible to list all pertinent references under each case with sufficient completeness to give the index more of the appearance of a bibliography. However, this would result in an extremely bulky volume.” (Garfield 1955, 108–109)
added to indicate the nature of the citing article (“an original contribution, review article, abstract, and so forth”) (figure 2.1 on page 32). Garfield described the production process with punched cards, stressing that “relatively unskilled persons can perform the necessary coding and filing”. The citation index would amount to a complete listing of all articles that had referred to a specific publication. He mentioned several possible uses of the index, amongst which “an individual clipping service” that the citation index could easily provide and “the tracking down” of the origins of an idea. The first application Garfield mentioned was, however, “historical”:

This would clearly be particularly useful in historical research, when one is trying to evaluate the significance of a particular work and its impact on the literature and the thinking of the period. Such an “impact factor” may be much more indicative than an absolute count of the number of a scientist’s publications (Garfield 1955, 109).

2.3.2 Propaganda

Garfield, now an independent documentation consultant, was not deterred by the silence that followed his proposal. He undertook several initiatives to make the citation index more popular, which increased his grip on the intellectual and practical difficulties in compiling this type of index. The editor of Chemical Bulletin invited him to write a short article (Garfield 1956a). This article stressed the citation index’s foundation in experience. It is moreover the first time Garfield

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67 Garfield refers here to citation studies (Gross & Gross 1927, Brodman 1944, Fussler 1949) that had been inspired by the problems librarians had in dealing with the growing literature.

68 Garfield advised amongst other the pharmaceutical company Smith, Kline & French.

69 The beauty of the citation index is that it achieves a posteriori indexing because a citation is experiential. The “logic” of all conventional scientific classifications has inevitably broken down with experience. Aristotelian logic has been a chain around the neck of the scientist and classifier.
CHAPTER 2. THE CREATION OF THE SCIENCE CITATION INDEX

directly linked his ideas about the citation index to the plea for a centralized clearing house by the famous crystallographer John Desmond Bernal in 1948:

Bernal proposed some time ago that a centralized reprint clearing house be established. Each scientist would then regularly receive papers in designated areas of interest. The proposal is excellent in its simplicity. Its execution is not so simple. How would one spell out his interests? (...) However, a reprint distribution plan based on the principle of the Citation Index could overcome this difficulty. (...) It would, in effect, be an individual clipping service.

Together with Mrs Margaret Courain, supervisor of the Research Files Division at Merck, Garfield produced an experimental citation index of patents, which he presented at the Minneapolis meeting of the American Chemical Society on 16 September 1955 (Garfield 1957). He informed the gathering that he had submitted a feasibility study to the Patent Office:

Some time ago a formal proposal was submitted to the Patent Office, suggesting that a feasibility study be conducted. If anyone is interested they can question the Patent Office on the matter. Pending action by the Patent Office, which seems doubtful, I am hopeful that Chemical Abstracts and/or Shepard’s Citations may be able to undertake the compilation of a Citation Index to patents.

Apparently, although Garfield was by now systematically campaigning for a citation index in different areas, he still did not wish to produce it himself. His main business was the publication and development of Current Contents, which was a success from the outset, and the exploration of innovative information products for scientists, medical doctors and engineers. For example, January 1956, he submitted a “tentative proposal” (Garfield 1956c) to the National Science

alike. Since the Citation Index is an arbitrary construct rather than a “logical” one, it can stand the test of time. Citations are permanent and unique, as are the works they identify. The significance of men’s writings may change, but their identities are fixed.” (Garfield 1957)

70In this presentation, Garfield again stressed the point of view of the literature users: “Every thought, idea or discovery can take on new meaning depending on the user’s frame of reference. (...) The Citation Index breaks this “barrier” by presenting subject matter in Bibliographical arrays which are neither alphabetical nor classified but associative”. (Garfield 1957) This was especially relevant to patents, since “Citations are provided for a variety of reasons, but principally to disqualify certain claims. Citations are also made frequently in order to restrict the scope of the applications.” Garfield pointed to the wastefulness of not using a citation index: “The lack of action in the Patent Office is particularly unfortunate since a system of this type for the exclusive use by the examiners could include references to abandoned applications which are not classified at all and which contain a wealth of citation information insofar as the citations resulted in abandonment. It is not unlikely that many searches have been repeated by examiners in part or in whole when an examination of the appropriate abandoned file would turn up much useful data. Examinators do often remember these abandoned files. However, it is difficult to see why they have to rely on their memories for abandoned files and not for published patents. Classification is used precisely because the human memory has its limits”.

71Chemical Abstracts had announced that they were studying the potential of a citation index.

72Sometime after the meeting, Garfield was told that although the Patent Office considered the citation index “a basically good idea”, it concluded that “the time required to compile and use it would not justify acceptance of the proposal” (Garfield 1957).
Foundation for a grant of the order of $50,000 a year to determine the information requirements of scientists. In May, Garfield submitted a proposal for research on mechanical indexing. NSF’s reply was characteristic of its general attitude towards private firms in scientific documentation: “When we discussed it on the phone, I tried to make it clear that the chances of approval would be better if it were submitted by an institution, in this case, the University of Pennsylvania.”

The grant agencies NSF and NIH would in the future repeatedly make the same point with regard to Garfield’s citation indexing proposals. Garfield returned the favour by being very critical of NSF’s policy and its way of handling scientific documentation needs. At the December 1955 meeting of the American Association for the Advancement of Science in Atlanta, he made a strong plea for a centralized national documentation centre (Garfield & Hayne 1955):

> The National Science Foundation does perform a few functions of such a center, but it is essentially a government counterpart of other philanthropic foundations that encourage and support scientific research. NSF has no centralized documentation apparatus. (Garfield & Hayne 1955)

Partly as a personal exercise, Garfield prepared a citation index of the Old Testament which he presented in 1956 to the American Documentation Institute in Philadelphia (Garfield 1956b). In this talk, Garfield presented a new idea, interpretative citation indexing. He sketched two possible approaches available to the indexer: “One he can limit himself to collecting all formal citations indicated in the text. (...) A second approach would be interpretative citation indexing. Based on the subject matter disclosed in the text the indexer himself can provide citations that relate passages to what has been published elsewhere. (...) This type of interpretative indexing is to be compared with exegesis.” (Garfield 1956b). The similarity with the procedure of a patent examiner is not coincidental.

### 2.3.3 Allen

January 1957, Garfield received the first serious support from a scientist. “Dear Mr. Garfield”, geneticist Gordon Allen wrote,

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73 Garfield to Brownson, January 25, 1956; Garfield to Brownson, October 29, 1963.
74 Brownson to Garfield, July 18, 1956.
75 Helen Browson, author of this letter, was NSF’s Program Director for Scientific Documentation. (Brownson to Garfield, July 18, 1956).
76 In their presentation, Garfield and Hayne linked documentation to the prevailing fascination with spying: “documentation is the forerunner of intelligence”. Said Garfield: “The CIA’s scope is totality of information pertinent to the nation’s safety and progress. The time has come for an analogous scientific body.” (Anonymous 1956)
77 Garfield to Packard, February 22, 1956.
78 Garfield also stressed in this presentation the importance of very precise references: “Thus, in a citation index there would be given the specific page of the citing as well as the page or passage of the article cited. This is extremely important, because the citation index is designed to deal with complex thoughts usually not revealed by the seemingly specific subject headings assigned by indexers”.
79 Allen was at the time with the Department of Health, Education, and Welfare of the National Institutes of Health.
Since the appearance of your article in Science two years ago, I have been eagerly looking for some news of steps toward a citation index. I have urged the American Society of Human Genetics to take some initiative in the matter, but they are already involved in the construction of a subject index in human genetics.

The references I have seen to your suggestion (for want of a citation index, I probably have not seen all of them) have been disappointingly cool, and I wonder if you have received any personal letters that were more enthusiastic.

If a group of interested persons were brought together, they might be able to make some headway.

Garfield’s reaction shows the impasse he had reached with regard to the citation index:

It was most kind of you to write me concerning your interest in citation indexes. I wish that I could report that we were well on the way towards preparing a comprehensive citation index to scientific literature. This is far from the case. However, I am more than ever convinced that a citation index is an absolute necessity in this era of voluminous publication. In my own personal reading and literature searching, I constantly see the need for citation indexes.

Garfield sent Allen a copy of the paper he had published in the Chemical Bulletin. He also told Allen that he had had “frustrating” experiences with the National Science Foundation. More than a year later, Garfield heard that NSF was planning to support research on citation indexes. He promptly inquired who was going to do the research and was told that he could submit a proposal.

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80 Allen had done this in 1956, when he talked to Sheldon Reed, then president of the society. (Allen to Macklin, April 9, 1959)
81 Allen to Garfield, January 24, 1957.
82 Garfield to Allen, February 15, 1957.
83 “I couldn’t have picked a better publication for burying an idea, but the former editor was kind enough to ask me to write the article which is more than I can say for some of the more ‘learned’ journals.” (Garfield to Allen, February 15, 1957).
84 “I had planned to submit some sort of proposal to the National Science Foundation for a grant that would enable me to continue research on citation indexes. However, I have found it very time-consuming and frustrating to deal with NSF and could not afford the time needed to prepare lengthy proposals. However, I would be delighted to get together with you or any other interested persons to work out ways and means of preparing a citation index to the literature of genetics and/or any other discipline.” (Garfield to Allen, February 15, 1957).
85 This statement seems to have been the result of a congressional hearing in which a congressman asked NSF why it didn’t shepardize the literature. As a result, NSF published the statement that it was considering the investigation of “a proposed method of bringing related material together similar in some respects to Shepard’s Citations, a respected method in the field of law, which has never been tried in the sciences” (Hearings, House of Representatives, Committee on Appropriations 1957). One of Garfield’s friends drew his attention to this statement (Garfield to Lederberg, May 21, 1959).
86 M. M. Berry to Garfield, August 22, 1957. This communication, to which Garfield referred in his research grant application of July 15, 1960 (Garfield 1960a), was probably a phone call.
87 Garfield recalled this episode a year later in his first letter to Joshua Lederberg (Garfield to Lederberg, May 21, 1959) and summarized it in his research grant application of July 15, 1960.
Together with Gwen Bedford, a researcher at the University of Pennsylvania, he prepared a draft feasibility study of citation indexes (Garfield 1958a) in May 1958 and sent it to NSF for commentary. At the same time, he wrote a paper for the upcoming International Conference on Scientific Information and informed Allen of both actions. Allen reacted sympathetically: “Your Unified Index to Science sounds like a tremendous advance over present indexing, and I hope you can get it started”. He could not, however, invest much time himself:

I am still interested in a citation index for science, but it does not appear that I can give any of my own time to it. The American Society of Human Genetics had already embarked on its own subject index when I brought up this matter, and is still too engrossed, I think, to consider any broader approach. However, I will tell them, at the next annual meeting, about your present plans and invite their interest and support. I’m afraid you can’t look to them for direct financial help, but is there something else they can do, as a society or as individuals, to help push the project along?

Two weeks later, Garfield officially submitted his proposal to NSF (Garfield 1958b). Its goal was “to determine the utility of citation indexes for science in terms of general usefulness, invariance in time, minimizing the citation of poor data, identification of the “impact factor”, and provision for individual clipping services”. The study should also develop “a suitable technical design for citation indexes”. Its motivation followed Garfield’s (1955) line of reasoning, albeit with an added emphasis on the index’s potential for “the encyclopedic integration of scientific statements”. Garfield moreover re-emphasized the potential of Bernal’s proposal for a central clearing house. This document also makes

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88 Gwen Bedford was with the Institute of Cooperative Research of the University of Pennsylvania. She had been associate professor at the School of Library Research, Drexel Institute. It was thought that she could possibly use the results of the project for her PhD dissertation, which would concern “the impact of the government research report on conventional scientific communication” (Garfield 1960a).

89 Garfield sent it on June 25 and heard on July 7 that Program Director Helen Brownson found it “suitable for submission to the Foundation”. She also expressed as her opinion that the salary levels “seemed quite high to us, for the Foundation ordinarily supports research and studies at universities where salaries are considerably less” and asked Garfield “to trim the budget as much as you can” (Brownson to Garfield, July 7, 1958).

90 Garfield sent Allen his paper and told him that he had “worked up” a research proposal (Garfield to Allen, July 24, 1958).

91 Allen to Garfield, July 30, 1958.

92 “The citation index for science can be the key to the “encyclopedic integration of scientific statements”, the basic synthesis which can weld disciplines and specializations. Operations research studies have established beyond dispute the value and considerable power of the results of the cross-fertilization of disciplines in the search for solutions to complex problems. We are now at the point where we need a tool which is suitable for tackling scientific problems on a broad front. The citation index is such a tool.” (Garfield 1958b, 3)

93 “From time to time, the idea of establishing a centralized reprint clearing house has been advanced (cf Bernal 1948). Each scientist would regularly receive reprints in designated areas of interest. The citation index offers an ideal basis for determining areas of interest which are realistic and practical. As a scientist’s frame-of-reference changes, so does his distribution list — and without lengthy exchanges of correspondence.” (Garfield 1958b, 4)
clear that Garfield’s “impact factor” was meant to be a tool for cultural historians mainly, and should not be read as an antecedent of present scientometric impact factors:

The index provides for a direct qualitative (and possibly a semi-quantitative) measure of the “impact factor”, i.e., and evaluation of the significance of a particular work and its impact on the literature and thinking of the period. This is the tool cultural historians need so that their studies can be rigorous in method and thus lead to meaningful results. (Garfield 1958b, 4)

The project was meant to be a two-year study, starting September 1958, which would be restricted to compiling the index. Garfield asked Allen to phone the office of Scientific Information of NSF in support of his proposal. Allen phoned Burton Adkinson at NSF’s office of Scientific Information, then explained his reasons in more detail in a follow-up letter:

The greatest value of a citation index, I think, would be its ability to connect any scientific statement with subsequent amendments, corrections, or retractions. Another great service that a citation index would perform would be to make readily available the latest work on any specialized topic. In summary, a citation index would, I think, do for our present system of science indexes what cross-references do for any simple index. By adding another dimension, it would increase tremendously the efficiency of any literature search.

One month later, NSF turned down Garfield’s proposal, but at the same time expressed its interest in a citation index:

In view of the interest in citation indexes as such expressed by the reviewers we believe that the actual preparation and subsequent testing of one might be desirable. It seems to us, however, that the scientific subject field of the experiment should be carefully defined either by a group of scientists or by a scientific literature group closely associated with a specific subject area. We think the preparation and testing of the index also should be under the cognizance of such a group. Therefore, I would like to inquire what your reaction would be to a possible situation in which a scientific society or literature group in a technical field appropriate for citation indexing would provide scientific direction for the experiment, and your organization, with its interest

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94 “Some of the people in the National Science Foundation need this kind of prodding in evaluating projects and I am sure that a favorable opinion from a reputable scientist like yourself would certainly do us no harm when our proposal comes up for consideration.” (Garfield to Allen, August 15, 1958)
95 Garfield had created his own company by now, Eugene Garfield Associates with offices in Spring Garden Street in Philadelphia.
96 Allen gave some examples of “self-perpetuating errors” in the human genetics literature.
97 Allen to Adkinson, September 5, 1958.
98 “Dear Gene: By now you will have received the Foundation’s letter saying that it cannot support your research proposal for a general feasibility study of citation indexes for science. The purpose of this letter is to ask your reaction to a possible modified approach to the problem.” Gray to Garfield, October 23, 1958.
and experience with citation indexing, would prepare the actual index. Such an approach, it seems to us, would combine the skill of the group most acknowledgeable in the techniques being studied with the scientific judgment and counsel of representatives of the “public” expected to use and benefit from the index.

### 2.3.4 A World Brain

Garfield took this as a flat refusal, proving once again NSF’s inability to deal with the tasks at hand. He did not stop his campaign, though. In November, he made his plea for a “unified index to science” at the National Academy of Sciences conference on scientific information (Garfield 1959). In this presentation, the idea of integrating scientific knowledge, already mentioned in Garfield (1958b), was further developed:

Since scientific research today is highly inter-disciplinary, the “selective” approaches of our traditional media, based on the old academic disciplines, can never give us anything more than makeshift tools, which do not function properly, considering the overall job to be done. Fragmentary approaches are not only inefficient but inadequate. (Garfield 1959, 674)

A unified and standardized approach to scientific literature searches was Garfield’s goal:

1. Provision for one logical starting point for all literature searches, regardless of subject.
2. Standardization of nomenclature, particularly in the areas of overlap between existing indexing services.
3. Provision for detailed indexing not possible in specialty indexes. An increased number of analytical entries per article would be economically and intellectually more feasible.
4. Elimination of all doubt as to whether individual articles had been indexed by specialty indexes, particularly in inter-disciplinary subjects where selectivity exercised by specialty indexes is necessarily arbitrary. Complete coverage of all individual articles becomes a practical possibility.
5. Economic utilization of machines for the compilation of the present specialty indexes and indexes to individual journals.
6. Economic production and distribution of scientific indexes by virtue of broadening the number of potential users. Mass production is the best known method for reducing product costs. (Garfield 1959, 675)

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99 Garfield’s paper grew out of an earlier paper he prepared while being at the Welch Medical Indexing Project, entitled “Unified International Scientific Indexes through Centralized Machine Indexing and Its Relations to Standardization of Nomenclature”. (Garfield 1959)
For Garfield, such a unification of indexes would mean a step towards realizing H. G. Well’s “World Brain”\textsuperscript{100}. In fact, this idea of a large, global information system was not only Garfield’s paradigm but also that of other major players in the history of the SCI. Garfield’s 1958 presentation was the first in which he explicitly put his citation index idea in this wider perspective: “The Citation Index has been discussed in previously published articles. However, this is the first time its use for consolidating references to and from the various abstracting services has been recommended. This feature of the Unified Index is significant” (Garfield 1959, 676).

2.3.5 Lederberg

In April 1959, Gordon Allen again tried to convince his American Society of Human Genetics\textsuperscript{101} to co-operate with Garfield to create a citation index. He told president Madge Macklin that Garfield’s submission to NSF provided “a rich opportunity for Human Geneticists to get a valuable free service”\textsuperscript{102}. Allen, who had been one of NSF’s reviewers of the proposal, informed Macklin of the reasons Garfield’s proposal had been turned down and urged the society “to appoint a panel of interested and competent members” to consult with Garfield. He also proposed recommending the proposal to NSF. One month later, Garfield received a letter from another World Brain enthusiast, geneticist Joshua Lederberg, which would prove the turning point in the history of SCI:

Since you first published your scheme for a “citation index” in Science about 4 years ago, I have been thinking very seriously about it, and must admit I am completely sold. In the nature of my work I have to spend a fair amount of effort in reading the literature of collateral fields and it is infuriating how often I have been stumped in trying to update a topic, where your scheme would have been just the solution! I am sure your critics have simply not grasped the idea, & especially the point that the author must learn to cooperate by his own choice of citations + thus he does the critical work. Have you tried to set this out in an adequate experiment? Would you look for support from the NSF? Of course you have to count on opposition from the established outfits, which have already succeeded in blocking any progressive centralization of the Augean tasks.\textsuperscript{103}

As he later explained to Garfield\textsuperscript{104}, Lederberg’s initiative was prompted by a science policy debate in the Genetics Study Section of NIH. The administration wished to evaluate its actual impact on research, and proposed, in the words

\textsuperscript{100}“A unified index to science could take many physical forms. In a large centralized science information center, this H. G. Wells type of “World Brain” might be a 3 by 5 inch card file, a random access electronic storage device, or a searching device such as Minicard or Filmorex. In this paper an alphabetic printed index is assumed.” (Garfield 1959, 676)

\textsuperscript{101}Allen was member of the board of this society.

\textsuperscript{102}Allen to Macklin, April 9, 1959.

\textsuperscript{103}Lederberg to Garfield, May 9, 1959.

\textsuperscript{104}Lederberg to Garfield, July 29, 1960.
of Lederberg\(^{105}\) “a number of rather fancy and inefficient schemes”. Lederberg recognized that a citation index would accomplish the purpose “at a negligible additional cost” and decided to contact Garfield. The latter was very happy to receive this letter:

> I hope you won’t be embarrassed by a show of emotion, but your memo almost brought tears to my eyes. It then seemed that over six years of trying to sell the idea of citation indexes had not been completely in vain.\(^{106}\)

He told Lederberg the whole story of his pleas for citation indexes, the support of Gordon Allen and the resistance he had met since 1954:

> As to opposition from the established outfits—there is no end to this. Chemical Abstracts pays lip service to Citation Indexes, but does nothing about them. Even my friends at Biological Abstracts and the Current List of Medical Literature who accept my judgement on many other conventional problems—look upon Citation Indexes as something impractical and unnecessary—particularly when there is so much more abstracting and regular indexing left undone.\(^{107}\)

He did not hesitate to inform Lederberg of his impressions of NSF, recalling the episode the year before:

> I tried to convince certain illiterates at the National Science Foundation to give me a small grant to conduct research on citation indexes. (...) Needless to say my proposal was turned down.

Lederberg was shocked by Garfield’s letter and “absolutely astonished that citation indexes are not long since a standard feature at the Patent Office”\(^{108}\). He advised Garfield to resubmit his proposal “to all the agencies who could be interested”\(^{109}\). Especially NIH “would be an excellent target”, since it “is anxious to evaluate its ‘impact’ on scientific progress, and how better do this than through your scheme”\(^{110}\).

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\(^{105}\) Lederberg to Garfield, July 29, 1960.

\(^{106}\) Garfield to Lederberg, May 21, 1959.

\(^{107}\) Garfield to Lederberg, May 21, 1959.

\(^{108}\) Lederberg to Garfield, June 18, 1959.

\(^{109}\) “I imagine your tactical error was in approaching the NSF at a time when, to put it bluntly, it was too broke to do more than a fraction of what it should. It is still not so well financed, and I think you might have better luck with another agency. In fact, the way to push it is to submit the same proposal, with due notice, to all the agencies who could be interested. These would include NSF, NIH, AEC, ONR, AFRC...” (Lederberg to Garfield, June 18, 1959). Lederberg also thought that the prospects would be better in biology or medicine than in chemistry; that Garfield should stress the job needed mainly money and machines, not professional manpower; and that starting with a review journal would not be such a bad idea.

\(^{110}\) Lederberg was himself member of a NIH panel with this task. During one such meeting, he recalled Garfield’s article in *Science* and decided to write the man (Joshua Lederberg, Interview, February 3, 1992, New York).
sons for NSF’s lack of support. Neither did he share Lederberg’s still sympathetic attitude towards the Patent Office. He even poured cold water on Lederberg’s suggestion of contacting NIH: “I think that I anticipated you on the idea of getting the NIH Div. of Research Grants interested. Their former librarian, Scott Adams, tried to get them interested but nothing came out of it”. Lederberg reacted promptly to the materials Garfield sent him along with his pessimistic assessment of potential support from the science funding agencies. What Garfield had taken as a flat refusal (section 2.3.4 (page 38)), he read far more positively: “I find myself rather more sympathetic with the viewpoint summarized in Dwight Gray’s letter of 23 Oct 58, and which I would interpret as a constructive basis for further dealings on your part.”

Lederberg’s interpretation was different from Garfield’s because the geneticist concurred with the reviewers’ doubts about the proposal. He wondered whether “in fact the concept hadn’t already been well enough sold to the NSF reviewers”:

My own feeling at the present time is that the utility and feasibility of citation-indexing are, in fact, self-evident; it is rather doubtful that any limited sample would serve to convince anyone else who did not already see the point. (...) I can easily see that $59,000 might be thought a wasteful expenditure if its main effect were to reprove the obvious, and especially if not very much more than this would be needed to get a useful product.

2.3.6 Re-establishing communication

Lederberg proposed Garfield “jump in” and ask for NSF’s assistance in organizing a scientific committee as suggested by NSF’s last letter on the subject. At the same time, he tried to convince Garfield that not only “dolts” but also quite recep-

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111“I can’t agree that in this instance the reason for the turn down was the financial condition of NSF. (...) in the Office of Scientific Information they go around pleading that nobody wants to do research in documentation and always have. What they mean is that nobody wants to do the kind of research they want. (...) They also give out money for “popular causes” like translation of Russian stuff—regardless of its scientific value. You can’t imagine how frustrating it has been in the past five years (or maybe you can) to have had at the helm of scientific documentation activities in NSF a woman who was neither a scientist or an information specialist, but just a good secretary (a Spanish major) who worked her way up by taking good notes at meetings and preparing reports for her bosses. I would never say this publicly, but that is the absolute truth. I tried for five years to get some kind of support so I wouldn’t have to go “commercial” but it was closing battle. I even got myself temporarily affiliated with the Univ. of Pa. ICR and the Franklin Inst. and couldn’t make a dent. (Garfield to Lederberg, June 23, 1959).

112“I am sorry but your are trying to give the Patent Office people credit for more intelligence than they have. You don’t know how backward they are. It is such a tradition bound organization that even their approach to machines, which they are investigating, is completely archaic. I suggest you meet their Dir. of Research some day if you want to be convinced. They did not reject the Citation Index on the grounds you suggested—it was purely on the grounds that they didn’t think it was worth the effort. (...) And Congress wonders why it takes over two years to get patents and sometimes longer.

113Lederberg to Garfield, June 26, 1959.

114Lederberg to Garfield, June 26, 1959.
tive people worked in the funding agencies. In other words, Lederberg tried to teach Garfield how to handle these institutions. The latter assured Lederberg that he was not crying wolf: “I think you will find, if you haven’t already, that I am really a very reasonable man and that I do not have a persecution complex about the NSF.” He accepted Lederberg’s critical judgement of his proposal, was “heartily in favor of forming what you call a “consumers group”, and acted accordingly by re-establishing contact with NSF’s Program Director for Publications & Information Services Dwight Gray:

Dr. Lederberg suggests that I ask your assistance in establishing a group of scientists who would advise us on just exactly what course should be pursued in regard to the development of citation indexes. Since there has been interest indicated for the genetics literature, I would like to suggest that this field be the “test” field and that possibly another field could be tested simultaneously. This second field might be fishery biology as the FAO, Rome had indicated a definite interest in helping out in this.

He also informed Gray that the American Society for Human Genetics was considering whether it could play some role in this project. At the same time, he informed Allen of Lederberg’s support for citation indexes. Allen immediately informed Lederberg that he had been talking about citation indexes to human geneticists “for several years without striking a spark of interest except in Everett Dempster.” Maybe microbial genetics, Lederberg’s specialty, was better suited? Allen also invited Garfield to come over and discuss the problems of a citation index in more detail. Garfield meanwhile tried to interest the Air Force in a citation index. Lederberg wrote Allen that he thought that it might be better to start with a block of journals, instead of a defined area of research.

Allen agreed with this, as did Garfield. Garfield also got the idea of enrolling scientists themselves in compiling the index:

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115“I would make a distinction between the “dolts” you deal with in the Patent Office and in NSF, AEC etc. I don’t know the Information specialists, but have the highest regard for the researchgrant people in all these agencies” (Lederberg to Garfield, June 26, 1959).
116Garfield to Lederberg, July 6, 1959.
117“I can well see the shortcomings of the proposal, particularly in retrospect.” (Garfield to Lederberg, July 6, 1959)
118Garfield refers to the Biology Division Library of the Food & Agriculture Organization, which maintained a citation index in card form (Garfield to Allen, April 11, 1959).
119Garfield to Gray, July 6, 1959.
120The two geneticists had known each other for a long time but had been unaware of their mutual interest in citation indexes. Garfield informed Lederberg of Allen’s involvement in his first reply to Lederberg’s initial letter. Lederberg wrote him that he had known Allen “for a long time” but had never discussed citation indexes with him. July 6, 1959, Garfield wrote to Allen and informed him of Lederberg’s interest. By that time, the two scientists had not yet discussed the matter with each other. (Garfield to Allen, July 6, 1959; Garfield to Lederberg, May 21, 1959; Lederberg to Garfield, June 6, 1959).
121Allen to Lederberg, July 8, 1959; Allen to Garfield, July 8, 1959.
122Allen to Garfield, July 13, 1959.
123Garfield to Allen, July 15, 1959.
124Allen to Garfield, July 17, 1959.
125Garfield to Allen, July 20, 1959.
I intend to write a series of letters to librarians, literature scientists and laboratory scientists, suggesting that they volunteer their time for Citation Index work. He would be assigned a particular journal to work on. After all of the file cards had been sorted by journal, we could then turn over to the editor of each journal an individual Citation Index for his journal.\textsuperscript{126}

NSF evaded committing itself and in a preliminary response\textsuperscript{127} proposed that the geneticists themselves should take the initiative to form an advisory committee\textsuperscript{128}. Therefore they approached NIH as well. Lederberg talked to Katherine Wilson, executive secretary of the Genetics Study Section at NIH\textsuperscript{129}, and so later on did Allen\textsuperscript{130}. Lederberg moreover exerted pressure on NSF to act\textsuperscript{131}. By now they were both lobbying as well as developing practical solutions to the many problems of compiling a working citation index\textsuperscript{132}. An important issue was where to start. Lederberg had his doubts about the choice of genetics:

Frankly I don’t think that “genetics” is the first best circumscribed field for a tryout in C.I. I think “physiology” would be much better. But we need it in all of science and any move to get started is a good one. If you C.I. just a group of genetics journals as a closed group I’m afraid you wouldn’t generate much improvement over what most people in the field remember from their own experience.\textsuperscript{133}

Garfield felt the same way. He preferred to start with general science journals like Nature. This would, however, exacerbate the problem of the size of the index. Having each scientific journal publish a citation index as a yearly supplement might be a solution\textsuperscript{134}.

\textsuperscript{126}Garfield to Allen, July 20, 1959.
\textsuperscript{127}Program director Gray was in Europe when Garfield’s letter of July 6 arrived, so the matter was handled by Assistant Program Director Tolkan.
\textsuperscript{128}“I am not sure that it would be appropriate for the Foundation to organize a committee of geneticists to advise on a citation index project in the field of genetics. Rather, if geneticists are interested, I should think they would more properly and more effectively take the initiative in setting up such a committee, which might function through the American Society of Human Genetics or the Genetics Society of America, or at least have the endorsement of one of the societies.” (Tolkan to Garfield, July 16, 1959)
\textsuperscript{129}Lederberg had just resigned from this panel — which had triggered the idea of contacting Garfield in the first place — so he could not “nurse the idea along” as well as he could have, he wrote Garfield (Lederberg to Garfield, July 9, 1959).
\textsuperscript{130}Allen to Garfield, July 31, 1959.
\textsuperscript{131}All three felt that they had to overcome stubborn resistance, overcoming of which required careful tactics. “NSF may have deflected our ploy on CI but I am still thinking what might be done. Let me know if there are any further developments—there should be!” (Lederberg to Garfield, undated, between July 14 and August 3, 1959).
\textsuperscript{132}For example, Lederberg sent Garfield some of his published work, as samples to work from, and got reprimanded by Garfield about his imprecise way of referring to other researchers: “citations ought to be a little more specific. I don’t understand why citations frequently aren’t more specific as to page number.” (Garfield to Lederberg, July 14, 1959).
\textsuperscript{133}Lederberg to Garfield, undated, between July 14 and August 3, 1959.
\textsuperscript{134}“I got a good idea, I think, for overcoming the problem of size of a CI. Why can’t each journal issue a yearly supplement, just as they do their index, which would be a citation index for that year?” (Garfield to Lederberg, August 3, 1959)
In August, Garfield discussed the citation index at NSF and NIH. He also had a thorough discussion with Allen about how to show the power of citation indexing: “To really demonstrate the value of a citation index we should, somehow, come up with as complete a citation index as possible to a selected list of journals and/or articles.”\(^{135}\) Garfield and Allen concluded that “some mechanical method” should be developed for copying the citations.\(^ {136}\) The main area for intellectual problems would be the specification of the “kind of citation”. Garfield hoped that the citation index would help standardize scientists’ referencing behaviour:

I believe that citation index research will pay off handsomely in the future in that this research will characterize all the different ways in which people “cite” the earlier literature. We will then be able to provide editors with a guide to standardized citation practices. Further, they might be influenced to adopt a notation or terminology that would indicate to the reader and the bibliographer the “nature” of the citation. In this project we would try to characterize, for a selected list of articles, each citation as to whether it was:

1. Review article (Rev.);
2. Communication (Com.);
3. Editorial (Edit.);
4. Errata (Err.);
5. Translation (Tr.);
6. Abstract (Ab.);
7. Book (Bk.);
8. Discussion (Disc.);
9. Summary (Summ.);
10. Bibliography (Bibl.);

Book Review. I have purposely left out: refutation, confirmation, etc. I have also left out any mention as to whether pertinent portion of citing paper is experimental, theoretical, introductory or whether it is a use of a method cited or use of “material” cited. These are points to be investigated later.\(^ {137}\)

He also preferred to indicate page numbers to “speed up locating the pertinent statements”. After his discussions with Allen, Garfield personally ran some tests on samples to discover the characteristics of the references, the average number of references in an article, and the speed with which a citation index could be compiled:

Scanning 1500 articles took about 15 hours in five sessions of three hours duration. I could sometimes scan as much as 200 articles per hour. It was never lower than 100 per hour. Depending upon motivation skilled clerks could scan at an average rate of about 100 articles per hour.\(^ {138}\)

His conclusions were that the citation index was indeed feasible.\(^ {139}\) From the tests, Garfield concluded that the project should be a three part program:

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\(^{135}\)“Compiling a citation index to a selected list of articles would increase problem of scanning the bibliographies and references in articles from which citations would be taken. For example, if a paper in “Nature” is included in our sample, we would have to carefully examine citations to Nature. However, this would offset the cost of handling a larger quantity of citations in non-genetics articles.” (Garfield to Lederberg, September 9, 1959, originally prepared August 15, 1959).

\(^{136}\)Two years before, Garfield had discussed these matters with the National Library of Medicine, which could build a special-purpose camera for about $1,000.

\(^{137}\)Garfield to Lederberg, September 9, 1959 (originally prepared August 15, 1959).

\(^{138}\)Garfield to Lederberg, September 3, 1959.

\(^{139}\)“The conclusion to be drawn from test#2 is that we can compile a complete citation index to all the general science journals, making the sample not only interdisciplinary but permanently useful when it is finished. The work will not be wasted. This ties in beautifully with another idea I had
In conclusion, pending comments from you, G. Allen and others\textsuperscript{140}, I feel that a revised proposal to NSF should be based on the following three part program of Citation Index research.

1. Mechanically (photographically) pick up all references found in a specified list of genetics journals and articles, the latter based on some well known genetics bibliography. From these eliminate undesired references.

2. Scan all Current Content journals for references to all articles appearing in a specified list of genetics journals and a specified list of articles or authors.

3. Scan a large list of journals from all representative scientific disciplines for references to general science journals including Nature, Science, Proc. Natl. Acad., etc.\textsuperscript{141}

This would result, according to Garfield, in three different indexes: a complete citation index of a list of genetics journals, a complete index of a list of articles in non-genetics journals and an index of all articles in general science journals. Garfield wished to scan “at least the last five years of the literature, preferably more”. They could be published as “individual journal articles or supplements”, or as a single combined “Citation Index to Genetics”. Testing should be done by handing out copies of the index to “geneticists and various libraries” and awaiting their comments. Lederberg “strongly disagreed” with “the addition of interpretative material” such as classifying the type of references or subjects:

I also disagree that you should attempt any subject classification, e.g., genetics, on the grounds that this defeats the main advantageous purposes of CI, namely a mechanical system of classification. If you start analysing the references, you might as well start trying to analyse the content of the paper, and you are back to abstracts.\textsuperscript{142}

He also emphasized the necessity of differing from the traditional, disciplinary approaches: “a good general CI will be of greater value to Genetics than a too specialized run that sticks too closely to the discipline”. He therefore preferred the citation index to the general science journals, Garfield’s research program’s third part. Contrary to Garfield, Lederberg had the strong feeling that there was actually no further research needed:

If you stick to your guns on the original principles of CI, I am sure you will find it widely used as a research tool, and that further perfections will evolve.

\textsuperscript{140}Garfield sent a copy of his letter to Gordon Allen, Katherine Wilson (NIH), and Connie Tolkan and George LeFevre (NSF).

\textsuperscript{141}Garfield to Lederberg, September 3, 1959.

\textsuperscript{142}Lederberg to Garfield, October 6, 1959.
What we need more than anything else is to get it going. (...) I know that you yourself will be keener to do the kind of analysis you’re discussing than just to go ahead with CI as is. But I think there will be much more support for you if you can demonstrate what CI can do. (...) My main aim, as you know, is to encourage you to get on with the work as simply and straightforwardly as possible. If it works out as well as it must, you should have little concern for enthusiastic support for your own research using CI.143

2.3.7 Growing support

The idea of citation indexing scientific literature started to catch on slowly. The board of the American Society for Human Genetics reacted sympathetically144. NIH’s Katherine Wilson urged Gordon Allen not to wait for action from her group but proceed with their plans145. At NSF, the mood was changing. “Things have changed, in that they are now listening very carefully because you and the genetics people have shown some interest”, reported Garfield146. He had even been told that he did not have to stick to $29,000 per year, the amount of money did not matter provided the idea was accepted147. The foundation insisted on a committee of scientists, however, and was not willing to take up Lederberg’s idea to form it itself148. The Human Genetics Society was also reluctant, so Garfield started phoning researchers himself149.

Lederberg paid him his first visit in Philadelphia the same month150. They discussed the committee in more detail. Lederberg had by now developed firm opinions on the way the project should be organized. As Garfield reported a few days later to Gordon Allen151, Lederberg stressed that the committee should not meet too often152, that the project should run for at least three years and that it

143Lederberg to Garfield, October 6, 1959.
144The board had met August 30, 1959 at State College, Pennsylvania. Gordon Allen explained the principles of citation indexing as a scheduled item on the agenda. None of the board members present expressed doubt of the potential value of a citation index, Allen reported to Garfield and NSF director Dwight Gray. The society did not, however, see it fit to take action as society. Allen got permission to report “their favorable response as individuals”. (Allen to Garfield, October 26, 1959)
145Allen to Garfield, October 6, 1959.
146Garfield to Lederberg, October 9, 1959.
147Garfield to Lederberg, October 9, 1959.
148Garfield to Lederberg, October 12, 1959.
149By that time, Garfield only had the support of Allen and Lederberg, with the added practical problem that Lederberg worked at Stanford and Allen in Washington. Therefore, he wished to concentrate the committee either in the West or in the East of the States. He asked Lederberg to get “two or three nearby in or out of genetics”. He clearly counted on Lederberg: “Finally, if I am being presumptuous in assuming you can work on this committee at all then let me know that too, which admittedly would be a disappointment, but I’ve gotten used to that. In that case we would again select all Easterners” (Garfield to Lederberg, October 12, 1959).
150This meeting took place on the Friday before October 29, 1959, and was the first time the two men met (Joshua Lederberg, Interview, February 3, 1992, New York; Garfield to Allen, October 29, 1959).
151Garfield to Allen, October 29, 1959.
152He was willing to attend one meeting per year. An optional “local committee”, comprised by Garfield, Allen and people from the East coast, could meet more often.
should process literature produced during the previous five years. He further urged Garfield to apply both to NSF and NIH. This would enable coverage of all types of journals. Garfield wholeheartedly agreed to this: “Since the file that would result from this would enable us in the future to do anything but attack the literature comprehensively”\textsuperscript{153}. This was underlined by their conclusion that “it costs only about one cent per citation to prepare a punched card for subsequent use in a computer”. Hence, it would be no more expensive than selectively entering citations to a particular set of journals. Their discussion did not turn up anything really new, but it did lay the ground plan for the citation index project as it would emerge later. Stimulated by NSF’s remark that money was not the main problem, they increased their budget to around $200,000 for an eight-year period. This meeting was also the first time they discussed the idea of a national newspaper for science\textsuperscript{154}. Later, Garfield attended a meeting at NIH where he presented a dummy of a tabloid size newspaper. This newspaper would have listings of science communications as well as citation indexes to them.

2.3.8 Delay

Although in October 1959, Garfield intended to submit the proposal “in the very near future”\textsuperscript{155}, it actually took a little over five months. One reason for this was that another proposal Garfield had written at the request of NIH was subsequently turned down. Once again, the profit-making status of his company proved an important obstacle\textsuperscript{156}. Not only did this make Garfield wonder whether his proposals had any chance at all\textsuperscript{157}, it also stimulated him in changing the organization of his work. He contemplated the formation of a non-profit institute but was advised not to do so\textsuperscript{158}, by amongst others Chauncy Leake\textsuperscript{159}. He did, however, settle on a new name to bestow a more “acceptable” status — the Institute for Scientific Information\textsuperscript{160}, although this took time\textsuperscript{161}. Garfield was moreover involved in “a lot of work” because of the production and increased acceptance of \textit{Current Contents}\textsuperscript{162}: “This has slowed down somewhat my prepa-

\textsuperscript{153} Garfield to Allen, October 29, 1959.
\textsuperscript{154} Garfield to Lederberg, November 17, 1959.
\textsuperscript{155} Garfield to Allen, October 29, 1959.
\textsuperscript{156} Garfield to Lederberg, October 12, 1959.
\textsuperscript{157} “Since it takes a lot of time, money, and energy to prepare these proposals I am not anxious to work them up if we are beat before we start.”
\textsuperscript{158} The main reason was a possible conflict of interest between Garfield Associates and its affiliated non-profit institute.
\textsuperscript{159} Garfield to Lederberg, October 12, 1959.
\textsuperscript{160} “In this way we would have the benefit of an “acceptable” name and none of the legalistic difficulties of forming a non-profit institute.” (Garfield to Lederberg, October 12, 1959)
\textsuperscript{161} In March 1960, Garfield explained the delay in a letter to the members of the prospective advisory committee Lederberg, Allen, LeFevre, Melnick, and Spiegelman: “Perhaps you have wondered why I have not written sooner concerning our plans on Citation Index research. This letter is meant to advise you that I have not given up the idea of pursuing this project. However, recent developments have necessitated a brief delay in forwarding our revised proposal to the National Science Foundation.” (Garfield to Lederberg, Allen, LeFevre, Melnick and Spiegelman, March 14, 1960).
\textsuperscript{162} Garfield to Lederberg, March 14, 1960.
ration of a proposal for citation index research.” In December 1959, he was told by NSF that it was highly improbable that funds would be available from the year’s fiscal budget “as a result of heavy grants to Chemical Abstracts and Western Reserve Univ.” Garfield decided to defer submission of the citation index proposal to “on or before March 1st”. Moreover, Garfield had written several other proposals to NSF which also needed funding. He had submitted a grant proposal for a Current Contents edition for space and physical sciences, as well as a proposal for another major project, the Index Chemicus. The latter project was one of Garfield’s top priorities. Garfield was also working on a “Copywriter”, a device which would, Garfield hoped, also help to compile a citation index.

The citation project was delayed but had not completely ground to a halt. Garfield continued computing various ways of compiling the citation index, focusing on the comparison of comprehensively processing all citations with the selection of citations to a journal like Science. He also paid renewed attention to the problem of publishing a future citation index. This did not prevent Lederberg from becoming a little worried. He inquired whether Garfield had considered creating the possibility for private investors to buy stock in his new Institute for Scientific Information. This question was motivated by “some anxiety about getting Citation Indexing under way”, Lederberg explained. He understood Garfield’s “being fed up with the federal grants situation” and proposed two possible other avenues: getting help from private foundations (notably the Rockefeller Foundation) and “raising capital by public subscription”,

which I bet you could do on SCI* alone, apart from your other important contributions. It is refreshing to see the kind of action you have generated while everyone else is talking, and while we have disagreed on some minor details, you certainly do have my confidence in the way indicated by my inquiry.

* SCI = Science Citation Index (free gift to you).

Note that Joshua Lederberg coined the now famous name Science Citation Index, which he especially liked because the abbreviation SCI stresses the link with science. He stressed the importance of the citation index, which in his opinion

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163 Garfield to Lederberg, November 17, 1959.  
164 Garfield to Lederberg, December 14, 1959.  
165 Garfield to Lederberg, March 14, 1960.  
166 Lederberg suggested two private firms to Garfield that could help develop his Copywriter (Lederberg to Garfield, received April 4, 1960).  
167 Garfield to Lederberg, November 17, 1959.  
168 “As a query in passing, is there an opportunity for private investment in your new Institute for Scientific Information? I might be quite interested myself; if you were interested further I suspect some of my colleagues would be too, though I am sure you do not lack very much in this direction.” (Lederberg to Garfield, April 8, 1959).  
169 Lederberg to Garfield, April 23, 1960.  
170 Lederberg had some recent experience when he wrote this. The Rockefeller Foundation had supported his planetary microscope evaluation project “knowing that NASA was embroiled in the usual government red tape”. (Lederberg to Garfield, April 23, 1960) Garfield accepted Lederberg’s idea to call the index the SCI rather off-handedly: “Incidentally, thanks for the gift”. (Garfield to Lederberg, April 26, 1960)  
“beats the other projects hands down in importance”. Garfield tried to reassure Lederberg:

As far as the relative importance of the various projects is concerned, I agree with you wholeheartedly that SCI is at the top of the list. That is why I want to put my best foot forward on it. I don’t want to mess it up by careless planning and thinking and, as you know, there are definitely some problems in carrying it out.\textsuperscript{172}

He wished moreover to stick to trying to get a grant from NSF, not believing in the stock selling potential of the SCI, nor having had encouraging experiences with the Rockefeller Foundation\textsuperscript{173}.

2.3.9 Submission

One month later, Garfield sent in his proposal to NSF and asked Lederberg to support it\textsuperscript{174}. Lederberg promised to do so\textsuperscript{175}, and wrote the same day to NSF in no uncertain terms:

I am sure there would be little point in adding to the testimony of scientific exasperation at the tremendous problem of coping with the existing scientific literature. There is no one solution to this problem; albeit the topical abstracting services do perform a useful function. To my own mind, and this is a considered conclusion, the Citation Index would be of inestimable value in improving the efficiency of scientific research insofar as this depends on useful access to the literature. (...) I am deeply and enthusiastically interested in the success of Mr Garfield’s proposal and am happy to have the opportunity to support this endeavor by serving on his advisory committee.\textsuperscript{176}

Lederberg stressed SCI’s “special value in interdisciplinary work”, especially if it would be compiled “on a global scale so as to encompass the entire literature”:

If this problem were entirely in my own hands, and this is the main point on which I differ with Mr. Garfield, I would not confine the initial efforts to a single field but would waste as little time as possible in securing comprehensive coverage.\textsuperscript{177}.

\textsuperscript{172}Garfield to Lederberg, April 26, 1960.
\textsuperscript{173}“I have tried to work with the Rockefeller Foundation on other matters, such as Current Contents, and drew a complete blank. Generally they are not terribly interested in problems related to scientific documentation. (Garfield to Lederberg, April 26, 1960).
\textsuperscript{174}“A call to either of these fellows would not hurt. The last time you said something to somebody about citation indexes things started to happen.” (Garfield to Lederberg, May 23, 1960).
\textsuperscript{175}“As you suggested I will write to NSF myself to verify my own specific interest in the program. I have in mind also communicating with George Kistiakowsky, Scientific Adviser to the President, but it may be better to wait to see what happens at NSF as this would amount to going over their heads” (Lederberg to Garfield, May 27, 1960).
\textsuperscript{176}Lederberg to Adkinson, May 27, 1960.
\textsuperscript{177}Lederberg also expressed this preference to Garfield: “I think the proposal for a separate Citation Index to each journal is an ingenious one but really a stopgap. The full program, in my opinion, would call for an independent publication of the unified SCI” (Lederberg to Garfield, May 27, 1960).
Lederberg found Garfield’s proposal “quite impressive”\textsuperscript{178}. Allen nevertheless proposed “a radical change in the design”. In the draft proposal, Garfield opted for a selective scanning of citations to genetics journals. Allen agreed with Lederberg that they should “not fool with the selective scanning, but go immediately to full coverage for a trial period”: “I still feel that, if the necessary budget and facilities could be obtained, complete coverage would be a better approach”\textsuperscript{179}. He also provided Garfield with a set of biochemistry articles that proved the usefulness of the SCI\textsuperscript{180} and gave him advice on the precise formulation of several crucial passages in the proposal\textsuperscript{181}. Garfield changed the proposal in accordance with Allen’s comments and proposed gently steering towards a comprehensive compilation:

I am glad that you agree that the comprehensive approach would be more satisfactory. My plan has been this—to wait for NSF’s reaction, but no later than two weeks from now—and then send in a proposal to NIH and in this proposal we will go for the comprehensive approach and sacrifice the number of journals covered—particularly in the physical sciences—so that we can have a low enough budget to do what we do completely. (...) I don’t think there is any opposition from our committee to the comprehensive approach. It is a question of how to convince the NSF people.\textsuperscript{182}

\textsuperscript{178} Lederberg to Garfield, May 27, 1960. The other members of the committee reacted favourably as well. LeFevre thought it was okay (Garfield to Lederberg, June 1, 1960), while Allen wrote that the application looked “very good” (Allen to Garfield, May 27, 1960).

\textsuperscript{179} Allen emphasized this in a renewed statement of his position: “If I am in any way responsible for the selective scanning idea and concentration on genetics, I now wish to reverse myself. If others on the committee express this same view, perhaps you should consider reducing the number of journals scanned, and reading out all the citations in those journals. This would be more accurate, would really cut down the labor of compiling a more extensive SCI in the future, and would still permit you to sort out the journals cited to permit compilation of citations in genetics and general science”. (Allen to Garfield, May 27, 1960).

\textsuperscript{180} “each one appeared, when published, to be correct and final, but each has been subsequently disproved or significantly extended. A person who read one of the earlier articles would be unlikely to suspect its inadequacies, and without a citation index at his elbow would probably proceed in ignorance of the current state of knowledge.” (Allen to Garfield, May 27, 1960)

\textsuperscript{181} He advised Garfield to revise his statement about the choice of genetics as the field for the citation index: “you singled out genetics as a discipline differing from others in its indexing requirements. Perhaps I did once say that genetics was particularly in need of a citation index, but what I think I meant, and certainly what I would say now, is that many illustrations of the need can be found in genetics. I believe that every area of active scientific advance would benefit about as much as genetics.” He moreover showed Garfield that the mathematical relationship between the number of references in an article and the number of citations was far from straightforward, mainly because science is not in a “perfect steady state but it is growing and expanding rapidly” (Allen to Garfield, May 27, 1960). This was not only a theoretical but a practical question as well, because the number of citations to be processed would determine the compilation costs. Garfield thought about this himself a lot since he had found that the average number of references per paper in the biological literature was 15. He concluded that he did not have to be very worried about the average length of citation index entry. Garfield double-checked the labour involved by having the bookkeeper type in citations for two hours. (Garfield to Allen, June 1, 1960, and June 3, 1960).

\textsuperscript{182} Garfield to Allen, June 1, 1960.
One day later, Allen sent his set of biochemistry articles together with a drawing of the citation relationships between them. Gordon Allen had, in other words, drawn the first citation network:

The arrows indicate the direction in which one would be led in a conventional literature search, starting at any point on the network. A citation index would permit one to trace the arrows in the opposite direction, and hence to find all the articles no matter where on the network he started.\(^{183}\)

He emphasized that this small network was an extract from “a considerably more voluminous literature on the same topic, all tied together with citations”. Garfield reacted strongly:

The material you sent me is magnificent! This must have been a great deal of work. It is fabulous. Why didn’t we think to do this before. I didn’t have this in mind when I said I had some examples of the power of the Citation Index. I merely meant specific articles which could be traced through a CI. (...) I once had the idea that some type of network theory could be used with Citation Indexes. I am now convinced more than ever, from your example, that this will be true.

NSF’s first reaction to Garfield’s submission\(^{184}\) — in a letter to Lederberg — clearly demonstrated its ambiguous attitude towards citation indexing:

This technique is one of a number about which people long have had a variety of subjective opinions — some pro and some con — without there having been any really sound experimental investigation of the idea. The citation index approach would seem to have considerable promise for leading the researcher rapidly and efficiently to the significant literature in a given subject area with, perhaps, the major element of possible weakness being the fact that its effectiveness necessarily depends on the care and seriousness with which authors of papers select the references they cite.\(^{185}\)

This attitude was “not altogether negative”\(^{186}\), though the reaction annoyed Lederberg who began to understand Garfield’s impatience:

In fact, I am beginning to get a glimmer of understanding as to the basis of and the magnitude of the hostility or misappreciation for SCI. Just because it is a tool which will be handled automatically by scientists themselves it will tend to give less scope and importance to professional information-handling bureaucracy.\(^{187}\)

\(^{183}\) Allen to Garfield, June 2, 1960.

\(^{184}\) NSF had also been approached by Allen who, like Lederberg, indicated his preference for a comprehensive citation index (Allen to Adkinson, June 6, 1960).

\(^{185}\) Fry to Lederberg, June 3, 1960.

\(^{186}\) Lederberg to Garfield, June 21, 1960. Garfield did not fully trust this: “sounds promising but so did it the last time I sent in a proposal. They never discourage you before you do all the work!” , he wrote to Allen (Garfield to Allen, June 24, 1960).

\(^{187}\) Lederberg to Garfield, June 21, 1960.
Garfield could not agree more:

I am glad that you are independently getting a glimmer of the kind of hostility I have encountered—not only with regard to the citation index, but with regard to any ideas which tend to free the scientist from dependence on an intermediary—whether it be the librarian, the government bureaucrat or what have you.\(^\text{188}\)

On June 9, Garfield got a telephone call from NSF, telling him that it would take at least four if not five, months before NSF could make a decision\(^\text{189}\). Two weeks later, Garfield sent his proposal to NIH (Garfield 1960\(^\text{b}\)), which was “almost the same as to NSF except I added a modification suggesting the comprehensive approach”\(^\text{190}\). He also included Allen’s citation network sample as an appendix\(^\text{191}\). By now, Garfield had actually decided to go on with the citation index regardless of the reactions of NSF and NIH:

I will let nothing stand in the way of getting a citation index going. (...) The more I think about C.I. the closer I get to agreeing with you that it might be turned into a practical ISI project regardless what NSF and NIH decide.\(^\text{192}\)

Moreover, he moved to form a non-profit organization\(^\text{193}\) “to take over the work of the SCI project”\(^\text{194}\). On July 7, NSF phoned Garfield to suggest some minor changes. Garfield included most of them\(^\text{195}\) and resubmitted the proposal on July 15 (Garfield 1960\(^\text{a}\))\(^\text{196}\).

\(^{188}\)Garfield to Lederberg, June 24, 1960.
\(^{189}\)Reasons given were the summer vacation and the end of the fiscal year (Garfield to Lederberg, June 9, 1960).
\(^{190}\)Garfield to Lederberg, Jun 24, 1960. Garfield included “almost all” changes Allen had recommended (Garfield to Allen, June 24, 1960).
\(^{191}\)He also showed them to Lederberg: “I thought you would appreciate seeing this rather interesting “network” diagram G. Allen worked out (Garfield to Lederberg, June 24, 1960).
\(^{192}\)The latter part of the quote is pencilled in ink, after Garfield’s secretary had already typed the letter (Garfield to Lederberg, June 24, 1960).
\(^{193}\)This was partly motivated to prevent conflict of interest situations. An example of this surfaced in a discussion between Allen and Garfield about Allen’s position in NIH. Allen had still not heard from NSF when he informed Garfield that his name on the application to NIH might “carry bad luck”. He had “a strong impression” that Walter Burdette, chairman of the NIH Genetics Study Section, mistrusted or disliked him and that he might be accused of a conflict of interest (Allen to Garfield, June 28, 1960 and July 11, 1960). Garfield did not like the idea of deleting Allen’s name at all: “Unless you give me some strong reasons for eliminating your name I see no reason to do so. Naturally I don’t want to jeopardize (sic) the project but I also don’t intend to desert my friends for a few sheckels. Let the truth be known. You were one of the first to take up my proposal and I am grateful to you for it and for the great deal of time you have put into it (Garfield to Allen, July 7, 1960). Garfield later reassured Allen, informing him of the formation of a non-profit organization: “I suppose one could still have a conflict of interest regarding a non-profit organization but I suppose that it would be harder for anyone to say it was for “profit” reasons.” (Garfield to Allen, July 26, 1960) Allen complied: “Removing my name from the application would not hurt me in any way, and might help you, but perhaps it is best to reveal my association with the idea, for better or for worse. As for myself, I am proud of the association. (Allen to Garfield, July 11, 1960)
\(^{194}\)Garfield to Allen, July 26, 1960.
\(^{195}\)Garfield to Allen, July 7, 1960.
\(^{196}\)NSF acknowledged its reception July 21, 1960 (Gray to Garfield, July 21, 1960).
2.3.10 The genetics proposals

The proposal for a genetics citation index to NSF was thoroughly rewritten and no longer comparable with the feasibility study applications of 1958. Bringing the scientist up-to-date on a particular paper was the first application mentioned. The overcoming of “artificial dividing lines” between existing abstracting services was mentioned as “a special advantage” of the SCI. The proposal moreover stressed that it was not meant to replace existing subject indexes and that the SCI would be especially useful for writers of review articles. These applications were supported by Gordon Allen’s citation network (figure 2.2 on page 54) which was added in the appendix. Garfield made clear that “one of the most attractive features” was the susceptibility of the index to “complete mechanization”:

Compilation by a staff of trained scientists is not necessary in order to index papers as the “indexing” has already been done by authors in providing citations to earlier papers. Compiling the SCI is almost completely a routine task of copying citations in new papers, sorting them in order by journal, year and page (so that all references to the same paper will be brought together), and then distributing the information either as a printed bibliography or in card form. (Garfield 1960a)

This argument was reinforced with the tabulations and testing Garfield had undertaken during the previous months:

The primary factor determining the feasibility of compiling a SCI is a quantitative one. The first general impression is that there are so many references in the literature as to make a SCI huge and unwieldy. Fortunately this is not true, as extensive preliminary studies have shown.

These computations relied heavily on Garfield’s experiences with *Current Contents* which by this time processed around 600 journals. Sixty per cent of the journals needed for the genetics citation index were already in house thanks to *Current Contents*. Garfield based his reasoning on his finding that the average number of references per article in the biological literature was fifteen. The CC-journals published 125,000 articles per year. Extending coverage to 1000 journals would lead to three million citations per year. Based on the test runs he expected production costs would be two to three cents per citation, i.e. between sixty and ninety thousand dollars per year. Garfield found it a surprisingly low figure when compared with the several million dollars needed to abstract and index the same amount of articles in the conventional way. He subsequently showed

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197 The appendices of the proposal to NSF (Garfield 1960a) consisted of: Gordon Allen’s sample of biochemistry references; the diagram of its citation network; the actual appearance of a printed citation index of Allen’s sample; the physical science journals covered by *Current Contents*; the life science journals covered by *Current Contents*; Garfield (1955); Adair (1955); Garfield (1957); Seidel (1949); Hart (1949); Garfield (1956a). The NSF proposal and its first three appendices were added as appendices to the NIH proposal (Garfield 1960b), while 40 copies of the articles on citation indexing were separately attached for the reviewers.

198 An alphabetical list of the life science journals covered (slightly more than 500) was added to the proposal as an appendix (Garfield 1960a).
that selective compilation could actually lower the number of processed citations while at the same time its production per citation would be four times more expensive. Therefore:

> As the number of selection criteria increases and the number of pertinent references found increases one rapidly reaches the point where it is cheaper and more efficient to process every citation. (Garfield 1960a, 4)

In the proposal to NIH, Garfield was even more explicit:

> Allen and Lederberg (and myself) are of the opinion that the comprehensive approach is more sensible, economical and productive. If I were in a position to ask for the ideal support I would ask for sufficient funds to cover the compilation of a complete Science Citation Index (SCI), covering both current and old literature, utilizing evaluative and editorial techniques for building the Citation Index into a true encyclopedia of unified science. (Garfield 1960b, 5)

In the face of “the immediate practical difficulties of obtaining such unlimited support”, he sketched the compilation of “a body of information which could be added to without difficulty if further additional support were found later” (Garfield 1960b, 5). In his proposal to NSF, Garfield proposed to follow the selective approach “in order to keep the budget of this research project as low as possible”. The money thereby saved could be used to process a five year backlog which was necessary to demonstrate the value of the citation index. This resulted in the proposal to NSF (Garfield 1960a) to construct a citation index by scanning a
CHAPTER 2. THE CREATION OF THE SCIENCE CITATION INDEX

J. AMER. CHEM. SOC. 63, 69 (1941) #1
    Zanker, V. Z. Physik. Chem. 199, 225 (1952) (#4)
    Lawley, P. D. Biochim. Biophys. Acta 19, 328 (1956) (#6)
    Schoenberg, M. D. in press (#15)

COLD SPRING HARBOR. SYMP. XII, 131 (1947) #2

Figure 2.3: First lines of the example of the actual appearance of a printed SCI as included in the proposals to NIH and NSF.

list of 1000 journals and processing all references to 43 specified genetics journals as well as all references to 22 specified general science journals. The proposal to NIH, on the contrary, entailed the processing of all references from the specified journals, punching them into IBM cards which were subsequently sorted mechanically. Only then would the citations to genetics journals be selected and printed (Garfield 1960b). If both NSF and NIH were to decide to support the project, the NSF funds would be used to “cover backlog” (i.e. process older literature) and the NIH money could be directed to increase the coverage of current literature.

The index would be published by providing the editors of the journals with an individual journal citation index which could then be published as a yearly supplement. This was only an intermediate mechanism, though. Garfield wished to keep the option of a separate publication open and wrote that he was “in correspondence with editors on the publication problem”. As figure 2.3 on page 55 shows, Garfield was by now thinking of printing the SCI in bibliography style. The total budget was estimated at $156,000 for three years.

2.3.11 Convincing NSF and NIH

In order to win over NSF, Garfield prepared the formation of a non-profit organization, “The National Documentation Center”. He first sounded out Lederberg about this, inquiring whether this would lessen his interest in future ISI stock.

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199 Garfield did not conceal this difference: “This method stands in sharp contrast to an alternative method proposed in a co-pending application to the National Science Foundation.” He explained this by referring to the existing “dichotomy of opinion on how to approach the problem of studying citation indexes” (Garfield 1960b).

200 “Essentially the same proposal has been submitted to the National Science Foundation. If support were received from both NSF and NIH then each grant would be used to support additional research. The NSF funds would be used to cover backlog, NIH to cover current literature.” (Garfield 1960b, 3)

201 Eight scanner-key punchers, “if feasible part-time graduate library students”, would produce the punched cards. Part-time project supervisor would be Gwen Bedford, University of Pennsylvania, who had also written the 1958 feasibility study proposals.

The geneticist agreed that “if this will help to secure and maintain federal backing it would be desirable to entrust the development of SCI to a non-profit organization.” He was less certain about the name: “it has some connotation of a governmental activity and if this is felt and thought to be presumptuous, it might do more harm than good.” When Garfield informed NIH of the NDC plan, secretary Wilson reacted immediately, telling Garfield that this would have “an important impact at NIH.” By this time, Garfield had experienced the effects of the new name of his institute:

> You would be amazed (...) how many doors the new name has opened for us. There certainly is something in a name!! I have never seen anything like it.

Nevertheless, Garfield did not really like the non-profit angle and meant to pursue this route only if it was necessary to convince NSF and NIH.

Lederberg, thinking about how to get NIH support, suggested that Garfield would bring up the advantage of the SCI in evaluating the impact of funding with Katherine Wilson:

> Quite seriously with so many agencies anxious to know just what their real effect is, a quantitative measure such as SCI would very readily furnish would be a very valuable tool for them.

Garfield agreed, telling Lederberg that in the past he had tried to sell SCI to the Air Force “for exactly the reason given in your letter.”

At NIH, discussion moved to the issue of publishing the SCI. November 7, NSF resumed contact with Garfield, explaining that the delay had been partly caused by the similarity of both of his proposals to NSF and NIH. The agency

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203 Lederberg to Garfield, August 1, 1960.
204 Lederberg therefore prosed to find another adjective than “national”.
205 Garfield to Lederberg, August 13, 1960.
206 Garfield to Lederberg, August 13, 1960.
207 Lederberg to Garfield, July 29, 1960.
208 Garfield to Lederberg, August 13, 1960.
209 Wilson put Garfield in contact with Burdette who asked him whether Garfield could get out a printed publication within the budget outline. Garfield got the impression that NIH wished to subsidize the compilation while he would carry the responsibility to publish the SCI “at a reasonable price”. He concluded that a non-profit organization might not be needed but was not sure. Burdette also asked Garfield about the advisory committee which made him anxious that Allen might have been right after all. Garfield did not know what to make of this (Garfield to Lederberg, September 21, 1960), but neither did Lederberg: “I don’t know what to make of all the cross-currents at NIH” (Lederberg to Garfield, approximately October 7, 1960). 210 I am sorry it has taken so long since we saw you to get in touch with you regarding the citation index proposal. You probably have thought the train disappearing permanently with us after our dash to the station with you. (...) The delay has stemmed partly from press of other work and partly from some lag in getting together with the NIH people to pool our ideas of just what each of us would want from the overall project and who would pay for what, assuming the two agencies consideration of your proposals results in a pair of green lights.” (Gray to Garfield, November 7, 1960).
211 “The fact that the two proposals as they now stand are almost identical although not covering (and not intended to cover) mutually exclusive tasks makes them a little difficult to process.” (Gray to Garfield, November 7, 1960).
asked whether Garfield could not prepare “a single, composite budget which will give a total itemized breakdown that will better lend itself to joint discussion by NIH and ourselves”. NSF was primarily interested in “a sound test of the value of citation indexes as a bibliographic tool”, whereas NIH’s primary goal was to obtain a usable genetics citation index. Garfield consequently revised the project budget and increased it to $100,000 per year basing it on the approach: “all journals processed will be processed comprehensively, i.e. every citation in every article will be ‘carded’”. Garfield reported to Lederberg: “We are definitely making significant progress on the Citation Index Project. (...) It appears that everyone now is enthusiastic about the “comprehensive approach”. Two weeks later, he estimated that the project “has better than 75 per cent chance”. On December 26, he could at last break the big news to Lederberg:

Dear Josh,

The official note that NIH approved our grant came in the other day. This was quite nice xmas present to say the least.

and to Allen:

Dear Gordon,

Santa Claus was very good to us. We learned that NIH approved its half of the revised budget which NSF asked me to submit based on 100,000 per year for three years.

Lederberg congratulated Garfield. Allen, who had not heard from him since that summer, was relieved:

This is wonderful news. You had been silent about it so long, and I thought so long after the date when a decision should have been made, that I had about given up hope.

NSF needed more time than NIH. Garfield was told that he would not hear until “after New Year”. “Apparently, there is some difficulty in view of the fact that NIH is making a grant to us but NSF is negotiating a contract”, concluded Garfield. Only two months later was this problem solved. By now, Garfield

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212 By now, the number of key punchers was increased to thirteen. The budget was also signed by Marvin Schiller, who had become Associate Director of ISI. (Garfield to Gray, December 7, 1960)
215 Brewer to Garfield, December 15, 1960; received December 23, 1960. The “notification and statement of grant award” granted $49,450 per year for three years.
216 Garfield to Lederberg, December 26, 1960.
217 Garfield to Allen, December 26, 1960.
218 “Congratulations + happy new year” (Lederberg to Garfield, undated).
220 Garfield to Lederberg, December 26, 1960.
221 “It seems that the National Science Foundation bureaucrats made a real big issue out of the question of negotiating a contract with a profit-making organization thus holding up their processing or our grant. However, somebody finally decided to do some thinking and came up with a rather simple solution for getting around the law which prevents NSF from making grants to profit-making organizations. (...) NSF will presumably make a grant to NIH and then NIH will make the grant to us.” (Garfield to Lederberg, February 13, 1961)
“was all set”, having received his first NIH check\textsuperscript{222}, and was “trying to move ahead now as fast as possible”. ISI moved to a new building and prepared to float the public stock. The idea of the formation of a non-profit National Documentation Center was shelved\textsuperscript{223}. However, in March 1961, work had not yet begun. NSF was still in the process of transferring its half of the project budget to NIH\textsuperscript{224}, which held things up. Garfield had also been busy with other projects. He had finished his doctoral dissertation, ISI had been moving to its new building, and last but not least the prospective project supervisor, Gwenn Bedford, left Pennsylvania University for Michigan. Garfield had to lead the project himself. In May, Garfield received the signed contract from the National Science Foundation: “This means that the Citation Index project is now finally started”\textsuperscript{225}.

\textsuperscript{222}The first payment was $12,364 (Brewer to Garfield, December 15, 1960).

\textsuperscript{223}Garfield to Lederberg, February 13, 1961. Garfield inquired tentatively whether Lederberg would wish to be member of the Board of Directors of ISI (Garfield to Lederberg, February 13, 1961). The geneticist wondered if he “could not be much more useful” if he remained a proponent without a formal status (Lederberg to Garfield, March 1, 1961). Garfield “bowed to his wisdom” (Garfield to Lederberg, March 17, 1961).

\textsuperscript{224}Garfield to Lederberg, March 17, 1961.

\textsuperscript{225}Garfield to Lederberg, May 17, 1961.