

Eugene Garfield 1925-2017: Informatician Nonpareil

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Eugene Garfield Memorial Issue

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From the Editors

Eugene Garfield (1925-2017) Informatician Nonpareil

Eugene Garfield, the architect of citation analysis and the founder of the Institute for Scientific Information (ISI) passed away on 26 February. Garfield's contribution to the field of data analytics and scientometrics is unrivalled. He evolved an indexing system for literature reporting research, based on the analysis of citations used within a specific paper. Works earn an impact factor, which is a measure of citations to other science journals that serves as an indicator of their significance in the subject. Garfield's *Current Contents*, was the earliest effort to compile and disseminate important information on scientific literature.

Garfield's Science Citation Index (SCI); the embodiment of bibliometrics and scientometrics, revolutionized the methods to measure the impact of scientific research. 'I had always visualized a time when scholars would become citation conscious and to a large extent they have, for information retrieval, evaluation, and measuring impact,' Garfield stated. 'I did not imagine that the worldwide scholarly enterprise would grow to its present size or that bibliometrics would become so widespread.'

SCI is a system that charts connections between pieces of scientific literature. It is an intellectual achievement, as well as an information engineering marvel covering millions of records from numerous subject fields and communicating over worldwide networks. These databases became the foundation of the on-line research tool now

known as Web of Knowledge. And it has now become accessible electronically via the Web of Science.

Garfield made significant contributions to the understanding of the growth dynamics and internal organisation of the research enterprise, building largely on data from the SCI and later variants produced by the ISI. He popularized numerous terms now familiar to scholarly research and publishing such as citation classic, research front, and publish or perish through the regular columns he wrote in *Current Contents* and *The Scientist*.

Garfield and his works have inspired information professionals to create solutions that can help researchers in advancing the frontiers of human knowledge. He enabled information retrieval to scale up, basically creating the entire field of Informatics as we know it today.

Garfield was never afraid of criticizing politicians and administrators for their wrong policies. He pointed out the need for democratizing science advice. He urged elected officials for making scientific decisions based on speculation. He strongly criticized former President Reagan's science advisors for serving as 'advocates of the administration's science policies, rather than as objective channels for communication between the President and the science community.' He wrote: 'Few would deny that the science adviser has a challenging

assignment. He must brief the President on many varied expert opinions on science and technology matters including those that deviate from the government policy. It is difficult in the absence of a President's Science Advisory Committee (PSAC), which once provided informed and wide-ranging debate in the executive branch. Can one person really be expected, however, to represent the science community's multitude of often-contradictory views? Personal experience and professional contacts will strongly shape the information and advice offered to the President. The current trend toward a partisan role for the President's science adviser only, underscores the need for democratizing science advice in the executive branch by reviving the PSAC. But even it may not be adequate today in light of the increasing number and varied nature of science and technology issues that the President faces. A small group of scientists cannot be expected any more to provide informed judgment on all science matters by themselves'.

Garfield opined that 'the knowledge, experience and judgment of the nation's entire body of scientists and experts represents a rich resource; but that is largely under-exploited by the government'. He suggested an electronic bulletin board on which they could 'post' their views on science and technology issues. Officials and policy-makers in the executive, legislative and judicial branches of government can read that. Also it will be transparent to the people including the science community. One with a different opinion also can respond. The dialogue will continue and there will be a large and diverse body of expert opinion on a specific topic, like the need for building the Super conducting, Super collider or for mapping the human genome. Even if consensus is reached or not, decisions can be based on informed opinions. By connecting the public to the science community's expert views on various issues, the bulletin board would play an important role in creating

awareness. Policy-makers will pay more attention to the opinions expressed because they know that the public is also reading those views.

According to *The Scientist* 'Eugene Garfield was a genius of a very special type. Not only because he had an incredible imagination and brain, but he had incredible tenacity and courage.' He left his mark on science and his life and work will surely inspire generations of scientists in advancing the frontiers of knowledge.

We dedicate this issue of *Informatics Studies* to Eugene Garfield. An obituary by Ernesto Spinak, who closely watched his work for decades, remembers his findings that revolutionized the field of documentation. The article by M Jayapradeep and Bino Jose 'Eugene Garfield: The Ultimate Impact Factor' gives a bird's eye view of his life and work stressing on SCI and subsequent variants created by his ISI.

Transcript of an interview with Garfield conducted by Henry Small in 2007 for *Web of Stories* presents in his own words Garfield's childhood, education, career, research and development, business ventures, his views on NGOs, the future of Open Access, Big Science etc which can guide academic administrators, science policy makers in governments and scientists. First published in text form, this interview reveals the life of one of the greatest masters of the universe of knowledge and a successful corporate manager of information industry who developed magical products that can ensure quality of scientific and scholarly research.

We thank University of Pennsylvania, Thomson Reuters, and *The Scientist*, for the papers by and on Garfield made accessible for reference, which enabled us to compile this volume in such a short time. *Informatics Studies* is profoundly grateful to Ms. Meher Garfield, Dr. Henry Small, and Prof. Subbiah Arunachalam for the timely support, and Grazyna Dobosz, Editor, *Web of Stories* for arranging to grant permission to print the monumental interview of Garfield.



In Memoriam: Eugene Garfield

Ernesto Spinak

Abstract

Eugene Garfield developed a theory about using the article's bibliographic citations as an information retrieval procedure more efficient than those in use during 1950s. He undertook the development of a complete citation index to show the propagation of the scientific thought. He founded the Institute for Scientific Information (ISI). There he created Current Contents, the SCI and other products now collected in the Web of Science, the Journal Citation Reports (JCR) and the Impact Factor. Garfield theory corresponds, in the era of Internet, to the development of information retrieval algorithms such as Pagerank, of Google, that uses structured citation between websites through hyperlinks. Garfield revolutionized the world of scientific research and science evaluation.

Eugene Garfield was born in New York in 1925 to a family of immigrants, and earned a PhD in Structural Linguistics at the University of Pennsylvania in 1961. From the experience during his student years on indexing of the National Library of Medicine database, he developed a theory about using the article's bibliographic citations as a better information retrieval procedure than thesauri, subject headings or free vocabulary, which were research hot topics in the 1950s due to the emergence of large computers.

Garfield undertook the development of a complete citation index to show the propagation of the scientific thought, based on the ideas of the Memex presented in Vannevar Bush's famous 1945 article 'As we may think.' Along with other great men of his generation – Robert Merton with the Sociology of Science, and the quantitative theories of scientific citations by Derek J de Solla Price – who were the parents of what we know today as scientometrics. All the others, who made their mark in this specific area came following their footsteps.



Eugene Garfield 2007.

Photo from Chemical Heritage Foundation

Garfield was the founder of the Institute for Scientific Information (ISI) in 1960, located in Philadelphia, Pennsylvania, which was transferred to Thomson Corporation in 1992, later in 2008, to Thomson Reuters, and from 2016, to Clarivate Analytics. He was the creator of Current Contents,

including the Science Citation Index (SCI), the Social Sciences Citation Index (SSCI) and the Arts and Humanities Citation Index (A&HCI), now collected in the Web of Science and the Journal Citation Reports (JCR), the Impact Factor, and along with Henry Small, of Research Fronts detected by co-citations. His bibliographic production exceeds 1,000 documents that can be seen in his personal homepage.

Garfield's citation indexing theory says that if we have an interesting or relevant article that cites certain authors, and other articles that cite those same authors, surely these articles will also be of the same interest or relevance. Garfield based this indexing method on the so-called citing culture; i.e., the more cited a paper is, the greater is its importance or its impact in its scientific field. This Garfield theory corresponds, in the era of Internet, to the development of information retrieval algorithms such as PageRank, used by Google, that uses structured citation between websites through hyperlinks.

Although the ISI has been criticized for establishing idiomatic biases in favor of English, the truth is that it has revolutionized the world of scientific research and science evaluation, marking a before and an after. Indexing by citations, the research fronts and the journal impact factor have been theories and tools on which modern Bibliometrics has been supported.

One of the last giants of documentation has gone.

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Eugene Garfield: The Ultimate Impact Factor

M Jayapradeep and Bino Jose

Abstract

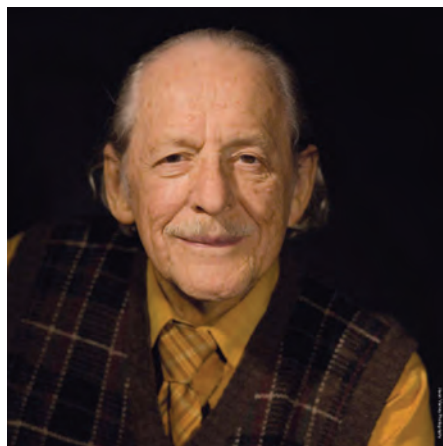
Eugene Garfield's invention of the Science Citation Index (SCI), the predecessor of the Web of Science, revolutionized the world of scientific information and made him one of the most visionary figures in scientometrics - the quantitative study of the scientific literature; as well as all areas of Informatics. Throughout a long and active career spanning seven decades, Garfield made significant contributions to the understanding of the growth dynamics and internal organisation of the research enterprise, building largely on data from the SCI and subsequent variants created by his company, the Institute for Scientific Information - Current Contents, Sciences Citation Index, Social Sciences Citation Index, the Arts and Humanities Citation Index and the like. He launched the idea of a journal impact factor, and created the Journal Citation Reports in which citation analysis is applied as a tool for journal evaluation. There is no other scientist whose work has influenced the field of Informatics to such an extent and for such a long period of history. This article gives an outline of his life and contributions.

Keywords: Citation Analysis, Science Citation Index, Web of Science, Impact Factor, HistCite, The Scientist, Scientific Research, Scientometrics, Infometrics, Informatics, PageRank.

Introduction

The contribution of Eugene Eli Garfield (September 16, 1925 - February 26, 2017) to informatics in regard to its services for scientific and scholarly research is unparalleled. He was a unique personality who influenced Information Science and Knowledge Management Scenario. Garfield has mainly pursued this career outside of academia. Technologies he has produced have revolutionized the retrieval of scientific information, and he has made considerable advances in the fields of the history and sociology of science. (Cronin and Helen, 2000)

Garfield, originally a linguist turned businessman; marketed language technology and Knowledge in a way, which no one ever



dreamed (Clarivate Analytics, 2017). He is innovator of the bibliographic control and entrepreneur with a cause. He is best known for his work in developing the field of

citation analysis - a measurement of the impact of an author, an article or a publication based on the frequency of its citation by other researchers.

Garfield founded the Institute for Scientific Information (ISI), located in Philadelphia, Pennsylvania where he did most of his research and development work. He and his institution have played crucial roles in the landscape of Informatics, ie from documentation to informatics through information science. He was President and CEO of the ISI from 1960 to 1992. His contribution to citation analysis arguably began when he founded the ISI. At ISI he went on to devise the journal impact factor, a statistical framework for quantifying the reach of a particular journal in the scientific community based on citations of its publications and the Journal Citation Reports, an annual publication detailing the impact factors and other citation data for thousands of scholarly journals.

According to Garfield, the citation index can help a historian to measure the influence of an article that is, its *impact factor* (Garfield, 1955). The creation of the Science Citation Index made it possible to calculate impact factor which ostensibly measures the importance of scientific journals (Wikipedia, 2017). It led to the unexpected discovery that a few journals like Nature and Science were core for all of hard science. However, the same pattern was not followed for the humanities or the social sciences.

Early Years

Garfield was born in 1925 in New York City as Eugene Eli Garfinkle, and was raised in a Lithuanian Italian Jewish family. He came from an educationally weak family back-ground. His father was a newspaper-magazine distributor and mother, a housewife. His parents were separated before he was born. His stepfather was a butcher who later changed to taxi-cab driving. His uncle who was a coat and suit manufacturer

by profession became his guardian. Garfield was not his original name but name of father's firm, the Garfield News Company. Political and other trivial discussions were a regular affair at his home but not on science or any educational or academic matters. There was only one who finished college education from his family. After primary education he attended a science high school at Stuyvesant. He had no real mentors throughout the high school days. As a student he showed much enthusiasm to study foreign languages and due to this he was later transferred to DeWitt Clinton High School in the Bronx. He was not a good student in high school except for mathematics. He did not even encounter a scientist or teacher who could have made an impact or steered him in right direction for further education during his high school days as well as undergraduate days. However, during the last year at Clinton High School, he came across Wilmer T Stone, an English teacher and former journalist and got some direction for his further studies. At 17, he started out in chemical engineering at the University of Colorado, but it was wartime, so he left soon for San Francisco, worked in a shipyard, and after the war, he returned to Berkeley.

Garfield studied at the University of Colorado and University of California, Berkeley before getting a Bachelor of Science degree in chemistry from Columbia University in 1948. During the early 1950s he worked on the Welch Library indexing project at the John Hopkins University School of Medicine, sorting and indexing documents from medical papers and journals. With a graduate degree in library sciences from Columbia University, Garfield later went into business as a 'documentation consultant'. He was President of Eugene Garfield Associates, which he established; from 1954 to 1960. At the same time he worked on his doctorate in structural linguistics at the Department of Linguistics at the University of Pennsylvania, which he completed in 1961 by developing an

algorithm for translating chemical nomenclature into chemical formulas (Garfield, 1961).

The science citation revolution began more than half a century ago. Garfield, then a young librarian pursuing a PhD in structural linguistics, started wondering about that most prosaic of bibliographic tools - the footnote. Most people think of footnotes as reaching backward in time to a document's sources. But he realized that they could reach forward, to - future footnotes would cite the original article. 'The citation becomes the subject,' said Garfield; the founding father of modern citation analysis two decades back; 'It was a radical approach to retrieving information' (Garfield, 1955).

Research and Development

Citation indexing was conceived in the early 1950s as a way to monitor, organize, and retrieve published scientific and scholarly literature. Citation indexing was then not a new concept; it had been implicitly being used in legal citator such as Shepard's Citations. But citation indexing concept had not yet been applied to the literature of any field of scientific research. Shepard's Citations came into existence in 1873 to provide the legal profession with a tool for tracking subsequent decisions based on cases decided by federal and state courts (Adair, 1955). Citation index was a very old instrument, but it was a new idea to Garfield. Adair introduced citation indexing to Garfield, who also owed intellectual depth to Shepard, who produced citator for legal cases in 1873, but the citation index concept was older than that. Garfield quickly realized that citation indexing held great promise for organizing, accessing, and evaluating the ever-expanding scientific literature, and applied it.

After finishing the Project at Johns Hopkins, Garfield in constant communication with William C. Adair, a former Vice President of the company that published Shepard's Citations had come to the conclusions that

citations referenced in science journals could be used to provide a new and valuable way of indexing science literature (Adair, 1955). Garfield wanted to learn the technique of Shepard's citation indexing. He set off to the Enoch Pratt Free Library and found a copy of 'Shepard's Citations' and was overwhelmed with emotions. The method used made a strong impact on him. 'Shepard's Citation Indexing outlined the method for him to link up all the threads for science information to be indexed based on reviews. He realized that citation indexing held great promise for organizing, evaluating and accessing the ever-expanding scientific information and literature. He still had to wait for the right time for fructification of the ideas (Garfield, 1955).

Garfield was deeply involved in the research relating to machine generated indexes in the mid-1950s and early 1960s (Reuters, 2010). It was at ISI that he developed the Science Citation Index (SCI). Officially launched in 1964, the SCI recorded the bibliographical and citation contents of the world's most influential journals, allowing users to follow citation links to access specialized research most relevant to their own work. In addition to the ISI's SCI, a system used to chart connections between pieces of scientific literature was developed which later became accessible electronically via the Web of Science. He launched Current Contents, an early effort that sought to compile important information on the scientific literature.

Garfield developed an indexing system for science literature, based on the analysis of citations used within a given work. Works earn an 'impact factor,' a measure of citations to other science journals that serves as an indicator of their importance in the field. The more citations in reputed journals, the higher the impact factor. The ISI sold subscriptions to their publication the Science Citation Index and over time, grew to include the Social Sciences Citation Index (SSCI) and the Arts and Humanities Citation Index

(A&HCI), Current Contents and Index Chemicus (Garfield, 2005) which were his initiatives. Garfield was responsible for most of those innovative bibliographic products from ISI.

Garfield also served as an adjunct Professor of Computer and Information science at University of Pennsylvania during his career. In 2003, the University of South Florida, School of Information was honored to have him as lecturer for the Alice G. Smith Lecture. He was awarded the University of Barcelona's honorary degree the *Doctor honoris causa* on 14 Jun 2016.

The Institute for Scientific Information

Garfield founded the Institute for Scientific Information (ISI) in 1960. ISI was the incarnation of Eugene Garfield Associates - Information Engineers Consultancy founded by Garfield himself that was changed shortly afterwards to the ISI. It offered specialized bibliographic database services, citation indexing and analysis, a field pioneered by Garfield. It maintains citation databases covering thousands of academic journals, including a continuation of its longtime print-based indexing service - the SCI, as well as the SSCI, and the AHCI. All of these are available via ISI's Web of Knowledge database service. This database allows a researcher to identify, which articles have been cited most frequently, and who has cited them. The database provides some measure of the academic impact of the papers indexed in it, and may increase their impact by making them more visible and providing them with a quality label (Diego Varela, 2013).

The ISI also published the annual Journal Citation Reports which list an impact factor for each of the journals that it tracks. Within the scientific community, journal impact factors play a large but controversial role. Garfield himself was wary of potential misuse of journal impact factors which were initially used internally at ISI to identify the best journals to index. He Garfield spoke

out against the use of the impact factor as a shorthand way to rank publications, researchers, or institutions - a practice simultaneously decried and used by many.

A list of over 14,000 journals maintained by the ISI includes some 1100 arts and humanities journals as well as scientific journals. Listing is based on published selection criteria and is an indicator of journal quality and impact (Clarivate, 2017).

ISI publishes Science Watch, a newsletter which identifies every two months one paper published in the previous two years as a 'fast breaking paper' in each of 22 broad fields of science, such as Mathematics (including Statistics), Engineering, Biology, Chemistry, and Physics. The ISI also publishes a list of 'ISI highly cited researchers', one of the factors included in the Academic Ranking of World Universities published by Shanghai Jiao Tong University. He was the founding editor and publisher of The Scientist, (Wikipedia, 2017B), a news magazine for life scientists stated in 1986. In 2007, he launched HistCite, (Wikipedia, 2017C), a bibliometric analysis and visualization software package. The databases the Web of Knowledge he started form the foundation of the online research tool now (Clarivate, 2017B).

In 1988, JPT Publishing purchased the ISI for \$24 million. In 1992 the ISI and its citation index was acquired for \$210 million by Thomson Scientific & Healthcare and was known as Thomson ISI and has become part of the Intellectual Property & Science business of Thomson Reuters (formerly the Thomson Corporation) and it formed a major part of the science division of the company. In October 2016 Thomson Reuters completed the sale of its intellectual property and science division; which is now known as Clarivate Analytics, an independent company that owns and operates a collection of scientific and academic research subscription-based businesses including the Web of Science.

Publications

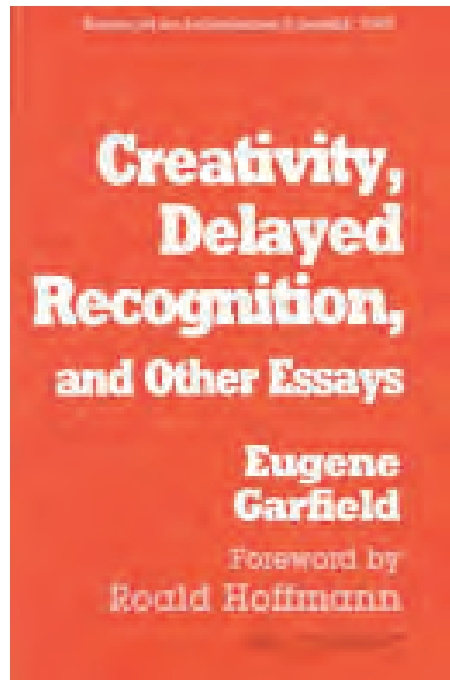
Garfield has published numerous books and articles on scientific information retrieval and related topics. Through his regular columns in *Current Contents* and *The Scientist*, he helped to popularize a number of terms now familiar to many in scholarly research and publishing, including ‘citation classic’, ‘research front’, and ‘publish or perish’ (Susannah, 2017). His books include ‘Citation Indexing: Its Theory and Application in Science, Technology, and Humanities’, ‘Dictionary of the Russian Language’, and ‘The Use of Citation Data in Writing the History of Science’. His ‘Essays of an Information Scientist’ (1962-1993 in 15 volumes by ISI Press) is the reproductions of his column *Current Comments* in ‘Current Contents’ and is considered a classic in the field of informatics. It is now available completely at his personal home page (Garfield, 2017C).

Garfield has published numerous books and more than 1000 research papers, popular articles, and reviews on scientific information retrieval and related topics. A number of his own contributions to the peer-reviewed literature went on to become citation classics in their own right, including his landmark 1955 Science paper ‘Citation indexes for science’ in which he proposed the creation and applications for what would eventually become the Science Citation Index as well as the 1972 paper in the same journal ‘Citation analysis as a tool in journal evaluation’ discussed his rationale and approach for creating the first journal Impact Factors (Susannah, 2017).

Garfield saw ‘The Scientist’ as a trade publication for working scientists. ‘He had the idea that science really needed a readable, engaging, often argumentative publication that didn’t also publish original research and could take a step back and look at policy,’ said Oransky. ‘I used to remember him coming to editorial meetings when I was at The Scientist. One needed to have the facts

straight when Dr. Garfield was in the room. He made sure you learned something in his critiques (Oransky, 2017). Garfield wrote in the editorial for The Scientist’s 25th anniversary issue. ‘There is still an important place for a publication that addresses issues such as tenure, science policy, ethics, and funding while covering key research findings and scientists who may escape the notice of traditional scientific journals’ (Garfield, 2011). The Scientist’s Editor in Chief Mary Beth Aberlin said ‘He established The Scientist as a ‘labor of love’, More than 30 years later, it is an honor to carry on his legacy’ (Scientist, 2017).

A list of Garfield’s complete works, with open access to the original documents in most cases, along with interviews and personal documents, is available on a website hosted on the server of the Library of the University of Pennsylvania, a centre at which he has lectured on computer and information sciences since 1974 which is worth reading even now. They contain writings of the early years of his *Current*



Contents columns and his intense correspondence on the creation of *SCI*. In these texts, we can find the theoretical bases for citation indexing, considered in computer terms, even though the limited capacity of calculation and digitization of information at the time did not lend itself to this approach. To learn more about Garfield and his work, we could refer to the book dedicated to him in 2000, *The Web of Knowledge: a festschrift in honor of Eugene Garfield* (Cronin and Helen, 2000).

Current Contents

Current Contents is a rapid alerting service database from the ISI that is published online and in several different printed subject sections. Current Contents was first published in paper format, in a single edition devoted only to biology and medicine. Other subject editions were added later. Initially, it consisted simply of a reproduction of the title pages from several hundred major peer-reviewed scientific journals, and was published weekly, with the issues containing title pages from journal issues published only a few weeks previously, a shorter time lag than any service then available. It consisted of an author index and a crude keyword subject index only. Author addresses were provided so that readers could send reprint requests for copies of the actual articles.

Current Contents enjoyed great popularity among researchers because, removing the congenital delay of traditional bibliographies, it allowed an increasingly wider audience to access, on a weekly basis and often ahead of print, the most up-to-date information on scientific journals' forthcoming contents. It was published in seven editions, including Life Sciences; Physical & Chemical Sciences; Agriculture, Biology & Environmental Sciences; Clinical Medicine; Engineering, Technology & Applied Sciences; Social & Behavioral Sciences; and Arts & Humanities. Each weekly issue was furnished with 'Current Comments', an essay written by Garfield himself. 'These editorials indicated

Eugene Garfield's enthusiasm, curiosity and depth of knowledge.

Citation Indexing

Citation indexing was initially designed for information retrieval, but it also provided data for evaluating the impact of a particular work of research, based on how frequently the work was footnoted or cited by other researchers. Citation analysis and infometrics pioneered by Garfield generated numerous citation databases, including the Web of Science and the Journal Citation Reports, which report impact factors and other citation data for thousands of scholarly journals.

Currently embodied and greatly expanded in the online Web of Science, Garfield's original invention Science Citation Index continues to serve as a reliable and efficient guide to the sprawling world of research. The Web of Science's body of citation statistics, documenting the most influential research in any given field as directly judged by researchers themselves, provides the basis for assessing and benchmarking the research performance of individuals, institutions, nations, and regions.

Advantages of Citation Indexing

When asked about the advantages of a citation index over a subject index by Chungji Kim, Garfield explained the possibilities of citation index, taking Medline as an example. 'Medline is based on a subject index method used in Index Medicus. It uses MeSH to control vocabulary. Index terms have to be determined in advance, and professional indexers are involved. So a subject index is vulnerable to scientific obsolescence, subjective judgment, and considerable delay. SCI, on the other hand, is a hybrid system based on natural language generated not only from title words but author-provided terms as well as expanded keywords, called 'KeyWords Plus.' In other words, SCI has its own system of self-generated index terms, which is also well suited for man-machine indexing and derived from the papers which

are cited in current articles. However, the piece-de-resistance of SCI is the Citation Index proper, the cited-reference index, which completely bypasses language all together (Chungji, 2000).

‘You have to understand the whole philosophy of what citation index means. Any document can be a point from which indexing can begin. Let say that you are writing a paper about Subject X. Even though you do not put the word X in the title of your paper, people understand that your paper addresses a very specific topic of Subject X. There could be hundreds of papers about the subject of cancer radiology, for example, but each paper is not just about cancer radiology; it is much, much more specific’. When Henry Small asked about his view on Citation as a form of currency Garfield stated ‘There’s some people now who are circulating the idea of citation as a financial reward. It is an economic activity for, the more citations you are going to get you, say tenure; it’ll get you a higher salary, that’s an economic reward. I certainly didn’t think so and I believed that citation was an emotional compensation; you get a great thrill out of having your work cited, or quoted (Arunachalam, 2007).

Science Citation Index (SCI)

SCI, created by Eugene Garfield was officially launched in 1964. The larger version SCI Expanded covers more than 8,500 notable and significant journals across 150 disciplines, from 1900 to the present. These are alternatively described as the world’s leading journals of science and technology because of a rigorous selection process (Thomson, 2010). The index is made available online through different platforms, such as the Web of Science (Thomson, 2010B) and SciSearch. (There were also electronic and printed editions, covering a smaller number of journals. This database allowed a researcher to identify which later articles have cited any particular earlier article, or have cited any particular author, or have been cited most

frequently. Subsets of this database, termed ‘Specialty Citation Indexes’, such as the Neuroscience Citation Index, the Chemistry Citation Index, etc were also available.

Impact of Science Citation Index

In an interview conducted by Istvan Hargittai for Chemical Intelligence, Garfield was asked to comment on the impact of his introducing SCI which changed the way scientists are employed, professors at universities are given tenure, and research journals are judged for their quality, he replied (Istvan, 1999) ‘If there is any truth to the statement, I’m not acutely conscious of it. Of course, CC and SCI are widely used, but I don’t hear people say much about it. If you use SCI especially for evaluative purposes, you don’t advertise it. If the SCI is used in tenure evaluations, hopefully it is done intelligently. I described this in an essay on faculty evaluation, one of my most popular. Undoubtedly, this use of citation analysis is due to the paucity of objective data for such evaluations. I can’t imagine how you would evaluate the impact of my work. How would you measure it? The Internet is having an impact but how would you measure it? When we talk about intellectual impact, it is very subjective - economic impact is another thing’.

‘Nevertheless, I do find it hard to keep up with the large literature involving journal impact factors. I am especially frustrated that I can’t respond to the portion containing mis-statements or misuses. There is much controversy about the validity of impact factors, which are used for many purposes. As you have implied, SCI and Journal JCR data have become institutionalized. People often criticize the impact factor because it is so pervasive. Editors, especially of new journals, are using JCR to demonstrate how quickly their journals are accepted or whether they measure up to the best-known journals. Some of the most respected journals do not hesitate to use impact factors in their advertising’

Chemistry Citation Index

Garfield, a chemist by training, also introduced the Chemistry Citation Index. His original 'search examples were based on his own experience as a chemist'. In 1992 an electronic and print form of the index was derived from a core of 330 chemistry journals, within which all areas were covered. Additional information was provided from articles selected from 4,000 other journals. All chemistry sub disciplines were covered: organic, inorganic, analytical, physical chemistry, polymer, computational, organometallic, materials chemistry, and electrochemistry (Garfield, 1992). By 2002 the core journal coverage increased to 500 and related article coverage increased to 8,000 other journals.

The Social Sciences Citation Index

The Social Sciences Citation Index (SSCI) is another commercial citation index product developed by the ISI on the lines of the SCI. The SSCI citation database covers some 3,000 of the world's leading academic journals in the social sciences across more than 50 disciplines. It is made available online through the Web of Science service for a fee. The database records articles, which are cited by other articles. Philip Altbach has criticised the SSCI of favoring English-language journals generally and American journals specifically, while greatly under representing journals in non-English languages (Altbach, 2005). In 2005, economists Daniel B. Klein and Eric Chiang conducted a survey of the Social Sciences Citation Index and identified a bias against free market oriented research. In addition to an ideological bias, Klein and Chiang also identified several methodological deficiencies that encouraged the over-counting of citations, and they argue that the Social Sciences Citation Index does a poor job reflecting the relevance and accuracy of articles (Klein and Chiang, 2004).

The Arts & Humanities Citation Index

The Arts & Humanities Citation Index (A&HCI), also known as Arts & Humanities

Search, is a citation index, with abstracting and indexing for more than 1,700 arts and humanities journals, and coverage of disciplines that includes social and natural science journals. Part of this database is derived from Current Contents records. Subjects covered are the Arts, Humanities, Language (including Linguistics), Poetry, Music, Classical works, History, Oriental Studies, Philosophy, Archaeology, Architecture, History, Religion, Television, Theater, and Radio. Available citation (source) coverage includes articles, letters, editorials, meeting abstracts, errata, poems, short stories, plays, music scores, excerpts from books, chronologies, bibliographies and filmographies, as well as citations to reviews of books, films, music, and theatrical performances. This database can be accessed online through Web of Science. It provides access to current and retrospective bibliographic information and cited references. It also covers individually selected, relevant items from approximately 1,200 titles, mostly arts and humanities journals but with an unspecified number of titles from other disciplines. The Arts & Humanities Search remained accessible via Dialog, DataStar, and OCLC, with weekly updates and back files to 1980.

Journal Citation Reports (JCR)

JCR is an annual publication integrated with the Web of Science and is accessed from the Web of Science-Core Collections. It provides information about academic journals in the sciences and social sciences, including impact factors. JCR is based on citations compiled from the SCI Expanded and the SSCI. There are separate editions for the sciences and the social sciences; the 2013 science edition includes 8,411 journals, and the 2012 social science edition contains 3,016 titles. The issue for each year is published the following year, after the citations for the year have been published and the information processed. The publication is available online (JCR on the Web), or in CD format or in electronic format; it was originally published in print, with the detailed tables on microfiche.



The Scientist

The Scientist started by Garfield in 1986 is a professional magazine aimed to provide print and online coverage of the latest developments in life sciences research, technology, and business. The magazine has been published monthly

Its closure was announced in October 2011. But then the LabX Media Group purchased it. It continues to cover Science policy, Careers, Financial topics, Groundbreaking research, Industry innovations, Economics of science, Scientific ethics, Profiles of scientists, Lab tools, Hot papers, Product spotlight, guides, and surveys on salary, Best places to work, etc. (Brainard, 2011).

The Scientist confers awards for the top innovations in science technology based on the nominations submitted. It confers awards to the Laboratory website, video and multimedia recognizing those laboratories which were best at using the Internet to further science.

Online

The Scientist has an online version now. It provides the content of the print version by providing life science news and interactive multimedia features. The current month's magazine content, news blogs, and podcasts are freely available with

registration, while premium subscribers are also given access to the magazine's archives of articles, news, and editorials. The Scientist launched in 2011, a Facebook page to deliver its content in the social media realm. The page now has more than 1.5 million page likes and fosters a highly engaged community of readers. The following year, The Scientist launched special interest Facebook pages to share the latest research developments in six different life science topics. These pages boast a combined viewership of more than 2.6.

Web of Science

Web of Science (previously known as Web of Knowledge) is an online subscription-based scientific citation indexing service that provides a comprehensive citation search. It gives access to multiple databases that reference cross-disciplinary research, which allows for in-depth exploration of specialized sub-fields within an academic or scientific discipline (Thomson, 2016).

A citation index is built on the fact that citations in science serve as linkages between similar research items, and lead to matching or related scientific literature, such as journal articles, conference proceedings, abstracts, etc. In addition, literature which shows the greatest impact in a particular field, or more than one discipline, can be easily located through a citation index. For example, a paper's influence can be determined by linking to all the papers that have cited it. In this way, current trends, patterns, and emerging fields of research can be assessed. Eugene Garfield stated 'Citations are the formal, explicit linkages between papers that have particular points in common. A citation index is built around these linkages. It lists publications that have been cited and identifies the sources of the citations. Anyone conducting a literature search can find from one to dozens of additional papers on a subject just by knowing one that has been cited. And every paper that is found provides a list of new citations with which to continue the search'.

Web of Science is described as a unifying research tool, which enables the user to acquire, analyze, and disseminate database information in a timely manner. This is accomplished because of the creation of a common vocabulary, called ontology, for varied search terms and varied data. Moreover, search terms generate related information across categories. An evaluation and selection process based on criteria such as impact, influence, timeliness, peer review, and geographic representation determines acceptable content for Web of Science.

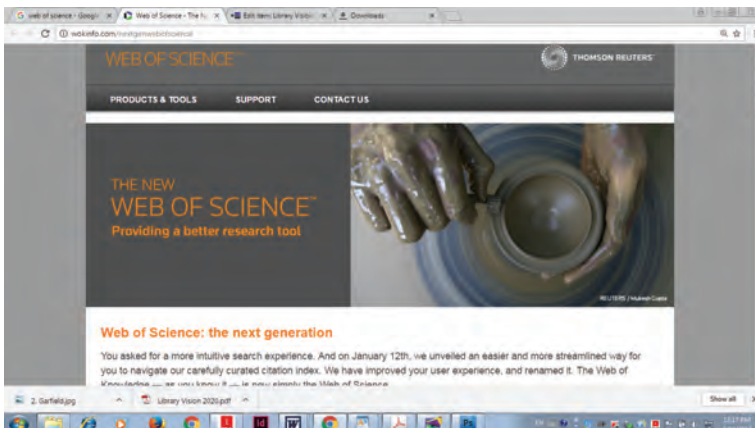
Web of Science employs various search and analysis capabilities. First, citation indexing is employed, which is enhanced by the capability to search for results across disciplines. The influence, impact, history, and methodology of an idea can be followed from its first instance to the present day. Second, subtle trends and patterns relevant to the literature or research of interest, become apparent. Broad trends indicate significant topics of the day, as well as the history relevant to both the work at hand, and particular areas of study.

Before ISI's Web of Science, scientists and researchers had very inefficient methods for finding and tracing other scientific documents. 'The citation database was not just an intellectual achievement, but also an engineering achievement,' said C. Sean Burns, a faculty of University of Kentucky who

received support for his research under Beta Phi Mu/Eugene Garfield Doctoral Dissertation Fellowship.

HistCite

HistCite is a software package used for bibliometric analysis and information visualization developed by Garfield. HistCite can answer typical questions asked by bibliometricians. The main purpose of the software is to make it easier for individuals to perform bibliometric analysis and visualization tasks. HistCite converts bibliographies into diagrams called historiographs to give information visualizations. Historiographs are based on the citation relationships between the papers in a bibliography. In a historiograph, each paper in the bibliography is represented by a symbol selected by the user. The symbols are arranged over a timeline of the publication dates of the papers. By changing the time frame of the analysis, the resulting historiograph can form a snapshot of a specific period or an in-depth look at the total history of a subject. Once a historiograph is created for a bibliography, it is easier to see and understand the subject's key publication events, their chronology, and their relative influence. HistCite is currently set up to import bibliographies created by searches of the Web of Science database. Bibliographies from other sources can be manually entered into HistCite.



Related Records in Citation Indexes

Thomson Reuters quotes the statment of Garfield; referring to the uniqueness of Related records: 'Using SCI CD-ROMs or one of the ISI websites, the Web of Science, you can find a particular paper of your immediate interest, let's say Paper A, by just typing any KeyWords, author name, address word, journal name, the year of publication dating almost back to 1945 now, or any combination of these in *General Search*. This is how you conduct a traditional subject search. But once you locate Paper A through one of these categories, you can find not only all the references that Paper A cites but also the papers that cite Paper A. In addition, you can do a specialized kind of citation search, called *Related Records*, in which you can find all the other papers that are closely related to Paper A. At the top of the *Related Records* summary list is the paper that is most related to Paper A; this paper may or may not cite Paper A, but these two papers are closely related because they cite many of the same papers; they have references in common. Now, you can go down the list, or you also can go sideways to see which other papers have cited this most closely related paper of Paper A. You may find hundreds or thousands of related papers. It is up to you how far you want to go with this expansion, but you are able to judge at each step of the way from the title and its abstract whether or not you are getting closer to what you are looking for. We started this method years ago. Medline or its website, PubMed, now recently adopted the term *Related Articles*; this is similar but not the same and certainly is not based on citation relatedness but other semantic criteria' (Thomson Reuters, 2017).

'In the printed versions of SCI, we could not do this kind of search very quickly. Long before the Internet began, we started, in 1988, to provide CD-ROMs. Using thiem or Web of Science, you can do a citation search in 10 minutes that used to take hours to do. If

your institution has a site license for Web of Science, you can combine all three citation databases: SCI, SSCI, and A&HCI' (Thomson Reuters, 2017).

'You could say that citation analysis is like fishing. You start with a line and hook because you want one fish. Then you want to know which school of fish that one fish is connected to. You decide what size of a net to use; you can expand your search with a big net, but you also can narrow it down using a small net'. (Chungji, 2000).

Garfield as Librarian

Although Garfield apparently never worked in a library, Garfield did have a Masters Degree in Library and Information Science and is often referred to as a *librarian* (Eric, 2010). In his early career, he had very close connections in the medical library. When asked about his opinion on calling him a Librarian, Garfield said (Eric, 2010) 'If you want to call me a librarian I don't mind. some of my best friends are librarians'. He told about his library-related work and information engineering: 'I worked in the Johns Hopkins Welch Medical Library before I went to library school. I was there because that is where the Welch Project had one big room where we did experiments with IBM punched card machines and especially the 101 Statistical Machine. The project was sponsored originally by the Army Medical Library, which then became the Armed Forces Medical Library, which was later transformed into the National Library of Medicine. When the Project Director terminated me I was advised to get a degree in library science if I wanted to make any headway in the field. That was probably not true since I had several job offers but it proved to be an interesting turn to my future role as an entrepreneur in the library field. A large percentage of my future 'clients' were librarians and I was active in the Medical Library Association but in some ways never accepted because I was working in a for profit organization once I started Current Contents and then the Science

Citation Index. The then cub reporter for Science magazine -William J. Broad who has gone on to become a well known science writer for the New York Times described me in an article as 'librarian makes millions off mere footnotes.'

'Although I don't mind being called a librarian, I think it's more accurate to call me an information engineer or information scientist. I think the scientific community considers me a science communicator. I always considered myself an information engineer, but was forbidden to use that term by the Engineering Licensing Board of Pennsylvania. So I called myself an information scientist. Today I am usually referred to as a scientometrician or bibliometrician but a more accurate term might be citation analyst'(Eric, 2010).

Garfield and Google

About his work on citation analysis and Google, he stated to Eric Rumsey that (Eric, 2010) 'Guy Gugliotta got the sequence mixed up when he said that the idea of the Science Citation Index came before I began my doctoral program in structural linguistics. It started while I was still at the Welch Project in 1953. After I left there while at library school I wrote my primordial paper for Science magazine in early 1954 but it didn't get published until 1955, a year after I finished Library School at Columbia. By then I had gone to Philadelphia and started my small company Eugene Garfield Associates that published Current Contents. Encouraged by my best friend Casimir Borkowski I signed up for my doctoral degree program in structural linguistics in 1958 and continued until 1961 when I got my doctorate in what is best described as chemical linguistics.'

'I believe you will find that in the original patent granted to the inventors of Google, the patent examiner pointed to the connection between page ranking and the journal citation reports. I had heard that William Arms had told them while at

Stanford about the Science Citation Index. (Eric, 2010).

Librarians don't talk much about Google having its origins in the library world. But it's well-established that Google Page Rank is built on Garfield's citation-analysis work done in the 1950's, which led to the standard library reference tool Science Citation Index, and later Web of Science. So it is to be considered that Google grew from library roots (Eric, 2010).

Garfield's work led to the development of several information retrieval algorithms, like the HITS algorithm and PageRank. Both use the structured citation between websites through hyperlinks. Google co-founders Larry Page and Sergey Brin acknowledged Garfield in their development of PageRank, the algorithm that powers their company's search engine (Richard, 2017). Gary Price, in 2004, referred to Garfield as The Father of Citation Analysis. So, if he's the 'father' of citation analysis, and citation analysis played a key role in the development of PageRank and Google, it's a short step to Garfield as Google's grandfather (Guy, 2009).

Some three decades before the concept of the hyperlink and the World Wide Web crossed anybody's mind, Garfield had figured out how to connect the immense body of scientific knowledge into a network.

Views on Selection of Nobel Laureates

When Istvan Hargittai asked about his views on the connection between publishing, impact, and the Nobel Prize, Garfield stated that 'It became an interesting game. But I never tried to predict who would win the prize. It was more relevant to suggest the fields that might be recognized. We might have predicted a prize for nitric oxide. Certainly, among those names would be Salvador Moncada. Moncada was certainly among the most cited authors. Nobel prizewinners are almost invariably well cited. The Nobel Committee didn't include him, and it has created a lot of controversy. I'm

not suggesting that the committees should select on the basis of citation analysis, but they should be aware of the most cited scientists for each field considered. The same thing happened to Moncada for the Lasker Award. Something odd is going on there. I find it very strange that members of many lesser prize committees prefer to choose Nobel laureates. Why not pick someone who hasn't been so visibly recognized. I have fought this battle many times. Most award committees like to play it safe. I think awards should go to people for whom the award would make a significant career difference. Why give a lesser prize to Nobel laureates? They've already had the highest recognition. But there will always be exceptions even to that generalization' (Istvan, 1999).

Contributions Garfield Loved

When Istvan Hargittai asked him to single out what Garfield consider to be the most important thing he has done he stated that 'To many people, Current Contents had the most pervasive influence. Current Contents is a ridiculously simple idea. Curiously, there has never been a scholarly article written about Current Contents. But it is still the model that has been adopted and copied. Its simplicity is what made it so successful. You say that I have a strong influence on science. Well, for a 25 year period I had a captive audience worldwide. The readership was larger than that of Science or Nature. The number of printed copies was as high as 40,000, but the average readership was tenfold that number. Some copies were read by hundreds of people in Eastern Europe and China, where they also copied it. When I went to Eastern Europe and elsewhere, people respected me because I did not attempt to criticize their political systems. I used citation data to demonstrate the relative strengths of their science. I didn't have to tell them what they knew. Rather, I provided them a window on the rest of the world. Since Current Contents was just a bibliographical tool, the Russian censors did not touch my essays. They allowed my essays

to go through. Of course, I was proselytizing about citation indexing and not capitalism. Many people still think that I'm writing those weekly essays. Recently, I met a senior scientist who said that he loved my essays and read them every week! I wrote the last one six years ago. Maybe he's thinking of my occasional editorials in *The Scientist*. In fact, many people don't know that *The Scientist* has not been an ISI publication for 10 years' (Istvan, 1999).

Clarivate Analytics

Garfield is also considered founding father of Clarivate Analytics. Web of Science which is presently owned by them continues to serve as a reliable and efficient guide to the sprawling world of research. Clarivate Analytics accelerates the pace of innovation by providing trusted insights and analytics to customers around the world, enabling them to discover, protect and commercialize new ideas faster. Formerly the Intellectual Property and Science business of Thomson Reuters, it now owns and operates a collection of leading subscription-based businesses focused on scientific and academic research, patent analytics and regulatory standards, pharmaceutical and biotech intelligence, trademark protection, domain brand protection and intellectual property management (Clarivate Analytics, 2017 A). Jay Nadler, Clarivate Analytics' chief executive officer, states that Garfield's work 'shaped the way that research is accessed and evaluated across the globe'. Now Clarivate Analytics is an independent company. It has over 4,000 employees, working in about 100 countries. It owns well known brands that include Web of Science, Cortellis, Thomson Innovation, Derwent World Patents Index, CompuMark, MarkMonitor and Techstreet.

Innovation and Entrepreneurship

Garfield has always combined his academic professional training with creative salesmanship and entrepreneurial spirit with an innovative vision of information retrieval tools. His entrepreneurship led him to

market his contents service, *Current Contents*. Eugene Garfield Associates-Information Engineers consultancy the forerunner of ISI carried out documentation projects for various chemical and pharmaceutical companies and this professional contact with chemical information and documentation inspired him to explore the common ground between automatic indexing, automatic translation and the automated processing of chemical formulas. This explains why he went on to gain a doctoral degree in 1961 from the Department of Structural Linguistics at the University of Pennsylvania, with a thesis on an algorithm for translating chemical names to molecular formulae. He had already begun to explore this area in 1960, when he indexed chemical compounds in the *Index Chemicus*, which is still used on the *Web of Science* portal.

Outside of his scientific and publishing endeavors, Garfield was never afraid of entering the political fray. He called out elected officials for making scientific decisions based on conjecture in 1986, and admonished former President Reagan's science advisors for serving as 'advocates of the administration's science policies, rather than as objective conduits for communication between the president and the science community.' In 1987, he wrote an editorial, 'Let's Stand Up For Global Science,' in which he argued that the United States ought to continue contributing funds to UNESCO in order to ensure the continuity of the agency's science programs (Krisch, 2017).

He was wonderfully clever, perceptive, and generous as an employer and he was great company, says Richard Gallagher, president and editor in chief at Annual Reviews and editor and publisher of The Scientist from 2002 to 2010 (Scientist, 2017). The ISI office had a state-of-the-art childcare facility attached, maximizing convenience for the staff. And he trained and supported many of the female leaders in the publishing industry today. The awards that he inspired also give an indication of his interests,

including The Eugene Garfield Residency in Science Librarianship and the ALISE Eugene Garfield Doctoral Dissertation Competition. He was actively involved in Research America (Richard, 2017). 'He was a businessman and an entrepreneur, but his first love was the 'data' said David Pendlebury, an analyst at Clarivate Analytics who worked with Garfield for more than 30 years (Scientist, 2017). He was a genius of a very special type. Not only because he had incredible imagination and brain, but he had incredible tenacity and courage. 'He never lost touch with his humble beginnings, he never became arrogant with his success' said his wife Meher (Scientist, 2017).

Dreams of Garfield

Istvan Hargittai asked some fifteen years back about the dreams that Garfield would like to materialize and he stated that 'When I sold ISI, new management almost immediately discontinued certain projects. The Atlas of Science we had started has changed its name to Research Reviews. We used the results of our global co-citation analyses to identify the newly emerging research fronts that needed to be reviewed and the new partners discontinued it because it would have taken years to make it profitable. But it would have been an encyclopedic treatment of current science. Current Science understood the mapping concept very well but, to my knowledge, it has not used co-citation mapping to produce an international encyclopedia of science. Systematic examination of the literature is necessary to identify what is not being reviewed. Annual Reviews produces about 30 annual review volumes in print and online each year. They have not used citation data as yet. Their methods for choosing topics are purely subjective - not that it is bad, just different. Derek Price used to say that for every 50 new papers in each field, you need a review, which then becomes the paper that people cite as a surrogate for those references. One could do an interesting historical mapping based on the network of review

papers. It has not happened' (Istvan, 1999). 'There are lots of things I wanted to do that have not happened. I wanted to publish a constantly current dictionary of science. What could be a better source of new terms than the ISI database? There is constant input of new terminology. The nomenclature from indexing services is not systematically being exploited to compile dictionaries. Libraries spend a lot of energy compiling thesauri. Most of those terms eventually do get into dictionaries, but it could take many years. I would also like to see the algorithm finished for creating historiographs. The Citation Index is a gold mine for the history of science. Mapping all the key references for a given topic, you should be able to graphically portray the development of a field'(Istvan, 1999).

'I find it is very frustrating that so many scientists are ignorant of what they could do with information retrieval systems. I think it is important not only to be literature-minded, but to develop citation consciousness. I'm not sure how you teach this. It requires indoctrination by informed mentors. I also wanted to use ISI Press to launch a systematic series of scientific biographies. It would have been an extension of our Citation Classics series. From ISI we published 4000 of those in Current Contents, and 2000 were reprinted in a series of books called Contemporary Classics in Science. We could have easily published many more thousands of Citation Classics(Istvan, 1999).

Lessons from His Life

When Istvan Hargittai asked what Garfield would consider the lessons others should learn from his career he said 'Too often, people are afraid of failure. They worry that they cannot manage financially. Money never drove me; it came to me. Nevertheless, if I had worried about money, I might never have achieved financial success. I don't know what accounts for this quality of persistence.

My mother never stopped until she finished the task at hand. You learn certain doggedness. I grew up working. When I was 9, I was delivering orders in a grocery store and worked in a laundry for hours just to earn a quarter. Later, I went to work for my uncles. I delivered orders in my Uncle Lou's liquor store. Then I worked in the garment district after school and summers. Maybe that was another reason that I didn't do that well in high school. I certainly enjoyed the work but realized I didn't want to remain in the garment business, much as my uncle Sam would have liked me to (Istvan, 1999).

Awards and Recognition

Between 1953 to 2010 most of the countries of the world, reputed universities and international research organizations honored Garfield with prestigious awards. Grolier Society Fellow, Columbia University (1953-54), Fellow, American Association for the Advancement of Science (1966), Award of Merit, American Society of Information Science (1975), Hall of Fame Award, Information Industry Association of America (1977), Herman Skolnick Award (1977) and Patterson-Crane Award (1983) of American Chemical Society, M.D. (hon. caus.), John Price Wetherill Medal, Franklin Institute (1983), Derek J. de Solla Price Memorial Medal, Scientometrics (1986), Dr. honoris causa, Vrije University, Brussels Belgium (1988), Ph.D. honorary, State University of New York, Albany (1990), Honorary Fellow, Society for Technical Communication. America (1991), Honorary Foreign Member, Institute of Marine Biology, Vladivostok, Russia (1993), M.D.(hon. caus.), University of Rome, Tor Vergata, Italy (1993), Charles University, Czech Republic (1995), Honorary Degree from Far Eastern University, Vladivostok, Russia (1997), Honorary Membership in the Russian Academy of Sciences, Vladivostok, Russia (1997), Professor Kaula Award for Library & Information Science, India (2000), Winifred Sewell Prize for Innovation in Information

Technologies in Biomedical & Life Sciences Librarianship (2001), Los Angeles Chapter - American Society for Information Science & Technology (ASIST)- Visionary Leader - Contributions to Information Science (2004), Honorary Degree from Real Academia de Medicina Del Distrito de Granada (Andalucia Oriental) Spain (2007), The Chemical Heritage Foundation - Richard J. Bolte, Sr. Award for Supporting Industries (2007) are only a few among them.

Open Access and Collaborative Authorship

To Henry Small; Garfield opined that 'Basically, the idea of Open Access (OA) is to have things available free of charge to the public. I cannot easily see the publishing industry ceding multi-billion dollars in profits that they're making out of, so called, value addition, that they do. Wikipedia like projects use community authorship.... collaborative. But these works are not properly authenticated; and so they're not going to be accepted, and you won't get them authenticated unless they get people paid for doing the work that's required (Small, Henry 2007).

Future in Science Communication

When asked about the currently available search engines and their index systems in the Web and the improvements needed, Garfield replied 'When I use the Web, I sometimes find it very difficult to search. Search engines can be very frustrating. The people who are running these engines have ignored 40 years of machine-indexing research. Of course their files are huge, but there would be a lot of value to their doing more research... I think the providers of search engines could learn some from our experience on ASCA to better meet their users' needs' (Chungji, 2000).

Garfield Will Live Through His Works

It is not easy to express the unique talents of this multi-faceted person, known as Gene

to his friends and colleagues. Garfield was the forerunner of indexing systems for scientific literature based on citations, so also based on the functional, intellectual and symbolic networks that authors establish between each other when they write their papers and cite the studies that they have drawn on in their research. It is a huge task to sum up the life of Eugene Garfield, the forerunner of Informatics. His work enabled information retrieval to scale up and it created, basically, the entire information science field as we know it today (Burns, 2017). The demise of Dr Garfield, who preferred to be known as the *Citation Analyst* is a great loss to the scientific community in general and informatics field in particular. His eternal journey through the field of innovative and entrepreneurial Informatics which he reined with a cause for more than half century will mark an inexplicable void till an incarnation take place either in business or Open Access Infospace.

Garfield and his work have inspired generations of others to create solutions to help researchers to build upon the work of their colleagues and predecessors in advancing the frontiers of human knowledge (Susannah, 2017). In the field of Informatics, Eugene Garfield stands alone as the immortal giant of quality ensuring technologies for scientific and scholarly research and certainly the success of his Science Citation Index and resultant products including Web of Science are without parallel. Borrowing the words of Garfield himself on one of his great predecessors Henry E Bliss, let us state ' Individuals live on in the works they leave behind. Garfield's work deserves to be sustained and its potentialities fully utilized to ensure the quality of research and development in all the fields of science and for sustaining life on this planet. Since we can no longer depend on Garfield himself, others needs to be stimulated to carry on his work. Garfield is dead. Long live Garfield'.

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Eugene Garfield: Pioneer of Informatics for Scientific and Scholarly Research

Interview by: Web of Stories

Abstract

Eugene Garfield (1925-2017) was an American scientist and publisher. In 1960 Garfield set up the Institute for Scientific Information which produced, among many other things, the Science Citation Index and fulfilled his dream of a multidisciplinary citation index. The impact of this is incalculable. Without Garfield's pioneering work, the field of scientometrics would have had a very different landscape, and the study of scholarly communication would have been considerably poorer. Transcript of an Interview of Garfield conducted by Henry Small in September 2007 is published here for the first time. It begins with a discussion of his childhood and education. He recollects memories of his career from jobs at Welch Medical Library, Smith, Kline & French, teaching at University of Pennsylvania, and starting of Eugene Garfield Associates and Institute of Scientific Information. His research, and development of products that revolutionized knowledge management scenario are described. His views on Informatics, NGOs, economic aspects of Open Access, quality of research, impact factors, and Big science are discussed. The interview reveals the life a person born to poor uneducated parents; facing the challenges of academic and business world becoming one of the greatest masters of the universe of knowledge developing magical products that can ensure quality of scientific and scholarly research.

Family History

My grandparents were orthodox; but I don't ever remember my grandmother saying one word about anything religious. My grandfather went to synagogue several times a week, although he was not a very well educated man. Once when he and I were traveling together on a trip in an automobile with my uncle down to Florida, which was a, kind of, important year in my life because that caused me missing, a half of my course in school -because I'd gone to Florida. I had to make up the course, and that meant that I had to go to summer school. But I found out from my grandfather, when we were traveling, that he had done two things when he was brought up in what was then Lithuania part of the Russian Tsarist empire. And, we were traveling across a river in Georgia somewhere and he sees there are two guys standing on the raft or log, loggers,



pushing logs around, and he says to me, 'That's what I used to do in the Old Country'. I said, 'But I thought you were a tailor', because when he came to New York

that's how he made his living and he made pants and sold them on Canarsie Street in New York. He said, 'Yeah, but that was only in the winter time, in the summer time I worked, as a logger'. As I understand it, he could write Hebrew but my... grandmother's youngest sister who came to live with us when I was about 4 years old and with whom I actually shared the bed with, for several years until she found her own place, she came over in 1929 to New York. She was extremely well educated because what happened was when my great aunt was born my great grandmother died in childbirth, and so the two- my grandmother and her sister, Ida, were put in foster homes. One, my aunt went to live with a very well educated family, I don't know what they did, business, but she learned to read, and my grandmother never learned to read, but she became a fantastic cook because she used to have to take care of this huge family and prepare meals for parties and so forth. So, they were not, you know, very highly literate people but I can remember not believing that my mother, grandmother couldn't read, because I was sitting in the kitchen with her and I pointed to calendar, can you read those numbers? She said, 'Yeah, I can read numbers'. So she read the numbers on the calendar. But, I couldn't, kind of, grasp the idea that she could not read English. Many years later my, youngest uncle who is only 6 years older than I am, and still alive, tried to teach her to read but unfortunately she died relatively young. She died of breast cancer when she was 58.

My Family and My Father

My grandmother was a very important part of my life because my mother became a mother at the age of 16. My sister was 2 years older than me. So my mother was only 16 years older than my sister. And... because my father basically deserted her, she had to go to work and so we spent a lot of time with my grandparents.

Did you ever meet your father or...?

Oh, yeah. It was a very tempestuous family life. I once wrote up as... Barry Blumberg and I belong to this club of 80-year-olds, people who were born the, 1925, and we, we've been up to his place in Maine, every 5

years he has a gathering of six or seven scientists who were born that year, and they asked me to write my autobiography so I described all this. Basically I have a theory that probably my mother, I have no proof of this, but my mother probably got pregnant and my uncle was livid. She had five brothers and her oldest brother was a relatively successful businessman in the garment district. He owned a company called Wolf & Barse, and I... later on I actually worked there. As I understood it, he got a hold of my father and, beat him up, or they had a fight, and the two of them hated each other from that day forward. And, every chance either one of them got to express that anger it got out, it affected us, you know, because on the one hand... I'm not saying my father was actually not an angel. I don't know, till my sister was born, they must have lived together and then he left even before I was born. The interesting thing about that is he later married the woman who then became his sweetheart, and they stayed together until he died, so he was married to the same woman, who I did meet. Her name was Anne. She was actually nicer to me than he was, but I did not see my father until I was about 5 years old. He came... I can still remember him coming to... I think it was three-room apartment on the fifth floor of a tenement building in the East Bronx. That's when my aunt came to live with us. We stayed in one room, my sister was in another bed, my mother must have slept in the other room, and then there was the kitchen. There were these two beds, my father was sitting on the one bed and I was sitting on the other looking at him, and he says, I want to show you something, and he pulls out a gun. My father was a bootlegger. Why he showed me the gun, I don't know, but you have to think about that he was probably, about 23 years old, and when I think about... by the time I was the father also, because later on when I got married I was 21 and Stefan was born in 1947, I was born in '25, so by the time I was not even 22, Stefan was born. So then after that I didn't see my father again for 5 years. He did everything possible to avoid helping my mother. He for 10 years did not give us a nickel. My uncle, on the other hand, supported us, so he became a, kind of, a

surrogate father even though the only time I would ever see him was Friday night we'd all meet and my uncle would come, frequently with business friends, to partake of my grandmother's cooking which was really outstanding. So every Friday night we'd go there and all my uncles would be there and my mother, as I said, had five brothers... she was the second oldest. The oldest was Sam, the one who hated my father, and then after them there were five other brothers, four others, Abraham, Nate, Hyman, and Bernard.

Childhood Memories

We lived... New York, East Bronx and West Bronx is like moving from Parkside Avenue to Mantua, from Mantua to Parkside, in those days. We at first lived in this. When you're a kid, you don't think it's so terrible that we lived in the... East Bronx a pretty rundown... they used to, they had these old tenement buildings that were all walk-ups. There was no such thing as an elevator. My mother, being a single parent, placed me as the first infant in what they called the first Jewish Lady's Home Nursery. There's a street called Fulton Avenue in the Bronx. Several years ago, I drove through there to see the old neighborhood, some of the things I could see. But it was right near the corner of Crotona Park in the Bronx. The Bronx has several parks but Crotona Park is a fairly big park, and right, that corner is where Crotona Park is, so I can remember that, and so we lived on the St Paul's Place, which is a street that's still there. But what they have on that street now, they've torn down all those buildings, basically, they have a big housing project, although some of the street is still intact. There was when we were kids an elevated ground on what I think was Webster Avenue, and that was eventually torn down, but on the corner was a candy store. In New York the concept of a candy store is unique. It's not, you know, when they say candy store they mean, it's more like a combination store for... candy store, soda, you know, soda fountain. You'd go there if you wanted... They had pinball machines that you could play, and you bought school supplies there, sort of, penny candies, and so on, so a lot of people used to hang out at

corner candy store. Now, my mother, when she would take us to the nursery, and every day when she came home she would take us to that candy store and we would, my sister and I, would share a chocolate milkshake. That was, you know, in those days you could probably buy it for a nickel or a dime, and I grew up on those things. Actually, it took me decades before I got out of the habit of drinking milkshakes. I still, once in a while I crave them, you know, but... when I went to college in, Colorado where by the time I was a freshman, they had a dairy in the centre of Boulder. I forget the name, but you could go down there and for 11 cents, this is 1942, purchase all the milkshake you could consume, loaded with ice cream and everything. So you could live off of that. Anyhow, we didn't have milkshakes. We had malted milks, so we also added malt to it. My mother would take us there, and she had a job doing what they call... those days poor working girls could, she, didn't have any, training or anything, well, she was 15 when she got pregnant. She graduated from grammar school but she had no particular skills, so she went to work doing piece work and what she did was putting rhinestones in costume jewellery. I guess they're prefabricated, molded settings and they make costume jewellery out of it, so that was her job. She'd get a batch of these moulds and then she'd get the, a whole pile of, stones or beads, or whatever the hell they were, and, I'll never forget, and banana oil. Do you know what banana oil smells like? If I smell banana oil I get - it's instantaneous, I was imprinted with that smell, because she would come home and we would sit around the kitchen table and help her put these stones with the banana oil into the thing, and she'd get probably a couple of cents for every one she did and she'd bring them down. That's how she managed to... she worked in... my uncle helped her.

Moving to the West Bronx and My Stepfather

So, that was the East Bronx, okay. Now, when I was in that part of town I attended... in New York all public schools have numbers. I attended public school number two in the Bronx. But, later I guess my uncle helped us

to move to the West Bronx, and in the West Bronx we lived on a street called Woodycrest Avenue, in an apartment house which does seem... was not a tenement that's for sure, but we had again a three-room apartment in a more... I guess, there were fewer cockroaches, you know, and things like that. But, typically, the back of every apartment in New York has a backyard, and there was, like, an alleyway and... I don't know, you've been to my house. I have a painting, which was given to me when I was in the Library School, which shows what a typical backyard in New York looks like. You can see all the clothes lines, everything. In, that alley vendors used to come all the time, so that you could hear people, just like, you see color, films of foreign countries, like, guys who would be... a guy would yell out, 'We, make keys!' or, 'We do, we sharpen knives!' all sorts of vendors would be coming through there and they would yell it out, 'I'm here if you have anything!' and if you wanted to get something done you'd go down and do it. So, anyhow, we lived on that street and down from that particular house, about 200 or 300 yards was the Woodycrest Branch of the New York Public Library. It was built on a strange hill. The bottom part of the library was the children's branch, and the upper part was the adult section. When I was about 9 or 10 years old my mother met my stepfather. I don't remember how they met, but that was an unusual, kind of, arrangement because he was also a relatively uneducated person. I don't think he finished grammar school. He was 2 years old when he was immigrated from Italy and, so she was going with my stepfather for several years before they got married but that being, you know... in those days a Jewish woman and an Italian man, that was, sort of forbidden thing. The odd thing about all that was that my uncle, among others, was very opposed to it, okay. But he started dating my mother and he liked kids so he used to take us, you know, on... he had a rumble seat Chevrolet, I think it was, so we, he'd take us for rides that we would sit, my sister and I would sit in the rumble seat and we'd go for rides up in Westchester County because he was an itinerant manager of the meat department, butcher, he was an itinerant butcher for the American- Atlantic

and Pacific Tea Company. I don't know, they still have supermarkets, I guess, still. It was very well known then. Atlantic Pacific Tea Company it was called, but basically it was a supermarket. Since he was brought up in, Westchester County, New Rochelle, New York, he knew all these places to go to so we would go to Jones Beach or we'd go to Palisades Amusement Park, or all sorts of places that he took us to, and he was a very simple man with very few intellectual interests. That later on became a problem because my mother was really an extremely bright, intelligent woman, I mean, and probably suffered because of that.

Growing Up Near New York Public Library

I think my interest in information was, sort of like, inbred in the sense that I was fortunate when I was a child to live across the street from a branch of the New York Public Library. My first experience with title scanning was in the library where I used to go in the forbidden section which was the adult part of the library, not the children's section which I rarely visited, I didn't read too many children's books when I was a child, and I scanned the title of every book in the library. I still... in my head I can still see some of those books that would come to mind, even though I'd never read them.

Why Did You Do That?

Don't ask me. I mean, it was something that... just a curiosity, kind of, a thing, you know, just.. I remember the title, 40 Days of Musa Dagh always come to mind as one of the things that I saw and Red Badge of Courage, and so forth. I don't... think that amongst my friends, when I was little, there was nobody that was really interested in going to the library. You know, other kids were, playing baseball and, we'd spend every night at 10 o'clock, or 10.30 on the corner, unlike most of the kids who were home, I would be out there on the corner discussing the results of the baseball games that took place. Every kid in New York, of course, knows, is mathematically inclined because he's got to know the statistics of the game, of the players and everything. It's amazing how much kids know about the, statistics of baseball.

High School

I was interested in science, more because one of my uncles had a chemistry set, but I never was a big, a wild enthusiast about it. It was by the time I was 14. My uncles were Marxist, three of them, the other two were capitalist, so there was that constant family tension, and that's my mother's family. I never lived with my biological father. I don't remember how I got into it but I always had a romantic idea what the west was like and I always had my mind set on going out west to go to school. By the time I was to graduate I had applied, to go to the University of Arizona, and for some reason I didn't get in but I did get into the University of Colorado. I graduated from high school when I was not yet 17. I was good in math so if you were good in math you could... like, I got 100 on Regent's Exam, that got me into Stuyvesant High School. New York Stuyvesant High School was the, and still is, the leading high school for science. Although I haven't been in contact with them at all. I don't know how many other majors the people study at science, other than science. But when I look back I regret that there was nobody that mentored me there. They didn't take a kid and, you know, assign him to somebody... everybody was on their own, and if you were good enough to do it on your own, fine, but I never got into science and I don't know why I didn't particularly like biology, and I was attracted to languages. So, in high school I studied Spanish and German but I couldn't take more than one language and so at the end of the first year or two with Stuyvesant, I packed it in because... on top of which I had to travel all the way from the Bronx on the subway down to 14th Street, New York which - when I was telling you about the lecture coming up that's where it's going to be, on 14th Street - which was an experience in itself, going down there every day because in New York kids traveled when they were 9 or 10 years old, without the slightest hesitation, go from one end of the city to the other for a nickel. So, I left Stuyvesant and I went to high school in the Bronx, the DeWitt Clinton High School which is a huge school. It's more like a college. It always reminded me... at that time the school, for some reason, was on three shifts. When you

were a freshman you would go for the morning shift or... and if you were a senior or a junior you'd go to the afternoon shift, so I didn't have to get to school probably, until 12 o'clock every day, and I could sleep late. On top of which, they weren't all that demanding, as it were. I don't think... at Stuyvesant I think there would just be more of a challenge, but... so, I studied German and Spanish. I belonged to the Spanish Club. I remember, one of my Spanish teachers was a guy who was the editor of the *Ladino Newspaper* in New York. *Ladino* is the language spoken by the Sephardic Jews who were kicked out of Spain, and it's Spanish with Hebrew characters, which I attended the what do they call it, the Hebrew School or Cheda, for about a week, and I hated it, and I just wanted to go out and I wanted to play baseball and my mother was a very easy-going person and never, made me do anything I didn't want to do. So, I said, I don't want to go to this thing, and so I didn't, go, I never was *barmitzvahed*.

My Mother

She was an incredible reader. I never could understand how she could read so fast. I remember my first wife had the same talent. My mother could look at a page and have it, practically... she'd read a book so quick, I'd say that, tell me what... was the book about, and she knew it. I couldn't believe it. She also had a very large handwriting, and scribbled almost very quickly. So my mother was a speed demon in everything she did. Things that we used to remember, she became an extremely good cook. She was taught by a pro. Even my sister is, but my mother could prepare, incredible meals. If you were having say, eight people for dinner, she'd be standing up, she'd serve, and as soon as your plate was done she would take the plate, take the dishes and the dishes were washed and ready for the next meal by the time you got through with the meal. So, you know, we get in the habit. When I get finished eating I'd immediately take, a plate to the sink and I would wash it out before... because if you let it dry you have to work three times as hard to clean them up. People let the dishes pile up and then it's a huge job. She could knit. I have an afghan that my mother

knitted, not for me, but I inherited it from my, daughter and she could sit and talk to you and knit, you know, and she made all kinds of sweaters. She was incredible, talented woman. Later on when my mother got older and... I had already started ISI, I was making a reasonable living. I certainly was not poor by then. She started to work - she had, a job as a cashier in a movie theatre. There was a movie theatre in the Bronx, it was called The David Marcus Theater. It was named after a war hero. She was the cashier in that place and she worked for that, for them for several years and never made a mistake. You know, she handled all that money and always had things in perfect order. But then the boss decided to let her go because his wife wanted the job. That led to a very sad period. My mother eventually died in a State mental hospital here in Pennsylvania. I think she might have been somewhat manic depressive early on because when I was a kid I can always remember that at certain hours of the day she was very, agitated, but extremely affectionate. My mother was a very loving mother, she'd constantly praise us and never even once hit us. After she lost that job she got into a very depressed state and she joined a community centre where she started to paint, and it turned out she was fairly talented. I used to have some of her paintings here. I've got them at home. She was a fairly creative person and I always felt that if she had been educated she could've done a lot. That, and her being married to a man who had absolutely no interest in things like that, whose favorite pastime was sitting in front of the TV watching wrestling matches, and, not the real wrestling it was the phony ones. So, this is Ralph's father, and you know my brother, Ralph. Yeah, he's a very smart guy. Ralph has got certain personality characteristics just like his father. He's, kind of, a guy that can put off till tomorrow what could be done today, nothing is urgent.

My Uncle Sam

Of my five uncles the youngest one was not barmitzvahed. I mean, by that time, they were socialist. The three of them were socialists. Marxist is really probably the best way to describe it. My uncles on the other

hand, the two oldest uncles, were in the coat and suit business. They were and my uncle Sam was very successful. A very clever businessman who had several talents one of which was that he could when you buy material they don't bring you a huge bolt of material for you to select, they bring you a book of little swatches and you've got be able to judge, from that little piece of material what this thing's going to look like, if you put that material into a woman's coat or a suit, and he had that incredible talent to be able to buy the right, at the right price and right materials. Secondly, in that business, unlike typical Jewish men who are very few. By the way, I believe it's because they say it's because of the alcohol dehydrogenase. It's actually some kind of genetic thing. I myself, always comment about this. If I have one beer I just cannot consume much more than one or two beers, and I never could understand how other people could drink more than one. Now, it doesn't necessarily apply to other kinds of alcohol. Sol Herner was a big drinker, he was... and I always told him, you remind me of my uncle. My uncle learned to drink and that was the key to his success with selling, as a salesman, because he used to wine and dine his customers, you know, and so he had that talent. Then he had this, we all knew he had the ability to do very quick calculations in your head, so he would walk around... I started to work for him when I was about 12 or 13. I used to go down to my uncle's business which was... he had a loft in a building, 37th Street and seven, between 7th and 8th Avenue in New York which is the heart of the garment centre, and on the second floor. I would go up and I was, like what they call, the building clerk. There would be orders that we would have to write out an invoice for two coats or five coats, or whatever it is, and put that in... with the, coats and then they would be shipped out. Well, he would come around... The copy of each order would go into a drawer, and every hour or so and we used to have to work 18 hours a day. You would come and you'd work until midnight in that season. You've got to get the stuff out. If you don't ship them you don't do it you don't sell them. So, he would come around and add up - he'd quickly go through the...

invoices that I had done and then he had a little piece of pad, he always had one and a half inches high and he would write down his numbers, and everywhere you'd see him go he would be writing down notes. So, that's why I always carry a pocket notebook to write down because I never can remember anything without writing it down. So, then I used to work on Saturdays and I had a business which was retail. People thought they were getting bargains so they'd come to these wholesale places on Saturdays and say, oh yes, I'm a friend of so-and-so - because technically you weren't supposed to sell retail because if the, let's say, JC Penney or, other buyers of the, of... from the company would find out you were selling retail, you're undercutting your own customers. You're not supposed to be selling retail. But they would come around and they would say, well, I'm a friend of so-and-so, and we would sell them a coat and we'd charge them the same price they'd pay, pay in the store but they thought they were getting a bargain. However, those were cash sales, and I think that was a good way for them to make some money without reporting it, as, you know, part of the general shipment. So, I used to take care of those. When people would come in I would show them the coats and they would pick something out then they would pay for it.

Learning to Play the Clarinet and the Piano

When I worked for my uncle I was still going to Stuyvesant. I would work there during the summer it was a very busy time in the coat and suit industry because... you're now shipping and... winter calling, and in the winter you were doing spring, right. So, I would go to school and then work there. I was earning - at that time the minimum wage in New York was 30 cent per hour. I think this is around 1937 or '38. I was working there a while and I... said to my uncle, 'I would like to... I think I need a raise'. He says, 'What do you need a raise for, you're making 30 cents an hour, that's a lot of money'. I was only 13 or 14. I said, 'Well, I want to buy a clarinet and...'. So, he says to me, 'How much is the clarinet?' I don't know, I think it was, like, \$12 or \$15, it was

a Peddler clarinet which, in those days, was a very common brand of clarinet made in Elkhart, Indiana which I think is still a place where they make instruments. In Stuyvesant you could get free clarinet lessons, if you had, you could probably even borrow an instrument but you couldn't necessarily take it home, but I don't know, in those days. Anyhow, I wanted to have my own, and he says to me, 'Well, no as your boss I'm not going to give you a raise, but as your uncle here's the 15 bucks, go and get the clarinet'. So, that's how I got my clarinet. I used to... and so I got the clarinet. I was a freshman at Stuyvesant and the way they did things there, and this is how I wonder how I can be sitting, in a room of people, or... and not hear anything, you learn to focus. In order to practice, or to even take lessons at Stuyvesant, we would sit in the balcony of the auditorium of the school, and the guy would give you a lesson and there'd be somebody three feet away getting a lesson with somebody else. It was you just had to be able to, block out anything else that was around you. And the same thing when I traveled. I mean, people don't understand how I can read on the subway, or read in a moving car. I did all my homework on the subway. Remember, I had to go from the Bronx all the way down to Union Square and that was, a 45-minute ride from the, from 168th Street in the Bronx all the way down to 14th Street. So, that's how you develop certain habits, but after... so, that's how I first started studying the clarinet, and I didn't, I didn't... after I left Stuyvesant I don't think I continued it. I don't know why, but... my mother had originally wanted, had gotten us some piano lessons and I don't remember... I remember the teacher used to, if you made a mistake, would take your hands and bang them on the piano, I hated her. An incredible way to teach you to play. So, I didn't stay with that very long. My sister, I think, remembered that too. But we had in those days, when we lived... even in... we... couldn't afford it otherwise, people would literally pay you to take their piano away. So, it was enough to cover the cost of moving the piano in, because we couldn't afford a piano and, there were people who were... in 1929 an unbelievable amount of

people, who were - the Crash - they lost everything and they had to sell everything, and it was difficult. There were so many pianos available, you could... just for taking, you could take them but how are you going to take a piano? You've got to get the damn thing delivered up to, get it up to a third or fourth floor apartment. It was quite a feat. So, it reminds me of the time when we opened up the office at 1523 Spring Gardens Street and we were up, we had the third floor tenement for the office of ISI. It wasn't ISI then. We bought a desk somewhere, I don't know, a cheap desk and in order to get the desk, we couldn't bring it up the steps, we had to take it through the window. I used to tell the story about this. There was a kind of, a homeless alcoholic, or whatever he was, a homeless man who used to hang around here and he, always wanted to be, somehow be helpful so we were moving this desk. I remember my brother-in-law came and he was helping me move this desk. We were hauling this desk up through the window with a rope. This guy comes along and he, sees us doing this, he says, 'Let me hold this for a while for you', I had to do something, I forget what it was, and he, took a hold of the rope and as soon as I started to let go, the guy was flying out the window with the desk. Oh God, what an experience. So, but those desks were unbelievably heavy, you know, the old fashioned wooden desk.

Working Before University

Before I got to the University of Colorado, in the summer of 1942, when I went out to Colorado by train, took the train out to, Denver, I was met there by a friend of one of my uncles. I had over a month to go before school started. She said, 'How would you like to work for a month or so? I have a friend...' She had... a friend of hers was a guy named Carl Campbell who was a very interesting man. I had never met an albino before, and Carl was an albino. It is an interesting experience because their eyesight is incredibly poor and Carl was a guy who, if he had to read, he would be up in, like, in a double bunk, he would be up by the ceiling reading with the light right down by him. But he was an incredibly literate man. He just... eventually became a Professor of

Economics after the war, but he was obviously not qualified to go in the service and he was running the tool shed at a construction job in the Rocky Mountains. What was this job? It was... the army was just then building Camp Hale which later on became the Headquarters for the Mountain troops. So I went to work as a laborer out there. That was an unbelievable horrible experience working as a laborer. After a couple of weeks I got really sick and eventually hitch-hiked... had to hitch-hike all the way back to Denver and almost died on the way back because somebody had gotten a lift with some Indians and they dropped me off in the road and they said, 'Oh, you'll get another ride', and I waited for hours and hours, almost froze to death, while I was waiting for another ride to get into Denver. Eventually I got there and I remember staying... I checked into a hotel which was \$1 a night, the Milner Hotel, and they had a chain all over the country. You could stay at the Milner Hotel and get a clean sheet and bed for one buck. And, I slept for 48 hours. I was, oh, really sick. Then finally when I got out of it, then I finally went up to Boulder and signed up for my course work and so on. That was how I got to know something about Colorado.

Learning to Type and Army Service

When I was in high school somewhere after I left Stuyvesant, I was not all that well motivated at school. I got good grades in things that I could do without doing much work. When I had to make up... when I went with my uncle Nat and his wife drove down to Florida for a winter vacation. I told you about what he did in Russia, but because out of school long enough that I had to make up... I failed some course and I had to make up some history course or something like that, and in order to get more units they told me I could take electives that wouldn't require any homework. So, that's how I studied typing and office practice which means I knew how to use mimeograph machine, the old ways of doing business in those days. That typing course was a godsend for me, I mean, later on, you think of all the times you use typing in your life, and it was not a routine thing for people to

know how to type in those days. There were a lot of things that came from that. Later on even when I was in the army, I was telling my son the other day, how I was offered the job as company clerk at Camp Blanding because that was... I just visited Jacksonville where Stefan lives, and Camp Blanding is not very far away, that I was offered the company clerk's job with the top sergeant because I knew how to type, and also because he liked my mother's shipment of Jewish salamis. This guy was an Irish guy married to a Spanish girl and he just loved halvah and all, and all the delicatessen stuff that my mother would... I was going to starve to death in the army and so we'd, they'd send us these kosher salamis or boxes of halvah, and all sorts of interesting stuff. But, I volunteered for the mountain troops because the reason I was so wildly enthusiastic about being in a skier, it was because my wife my girlfriend then, was in Colorado. That's why I volunteered in the army to go to the mountain troops [10th Mountain Division].

Getting Drafted

When I went to school in Colorado which only lasted 6 months. It was during the war and I decided to leave school which upset a lot of people. I went out to San Francisco and I got a job working in the shipyards, as a welder. I was trained as a welder and I lived alone in San Francisco. I rented a very small apartment and while I was out there... I had met my first wife in Colorado but had left. She was a physics major and I eventually got a degree from Boulder and, then moved out to California later. But I then came back to... after working in the shipyards. the war was on and I knew that eventually I would be drafted, I don't know why I decided I wanted to be in New York when it happened, and, but a friend of mine, Richard Aspinwall who just died, just about a year ago, had joined the Merckhant Marine and my plan was to join, the Merckhant Marine Academy with him. So, when I came back to New York I immediately got a card from the Draft Board, so instead of just ignoring the card, which I was told I could easily have done I'd just gone to the Merckhant Marine Academy. When they accepted me, I just showed up to

the Draft Board and the next thing I knew I was out at Camp Upton. I was drafted, okay. But, I had been staying at...my mother was living in the Bronx and so I got drafted from that address, I guess, Gunhill Road in the Bronx. Now, after I went into the army, having lived in Colorado and now Faye was there, when I got drafted... I was telling the story... my... because I was just down near Camp Blanding is where I went for basic training, just outside of Jacksonville. They, asked... when you were in basic training, when you leave they ask if you want to join a, regular, paratroopers or mountains troops, I signed up for the mountain troops. They took me because Faye was in Colorado. If we were to get out in Colorado I'd be able to see her. So, that's what happened. I... showed up at the Camp Hale in Colorado after basic training. But, other story that I had to tell them was while I was at Camp Blanding, my mother used to send these packages of food, and I had a top sergeant who loved her stuff, and he said to me, you can become a company clerk, you won't have to go... on combat and so forth, why don't just take this, because he found out I could type, you know, that, same old thing with the typing. I said, no, I volunteered for the Ski Troops and that's how I went there.

Army Training and Getting an Ulcer

Well, what happened was that after I got to Camp Hale in the army, it was already late January or early February, and I was past the training sessions for skis but they made me a snowshoe man, or something that means you, carry machine guns and stuff like that. Also, you learn how to pack a mule. The animals were very important in mountaineering, but you weren't allowed to put more on a mule. You could put 100lbs on a man but not more than 200lbs on a mule. But at nighttime we used to sneak stuff on the mules. In April apparently the army decided to send these, the guys to fight in the mountains in Burma. So, they said, we need to send you down for some jungle training. So, they took this whole division, at least my regiment, and shipped them off to Texas in the middle of summer, I mean, in June, whatever it was, and they... the weather was incredible. So, I was down in, at

Camp Swift which was outside of Austin, Texas, and that's how I had my first encounter with Austin - a small town at that time, the capital of Texas, a university town. Having been in Colorado, these guys would go on weekend passes, they would all rush into the town and there'd be 10,000 soldiers marching up and down the streets looking for something to do, and I used to go out to the university to listen to music out in the, in the library. So, what happened was that they asked if I wanted to sign up for officers training school when they heard that I had a few months of college, and, so I did, and I went to OCS, Fort Benning in Georgia. When I lasted... I think I went through about 13 weeks or something like that, but then they had... the Battle of the Bulge occurred and it was some order to send everybody to different places, the poard of embarkation. Eventually I wound up at Camp Mead, Maryland. While I was there I started having symptoms. It turned out, they diagnosed that I had a duodenal ulcer so my mother - I'll never forget, my mother - they called my mother, she came down to visit me at the hospital, that poor man, and she, the doctor, captain or something, says to her, 'Mrs G, we've got bad news for you, we're going to have to, we're going to have to send your son home, he can't stay in the army'. She started to cry, you know. He thought that she was unhappy. Oh, that was really funny. So that's what happened. I got a medical discharge but, and I was able to go to school on the GI Bill. So that was an interesting period.

Columbia and Berkeley

Now, when I went to Columbia I had to pick a major. Now, I originally wanted to go to medical school and I had signed up for that, but they told me much later... I only, I was only at the summer session in Columbia and then I went back. By that time my first wife had moved to California and that's how I went out to Berkeley. She was already registered there. On the GI Bill, I took the pre-med program, but then when I got through my... the VA, or whoever was in charge of, I guess, it's the VA that does that, they said to me, you don't have enough GI credits to finish a medical education, under

this programme, which was called PL16, because you had a medical discharge you have to be able to be employed at the end of your education, so you've got to pick a different career objective, okay. So, that's how I decided to go into biochemistry. I picked a biochem major. So when I finished, however, 2 years at... Berkeley, my wife applied, I'd already been married by that time and she applied to medical school and got into Temple, and then we went, moved from Berkeley where we'd been living in Oakland, to New York. When I got to New York I signed up at Columbia but I couldn't get into medical school at that point and I wasn't going to be able to get the money for it, either. So, I became a chem major at Columbia and I went to Columbia to continue, and I graduated in '49 from there, and that was when I ran into the problem of finding a job.

My First Wife Leaves Me and Coping as a Single Parent

It's funny how, people think they're doing you a favor ... loyalty takes you in funny directions. When I met my first wife we were very much in love, she was 7 years older than me, and already... she was a... very brilliant woman, had almost like a photographic memory, she had been a physics major and decided to go to medical school after she'd gotten her degree in physics. But, when she was out in Berkeley during the war, moved out there, she worked at the Lawrence Radiation Lab and she met the fellow who was her, became her second, her next husband, and Marvin Weinstein, who... we're still good friends, but he was the beau, but she hadn't told me. She didn't want to tell me about it because she thought, you know, she didn't want to write me a Dear John letter. She wrote to me almost every day when I was in the army and I still have her letters. Marvin didn't want to say anything, so when I came out there she didn't say that they cared for each other and we got married, and then after we were married she decided that it was a mistake and she wanted to live with Marvin. So, what happened was she got accepted to medical school. We were... Stefan was born in January, we moved to New York around June or July when he was only 6 months old, and she had other children and her 5-

year-old son, Michael, was with and at that point, I was his significant parent. I mean, he didn't know any, anybody else but me at that point. So the two of them were with me in, this apartment in the Bronx, and she had decided that she was going to go to medical school regardless of what I decided to do, and she was going to live with Marvin, so she made a deal with her mother to send Michael out to live with his grandmother. I came home one day and she told me he wasn't there anymore and I could do what I wanted to do about the fact that she was going to leave. So, I decided, I don't know, rightly or wrongly, well, you're not going to take my son with you, already shipped off Michael. I considered my child, you didn't even ask me. Of course, with the legal issue she was entitled to do that. So I took Stefan with me. So, here I had this baby, 6 months old, I found out what it is to be a single parent in New York, and male. If you're a woman it's not so terrible, in those days it wasn't, anyhow. I had a lot of experience with taking care of kids in those circumstances and the bias against men is incredible. I could not... they would not accept my son in any childcare centre unless any one of the female members of my family refused to take him, okay. So, my sister was not unwilling to say she wouldn't take him, right, she convinced my mother the same, they wanted, in a sense forced me to leave the kid with them. So, I started working as a cab driver to support a nursemaid and I went to live with my son near Columbia University. I rented one huge room and this nursemaid was, Nellie I think her name was, she came to take care of Stefan while I went to work, or when I was in school, whatever. That didn't work out too long, very long, and then I, for while I found a... I shared an apartment with another... a woman who had a child and we shared the cost of a nursemaid, and I could leave the baby there. That also didn't last too long. I had some really interesting experiences. It turned out that she was married to a guy who was a homosexual and I had never had any experience in dealing with people like that before, and everything. So, that lasted for a certain length of time, and then finally what happened was that I decided that I would

leave Stefan with my sister. He went to live with my sister. That eventually proved to be somewhat disastrous because her husband was a, sort of, almost psychotic, and... so, somehow I managed to get through college, working. I worked, you know, this is... and I oftentimes I get the period before graduating and after, there's two different periods in which I had to work because later when I left and I'd already been to Johns Hopkins, I came back. I had to do a similar thing, to work and go to school at the same time. At Library School I had two different jobs, I had a scholarship and I had, and I also had worked in the Hickory, the Old Hickory Bookshop which was a, really a mail order medical history bookstore, and they, the people that ran that sponsor a scholarship IC at the, Medical Library Association.

Origins of Eugene Garfield Associates

When I started the company Eugene Garfield Associates our, letterhead was Information Engineers, okay. So, I've always thought of myself as an engineer and probably one of the reason, motivations for wanting to change, get ASIS to add the word technology to its title because I didn't think of myself as an information scientist, per se. Anyhow, it was, I think, about a year or so after, I don't know the exact date, I don't know that I even have correspondence, it might be in my file somewhere. I got a letter from some state organization, or some professional society of engineers saying, Mr Garfield, you cannot call yourself an Information Engineer because if you are an engineer you have to be licensed by the State of Pennsylvania, you've got to be a licensed engineer, and in order to do that you have to have graduated from an engineering, and accredited engineering school or done some other equivalent, I don't know what the rules are, and I don't even know if that's still true or not. I thought it was very funny because it was only a few years later that, I think it was either Sol Gorn or Morris Rubinnoff at the University of Pennsylvania, he asked me to come over to teach graduate students a course in information retrieval. So, anyhow, that was why I had to remove that designation from our letterhead, and by the time 1960 rolled around we changed the name from Eugene

Garfield Associates to Institute for Scientific Information which had an entirely different, kind of, history. I didn't know until quite recently, I think I read this either in some document about the history of the field of information science, or either in Andrew Brown's book about John D Bernal that Watson Davis had actually used a term, 'scientist institute for information', as a proposed entity that should be developed, and Watson Davis was a name well known to us because he was one of the original founders of ADI, the American Documentation Institute, which started around 1935. When I was still at Welch Library in Baltimore, ADI was still called ADI and you could not be a member of ADI, only institutions or societies could be members of ADI. It was more of a, cooperative, almost like a, non-profit trade group of academic institutions that were trying to develop new technologies for their use. So Watson Davis and other people like that were interested in things like microfilm and whatever, you know, so. I thought it was very amusing that they would almost have anticipated that name. At the same time, as I've often acknowledged, JD Bernal was now the inspiration to me because of what he proposed in Social Function of Science. When I looked at that book recently I realised how much I'd forgotten about it over the years. I mean, it, covers an amazing amount of material for a book that was written so far ahead of its time. He proposed... today people should know that he is probably the original... he would be supporting Steve Harnad and the Open Access movement, Vitek Tracz and others would be interested in that, too. But the irony of all that is that the publishers, the society publishers were just the same group that opposed Bernals ideas and as are now opposed to the Open Access thing. You know, nothing changes after - how many years now? I'd say about - originally '39 he proposed it - that's about 75 years, nothing much has changed. So... But, the origins of information engineering backing in ADI started as an institutional thing, and then, in 1952 they decided to make it a professional society and opened it up to individuals and that was when I first became a member. That's why I got to be, I think,

probably one of the first members as an individual, to join. I don't know the exact year - I think it was '52 - and I had already been, I had met Jesse Shera who was the editor of American Documentation, which is the predecessor of the Journal of ASIST, you know, and he asked me would I like to be an associate editor of the journal. I couldn't believe it, but he asked me. I thought it was supposed to be such an honorific thing, you know. That was how I happened to be in a position to eventually invite WC Adair, the former Vice President of Shepherd's Citations, to write an article about citation indexes for Science. Because, what happened was, well, now we're jumping back and forth. I don't know how you're going to handle all this. But, I had not only been interested in joining ADI but prior to that I had been involved in the information side of things simply because I learned about chemical information systems through, by accident through James Perry. When, I worked in the laboratory of Louis P Hammett as a lab assistant at Columbia University, he was the editor of the McGraw Hill series and I guess, just like I was attracted to the public library I was attracted to his personal library. He had a huge collection, the entire chemical abstracts, his own personal collection, and as the editor of the McGraw Hill book series he had access to a lot of material. At the same time, I also found indexes that had been created in - or I tried - I created indexes to all the chemical compounds that are... in the closets at Columbia University, probably 100 years of students who had created compounds and labeled them, and all they needed was purification and you could use them, and these guys were spending all kinds of time remaking this stuff. I said, 'Why, should you do it all over again when we can do it, have it done before?' So, that's how I, sort of, got interested in chemical information systems and, searching and so on, and I was just drawn to it.

Working for Louis P Hammett

When I graduated from Columbia University in 1949 with a chemistry degree, it was impossible to find a job. It was a real recession. Although I eventually got a job as

a lab technician, because I was a chemist, in a private company that did consulting work for companies. I temporarily had a job which took advantage of my knowledge of typing and I became a sales correspondent at La Salle University which is really a mail order school, for... you take courses in shorthand and as a result I could take free courses in stenotype and shorthand, and so I decided, with that background, I might someday be, use that and become a secretary. Billy Rose was a very famous promoter but he'd, he'd gotten there because he knew shorthand and typing. He was a champion typist. I think he could type 200 words a minute or something. So typing was, oh, like, 150, but he could take steno at some incredible speed. So I eventually got this job after working as a sales correspondent. Just a few blocks away I somehow got a job as a lab technician at a place called Evans Research and Development, and while I was working for this company which had various consulting assignments, for example, they had a client that was looking for synthetic sources of agar gels which had to be imported from India and they were worried about supplies, and their client produced something called Fast Teeth, I think. It was somehow connected with false teeth, or whatever it is and they also tested out hair shampoos. You could get a free shampoo if you wanted. Half your head would be with one shampoo and the other half with another shampoo. So, one day I was working in the lab and somebody comes over and says to me, 'We want you to help us out. Somebody was supposed to, meet with this client and take notes. We want you to come in and take notes'. So, they didn't know at the time that I could type and everything like that, so I went to this meeting and I took notes of this meeting, and I typed them up, and they were stunned. They thought, I could do that, much better than I could do lab work. So, they asked me if I wanted to stay on as a technical reporter, but just about the same time that I had gotten tired of working there. Sydney Bernhart my first cousin who lived in Brooklyn was getting his PhD with Louis P Hammett, for organic chemistry. Sydney was a brilliant guy, very unusual fellow. He was a

diabetic and always had health problems for most of his life but was well on his way to his PhD, and heard that I was looking for a job, and so he got in touch with me and said, there's an opening here to be a lab assistant for Louis Hammett, if you have the aptitude and are interested you can get your doctorate here. So, having already gotten my degree at Columbia I knew the place, but I didn't know anything about Hammett other than the fact that physical chemistry was something that scared the hell out of me, and I had taken physical chemistry from another professor Alfred - he was a well known physical chemist as well. But I didn't know how famous Louis Hammett was at the time I went to work for him. Later on the irony of it is that he went on to become, not only a winner of the National Medal of Science, but he was a member of the Weinberg Committee that did the Weinberg Report on Science Information in Government, which I mentioned just the other day when I was down in Oak Ridge, telling these people that Al Weinberg, coincidentally, brought together all these people that I had known on that committee, Josh Lederberg, and so forth. So, anyhow, I went to work for Hammett and... with the idea that I would eventually get my doctorate. But while I was working in the lab... I got exposed to working with ion exchange resins which were produced by Rohm and Haas. How things come around. I'm now a very good friend of the erstwhile President of Rohm and Haas, John Haas at the Chemical Heritage Foundation. He lives right near me. He's a wonderful man. But... I would have to run his experiments on... and have to create these various esters. The whole idea was to measure the differences and the rate at which you could separate these esters on the ion exchange resins. So, in the course of the work, though, there were times when I had to use picrate and they're very explosive, and I had set off one or two explosions in the lab, and finally, as I've often pointed out, Louis P Hammett, very diplomatically, called me in, the office one day and said, I think it might be a good idea if you thought about modifying your career aspirations.

American Chemical Society Meeting and a New Job.

I decided to go down. I think that was the Diamond Jubilee Year of the American Chemical Society, and I went down to the meeting. I'd never been to a, scientific society meeting before. It was held in some hotel in New York and I go in there, oh, and I also had read in the paper an ad for the Director of Chemical Research at... the Ethel Corporation was looking for what was advertised as a Chemical Secretary. So at that time I could do typing and stenography and I had the chemical degree, so it was a, kind of, a natural fit, and so we arranged for an interview. When I went down to the hotel with the interview, I walked in there and I see a sign that says, I think it said, 'The vision of chemical literature'. I walked in the room and there was James W Perry on a platform, chairing this meeting, and I started listening to these people talking about different things, and I got the idea that here was a group of people who were doing for money what I was doing for nothing. So, I said to Jim Perry, I walked up to him after, sort of, the session closed, I walked up to him and I... said to him, 'How do you get a job in this racket?' and he laughed. He had just come out with the punch card book. He and Robert Casey of the Schaeffer Chemical, Schaeffer Ink Company, they produce ink, right, Schaeffer... and just produced the punch card book, Punch Cards in Science and Industry, and he also... James Perry was a chemical engineer. He had produced a very well known textbook on Scientific Russian, Teach Yourself Scientific Russian. So, after I said that, he said to me, well, he said, 'There might be a chance for you here... there might be... we might have an opening for you'. So, at that time James Perry had a project at MIT. I don't know all the politics about, and working for him, at that project, was Alan Kent who later on became... I think the editor of JASIST for a while and he also was the editor of the Encyclopedia of the Library Information, and the Dean of the Library School at Pittsburgh. I had a lot of contact with Alan later on. Madeleine Henderson was his assistant, I think. We've remained friends now ever since then. So, he says, 'Well, why don't we meet and talk about this'. So,

I invited him to come up to my mother's house in the Bronx, and he came up there, and she fed him and he loved Jewish food, and he liked to eat generally. So, he then gave me a copy of, of his book and he said, 'What we want you to do, you, kind of, have you come up and visit the project at MIT'. So, at some point a few weeks later he calls me and I go up to, Boston and he takes me around the MIT library. He introduces me to Norbert Weiner, who was in there. Also he was a friend of Calvin Mooers, interesting, but I didn't know that at the time. Calvin Mooers was the guy who invented the information retrieval and he was working on his master's degree. So, a few weeks go by and I don't hear from him. Finally, somehow I got in touch with him which, you know, in those days we didn't have cell phones. It wasn't all that easy to get hold of people. He finally... which I learned later but I wasn't aware of it at the time, they have closed his project down, or he didn't get his funding from whoever was funding him, and he was going to move from MIT out to Western Reserve, I think, they went to, or Case. I'm not sure where. You know, he and, Alf set up that metallurgical system, whatever. So... metal system. He calls me up and he says, 'Why don't we meet again?' and he said, 'Do you mind, I told you the job is going to be in Boston but will you care if it was in Baltimore, if you had a job Baltimore?' I said, 'I didn't know the difference, you know', I said. So, he says, 'Alright', and we met again. My mother fed him again, and then he said, I want you to... here's somebody I want to introduce you to, and he gives me the name of Dr Sanford Larkey who he knew as the Head of the Welch Medical Library, the Director, but also as Head of the Army Medical Library. He had the indexing project they were sponsoring.. to investigate machine methods and new methods of indexing. So, I go down to Baltimore and I meet Larkey, and all the time I'm talking to him I think I'm going to work for James Perry. I never knew that this was a completely separate thing. Well, he meets me and they were looking for who was a chemist because that was the work that was being done, partly by Mina - Williamina Himwich was a very wonderful woman who I replaced, so to

speak. I mean, she left the project and I couldn't really... She was a trained brain physiologist but she was very, bright in other things too, and they needed somebody to do the chemical nomenclature side of subject heading analysis, and so forth. Larkey had gotten, obviously, a recommendation from Perry or I wouldn't have been there, but unbeknownst to me I found out later when I was leaving the Welch Medical Library that Larkey had gotten a hold, or written to Louis P Hammett and he told him that Garfield is an extremely hard worker but not a particularly original thinker, and that was exactly what Larkey wanted, he thought. So, that's how I got the job. Was he surprised.

Working at the Welch Medical Library

Now, the Welch experience was another turning point because the interesting thing is I had met my second wife, Winnie, while I was an undergraduate at Columbia. When I went to work at Welch Medical Library, took that job in '51. She'd made up her mind and said- 'this was a decision point, either we... I go to Baltimore with you or we're finished'. So, we decided she would stay and I went to Baltimore. Stefan was living with my sister. My mother gave me a present for graduating, which was a lot of money in those days, and I gave the money to my sister to take care of Stefan. So, I went to Welch and at that project I started out... it turned out... San Larkey was a first-rated alcoholic. He had a medical degree and also a degree in literature from Oxford and, sort of, allegedly a medical historian, so he'd... how he fell into the job as Director of the Medical Library, I do not know, but he, did. I spent the next 2 years working for him on that project and essentially each day consisted of the two of us sitting across the table, it was located in one wing of the library, Welch Medical Library, and discussing... he would tell me all his war experiences as a medical. I don't know what, how, what he did in the army, and we'd discuss subject heading analysis in terms of what we're going to be putting in the subject heading authority list. That was 9 to 5, and at 5 o'clock he would go home and he and his wife had a big social life and she was a member of the Democratic National Committee, as I recall. And the evening was

when I started to do the research. That was where I got to know the people at the Army Medical Library and it became the Armed Forces Medical Library, and later, after I had left, it became the National Library of Medicine. Then I got to know Seymour Taine and Bob Hayne through - Bob Hayne was the subject heading authority editor, more or less, at... and Bob came to work for me later, much later. Then I met all sorts of leading librarians and scientists at that project. I met Estelle Brodman who was at the National Library. Dave Kronick was there. I met all these people, and when you meet enough bright people you learn quite a bit from meeting such a group. On top of which, the Welch Medical Library indexing project had an advisory committee of about 15 honorary consultants. Apparently serving on such a committee is, has some... people like to do this because, they get to go to a meeting once every 3 or 4 months and they, get their expenses paid and they get to meet other interesting people. It's like we have an advisory committee here and you... why would somebody do that because it wasn't a financially rewarding thing, it was an intellectually rewarding thing, and that committee consisted of a lot of incredibly bright people. Mortimer Taube was on that committee. Chauncey D Leake was the Chairman, and that's how Chancy and I became very good friends and he became a, kind of, mentor to me. Verner Clapp, the Chief Assistant Librarian at Congress was on the committee, Eugene Scott, it's all recorded.

Eventually, because there was no other project like this in the country. Everybody, kept coming visiting this project because it was the only one like it, and I met all sorts of foreign librarians who came in, the first time I ever met I met them at that committee, at that library and I would explain what the project was doing. What happened eventually was that they had arranged to get an IBM 101 statistical machine which was supposed to be the fantastic answer to all these irretrievable problems. I became an expert on wiring what we today call programming that machine. I mean, you didn't program it, you had a board where

you put the wires in to do the sorting and do whatever you... selecting and so forth. I had already learned how to use the other IBM machines because this machine was located separately from the Johns Hopkins Hospital which was across the street, and where all the other standard IBM machines were located, the tabulators, keypunchers, and so forth. We had our own keypuncher in the lab, and Collators and so forth. So eventually I started doing things with this machine that the IBM said were impossible to do. I mean, we developed... Calvin Mooers had developed something called superimposed coding, so I developed a concept of superimposed wiring and said, if he can do that with Zator, I can do that with wires, so that's how we developed, I developed that particular system. Anyhow, by the time... There were so many different things that I did on that project. I met the people at Chemical Abstracts through that project. I was a volunteer abstracter for CA, and they gave me the subject of Spanish pharmacology because I could read Spanish and I knew enough chemistry to do the abstracting and I learned the technique of abstracting and indexing. And, Charles Bernier was the editor, I met him, and EJ Crane, Dale Baker was working for them at that time, but he never came to the Welch project. And, so, what else? I did a study of abstracting and indexing services in the overlapping coverage, and so forth and then subject heading analysis, we did work with Sam... Bob Hayne, and eventually I went over to... the idea was that you introduced mechanisation so I went over to learn how they did, they put together the current list of medical literature. I developed an IBM punch card system for producing that system. There are these papers that I did, Preparation Subject Heading was by punch card methods. Well, today they're so primitive but you think about it, back then...

Starting Contents in Advance and Getting Sacked

What happened was that he didn't tell me, but San Larkey had been told that the project was going to be terminated in June of that year. Somewhere in the second year of the project I decided that in order for me to do

research properly I needed to have a current awareness system. So, I decided to produce what I called Contents in Advance, and what I did was I wrote to all these library journals and documentation journals and said, just send me your contents page and I'll put them together as a... publication, and that's how I started Contents in Advance. This made Larkey very unhappy. Why, I don't know, but he just didn't like the idea that I was doing this. Now, he was a man who liked to be in control. So I... at first... it used to be that everything he ever told me I ignored him because, the hours, during the day he would have one thought and then he'd go home and he'd drink himself to... blind and then he'd come in the next day and start all over again. It was like he'd forget whatever he said to me. Then one day he says to me, 'Well, you ignored my'... I kept on producing current, it was called Contents in Advance, he said, 'You ignored me so basically I'm terminating you, okay'. So, without even giving me my vacation time, he cut me off without my vacation pay and whatever, my job was terminated and the project ended.

As a Documentalist, You Make a Great Career

We had planned to have a public discussion of the project's different sub-projects, okay, subject heading lists, searching by the 101 statistical machine, various other topics. We organised the first symposium of machine methods in scientific documentation held at the Welch Library. We advertised it, we announced it. 300 people signed up, for about, I think for \$5 or \$8 you got a, free lunch with an all-day seminar, discussion, and a tour. The chef of the Johns Hopkins Hospital prepared us an incredible meal in the main room where they have the picture of the four famous physicians. There's the very famous painting of...

I haven't seen it.

You've never... have you ever been to the Welch Library?

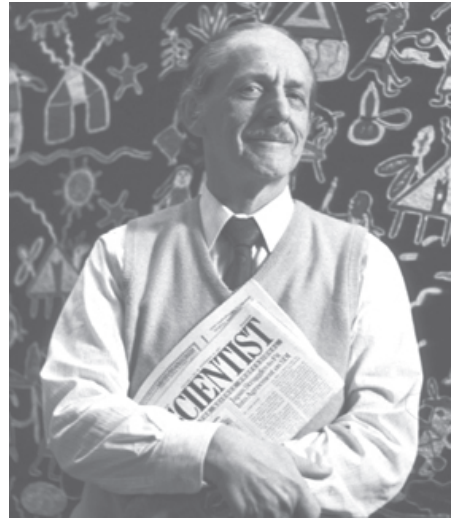
Once, but...

That, it's probably on the web. It's a very famous painting of the four physicians. Osler was in that, probably and Welch. I forget who the others are. Anyhow, it was a

wonderful meal, and one of the funny stories that comes out of that is that Ralph Shaw who was the Director of the National Agricultural Library and, very famous for the work he did on, with Vannevar Bush, the rapid selector, and so forth, he was a very funny man and incredibly articulate. He later became the Dean of the first Library School at Rutgers and later moved to Hawaii and became Director of the Library School out there and the library. But Ralph Shaw came to the symposium and he wrote back and he said, 'Dear Garfield, as a documentalist you make a great caterer'. You see, he'd, he liked the lunch. But, he and I became, you know, pretty good friends, and at one point he was going to hire me as an assistant professor at Rutgers later, and he had to withdraw the offer because somebody else had accepted that he wanted first, Lowell Martin, I think it was. But, Ralph was a guy who was an incredibly domineering person... Sam Lazerow and Norma Relcos who worked for him. When Ralph Shaw was out in Hawaii, at one point later on, during this Vietnam war, my son, Stefan, was in Vietnam and he was wounded and they sent him back to Hawaii to recover. I can't remember the reason why I couldn't go to see him, so I called Ralph Shaw in Hawaii and asked him if he would go see Stefan, and he did, and Stefan will never forget because when Ralph went to the hospital he took over the place and started bossing the nurses, telling them what to do, and so he's just that type of guy, you know, very domineering, bossy person. So, anyhow that's, I've often said, you know, all these people I met when Larkey terminated me that when I found out about the letter he'd got from Louis P Hammett when I went out I'd for some reason asked somebody to show me my personnel file. That's when I saw the letter from Hammett.

Going to Columbia Library School

San Larkey was as very vindictive. He even tried to prevent me from publishing papers on work that I had done at the project. There's a correspondence... and, my first paper was The Preparation of Subject-Heading Lists by Punched-Card Methods. It appeared in The Journal of Documentation, and he had a lawyer write



up a letter saying that this was, not permissible, whatever it is, there was no basis in law, I don't think, for preventing me from publishing this paper, but he just wanted to make my life miserable. But after a while it stopped and then I did another paper on the preparation of subject heading list, of printed indexes, that was how we showed how to do the Index Medicus or whatever it was, and, eventually it died down. He, never did much after that. He didn't have a very distinguished publishing career. But what happened was, having met Sam, and having met Seymour Taine, having met all these other people, they said to me, what are you going to do now? I said, 'Well, I've got to support my family, I don't have any means of support'. Seymour Taine, I think, said, 'I think it would be good if you were to go and try and get a library degree'. So, I applied to Columbia Library School and I got in for this. I went there for the summer, and while I was there... and I had to pay for that because... Columbia was not cheap, the Library School was not cheap. I met the Dean of the School, Carl White his name was, and he was a very interesting, kind man, and we got to talking and it turned out he had been a preacher or a... I don't know, a Doctor of Divinity or something like that, my mother thinks, but... he said to me, 'There is a brand new Fellowship being set up by the Grolier Society', and I didn't even know what the hell the Grolier Society was. It turns out the

Grolier Society was a name, not, there was a real Grolier Society, but the Grolier company produced the Encyclopedia Americana, okay. So, they had a form and they asked you to fill out this application and in my list of references I gave, as my references, the Director of the National Library of Medicine, the Chief Assistant Librarian at Congress, and the Director of the National Agricultural Library. So, I was a shoe in. I got the fellowship. That's how I was able to get through Library School. At the same time, I was working at the old Hickory Bookshop which is a job that Sam got me, and what they wanted me to do, they were a mail order, they sold old and rare medical books and the place was, sort of, chaotic, it was, everything was on shelves. What I did was basically I went in and I set up their, a numbering system. I put a number on each book and put it on the shelf, and catalogued it so you could look up what they had and where to find it, and saved a lot of space by doing it. Jo and Murray Gottlie, were very grateful that I did that for them, and I figured, you know, I earned, enough to help me get through school.

Getting a job with Smith, Kline & French

When I was at the Johns Hopkins project I had met all sorts of interesting people like John Mauchly, and Calvin Mooers came to visit me. I don't know if you remember Ellen Tornudd was the Director of... the leading librarian in Scandinavia. She's a real good friend. She's retired now. So, Ted Herdegen, from Smith, Kline & French, had come down to the project, and he heard that I was in library school and they wanted to set up an IBM punch card system for one of their drugs. He calls me up, and tells me that they have an opening, would you like to come down and work for 6 months to set up this system? My best friend at the time was somebody I'd met by accident while on vacation in, in Canada, Casimir Borkowski, do you remember that name? Well, Casimir was a very interesting guy, but then the point being that he was working for Léon Dostert at the Georgetown machine translation project from Russian to English, and I was supposed to go to work with them at the project. So, Ted Herdegen called me up and

says he wants to offer me this job as a consultant, and I said, but I'm already committed to go to... I've got this graduate post doc or whatever you call it... not a postdoc, a graduate teach... research assistant at the, the machine translation project. Now, a few days later I get a call from Casimir that the Government cut off their research money and the project was terminated. So, I got hold of Ted Herdegen and I was desperate for money by that time, I said, 'I'll come to work for you', and that's how I came to work for Smith Kline. I came down there. I graduated in June of '54 and while I was at library school was when I first wrote up the idea of the citation index. The paper that I eventually published in Science was written originally as a term paper.

Discovering Shepard's Citations

The vice president of Johns Hopkins University had been asked to be the introductory speaker at the project, at the symposium. In his talk, he made a remark that became quite frequently quoted later on, that scholars were going to drown in a flood of information. His name was Lowell Reed. I don't know if they had a PR team at the Johns Hopkins. He was vice president of the university, and was picked up by the news services and the headlines were, 'Man to drown in a sea of knowledge', or something, like, that. That produced all kinds of letters that we got from various people outside, even, academe who were interested in what we were doing. But in particular, I remember one... we heard from the librarian at San Quentin. It was interesting how... he was apparently one of the more progressive librarians in the country. But another person that wrote to me, or to the project, and I hope I still have the original, it was written in green ink, handwritten and it was a letter from William C Adair who was the former vice president of Shepard's Citations in New York. I had never heard of Shepard's Citations at that point and in his letter he said to me that way back in the '20s, way back, I mean, at that point we were talking 1953 so, it was 30 years back, he had had discussions with, some engineering or physics society about the idea of, what they call a citatory system for science. I think

basically he said that they had considered it but the project was totally impractical because, they were doing case law, and it's important historically, people don't even seem to know this, there was no citation index at that time for law journals. Later, Shepard's produced that citation for the law journals only after we started the SSCI, because librarians started complaining to them about why, didn't they have a citation indexing for journals. So anyway, they had figured out there was just too much literature. Law has got a lot of similar cases - it's nothing like the size of the scientific literature. So, nothing came out of it and then he said to me, he thought he would just call this to my attention and I didn't know what the hell he was talking about. So, I went down to the Enoch Pratt Free Library which is a central library in Baltimore and went into the reference room and that's when I saw what a citation, what a, what Shepard's Citations was. I literally screamed out, 'Eureka!' Chauncey Leake had told me to focus my attention on review journals, right. I had done an analysis of review articles and tried to figure out why they were so interesting and useful to people and they clearly were. He was one of the editors, I think a band of reviewers at the time and in fact, he used to write a column, review of reviews. I could see that basically a review article was, speaking in theoretical terms, just a continuous string of indexing statements, so and so has reported this, so and so has reported that, and it made sense. At the end of the sentence was this citation, right. So, if you took the sentence, that's an indexing of that document and the more I thought about it I thought, 'Gee, this is an incredible idea', and trying to convert those indexing statements to what, I didn't know how to construct the index from reviews. You know, I had the idea to use index reviews but how would you use them, as indexing statements? I would compare them to what Chemical Abstracts did in the way of indexing. It's a different level. They were at a linguistic level, this is a, different contextual level. So, when I saw Shepard's then I realised that the, index had to be inverted. Instead of having the indexing terms you had to have the document as the focus and then

the statements that fall in. That was basically it.

Getting My Paper Published in Science

So, I wrote Mr Adair back, I thanked him for his idea but I couldn't do anything about it, at the time. But I said to him I was just made editor, later after I left the project, I think I wrote to him and I said, I have been made associate editor of... wouldn't you like to write an article about your idea? I said, why don't you, and I told him what to do. He wrote this very short article which did appear, and then I took that article which I cited in my paper and wrote the paper for Science, which I submitted to Science and the reason I did that was because I had met Bentley Glass at Johns Hopkins. He was at in the genetics department at Johns Hopkins when I was there and he'd come over to the project, he was always very interested in literature. He was on the board of Biological Abstracts, and that's how I got to know them and Miles Conrad and so on. So, I wrote this paper and I send it to Bentley Glass who was the associate, one of the editors of Science magazine. He did a fantastic job of editing my paper, grammatically and otherwise and eventually, it took a year, I wrote the paper while I was at library school, after I'd left the project, it went through and they published it. So, now the paper came out in '55 I believe it was July of '55.

Looking After My Son Stefan and Getting Married

By that time, I had come to Philadelphia and worked for Smith Kline and when I came here in October or November of '54, I was staying at the Broadwood Hotel. My sister called me up, I think it was December or January and said, you've got to come to New York to do something, take Stefan with you because he can't stay with me anymore. Her husband, had a mental breakdown, and so forth. So, to make a long story short, I was working for Ted Herdegen and Ted was an incredibly compassionate man. He, I said, I went to him and I said, 'Ted, I'm afraid I'm going to have to leave, because I can't, I've got to go up to New York and take care of my son and work things out'. He said, he, Ted Herdegen, took it upon himself to find me a small house where he lived in south of

Mullica Hill in Jersey. This little house was located at the edge of an asparagus farm patch, okay. Three-roomed house. 'You'll drive to work with me every day, my sister will go and get Stefan from school and when you get home, we'll be there'. That's how I got... Stefan came down, and Ted Herdegen did this for me, and his sister took care of Stefan, and we did that until the summer of '55. That summer - an amusing story - I had bought a 1955 Ford and up until that time I'd had serious problems with my leg and - remember when I had the sciatica, that started back, I had injured my leg while I was driving a defective car a long time ago, I won't get into that but, I had a hand-control car set up. Somebody had told me about paraplegics driving hand-controlled cars so, I was driving this car with hand controls and I took Stefan, I said, 'We're going to go on a vacation and the vacation will be a trip out to Colorado, okay, and, among other things, you get to see where your grandmother came from'. We went to visit Mr Adair in Colorado Springs. I gave him a copy of the article and so forth and drove back, and on the way back we stopped off in Chicago and we went to see Winnie who had been my girlfriend in New York. She knew Stefan as an infant and that was it, we decided, after we saw her, we visited her in Chicago then she came and we got married and she move to Woodbury and that's another chapter. But, by then the paper had been published in Science, thanks to Bentley Glass.

Early Days of Current Contents

Now, what was a consequence of that, of course, nothing, nobody, at that time I wasn't thinking of producing the SCI myself. I mean, Current Contents was still Contents in Advance, they were still producing that thing and eventually I turned it over to a couple of librarians I knew who worked in the pharmaceutical industry. Anne McCann and some other librarian, Marge Couraine, I think it was. They were, I had met these people at library school and I was just working as a consultant. But, as a consultant, I decided to do a management edition. I thought, Contents in Advance was okay for librarians but was it going to be a commercial success? The field of management somehow

struck me as being potentially interesting so, as a documentation consultant, I put together Current Contents of Management and when I announced it, just so happened that the librarian, Richard Gremling at Bell Laboratories, had wanted to do the same thing for his staff. So, he contacted me and he said look, 'Look, this publication you're doing is fine but I want the same thing for Bell Labs but I want you to tear the cover off and put our cover on it'. Okay, so, he wanted a customised edition. The title of that, by the way, was unbelievable. The original title of Current Contents Social Sciences was Management's Documentation Preview. Documentation, automation of documentation. So, a very funny story about that one, too, because somebody recently asked me about Mortimer Taube. Mortimer Taube was... do you know anything about corded indexing, the history of that? Oh, my God. Anyhow, Mortimer Taube started a company called Documentation Inc and he was angry as hell at me because I called my company Documentation Inc, Docu, capital M and eventually when I started this Current Contents not only do I get the Bell Labs contract but a couple of guys who were from Washington DC contacted me. One of them was a PR man, the other was the owner of a printing press in Washington called the Kaufman Press. He saw an opportunity to get a printing client. For some reason they'd made some money on some deal and they wanted a, it was, like, a tax loss thing. They said, come on, we're going to become your partner, we'll help you print it and so forth and promote it. So, I said, fine. We had a long discussion and Harry Brager was the name of this PR consultant. He said the title of that publication is absolutely absurd. It's, it's an impossible thing to pronounce. Why don't you call it something simple? So he says... what is it? So, I say, he says, 'Describe it to me', I said, 'Well, I thought it was the current contents of the different journals', he said, 'Well, that's it, that's the name of your magazine'. So, that's why we called it Current Contents. However, he says, 'Okay, now we're going to plan a direct mail campaign', and I, you know, I'd been promoting, knew a little bit about direct mail but, I said this is not a publication. I didn't

think it was a publication for, how can I say, line managers, if I can put it that way. It was an academic, for academics, and people, librarians and so forth. They insisted that we make a mailing of 5,000 pieces or whatever, spend this huge amount of money in order to demonstrate that this thing was a killer application or not, okay. They bought a mailing list of executives who earned at that time, let's say, \$100,000 or more. I said to them, 'You are absolutely crazy, there's no guy who's earning \$100,000, that...' there's a guy who earns \$1m a year, are you going to sit around and read Current Contents? They insisted that was the right way to do it, well it wouldn't be the guy himself it would be his assistant, it would be the librarian or whatever. They made this mailing and it was total bomb. I think we got three or five subscriptions. The first subscription came, the Allan Wood Steel Company, some vice president, okay. So, after that happened they came to me and they said, 'Well, that's it, this thing it was a great idea but it just isn't going to fly, okay'. So, they said, 'We're out of here, you can, it's all yours, do what you want with it'.

The Genesis of Current Contents

During that time I get a call from Charlotte Studer, who later became Charlotte Studer Mitchell the librarian of the Miles Laboratories, and she knew me from the medical library of the SLA, and she said to me, 'You know, Merck and a few other companies do contents page services for their staff. We would like to have something similar but I don't have the resources to do it. Why don't you do it?' So, she gave me this contract to send them Xerox copies of all the contents pages, 150 biomedical journals and she said, 'Do what you want, you can sell it to anybody else, just deliver this to me, I don't care what you do, okay'. So, that's what I began to do.

Was that the first one of the sciences that you did?

It was called Current Contents of Pharmacological, Chemical and Life Sciences. Then I started printing and shipping her whatever her copies were but then other, I decided that in order to market it I had to make them buy 25 copies, no more no less than 25 copies.

Then Warner Lambert and the others, the ones that didn't have... I had thought that when we started this thing it would be so much cheaper than doing it yourself that the people who were doing it would gladly buy in. But that was just the opposite of what turned out to be the case. Just like in government, these people had such an entrenched bureaucracy it took 10 years before Merck would sign up, and even the idea had been started and Merck and Cyanamid they didn't commit for the longest time afterwards. Because people had, jobs or they had commitments or whatever. You learn a lot about human behavior when you get into marketing. So, I was selling Current Contents in groups to the pharmaceutical field. Nothing else had occurred to me. One day, I get a call from a fellow named Jack Gershon-Cohen. He was the guy who invented the mammogram. He was a consultant to the pharmaceutical industry. He said, 'I have been visiting one of my pharmaceutical clients and I see this thing called Current Contents. Why don't you sell that to me and to other people like me?' So, I said to him, 'Well, we just hadn't thought about it that way. What do you think it's worth to you?' So, I said, 'Do you think it's worth \$2 week?' Cos I didn't have a set up. I said, 'If you send me a cheque, if you want it for \$100 a year, I'll mail it to you personally every week, okay'. That was when we got the idea to sell individual subscriptions, okay. Now, next thing we found out, I get a call from the director of the McArdle Memorial Laboratories at the University of Wisconsin, which is where Josh Lederburg incidentally was working at the time, although it's not connected. Because the funny story about Josh is that the year he won the Nobel Prize was when he wrote to me about the SCI. He said to me, 'I can't afford a subscription to Current Contents, it's too expensive, \$50'. But when McArdle got the subscription, group subscription for \$30, then he bought it. I get this call from McArdle and he says, 'I see you sell this to industry and it costs \$1,500 a year, you know, for 25 subs'. He said, 'Will you sell it to us?' I said, 'Why not? I'll be glad to sell it to you, what's the difference to me?' He said, 'Well, why don't you give us an academic discount?' So, I said - though I always tell people this is

how I did all my major marketing research, you know - 'Why don't you, why don't you tell me what do you think is an academic discount?' He says, 'Oh well about 50%'. So, for \$750, he got the 30 subscriptions and that was how it started. Then, eventually, I don't know how I met him, Marvin Schiller came to work for me and he had just gotten his degree at marketing at Penn State, I think, and knew a little bit about direct mail and we did a direct mail campaign. So, that's how we began the subscription service of Current Contents.

Index Chemicus and My Doctorate Degree

Well, I think the official year we used, as a launch year for CC life is, like '58, pretty sure, that's about it. We had done this customs service in '57.

By that time you had your own business I presume?

Well, it was still consulting, I was still consulting for Smith Kline all this time, a couple of days a week with all these other things. Cos they had offered me a job and I had turned it down and well that I did because a lot of the people had said, oh, if you go to Smith Kline and you'll have security for the rest of your life. A lot of those guys were out of a job within a few years when they had a reorganisation. Then, you know, all during that time I was doing consulting for them, there were also... I was advised... we went into chemical information systems too. Don't forget the Index Chemicus started in 1960. So, Current Contents wasn't that old when we did these other things it's just that it helped finance, the other things.

You were in Spring Garden Street, is that where you were?

We were there until, yeah, and we were doing Index Chemicus from Spring Garden Street, 1960 is when we launched it. It was a very risky proposition, you know. Then at the same time, all this was going on after I went to work for Smith Kline, and my friend Casimir came up to Philadelphia and was getting his PhD in linguistics at Penn. He introduced me to Zellig Harris who was the chairman of the Department of Structural Linguistics, and I wasn't all that conscious

of how radical that was. Noam Chomsky was just finishing his PhD that year that I met Harris. And so, Casimir came to work with Zellig Harris and eventually got his PhD there and he introduced me to Harris. I found out about some of the work they were doing and I said to him, 'You know, you could get a lot of grant money', because he was talking about doing some kind of scientific text analysis and so forth. I said, 'Let me introduce you', I introduced him to Helen Brownson, I knew she was interested in languages and, she was, she around when you were still there, was she still there when you went? It didn't take 2 or 3 months, he had a half a million dollar grant to study the application of transformational analysis of statistical analysis to the scientific indexing. I don't know what, the project never went anywhere, nothing came out of it. They wouldn't listen to me. It just didn't go their way. But, as a result of all that he and I and Zellig Harris made a deal that I would come to work for my doctorate at Penn. They would transfer some of my credits from my doctoral program at Columbia and would call me, cos I'd already been going for a doctorate there, then, I couldn't get my interdisciplinary committee to meet, no less to agree on anything and I had to take a certain number of credits at Penn in linguistics and research and so forth and I got my degree. But my topic there, which was agreed upon, would be my algorithm for translating nomenclature into molecular formulas, which was, of course, on my mind because of the Index Chemicus. The Index Chemicus was a radical thing because nobody believed that you could produce an index to organic chemical literature with a 1-week turnaround or a 1-month whatever, you know. What I found out was that if you look, if you went through the chemical literature, you could, every new compound was identified by the fact that they reported a key empirical formula. So, why are you calculating it? People at chemical abstracts insisted that they'd first draw the compound, then name it and then index it. So, I said, I went to Smith Kline as a consultant, I said, 'Listen I have to figure out a way to get you', the CA was 3 years behind the literature, now how can you, it's impossible to run a research

department doing chemical synthesis without actual authority. They were 3 years behind can you imagine the amount of duplication that was going on in those days? So, I said I figured out a way to get you a molecular formula index within weeks and very few people believed me. So, I did it and the original idea was that all we did was, I looked for the chemical formula and that's how we identified a new compound. We took that and we didn't even need the name we just had the formula and we gave them the reference. But, still we wanted, I got into this thing about taking the name and I figured out that what they were doing was, when you take a name, it's just basically a substitutive process, for the structural diagram. So, you don't need to do the reverse, you can take the diagram and create the name and the formula but you can do the reverse. That's where the algorithm came from, that we could take a name and calculate the molecular formula by computer.

Handing in My Dissertation

But what happened at Penn, Zellig Harris had agreed on the whole thing when I figured out it worked, I wrote it up in 10 pages. Now Zellig Harris decided to go on a sabbatical, the year I'm supposed to get my degree. He says to me just before he leaves, 'There's another professor by the name of Hiz, supposed to be an expert on logic or something like that, who'll supervise your graduate work while I'm away'. So, I go to see this guy and I bring him my finished dissertation and he looks at it and he says, 'Are you serious?' I said, 'Well, yeah'. Now I'd proved... I said to him, 'We agreed, I proved it can be done, right, and that's what Harris agreed to'. He says, 'Well, you can't get a doctorate with 10 pages'. Then what happened was, I then decided that in order to satisfy his... I wrote this transformation on grammar of Geneva nomenclature, okay, which was, like, I don't know, 100 pages or something like that. I did a Zellig Harris type analysis of Geneva nomenclature roles, all right, I write this up. He had made up his mind that he was not going to make a decision about my doctorate until Harris got back which would mean I would be delayed for another year. But in the meantime, we

ended up playing this game. So, I would bring the dissertation in, and what I did was I put the full text of my dissertation on a Flexowriter. You remember what a Flexowriter is? An automatic typewriter with a tape. I run it through the tape, run the thing off. Sylvia Shapiro was working for me then and, of course, we used it for doing direct mail. I mean, those days people... we used to have a process called Hoovenized mail and personalised pictures. You type, it looks like hand typed letter but it's really basically a tape that's run off and then you just change the address. So, I put the dissertation on the tape, I run it off, I take it through to see Professor Hiz, he reads it through and he, didn't understand what the hell was going on, he didn't understand. He would look at it and he would, well, make a change here, make a change there and he'd go through it. Made about half a dozen changes Well, he figured that having made all these changes, I wouldn't be back for another week with another, you had to have a completely perfect typed manuscript. So, I get back the next day put the tape through and we got another. He was absolutely going crazy trying to keep up with, trying to make changes. So, finally I knew he couldn't make up his mind. I called Harris, I found out where Zellig Harris was, he was in Rome, Italy and I called him up at his hotel. And basically I said to him, 'Zellig, if you'd...' we were on a first name basis, this is the guy I helped him get a half a million-dollar grant. I said, 'Zellig, either you send a cable or call the chairman of the department to sign my diploma or whatever it is you have to do, sign the papers for my degree in June, or I'm going to come over there and I'm going to drag you out of bed'. Within 2 days there was a cable sent to the chairman, Hiz wasn't chairman, but the chairman was a comparative linguistics professor [Professor Hoeningwald]. He lives in Swarthmore all the time. That was it, but it was quite an experience using technology to beat him at that game. At one point he says, he makes this brilliant statement to me, he says, 'You know, my real advisor was the chairman of the chemistry department at Penn, Alan Day, he was the one that really got me through cos he understood what I was doing, you

know'. Hiz says to me, 'I studied Polish chemistry'. What's that supposed to mean, what's the difference, like, Polish notations supposed to be something different? But, that was how I got my doctorate while I was developing ISI.

A proposal for the Genetic Citation Index

We had already been working on the Genetics Citation Index at the same time I was also doing my doctorate and the Index Chemicus. It's amazing when you think back how many different things we were doing, back in those days.

After your '55 paper, what was the next big impetus?

Well, I think one of the key events was the fact that Dr Lederberg wrote to us, he wrote this classic letter in which he said, and I paraphrase, for lack of an access to a science citation index, I don't know what happened to your paper of 1955 in Science. As a result of that, I wrote him this long letter saying how difficult it had been for us to find any support for the idea amongst the granting agencies, for a variety of reasons. He thought that... and frankly I had never really heard much about the NIH in those days although I guess I must have known a little bit and it wasn't till a little later, I discovered such a thing as... that NIH grantees had funds that they could buy subscriptions to Current Contents if they thought it was considered relevant to their grant. That was a major marketing coup when we were able to write on a brochure, these, you can charge this to your grant, and it made a big difference. But anyhow, Dr Lederberg wrote to us about '59, I think it was in the year, so, after he got the Nobel Prize. And we met, he came down to Philadelphia and we met here and he and I had a long talk. We were walking, we walked, he was staying at the hotel called the Penn or something like that, at 40th and Chestnut or, I think it doesn't exist anymore. But we walked around the block several times and he said, why don't I write up a grant proposal to the NIH and that would be submitted to the genetics study section, and that was when we created this proposal to do the Genetics Citation Index.

The Indexing Project

We produced three experimental indexes, we wanted to see the effect of chronological

depth. So, we had one index that was 15 years of one journal, I think the American Journal of Human Genetics or something, like that, or a few journals. We had another index that was 5 years of genetics-related journals and then we had 1 year of the science citation index, which covered 600 journals. We then created a profile that would match that big file and pull out of it those things that were deemed to be relevant to the genetics portion of it. Not an easy thing to do, in fact, it was the project, in a sense, expressed indirectly the incredible changes that were taking place in molecular biology which is a very multi-disciplinary topic and some of the critical literature was appearing on a double helix in those days and related topics were in physics journals, Reviews of Modern Physics and so on. So, the multi-disciplinary nature of modern molecular biology which was being catered to, so to speak, by our taking the multi-disciplinary approach. But that was the whole problem all along, as I had said that in a 1958 paper that I presented at the International Conference of Scientific Information, you needed a unified index, not... separate disciplinary indexes. Now, in a practical sense, Chemical Abstracts may have boasted that it was a chemical abstracting service but the fact is that they stretched out their interpretation of chemistry into so many different parts of the literature that you couldn't, you could argue that in a sense they were multi-disciplinary. They never were complete in any different field other than chemistry. The way in which they did their abstracting never ensured that they were absolutely complete even for the best chemical journals because they were individual abstracts that were assigned to people, not journals, it wasn't a journal-by-journal approach. Anyhow, the indexing project produced a very interesting product and we... part of the grant included the distributing. I believe it was 1,000 copies of the Genetics Citation Index which we have, I still have one or two copies of it on a shelf. We then proposed to NSF that they would publish it, with sole house support publication of the SCI. They wouldn't hear of it and I had been trying all along to get other groups to get involved and we then decided that we would, and NSF agreed to

take the 1 year index that we had produced and only one copy of course and print it. We offered it for sale and when we did sell that, it came out to be a five-volume set. Five-volume and we sold it for \$500, which in those days seemed like an outrageous price to people.

Financial Problems for the SCI

We took the chance that we could finance the project ourselves and it was a, decision that almost bankrupted us because the response to subscriptions selling was a lot slower than we ever anticipated. People today who take the Science Citation Index (SCI) for granted, forget how conservative a lot of people were. Not just librarians, but even scientists, who many of whom, to this day, have never used the SCI. I mean, it is not a necessarily an obvious thing for people to do, even now and if you don't teach them to do citation searching, they don't learn it by default or somehow. Even if they use Google, I imagine you would think that people would understand, I don't even think some of them understand how it's done. So, anyhow, we got to the point where we thought we were going to be under-financed and that was when somebody on our board suggested that we bring in some outside investors. Now, the company had actually tried to go public a couple of times, at least one time before that. Each time it happened it was like Wall Street; it's like trying to go for an IPO last week when the market hit the bottom. We did that on two different occasions and it just was incredibly bad timing so, we never worried about going the public route. We got this group of investors and I don't remember who lined them up but they were... what happened was that somebody at the, one of the investment banking firms in New York had a bunch of cronies who thought this was going to be a good investment. Each bought about \$50,000 worth of what they called convertible debentures and invested half a million dollars I think they got about 20% of the company for that. It wasn't 6 months or a year that we paid off the debt. A debenture is just basically a loan, which is secured, and the investors get the stock free. So, you pay them off the money they loaned you and

then they get the stock left over as a bonus. So, that's how we got all these outside investors involved. Well, one of the events that occurred, after we got the investment, was a year or so later, four of my vice presidents decided that I was not interested in making money for the company. That I was only interested in academic ideas and that I didn't behave in such a way so as to guarantee that the company would become profitable. Especially with regard to chemical, the chemical indexing product. So, one of the first things they tried to do was try to change the Index Chemicus into a glorified version of Current Contents and that bombed. Then eventually, they decided that the only way that the company could succeed was if they took over the management of the company and eventually kick me out. So, I got separated from, the four people involved, I don't think I need to name them now, but they were all officers of the company and I told them to take a flying leap and they all left with the exception of one who stayed on ostensibly, but only to hide the fact that he was planning to join them after his stock options or his ownership in the stock, was final. He had a piece of the company but he wouldn't get it for another year or so, he waited and then he announced that he was leaving. Well, they did leave and they formed this company called Information Company of America and they started some service, I forget what it was, and I think we filed a lawsuit against them for whatever, I don't know what the grounds was, for unfair competition or whatever. Current Contents knock off I think? They started something similar, but different and they eventually went under. It didn't take long, and I convinced Irv Sher to come back, and he had regretted that he had done that and went on, stayed on and did a lot of great things for the company afterward.

Problems for Starting The Scientist

I had the idea that, the function, people forget that the two functions are pretty much, we stress so-called current awareness functions that journals and abstracting and other things play which today you can get so easily by just getting so many different things free on the web you find out, you could put

together a contents based service yourself, you could put together getting free downloads of the contents of individual journals or whatever. But in those days it was, you had to go through individual journals, you had to do those by yourself. There was also this business of this amount of money that was spent in publishing papers. So, the idea I had was, why not, the expensive part of all this it seemed to me was the way in which these things were produced, the printing and other costs. So, I had the idea to produce what I would call them, like The New York Times. We had, the idea was a tabloid format that would be a daily newspaper of science in which people could publish their results, just the way they do in Nature and things like that. We did calculations to show that we could produce a paper approximately the size of The New York Times that would accommodate 25,000 articles a year. Well, that, that's more than critical mass for the most important part of science. I mean, how many articles a year are there that really report something all that new? So, that was the essence of my idea for the newspaper of science. We produced mark-ups for it, something called The Daily Scientist and we still have samples of that around here somewhere. But, clearly that, NSF wouldn't go for it. I could, in hindsight I can understand why they could never, just like they wouldn't go for open access even if they could because all the societies would be opposed to anything like that. So, and how, if you started something, unless you were extremely well financed, how would you get something like that off the ground? The other factors were, would people be willing to publish in a thing like that, if they didn't have the, kind of, peer review they expect and the sociological issue of why do people publish where they publish. Now, Josh Lederberg also had ideas for a system he called Citel, that's available in his archives. So, the idea of communicating what was new in science in efficient ways has been around for a long time. A lot of different people had a lot of different ideas. I know McGraw and Hill had talked about doing a newspaper of science and it was only after, it was in 1986 that we launched The Scientist, I did that, was probably one of my big mistakes. What

happened was a very sad thing that occurred here in ISI was we had made an agreement to launch The Scientist with great encouragement from a guy named Nicholas Wade [sic - should be Valery] who had been one of the editors of the New Scientist in England and he was working for The Economist. You, and you were here at the time and The Economist wanted it to be, it would be a joint venture with The Economist and they knew all about publishing a publication of that kind. What happened was, this incredible thing where the guy who was the disgruntled former vice-president of Data Processing, that guy Campbell, he planted some software around the company and then he called the FBI. The FBI came in and closed us down that day. Do you remember that day?

Absolutely.

As a result of that, the, The Economist backed out. That was the night that we had signed the agreement. I didn't know that. Then, when they did back out I said to him, 'Well, look, they backed out are you still willing to go ahead with your plan?' he was going to move to Washington be the editor on, yeah, go ahead. So, we ridiculously agreed without any formal written agreement and rented a space in Washington and opened up the office and hired Tammy Powledge and all the others and there we were, and he'd backed out. Said he didn't want to leave The Economist that they were not going to support the project, they had told him they weren't. So, I had to make that decision, a critical decision which every financial advisor I had told me not to do it, but I went ahead and I supported it. Now, later when, when we sold the company to... JPT, they lied to me when they said that they intended to help support, get The Scientist going, they would give it a real shot. They knew in advance, what they were going to do. It was not even 6 months that they said, we're closing it down, you can either close it altogether or you can have it... take it for \$1, and that's how I come in to it. I supported it all those years until Vitek became my partner.

Ups and Downs of the Business

People think it was always, easy to get where we were but it's a lot of ups and downs in

this company. We had a lot of successes, we had a lot of failures too, nothing ventured, nothing gained. We've, at some point after a while I just felt being a manager was not what I was cut out to be and the company got so big there were personnel problems to deal with and very hard. So, now it's always been very intellectually stimulating to be, working with scientists and providing a good service and I think the people in the company enjoyed working here because we, tried to have a pretty open, kind of, collegial atmosphere. I just think we always managed to get people who worked hard without having to be watched day and night and I think that's generally paid off. I mean, it's not the typical corporate way of doing things, but I think we've had a very good family life here.

Impact of Information Systems on Scientific Progress.

What's the value, the impact of scientific information systems on the progress of science. How do you demonstrate that? I said that it's similar to the difficulty of measuring the economic impact of basic research. It's a truism, everybody believes in it, but proving is something else, you know. When you're a chemist, you're trained to use literature, right? They take it for granted that your time is well spent if you spend time in the library. Wasn't always true that way, you know. There were times in the past, especially in Germany and other countries, a scientist could, had to have permission to go into a library. He couldn't be wasting time out of a lab; they wanted him to work in the lab. As though every minute of the day spent in the lab could be productive. I guess you could watch the bugs multiply faster. But, I can't... anyhow, it turns out that there are people apparently now who are trying to, in terms of science policy analysis, to demonstrate the value of information systems. I mean, we, take it for granted that the marketplace determines that. If it wasn't so important to industry and academia why would they be spending all these billions of dollars in paying for information systems, there seems to be some value there.

It was important to scientists' careers to have the citation index. I mean...

Well, it's important now because using citation, frequencies and measurements have begun to be incorporated into the, evaluation process, the tenure process, the research assessment exercises and so forth. But it wasn't always that way. I mean, in the past they just relied on subjective opinions and I keep getting confronted with this idea that I'm responsible for a Frankenstein, that we created this thing and there are a lot of people who hate us for it.

You're being imitated, you're being imitated by all these other players.

Yeah, but I mean, still you get all these, what's the word, critical remarks and dig out the latest versions of... editorials by people who are so upset by the fact that numbers are being used to evaluate people, as though they were never used before. As though peer review was a far better reliable, you know, subjective views are more reliable, maybe in some cases they are better. Somebody has an intuition about somebody, somebody who has got high numbers, but you think he's a jerk and other people who have low numbers, you think they're brilliant. I mean, that's a personal opinion. But what's the collective opinion? You did that study on the collagen thing which was a classic which shows that in most fields anyhow you probably find that 95% of the time, the people that turn out to be the most respected are the ones with the highest citation counts or certainly among the top five or ten, right?

They'll be perceived as the leaders in the field.

Well, all these snide remarks that people make about citation analysis, I always tell them at the meetings, I never encountered a Nobel class scientist who hadn't published a citation classic. You look up their records and in fact, more than one, several, frequently. If they haven't there's always a very good explanation for it, that in fact, you could then use the citation history to trace the reason why there was no citation recognition, it was a long-term delayed recognition. So, on the one hand people are always citing examples of delayed recognition of which there are very few, by the way. But Wolfgang and I did a paper in Scientometrics a couple of years ago, we did that study of how rarely you can find them yet people have this impression

that it's a widespread phenomena and therefore you can't use citation analysis frequently. If it was very common you wouldn't want to rely on it but it's really such a rare thing.

Impact Factors

Irv Sher and I when we did the genetics project we created the initial... we had the journal list of really important biomedical journals from Current Contents. We couldn't have published Current Contents. At that point it was, let's say, 4 or 5 years old. Medical librarians and all those people said these are the 100 most important journals that we were covering, all along. Now, would there have been a really significant journal that they would have said, don't put it in there. So, it's kind of almost silly that they would think was such a huge exercise to get the most important journal. We all knew what the journals were, whether we were called subjective or not. Now Estelle Brodman who was a medical librarian and I loved her as a person and as a historian, had this, she didn't like the numbers game either. She thought that, she did a paper on choosing physiology journals and she objected to the fact that our ranking gave too much weight to certain types of journals, I guess review journals because of their inordinate citation. Well, they're still important. Beyond that, anybody knew what the leading hundred... I guess once you get past the first few hundred titles then you can get into these squabbles about what is or isn't the next journal to add. So, Irv and I said, what are we going to use as the criteria. We know what we start with. Then we did this journal citation analysis, the primitive JCR and we called it, was the Journal Citation Index when we first did it and we did it because we thought it would better to maybe publish the citation index, not sorted by author but sorted by journal. Which, in a sense, was the way Shepard's Citations was organised. They put more emphasis on the vehicle rather than on the individual, the individual case or whatever. So, you'd have it by journal. Well you could of had that. So, we did a journal sort in order to see what it would look like. There were a number of advantages to the journal sort if we had done

it. We could have eliminated... at that point, we recognised that if an author had his name spelt differently, when you did a journal type sort, it would be brought together regardless of the spelling of the cited author. But, we had to say well, most people were used to citing author journals, that's why we went that way. Then it was just a trying to make a decision between these small and big journals so, this was a way to normalise size. Now, we did think about the concept of impact, even in 1955, we mentioned that in our paper but it wasn't the formal impact factor. Today when people talk about impact factor, they don't always mean journals, they're talking about an author's impact, which is a very confusing thing to do.

Or institutional impact or country impact, all sorts of things.

No matter how you slice it, there's always going to be somebody who's got an objection. All along there was always the criticism that it was mostly English that we're biased the game in favor of the English language. Then, whenever you did a study to disprove that, they'd find some other objection. When it wasn't, you should be comparing European journals and now the Germans object when we evaluate their journals. Then you tell them, if you were a German scientist, where do you think you're going to publish a really hot paper? In the German journals or in the international journals? It's the authors that really determine the game, not us. So we are selecting where the action is so to speak. In the beginning, we were emphasizing the fast moving fields, molecular biology, biochemistry and so forth and so there was somewhat that bias. So, what we maybe didn't do from the early days is quickly enough pick up on the slower moving fields. I mean to this day, I question whether we have too many, let's say, taxonomy journals missing from the ISI coverage but there may be some more fields like geography, but how you would pick up that stuff I don't know, there's so much low impact material that after a certain point, you can't even discriminate. You know what's the difference between something with an impact of point one or point two? Yet people will make a big thing out of that.

Historiographs

I believe that Bernal himself did some kind of a historiograph, he didn't call it that, they didn't create it out of citations, they just created a genealogical map of the field. Now, Gordon [Allen] when he was on the committee had this idea of tracing the citation, the history of nucleic acids, that's what he did at that time. I believe that those kinds of genealogical maps existed before and I believe Bernal did one and I couldn't remember where I saw it, you know it's something you can't ask an index to find, how the hell would you look for it? They don't index graphs as they might, but even if they were indexing how would you look for it? You might have to look through all his papers. So, it's, sort of, like saying, find me a paper that contains a structural diagram. Other than that, I don't know. To me, when Irv and I were talking about this back in '64, when we did that DNA thing my interest in this whole thing, was in a sense that the citation indexes would be incredibly valuable to a historian of science. I hadn't thought about going into that field. When I was at Hopkins I wanted to take my degree in the history of science and they had a major out there. They also had the Institute of History of Medicine right there at the Welch Library, top floor so it was a topic that I had been interested in. Then, I don't know what the evolution of the project... we got to talking to Harold Wooster at the office of air force research directorate and he, liked that idea. I said let's investigate what would happen if you used citations to track the history of a topic. That's how I did the DNA thing. Now I realised, when I was down at Oak Ridge last week and I showed that... historiograph from Mendel to Nirenberg is still the most interesting picture we've ever done. But, you really have got to go into that report in detail to appreciate what it's got there because Irv with all those overlays... I don't even know how you would produce it by a computer if you had to do it, if you took those overlays, photograph them separately on a computer, how would you merge them. It would be quite an interesting thing to try to do and I don't know what he thought was so interesting about well discovering which overlap the... weakened the links and the

strong links. It was just a kind of an intellectual exercise. Then the ability to generate that map algorithmically, it turns out to be not so easy. You take, cause you got the problem of how do you filter out the noise that develops in those things? We got a demonstration of the spallation neutron detector or producer, at Oak Ridge? It's, like, they spent about \$2 billion to build this thing and a guy by the name of Ian Anderson gave us a tour, personal tour of this thing and it was unbelievable how he explained that. But they have a map on the wall, if you could do your mapping exercises on there, all the overlapping and lines, you could do a map of 1,000 papers up there, would be beautiful. It's what Derek Price always wanted? That's what you need, a real command and control map.

Usefulness of Citations for Historians

When I started to write the paper I could see, I think I made a statement, how valuable it would be to historians to have this, kind of, information to be able to trace these developments. Today, now even as I work with the HistCite program ... you essentially create a citation index of the field you're looking at, you get this incredible time line. This is great for you. You've got a record of everything that historians or whoever has written on the subject have cited and that gives you an immediate rough... quick and dirty time line on the field. Now, it doesn't necessarily mean that you have to assign a greater importance to the more cited stuff because what you're looking for is the chronological development in the field. What Irv and I found in the Asimov thing were certain papers that proved to be what we call important links that Isaac Asimov had overlooked, that he agreed that he had overlooked, couple of papers, not very many. So, I think that's one of the weaknesses right now in the early stages of SciMate that unless you apply subject criteria, you're going to always over emphasise what gets highly cited versus the appropriate link. I saw that when I was trying to show, when I did this paper for the Bernal Symposium in Dublin. We showed linkages between JD Bernal and Watson and Crick and Rosalind Franklin and other people, but there weren't as many links

as I expected to find. But it's just, it turns out to be a very interesting tool to be able to take two people's personal bibliography and just run their two HistCite files together. You take my records and your record, you run the two together, who do we cite in common, how are they linked?

You, pointed out that some people have neglected to cite people that they should have, like Watson.

Yeah, Watson admitted to that, eventually. Even Einstein forgot, he just didn't formally cite them. Who knows what the reasons were, but he hasn't got very many references in his papers. They're probably implicit, you know, we used to call them the implicit references. So, if you were going to trace the genealogical tree of the field, you'd have to go in and add those, we call them 'pseudo citations'.

He claimed that he didn't know the literature that well and he would have cited them if he had known about it.

He didn't know the literature, Einstein claimed that?

That's what he said, back in 1905, he didn't know.

Well, Medawar would have said he was smart to have avoided it. He didn't know it but he'd learned it, somehow, right? He got the ideas.

JD Bernal, Politics, and Science

If Bernal had... by the way, did you get a chance to look at the biography of JD Bernal? I had the book. It's really wonderful. I met Andrew Brown in Dublin and he's a wonderful guy. But, if Bernal hadn't been so pro-Stalin, he would have probably won a Nobel Prize, I'm pretty sure of that. Do you know that he was an advisor to Churchill? And Churchill knew he was a communist, it's an incredible thing, I guess it's how people felt about Oppenheimer. General Groves was not stupid, he said I don't care if he's a communist or not, he's a great physicist. That's the way it was, I guess. I suppose that Einstein was probably accused of being, if not a communist, a Marxist, I don't know. As I mentioned, Bernal was also a Sephardic name. So, I tried to find out if Derek [Price] had ever had any personal contact with Bernal and it turns out

he was not he went to the University of London, but he didn't get his degree under Bernal. He just may have known about him. He knew about him. But, he didn't actually have any dealings with him when he was younger. I don't know when he first encountered the Social Function of Science but I encountered it early on because my uncle had given me a copy of that book and then when I went to the University of Colorado when we were talking about science and the war and everything, like that, Bernal's name came up. People knew about him then but only from that book because nobody was hearing about him during the war, he was working probably secretly. I don't know how open he was during the time of war, I guess he was just too busy.

Scientometrics, JD Bernal and Joshua Lederberg

In my talk I found I was able to trace down the evolution of scientometrics, the business of science of science. Derek, in his book, used that term, 'the science of science'... 'prologue to a science of science', which is the term that Bernal used. By the time, another 10 years or so had gone by scientometrics got into it because of Nalimov. Not clear to me when that precisely occurred but the journal came in and Tibor Braun was more susceptible to, Russian influence because of the Hungarian location. They knew about scientometrics but when did you first hear that term, can you remember?

I guess it was 1970, I don't know.

I don't remember when I first saw his, book the CIA translated, it was free, it was available but it wasn't talked about much, it was a government document.

I wonder if science of science was somehow a Marxist idea or a Soviet idea?

Well I'm pretty sure that's the way it was considered, they discussed it. His concept of... they gave him credit for, and I said about Alvin Weinberg who became his greatest exponent, big science is what he called it. Derek was talking about little science. It was Weinberg that made that distinction being at a place, like Oak Ridge, that's big science. He talks about science the way that Bernal

did so I said, it was implicitly a Bernalian, Weinberg, criteria for scientific choice. He wrote two commentaries for us in Citation Classics?

I remember something. Yeah... I never connected the criteria for scientific choice with Bernal, but now that you mention it.

Yeah, he doesn't say it. I don't think he ever, to my knowledge, cited it. I don't think he did but I'm sure he must have known about it. So, fascinating how all these things interact. Josh Lederberg, he's very familiar with his work. Did you know that he wrote this paper in the East German academy, he was invited to write a commentary about Bernal and the 50th anniversary of The Social Function of Science? When I went back and looked it up it, turns out Josh wrote a paper in that called The Social Function of the Scientist, based on using that 50th anniversary. Then, that paper was republished in an American book. I don't know who the publisher. I couldn't figure out what, I saw the paper listed in his bibliography and then I realised we had, Meher found the book in my library, on the shelf, it's sitting on my desk now, it's a collection of papers. It's called Science and Society, something, like that. If you've never seen it, Josh's paper is in there and it has to do with the whole concept of what he calls, truth telling, the ethics of science. So, Josh has some interesting comments. He said basically on the Marxist influence on his work he was blindsided by Stalin and he wouldn't actually criticise Lysenko - that was the biggest mistake he ever made. I think most people feel that was clearly an emotional bias although he had shaky feelings about it.

Were you at all... did you know about Vannevar Bush?

I remember reading that. I remember seeing an article by him in the Atlantic Monthly, but that occurred much earlier. I think his original, I'm trying to remember, there was something he wrote in the early '50s that I'd read but, I didn't know about Vannevar Bush before that. I heard about him through the rapid selector work and all that, you know, later. Essentially, up to 1951, till I went to that project, the only thing I ever knew about, I didn't, I didn't relate Bernal to this at all. I mean, to documentation. When I

was reading it, was the social... science and the state so to speak, and that's what Weinberg was talking about, the government is giving great support to science. Before that, science was private, the upper crust, elite and industrial, it wasn't government. The war is what really produced the connection between government and science.

Working for Smith, Kline & French

It was Ted Herdegen, who later on became one of my closest friends, and to whom the first issue of the *Index Chemicus* is dedicated. I guess online it wouldn't be possible for people to see those things quite as easily as when they were in print. Ted Herdegen had come to visit me, and only very briefly from having visited the Welch Medical Imaging Project, and when Smith, Kline & French was a relatively small local pharmaceutical company, and the president, the owner, the leading executives at the time were typical mainline Wasps and Smith, Kline & French was basically a Waspy company, although I later found out that they had one other guy by the name of Greenberg who stayed there quite a while, but he was a rarity. Anyhow, they started to become a larger company. They were the ones that sold Dexedrine and products like that. I think at that time Dexedrine was used for people with colds, it was like a nasal spray. So, they were very big over the counter market. I don't know how many prescription drugs they sold. Ted Herdegen had worked as a chemist for them many years before in the lab that apparently was located downtown, in Philadelphia somewhere. But when I came to Philadelphia, when he... when I came to work for them, they had a brand new building that is still there, 15 Spring Gardens. It was, you know, like a four or five storey building, and they had tableting and manufacturing in their offices and so forth, and in the labs. So they had a man by the name of Wes Scull, the director of chemical research, one of the medical research directors, had gone to Europe to find potentially new products, and they found this drug which was used for the treatment of nausea in pregnant women. It was I believe, a CNS depressant. But somehow when they did some testing and they found

out it was a very effective antidepressant drug, which was peddled under the name of thorazine. Thorazine is even used to this day for a variety of conditions, I think it may be even like a mild sedative, relaxant, CNS, relaxant or whatever. The year that I came here, 1954, the drug had made the cover of Time magazine, and this company suddenly became a superstar pharmaceutical company. I didn't have any stock or anything, and I don't even know if it was on the, stock market or not. This drug made them a fortune. Apparently what had accumulated was a large number of kinds of informal, formal case reports on the drug. What they wanted, Ted and the head of the department at Smith Kline was a fellow named Henry Longnecker, who was from the mainline family, as I recall, and they realised that they needed to have a more efficient way of processing all of the reports they were getting on the use of the drug. So, Ted had known that I was considered as the punch card guru in those days, and he had called me when I was still at library school. I think I said yesterday that I was planning to go to Georgetown with Casimir and Léon Dostert, but that fizzled, and so he hired me and I came down here. What we did was we set up a pretty straightforward coding system where each position on the card represented some particular side effect. What they wanted to do, basically, was to do statistical analyses to see how often certain side effects occurred. There were things that apparently... you couldn't draw those conclusions from just working with the drugs, some doctors might have noticed it. But it turned out that there were several significantly frequent side effects, like dermatitis and so forth. So, that was one of the reasons they brought me down, as somebody to set up this punch card system, so we had these coding sheets. What we did was first of all we got all these individual letters or documents that were given to the detail men who sold the drug for the company, the doctors gave them these reports, and then furthermore there was a lot of literature that had been published on the drug, even before it was marketed for the purposes they were selling it, to treat nausea and vomiting. So we went into the literature, and whenever there was a case

report in the published article we would treat that as though the doctor had submitted it, and that was how we used the published literature to build up sort of like an early evidence-based medicine, to analyse the effects. Also they used it to determine whether there was anything else they could do to market the drug for other users. That's one of the big payoffs for these companies, that they market for one thing and then they find out it's good for something else, so they make more in the by-product use than they do on the original. So, that's why I came to Smith Kline While I was there of course they got me involved in other things.

People at Smith, Kline & French and Paying Back Taxes

They had a group which included a fellow by the name of Leo Stevens. I mention Leo Stevens because he was an Austrian who had been in the ski troops, in the same 10th Mountain Division that I was in, but he was one of the original people who were brought over as ski experts. Also he was a refugee from the Nazis, I guess. The reason I remember Leo so well is because later, after he retired from Smith Kline, he came to work for ISI, and he and I went on lecture tours where he would take over answering, giving lectures in German and answering questions in German and so forth. He was in the translation part of the literature section of Smith Kline, so his knowledge of languages was very helpful. He was a very, nice and funny man, who was extremely Germanic in his ways. I mean he was so disciplined. When we went traveling he had a routine that he followed to the letter: as soon as he hit the hotel room he was on the phone telling the laundry or the housekeeper to come up and get his laundry and that, and we would have that taken care of right way and then he'd take care of other things. So, anyhow, he was in the literature section but also was a doctor in organic chemistry, I think. They had a group that was doing structure function analysis, that were hoping to... it was really early on, and there was a fellow by the name of, who had been an academic, by the name of Heger, George Heger, doing that kind of work. That's how I sort of got into coding of compounds. We had pretty

primitive coding systems then. We weren't using any kind of line notation or anything like that, or not even attempting to search structural diagrams or... Molecular formulas? There's a term for the basic coding structure. Anyhow they also had in that group a natural products division. I think they talk about going out and looking for plants, a fellow by the name of Bob Raffauf, I remember. He later on went to work as a professor at New England University, was in charge of going out throughout the world, looking for exotic plants that could be turned into potential natural products. They called it the natural products section of the literature department. So, there were a lot of interesting things going on. Also when I first came there I was living alone, and I'll never forget when I stayed at the Broadwood Hotel, and when I filed my income tax return I claimed my hotel expenses, as a deduction. Several years later, while I was on a trip to Paris, I get a cable or telegram from my wife telling me the IRS says you owe them \$500, something like that. It turns out that the tax accountant, the CPA in New Jersey, had filed my return and it turned out he illegally claimed my hotel expenses as a legitimate expense. I said well, I lived in New York. The IRS said you are spending 4 nights a week in Philadelphia, therefore that is your residence, and whether you keep your apartment in New York is none of our business. So, I had to pay them all these back taxes plus penalties. I found out that you can't use your accountant's stupidity as an excuse. It was pretty traumatic at the time, because that's when I first encountered having to borrow money to pay off debts.

My Son Stefan

I was there for a couple of months when I got this call from my sister that Stefan... I had to do something about bringing Stefan down. Ted had set me up in near Mullica Hill, and that's how Stefan came down and grew up as a country boy. That impacted his life forever. He hung out with all these farmers and mechanics and so when we moved and I got married to Winnie, we moved over to Swarthmore in order to get, have a much more academic preparation because we wanted the kids to go to college.

It was a disastrous decision in Stefan's case, because he was always a very mischievous kid, and he had been hanging out with all these farmers and mechanics, and he just liked to fool around and went out hunting. We had a stream right near the house where we had this big log cabin that we lived in, and out in the Thorofare to New Jersey, and when we moved to Swarthmore, Pennsylvania, he never could adjust to the academic. You know, these kids are all 90% going to college in Swarthmore, that is what you can imagine, and where he had gone to school down in Mullica Hill, it was probably just the opposite, 10% went to college. So, by the time he was about 14, I think it was, he'd had it and he ran away from home and went to live with his aunt in California, that's his mother's sister, and he grew... spent the next several years living with her. She was married to a black man, so he was living in a, black neighborhood and getting exposed to the civil rights movement and all that sort of thing. She was very much involved in that sort of thing. He then participated in... I don't know if you've ever heard of a CORE? They had a demonstration at the Cadillac car agency and I think they were protesting at the fact that they didn't hire blacks, or whatever it was, and there were dozens of them got arrested. They were doing a sit-in or something like that. Stefan was brought before a judge and they were given a choice, you can either join the army or you can go to jail. So, that's how he got in the army. From there within 6 months he was in Vietnam. He had signed up for a regular 3-year hitch. I think I mentioned before that when he got wounded in Vietnam ... I couldn't get there for some reason, I wish I could remember the reason why, but Ralph Shaw went to visit him. Now, what that does remind me, because I just came visiting Stefan in Florida, when... I don't know if you remember Mel Weinstock... Mel and I, lived together for a while and we shared an apartment at Society Hill Towers. Mel was a guy that never went to sleep. I mean, he claimed he could deliver on 4 hours sleep. Of course, when you were working with him, you would notice that he would doze off in the middle of a conversation. He used to hang out at the Middle East restaurant because he knew all

these women who were belly-dancers and so forth. It turns out that most of the women who were doing these exotic dances were... housewives with kids who would support with an extra job. When Mel... Mel was a very funny guy, great musician, he played a beautiful French horn, he played in the Haddonfield Symphony Orchestra. So I went over there a few times. One time I was in the Middle East restaurant, and the guy who owned it was this fellow Tayoun. I don't know if you remember but later on he was sent to jail for getting involved in some kind of maybe murder rap, but definitely corruption of some kind. But while I was there Mel introduced me and he said his son is in Vietnam, and somehow he said he likes baklava, which Stefan had had, I think, at some point, I had gotten him some baklava. Tayoun says, 'Oh'. A couple of months later I get a letter from Stefan, I probably still have it, Tayoun had sent him in the mail a huge baklava in a round tin, and he said when he received it they hadn't eaten for a couple of days, they were almost literally starving, but the mail was delivered even though they weren't getting any food, and they ate that whole baklava in one night, him and his buddies. You can imagine the amount of calories that there was. Stefan still tells that story gleefully. But Vietnam was not a great experience for any of them, I guess, and when he came back... he was wounded twice, and then finally he was transferred, when he finished in Vietnam he was transferred to Germany, and then he was in the tank corps, I think, in Germany. Stefan knew some German, I sent him to stay with a friend of mine that I knew in Munich for the summer and he learnt to speak German. To this day he still speaks to people in German. Also when he meets people who speak Yiddish he uses German to speak Yiddish. That was a by-product of moving to Swarthmore.

My Son Josh

My son, Josh, who I just visited in Tampa, was born in 1958 or '59, and he played football for Swarthmore. His mother was about 5 foot 11, so Josh is very tall, although my daughters were not so tall, surprisingly. But he's 6 foot 2. He went... he did go

through the academic programme very well and went through Trinity College in Hartford and graduated. He got his first degree in marine biology, and that sort of accounts for his interest in water sports. Then later, after he graduated from college, at some point Stefan came to work, he always, after he came back, he tried to go to college for 1 month, he went to Cheyney, after he got out of the army and Vietnam, and he lasted about a month, he just was not suited to an academic programme. But Cheyney was basically a black college, even though it was a state university or something like that, and he said he knew what reverse discrimination was there, to be a minority. He then became a truck mechanic, and eventually he convinced me to get him a Peterbilt truck, and he and Josh drove this truck across country. It was an ISI truck, we had the idea once that we were going to use this truck to sell our products like in a library on wheels. I don't think we ever carried that out. So, that's how Stefan happened to work for us for a while. Then Josh came to work for us for a while as a salesman. He worked on the sales force. Even though, if you meet him today he would make a fantastic sales person, he hated it. Eventually he went Minneapolis Honeywell had a computer school, do you remember that, was down on Market Street, took... programming, and then he came to work at ISI and he reported to Phil Sopinsky. I'll never forget when he came to me, and he was here I don't know how many months, and he said, 'Listen I'm not making enough money, you're not paying me enough'. So, I said to him, 'Why are you telling me? You go to your boss and deal with him about it, I'm not going to - this is not a system of nepotism, you know, I didn't hire you because you're my son, I hired you because you're qualified for the job'. He didn't buy into that, and he said, apparently he went to Phil, and Phil didn't think he warranted a raise, so he quit and went and found himself a job at Shared Medical. Do you remember that? When he got to Shared Medical he wasn't working for them, they were just at the beginning of this big boom in hospital information systems, he wasn't with them for more than 3 or 4 months when one of their clients, Tampa University Hospital

offered him a job. I mean, when I saw him just last week he took me past where they had brought him down and put him up at the Holiday Inn, and he had this wonderful... he had the combination of working in a computer center and also being near water sports which, he does scuba diving, windsurfing, snorkeling, everything. So, it was like he'd found heaven. Well... but at the same time when he was 17 or thereabouts he and I went for flying lessons. That's when I found out, when you fly one of these old planes they use mechanical brakes, just like an old-fashioned car. When you are taxiing the way you operate the brakes is just like a car. I had this problem with my injured leg, and I found that I just could not use that brake without, you know, causing the pain to come back, so. He continued taking the lessons, and then the hardest part was not learning how to fly, which is relatively easy, it's all the weather and the mechanics and the rules and the details, it's like going to school and having to learn all that stuff, and he ate it up, so. He became a flying fan and passed right away and got his solo licence. When he got to Tampa he eventually bought his own Cessna. So, when I was down there last weekend he flew us from Stefan's house in Jacksonville to Tampa to go to stay with him. As a result of that, even after he worked in the university computer centre in Tampa, he got fed up with the computer business and decided to go back to school and got himself a commercial pilot's licence. So, when he did it I remember I said, 'Josh, you know, you couldn't have picked a worse time to be looking for a job as a commercial pilot', because they were laying off all these pilots from all the airlines because there was a glut of them, and besides the Air Force had been turning out all of these guys. Well, he said, 'I don't care, I'm going... I like flying, that's what I'm going to do'. So, he went and spent the year and got himself a commercial licence, and then he started working for Mesa Airlines, which is a commuter airline. They actually do the flying and supply the pilots for US Air and these other commuter airlines. He worked for them for a while, and then a friend of his got him this job with this timeshare company, which is what he does now. So, every time he calls me up, every few

days, he's in a different place every day, flies around executives and the movie stars and what have you; they all each buy a piece of the plane. The reason all that is significant is that neither Stefan, Josh nor any of my other kids really were interested in the company. I mean, maybe if they had been, things would have gone a little differently, I would have felt well, let's let one of them take over, but that's the way it was. So, I have no regret about that. For a while I thought Josh might be interested, but he's a very different guy; he's interested more in football, I think, than he is in information science.

Job Security and Outsourcing

I always say a consultant is a euphemism for unemployment. Have you ever noticed that a lot of people who get laid off suddenly become consultants? They are available for whatever. I've known quite a few consultants in my time, people who, for whatever reason, find themselves without work. I always found it very amusing that people who worked at Smith Kline made fun of me because I refused to take their offer, take their, not a tenure job, but a permanent job with them. Because most of the guys I knew there were out of a job within 5 or 10 years, maximum 10 years after I had left, you know. The companies either get sold or their jobs get wiped out or there's some reorganisation. We even know at Smith Kline - I mean at ISI - how often that happens. Including even when I brought... you remember Dick Kollin when he came in, and he said we should cut down the expenses of the company by 10%, just like that. They tried to have an across the board 10% cut in the budget. You can't necessarily take 10% out of the computer budget the same way you could take it out of the marketing or any other budget, it's... but that's the sort of thing that happens when you get a corporate mentality, that the bottom line is the only thing that counts. That never was the case with me and ISI, we were worried about the people that were going to be affected by trying to do something like that. But today, as you can see, it doesn't matter whether they take the jobs; we originally get set up in Ireland and then move to India. Our, reasons for... we didn't really, in the beginning we never

outsourced. What we did was when we started in Philadelphia it was very interesting, once we got into doing the SCI, I had to hire a slew of key punch operators, okay, and it was no easy task to find in the city of Philadelphia key punch operators who were both technically qualified and literate. I mean, all we wanted was, bright high school graduates to do the work. It was a challenging key punching job, and the ones that got it loved it. But we just could not hire enough people, and for a long time I tried. This is how I got to meet, what was his name, Leon Sullivan, was the guy from the black community that tried to develop opportunities for black people. Another reason why this company was always open as far as equal opportunity employment, long before there was a law. We really tried valiantly to find sources of local people who could do the work, but we just could not find enough of them. So somehow we finally decided that we had to open up a branch and what we would do is go to New Jersey, which is today like going to India, it was almost a betrayal of Philadelphia but we just couldn't do it. That's how we got in. It was, I think it was Cherry Hill from the beginning and then it was Pensauken who was there first. Then what we did was hire people who worked... we had a night shift in Philadelphia, but then we had problems in Philadelphia because in a night shift you had to provide escorts at night; you worried about people coming out of the building at midnight and the streets were full of drug addicts or whatever, you know. In New Jersey, on the other hand, we were able to hire a lot of married women who could work. It was good for them to have a job like that. Irv and I were always in favor of flex time. I don't if you remember that. People used to object to Irv coming in here... he used to come to work at like 5 or 6 o'clock in the morning and go home at 3. I remember when people said how come that guy can go home at 3 o'clock, and I said if you want to go home at 3 o'clock you can come in at 5 o'clock in the morning, too. Irv, whenever he left here, he always had a briefcase full of work. He'd come back and he'd done a day's work at home at night, which his wife did not appreciate.

Irving H Sher

You could spend hours talking about Irv; he was a character, a real character. I never got to know him completely, but part of the reason for his strange hours was his religious habits. I think he was an orthodox Jew, but he also did other things that you would not expect. He was an expert in Chinese, and loved to eat Chinese food. Larry Hackett could tell you more about him because he worked with him, and Caroline Finn.

When did you meet Irv, do you remember?

I met Irv at Smith Kline French. That's how I met him. He was working there and when I finally set up the company we met somewhere and decided that he would be better off coming here. I know the guy was bright when I first met him. He was an unbelievably irritating person. He believed in the principal of... what is it he used to say... the bigger they are the harder they fall. He was totally defiant of authority, so if you ever introduced him to somebody who was the expert in the field he would give them a hard time, but in a very clever way; he was incredibly smart. I'll never forget the time when he tried to provoke Josh Lederberg, who is a guy that just cannot be provoked, and he kept on going after him, and finally Irv recognised that this guy is not only smart but diplomatic, he just wasn't going to get to his goat. A similar experience with John Tukey, and Irv really knew his statistics; John Tukey was the world's authority on mathematical statistics. Irv would take anybody on. He was a biochemist, he was just... anything, a linguist; he was really a kind of genius in a strange way, but he had this incredibly nasty habit of using four letter words that were unbelievable. You'd be sitting having this conversation in the office and he would say, 'Fuck that and this'. It was just weird; it was almost like he had Tourette's syndrome or something. But anyhow... so a lot of things we did were kind of bizarre. Once Irv made up his mind about something you could not change it.

I think, if I'm not mistaken, he may have come to work for us after Bob Hayne. Bob Hayne also, I think, had been at Smith Kline too. I introduced Bob to Ted Herdgen so that... both of them had been at Smith Kline.

But I knew Bob from before. I think I got him the job, introduced him to Ted Herdgen, and so they moved up. Irv was a graduate; he had a DSc, not a PhD, Doctor of Science, which you get when you graduate from Johns Hopkins School. He went to the School of Hygiene at Johns Hopkins, which had the bio statistics program, so he was both biologically and statistically trained. He was, just an unusual guy. So, he taught himself programming. He had his own system for shorthand. Anything he touched it was different. Now, on the other hand nobody else could replace him. If he did the program nobody else knew how to program it. So taking his stuff and modifying it was not easy. Anyhow he was a very important step in the growth of the company, I think. He was very, weird about different things, he absolutely would refuse to put his name on papers. I would say, 'Irv, we're doing this paper and you're part of all this'. He wouldn't care; I literally had to force him to put his name to a paper. I just put the name in and he wouldn't argue anymore. But he would edit the paper. He would do an incredible refereeing job. People hated him when he was a referee because he's one of these kinds of referees that not only said... he'd say things like, 'That stupid ass'. It would be a personal attack of four letter words. But you couldn't have got a better analysis of what was going on. He just was a very odd, strange guy.

Using Abstracts and the Company

In hindsight, if it hadn't been for JPT we might never have done abstracts, because I tried to get Irv... I said, 'Irv, there's a reason we can't compete with BA and CA and all these other abstracting services on an equal plane, it's because we don't use the abstracts'. Now, in the early days we used to think well, you'd have to create the abstracts, you couldn't just take them, but the author abstracts were available; it turns out nobody ever cared if you used the author abstracts. He said you don't need the abstract to retrieve the information. He would not see the importance of that. I think it was only, within 3 months or 6 months when Paul Neuthaler came here that they added abstracts, and Irv figured out of course how to do it. Once

you told him, once you told him to do something it was done. He would fight, he would fight, but if you said, 'Irv, I'm overruling you, forget it', once that happened... but he was just that way. But he would fight and make life so miserable for you until you'd convinced him. That was the sort of thing he did. The same thing we couldn't convince him about getting rid of the damn truncation. When we started out, we had a punch card and we were limited to it, but he kept on delaying the inevitable change so that you could accommodate the whole name. So, when you look at it there were still records with those damn truncation marks. Similarly in some way I regret that we used to have this non-journal code. How many times have I regretted that we don't have that still? If that had been left there it would have been a hell of a lot easier to do various kinds of bibliometric studies in the records. Because in the early days we had no trouble differentiating the book citations and journal citations. But those kinds of compromises were eventually made. The same thing with, I don't know whether I regret that we didn't keep on doing patents. When we first started the SCI we did 2 years complete US patents. Nobody saw the value of it, say well think of all the journals we could do, so we dropped the patents. Eventually somebody else picked up the patent citation index. That's a story all on itself.

At the Patent Office and 'Index Chemicus'

Index Chemicus started in 1960, but the way that happened was that there was a fellow named Bob Hart. Robert Hart was manager of some information department at Merck, and he was chairman of a Pharmaceutical Manufacturers Association (PMA) committee, and at that time, as they were always are, but particularly at that time the patent office was years behind processing patents, and so they convinced the then patent commissioner that the patent office would agree to use information that they would pay for compiling, in order to process patents in certain classes of chemistry. At that time the steroid market was just opening up, there were thousands and thousands of new steroid compounds being synthesised.

People like Carl Djerassi became famous for the birth control pill, and all these other drugs. So, they formed a sub-committee of the PMA and they put up the money to form a project in co-operation with the patent office. There was a fellow by the name of Andrews, I believe was his name, the head of this patent office section for investigating machine methods, punch card methods, and there was another guy who was a very active ASIS member, a short fellow by the name of Si Newman, from the patent office. What they did was we designed, like I had done at Smith Kline for the drug thorazine, for steroids we had again a card that had a direct code that you could search structural groups, it was not a Wiswesser line notation, it was just a straightforward coding system, and the amino group was one code and the hydroxyl group was another code, and it was a very simple-minded kind of coding system, but it was effective in terms of searching for these compounds. Among other things, we got a contract, Eugene Garfield Associates, to scan the literature and pick up new compounds and create a punch card for each new compound. We were doing this scanning of the literature for the patent office for a couple of years. It was as a result of that work that I became aware that you didn't need to do all this stuff that Chemical Abstracts was doing in order to create a molecular formula index, because all we did on the project was to look through a paper, and whenever they do the analysis they do the molecular formula, empirical formula, okay. So, that was a sign that you had a new compound. You know a child could look for it and find them; you didn't have to be a chemist in order to know it was a new compound. That was the definition of a new compound, as it were. There were occasional times when you would scan the rest of the paper and you might have found something mentioned which would have been considered and mentioned in the literature book had not been analysed for. So, only the compounds that had been subjected to a molecular formula analysis were listed. So, you might have 20, 30, 40 compounds in a paper, but they were all there, clearly identified. So, that's how I realised that... why do we have to limit

ourselves to steroids, you can do the same thing for any other compounds too. So, at the end of that... then at the same time there was a fellow named Dr Max Gordon, who was at Smith Kline also, a well known organic chemist, who had gone to... who was a former student of Alan Day at Penn, he had got his degree at Penn, I believe, and Max Gordon was head of a group, he had a local group in the American Chemical Society, who got together and wrote a letter of protest to Chemical Abstracts and said we're all members of the American Chemical Society, why the hell don't you get yourselves together and put together a current index to the literature, this is costing us a fortune because you are doing it the way you did it 50 years ago. The CA, of course, the American Chemical Society typically would not pay attention to these guys. And the situation... then Max, we got together all the people in the various drug companies, and we all used to meet in the ACS meetings, and we formed this group. To make a long story short, they had seen what I had done on the patent project, and I said to them I could produce this index for you for a cost of about \$25,000. At first I went to Max Gordon and tried to get Smith Kline to do it just for itself. Glen Ullyot, who was a super CA fan, said, 'You're out of your mind, you could never do that for \$25,000'. He wouldn't believe it, even though Glen is a wonderful man, we became good friends. His wife is still, by the way, active in the Chemical Heritage Foundation, I see her from time to time. But he and a lot of other people absolutely refused to believe you could do it. So, I said, 'Okay, Smith Kline, you don't want to do it, is it okay if I go and offer this to other people?' So, that's how the Index Chemicus got started. There were 12 companies that initially agreed that they would buy - I forget how many copies of the Index Chemicus, let's say \$5,000 or \$10,000 a year, I don't remember the amount, and that was not enough to cover the cost; it was still a high risk venture for us. We would then be able to sell it to other people who wanted to buy one subscription or whatever, and I forget what it cost in the first year. But that's how we got started. By the time we got started it had been converted from a purely molecular formula index into

a current awareness service with, with structural diagrams, and that was the key factor as far as organic chemists were concerned, that was the thing that was missing from everything else that was being proposed. That's why we cut and paste. We used to go into the journals and literally cut out pages of the journal and paste them down and make the abstracts and so on. So, that's how it got started. It was later on that we began to get involved with the Wiswesser line notation, which was another step.

Index Chemicus

Index Chemicus was launched in 1960. The contract for the genetic citation index, the grant was like maybe the same year, '61, '60 or '61, and then it was for a 3-year period that we did that. So, SCI, the '61 SCI was published in, what, late '63. Then we decided, on the basis of that, we would launch a quarterly service, which started in January of '64. Index Chemicus by then was in its fourth year, and still hadn't made a profit. It was at some point in the next few years that the four of us of the four horsemen of the apocalypse decided that you're never going to make any money on this chemical information stuff, and we think you would be better off getting out of the company, and we'll run it and we'll make a profit. But, as you know, it took a long, they still have, I don't know, they probably still now make money on it, but nobody ever tells me about it. I'm sure it wasn't making much money at the end of my tenure on Index Chemicus. But the organic chemists loved it, and probably still do

Using Molecular Formula for Retrieval and Citations

It was evolved you know, first it was just titles and lists of compounds, and then later they added diagrams, and then we added the author's abstracts, and then we added the indexing. The most, the critical thing was the updating of the formula indexes. Because what I found out when I was still at Welch Project, people did not use the name of a compound to find it; they never searched that way. The main way people found compounds was going in by molecular formula, the empirical formula, and then they would see the list of compounds that were

isomers of that empirical formula and that was how you found the literature. You didn't try to name it first, because 50% of the time you wouldn't think of the right name anyhow. You had to have a special knowledge to be able to name the compounds with the system. So, that's how I realised that the molecular formula was the key to retrieval rather than the name. So, to this day I think CA still regenerate the structural diagrams, and from there they name them according to the rules they create. The molecular formula is a by-product. They can do all sorts of things automatically now which couldn't be done back then. But I always say that's a good case of one of my least cited papers having the highest economic impact on science. It was the algorithm chemical names into formulas. That is the heart of searching chemical compounds these days, but very few people are writing papers about it and citing that algorithm. Which, I not only had in my thesis, but I published it as a paper in...brief, I had a brief half-page letter to the editor of Nature about my algorithm, it was called Automatic Translation of Chemical Formulas. When I published that paper in Nature, that letter, I was taking a calculated risk that, in order for us to establish priority, that they wouldn't prevent me from getting my degree. Because technically speaking, in those days, if you had published on it you couldn't get your doctorate, you couldn't use it as your dissertation any more. So, I published it but I knew that my dissertation advisor or nobody else would ever read Nature so nothing ever happened. So, that's the original reference, that paper in Nature. Then the dissertation is the other reference on it. Then, I think, later on I wrote another paper in the journal Chemical about it. But a lot of people forget that even my 1951 paper about SCI, about the citation index, not my most cited paper but the one that came later, in '72.

Oh, you mean the '55 paper?

Yes, the '55 paper is not my most cited. You'd think that. It's because of the journal evaluation thing and the impact factor. Of course the book is cited a lot. What I find amusing about the book is that people still

cite chapter 10... Tibor Brown was desperate, I think, for articles for his new journal in the first year at *Scientometrics*. So he asked me if he could publish chapter 10 of my book as a paper. That paper gets cited... it's one of the most cited papers in *Scientometrics*. But nobody seem to realise that it's a chapter from the book, even though I think it says it, but it still gets cited separately. You have to count that in. The book is probably cited more than any of my papers, I don't know how many times.

Current Contents and the FASEB Meeting

In those days when we started *Current Contents* we were very conscious of biochemistry and molecular biology; that was our focus. Anything that was in *Current Contents* if it was anything at all related to biochemistry or molecular biology they were our target. That was the reason why we did the impact factor based on 2 years. Everything was centred around what is the most important in serving that audience. So I attended the International Congress of Genetics in 1960, in which Irv and I gave... an abstract of our genetics project. It was '60 or '61, I'm not sure. I remember going there, in Schreveningen, near The Hague? They had a lot of conventions there. I got definitely sick there. Whenever I go to Holland I get this kind of nasal infection of some kind. It happened to me every time I've gone there, except, I think, also when I went to the International Conference for History and Science. Needham, Bob Morton was there, I think, and you were there, right? But in '61 I went to the International Congress of Biochemistry which was attended by 500 Americans, in Moscow, and I vaguely recall I met some of the people from the VINITI there that year, but it seems to me it's a little early, because SCI certainly was not in existence as SCI. The genetics citation index was still not very complete in '61. So, it was *Current Contents* that was being promoted, it was pretty much my life. We used to go to the FASEB meetings in Atlantic City. You know why the FASEB meetings were held in Atlantic City? There were only three places in those days where you could have a convention that was big

enough to accommodate FASEB, which had over 20,000 people at a meeting. Atlantic City, Chicago and New York were the only places that were big enough. I think there were similar circumstances in the American Chemical Society meetings, but now there are much more places where they can have these conventions. I think it's because of the hotel space; it's not just the convention hall. So, when we went to the Atlantic City convention we had exhibits; we bought exhibit space for our products. I'm jumping ahead now because I'm thinking of one particular meeting at Atlantic City with FASEB. We had already started the SCI, so it had to be a meeting in '65, let's say. Well, that year we decided to buy exhibit space at the FASEB meeting. Irv has this brilliant idea that we will promote the SCI by having a booth with a Chinese theme, with all these red posters, and he had written different slogans in Chinese. The other finishing touch was that we were going to have fortune cookies made up which had little slogans: 'SCI making successful scientists', or something like that. We ordered 5,000, 10,000, I don't know what the minimum order of fortune cookies we had made up okay, we bring all these things to the convention. Apart from the fact that the earlier days I was there I used to promote *Current Contents* by going on the boardwalk, I would give out copies to the guys on the boardwalk, they couldn't stop you from doing that, because in the early days I couldn't afford the booth. So, anyhow, we got this booth, we're giving out these cookies, people are coming around and we're giving out samples of *Current Contents* and the SCI literature. I think it was the second or the third day, it was a 5-day convention, there was a committee group of three or four guys come in and they say, 'We're from the FASEB exhibits committee, and we have to inform you that you are in violation of exhibitor rules, which are that there are to be no free giveaways'. What are you talking about? 'You are not allowed to give out free cookies'. So, we had to take all those cookies back. We were eating fortune cookies for months. I'll never forget that. Of course, later on it turns out that giveaways are... now they give you all kinds of stuff.

Attitudes of the NGOs

There was an incredible anti-private enterprise sentiment. I mean, these non-profits had their own way, everything they did, it was always holier than thou attitude that we can only do things right. That's why the American Chemical Society never wanted to do everything, because they thought whatever they did, that was the gospel. When I was at Oak Ridge I reminded them last week about the fact that the Federation of Abstracting and Indexing Services, when it was set up it was purely for non-profits and the government. You could not be a member if ISI could not be a member; Roy Hill [of McGraw-Hill] couldn't and so on. That's why me, Boris An•lov, Jeff Norton, Bill Knox and Saul Herner were the five guys that founded the Information Industry Association. Not only that, that IIA has become SIIA, the Software and Information Industry Association. Today, of course, it has changed; ISI is a member of NFAIS and so on. But in the early days, the government would not give any kind of grant to us. If they gave anything it was a contract, they set the terms. You know how often NSF tried to screw us out of our own data. Because Mort Malin had worked at NSF and he had negotiated some kind of a scholar's deal with them, and they used that, scholars were allowed to use our data for their own personal research. But they use that as a way to give the information to Francis Narin, give him the contract to do the work. That's how it all happened, and it took us years to get out of that bind

International Congress of Biochemists in Moscow (1961)

I went to the biochemistry meeting to promote Current Contents and to talk about the genetics index. Well, they had... these 500 American chemists were there, including Dr Greenberg from Smith, Kline & French and many other friends of mine who went to that meeting, and the year I went to that meeting, it was '61, the summer, I had my son Stefan with me. I'll never forget that year because that was the year when we had traveled to Moscow, when on the way back we went and stopped off in Leningrad and we went to the museum, the Hermitage. The

day we were at the Hermitage, Yuri Gagarin and his family were making a tour of the museum, and I'll never forget it. Can you imagine an American astronaut visiting the Metropolitan Museum of Art the day after he gets off the space shuttle? It was an incredible contrast in culture, right? So... but at the biochemistry meeting we were promoting Current Contents. My Uncle Sam, that I had mentioned to you, had retired by then, at a fairly early age actually, and he was traveling in a private automobile with a friend of his, they had gone to Poland and they knew I was in Moscow, and they came to Moscow by car. How they managed to do that I do not know, but they did. He was an incredible guy. He was staying at the Hotel National, which was where Lenin had stayed during the revolution, and there was a special room that was kept as a kind of museum, but you couldn't go in and you couldn't use it. He tried to book that room. The way he did it, being the drinker that he was, he's gotten involved in these drinking sessions with these Russians; he would out-drink the Russians, it was just incredible. So, he came over to visit me while I was staying at the Hotel Metropole with Stefan. All these Americans were there, and when you went to Russia in those days to the in tourist agency you would have to pay for everything in advance and you had to buy coupons which were used for your meals and for your buses, and for whatever you were going to do at the hotel, and you got one for every meal. So, you had a breakfast thing, you had a lunch, you had a dinner, you had a tea; so you get these coupons. Well, Stefan, he was 14 years old, being the kid that he was, he went around, mostly Americans couldn't eat that many meals a day and use all the coupons, he went around and people were giving him coupons that they hadn't used. He would just collect them like a kid collects baseball tickets, you know. He was sitting at the table with us and my uncle came over and he orders a bottle of Russian champagne. So, the waiter, we thought he was going to have to pay him in rubles because it wasn't a part of the usual meal, and we were not vodka drinkers so we had never ordered any alcoholic beverages so the waiter is hanging around, he's getting a little

impatient, and he sees Stefan playing with these coupons, and he walks over to Stefan, instead of waiting for my uncle to give him the rubles, and takes a few of these coupons for the champagne. Then we figured out... that was the realisation that the Russians considered these coupons had a certain value in rubles, so you could buy anything in the hotel with that stuff. So, we said okay, we ordered champagne for the whole room, because he had piled up all these coupons, we used these coupons to buy champagne. There were guys who spoke to me 20 years later who said, 'We'll never forget when we got the champagne delivered to our table from ISI'. Because remember, ISI was changed, it was named in 1960, '61 is the meeting. So, those are the little things... it was incredible, the stories you could tell. Stefan was amazing, he discovered the flea market on his own, he was a 14-year-old kid. Nobody ever questioned where he went, and so on.

George Vladutz and VV Nalimov

I think I had somehow gotten over to VINITI to meet the people there, Nalimov may have been there, but George Vladutz remembered me from that. He didn't introduce himself, but he had been there. Later he wrote to me. When he emigrated from Russia, which was a difficult thing for them to do, he wound up... the Jewish people who left Moscow went through Rome, and from Rome he called me or cabled me that he was there and we tried to help him out in some way, and eventually it worked out he came here, he worked for us. But he was the Russian authority on organic notation systems, research in literature and so forth. He remembered my visit in '61, I remember that; so, that's how we got to know George. He was a character in himself too. God, he was weird. When Beta Starchild was working here I had people say, 'Do you think George Vladutz is a KGB agent or something like that he's such a peculiar guy?' Because the way the Russians, if you live in a society like that you become so paranoid, I think, about the way people behave. He was so political, everything he did was manipulative. There's a certain bureaucratic approach to things he has, and other people I won't mention that

we know that behaved that way. But I think Nalimov had worked at VINITI at some point. I don't know the exact sequence because he had been in a gulag for about 15 years as, I think... his family had a history of religious protest and what have you. It's all, it's all documented. His wife is, I think, publishing a translation of his autobiography, or something like that. He then, being a polymath, and being a brilliant statistician and mathematician, quickly caught onto the idea of, you know, the use of measurement in science policy, and that's how he invented the word 'scientometrics'. As we've mentioned many times, it was eventually that document, that book was translated by some government agency, I don't know whether it was the CIA or whoever, they never put their name on anything, but it was available free as a document, a technical report. It was not cited all that much because it wasn't being disseminated widely. But because of the east-west connection in Hungary, Tibor Braun picked up on that, and Nalimov was an early member of the board, and you and I were on the board early on, and Derek, and that's how he picked up the word scientometrics, directly taken from the title of the book. So it was just basically an appropriate synonym for science of science, which was a very awkward term, I think. That would never fly in English; it's just too repetitive to say science of science. What does that mean. It's just the application of measurement to social science policy, science policy studies.

Russian Colleagues and Traveling on the Trans-Siberian Railway

I met him in... I went to Kiev at one point. He died because of the... Chernobyl. He died of cancer. He was out taking a... he was in Kiev, but he was apparently taking a sun bath or something like that, and they had not told anybody about the explosion, the meltdown, and a lot of people died of exposure to radiation that could have protected themselves if they'd known. I never got the full details, but that's what I found out later, because I don't know who found out for me or told me, but my friend... have you met Victor Vaskovsky? Do you remember when I went on a tour on the

Trans-Siberian railroad with him? Well he was born in Kiev, so he goes back... he had gone back to Kiev. I'm sure he knew about Dobrov. So, that was a tragic outcome of that. He would have been a brilliant contributor; he was smart, Dobrov. He had...

Did you ever run across Mulchenko?

I don't know. That's the guy who wrote... whose co-author was Nalimov. Did you meet him?

No.

I don't know who that person was. I never did meet him.

He was a politico or something.

I imagine we could ask Valentina, she probably knows who it is, or Jeanna, his wife. I still have contact with Nalimov's wife. It wasn't until, I think, '65 when we went back to Moscow, we had exhibits at the Moscow book fair in order to promote sales in Russia, which didn't turn out to be all that productive. The Russians had their system of controlled distribution, and they were not going to ever buy more than three or four copies of the SCI, come hell or high water. The guy who made that decision, his name was Aritunov, and we knew him because, I forget what the occasion was that brought the Russian delegation to the US, and they all came to visit ISI, and in fact I had that whole group of Russians come out to my house in Swarthmore, and it was a snowy night, and because of the snow... we couldn't get a car to take away, we had to march them through the Swarthmore campus to the train station to get them back into Philadelphia. I don't know if those people knew they had all these Russians trampling all over the place, but Aritunov spoke very good English, and had been a member of the Russian trade delegation during the war. So, he knew America very well. He controlled, he made the decisions of how many copies of any journal were bought in the entire Soviet Union, scientific stuff. I remember there was a library subscription at the Lenin Library, at the Moscow, what do they call it, the Moscow Public Library, and a third one at some other place in Moscow. Then later on there was an exception made... the library of the Academy

of Sciences, was another one, and then Victor Vaskovsky got, he was secretary of the far eastern branch of the Academy, got special kind of dispensation to buy the SCI for their far eastern branch. That's why, when he wrote to us, we put him on the board, the editorial board. Eventually he set up this tour. He took me on a tour to give a lecture about the SCI, the story, in Moscow, we went to Novosibirsk, and then we went to all these other places on the Trans-Siberian railroad. It was very fine. That is how I got to meet Pudovkin, he worked at the same place that Viktor did, at the Marine Biology Institute of Vladivostok. When we got to Vladivostok I actually didn't go into Vladivostok, because Americans couldn't enter Vladivostok; you could go to the airport or they had a separate station, about 60 miles north of Vladivostok, I forget the name of it... Nahodka was a special place where all foreign people were taken, and then from Nahodka you could take a boat out of there to Japan. But you could not go into Vladivostok per se. You might... I don't know if you could fly in there. They once took me to the airport, where I conducted a seminar at the airport actually. Later on... because what the reason was, we wouldn't let the Russians in San Diego, so it was a reciprocal thing. But even Russians couldn't go to Vladivostok; you had to have a special passport to get there even if you were Russian. It was like a secret city. The Russians are totally paranoid. I mean, everything they do has got to be veiled in secrecy, it's just incredible. They are used to it, and it's amazing to accommodate to that. But it's a little like the way of being with, having just visited Oak Ridge thinking about the Manhattan Project, can you imagine that town, what it was like. They've got a museum where they show some of that stuff now, it's quite interesting. Amazing what was going on and nobody knew what the hell was going on. Very few people knew they were building a bomb. It's incredible when you think of how many people worked on it.

Eastern European Readership

We had a lot of friends in Eastern Europe, because the interesting phenomenon was

that... maybe it was a quirk of the censorship system there, while Science and Nature magazine were highly censored, Current Contents was not treated like a normal journal; it was just like a bibliographical tool. Who would have questioned... like you didn't have to censor Chemical Abstracts, right? All they had to do was copy it. But in the case of Current Contents they apparently ignored the fact that upfront they had these editorials. So, it became a window on the west for the people in Russia who read, and other countries, especially in Eastern Europe, like Prague and Hungary whenever I would meet those people I would always say, 'You know, I feel a little strange; do you think I am not behaving morally because I don't comment on the fact that things aren't quite the way they should be for East European scientists?' They say you don't have to tell us what's going on, just tell us what is going on in the west, we don't need to know about what's going on in Russia or the east. That's why we were so well known to so many different people, that was the only stuff they could read every week. Now, on certain occasions I'd be conscious of it. That was another reason why I thought it would be interesting. We had the press digest, that was another. We could always pick things out of there of interest to them to give them some kind of a view of the culture. So, we were very well treated in all these other countries whenever we went there. Many places I never got to, like Prague.

Do you think they used the data to do science policy studies?

Where?

In Russia, Eastern Europe.

It's hard for me to know. I never did specifically ask anyone did the KGB use it. It's like saying does the CIA use it? How would you know? I can tell you this, it would have been nice if you could have been there when we gave lectures on the mapping of Russian science. When I gave these lectures in Moscow it was clear that we were telling them something about Russian science that they had never heard before. So, if they were using that data they sure weren't telling their own people about it. So, it was very

enlightening for us to point out here's where your activity is, as far as the western literature is concerned anyhow. Nobody, I guess, would have ever disputed it, no matter what I said; they wouldn't have agreed or disagreed. They'd be like... let's say we're completely wrong about what we're saying, are they going to tell us? But nobody ever questioned it. We identified... I mean, it was pretty accurate. I think it turned out to be a pretty good window on what disciplines they were really involved in. It turned out... all the leading scientists were well known to our leading scientists. When I went there I was always... this son of the Nobel, Kapitsa, and he knew me and came to different events we had, or lectures they would show up. Ovchinnikov was a leading molecular biologist, he died at a relatively young age later on, and there was a guy, Josh Lederberg knew him Spirin, he was famous, a Russian biochemist; all the leading Russian scientists were identified by us in one way or another. They may not have shown up as well compared to Americans, because of our bias, but still they were publishing and their work was known, especially those in physics. The secret work... we wouldn't have identified Americans doing secret work either.

Citation Classics

I was thinking the other day when I was in Oak Ridge, somebody said Alvin Trivelpiece is coming to the celebration and he is going to attend your talk. I don't know if you remember him. He was the one who became executive director of AAAS for about a year at one period; 20 years ago. I met him in 1987. He was the successor director to Al Weinberg of the Weinberg Report. So Bonnie Carroll said to me see if you can find out anything interesting to say about Alvin Trivelpiece, so I looked him up and he was the author of a highly-cited paper published in 1959. Well, I referred to it as a 'citation classic', but it was not one which we had invited him to comment on back then. I would have to look at the period of time, and so forth. But anyhow I keep reminding, we published 4,000 commentaries by authors of citation classics, so that meeting in Oak Ridge why... you know, the reason that we stopped doing that was because of

the stupidity of them wiping out that editorial group that we had. But in particular the stupidity of wiping out...I forget his name, we had one guy who was able to get seven commentaries a week published, six or seven.

Is this Eric [Thurswell]?

Eric who?

Eric...

He had some sort of a mathematical background, right?

I think his name was Eric, but I can't remember.

Well not one person... now, today - I could understand the excuse that well, you had to print it and everything - but today you can do it online. Why don't they do, they do these hot papers, why don't they do these citation classics? Who does it take to... so I'm going to write a note to Keith McGregor. And the reason, I'll tell you why it comes up: we have these 4,000 items, Meher Mistry put up... scanned those 4,000 classics, we had that work done at our expense in India, I believe, and put them up. They were not perfectly scanned, because the resolution at the time was like 98% and you really need 99 or something like that. And it is publicly accessible, okay. Why couldn't... why don't ISI continue to do it? I'll be glad if we could turn that file over to them and put that. People refer to my website; why don't they refer to the ISI website?

That's a great idea. We should keep doing it.

So, if you get one person doing it could take care of maybe even more than six or seven. I'm sure a procedure could be set up where it's almost like a journal. In other words, ISI could do a journal citation. I once wanted to do a journal Citation Classics, and it's kind of an autobiographical thing. You wouldn't have to limit it to one page, as we did.

Teaching at University of Pennsylvania

I think it was the new President of Drexel at the time, was his name Hagerty, they asked me if I was interested in becoming Dean of the School of Library and Information Science. I didn't know that one. I said to him, 'Well, that would be a part-time job right?' He said, 'Oh no, no, basically you'd

have to give up your company to do it'. I said, 'When Carl Djerassi was asked to be chairman of the department they didn't ask him to give up his stock in his company, and I don't intend to give up my work at ISI in order to do this, but it's a job that I think I could do if you were interested in talking about it as... I wouldn't consider it a full-time job, but certainly I could handle it'. It never occurred to him that I might not be interested in just leaving the company. It's just another job, I guess, to him. But I guess, the teaching part, they never did... during the times when the other, more conservative deans never asked me to come over there and teach. It's very strange, I thought. Later on they gave me an honorary degree, but they never thought it was important enough for me to go over there and do any teaching. But there's some kind of a mindset there, I guess, about that. Now, the people at the Moore school, when... they were interested in getting these government contracts in the early days, and they wanted to know what information retrieval was. There was just a good coincidence that Saul Gorn was an old friend of Sam Lazerow that I had met when I was still in Baltimore and he was working at the Aberdeen proving grounds, I think. He was a brilliant mathematician and basically the father of programming languages, I think is what they considered him, he was a guru of that sort of thing. He spoke to Morris Rubinoff at the Moore school, and said, 'Oh gee, why don't you come over and give this course at our graduate centre for information retrieval?' and that's how I got to doing the teaching at Penn. I think I mentioned that when I was telling you about the business about the information engineers: it's okay to teach but it was not okay to practice. That always proved to be an interesting experience.

Sharing Information and Encyclopedists

Brett had picked up on the idea that I had talked about in library school, I wrote a paper about what I called the librarians' encyclopaedia, and basically what it was a system for getting reference librarians to co-operate in sharing their FAQs, they call them today, I don't know what they call them, frequently asked questions. So, the idea was

that instead of people constantly looking up the same reference questions over and over again and doing the work, share the information. Well, back then in 1953 or '54 it wasn't all that easy to share information, and today it is. So, then Brett came along and wanted to start kind of an online shared reference service, I forget the name of it. I actually invested in it, and before it even got to the point of losing all the money, which it was about to do anyhow, he died suddenly. Brett was, a partner, I think, with Dick Kollin in starting Management Contents...

Magazine Contents. I gave him the name. I said what are you doing, he didn't have the right name, it's the Magazine Index. That's what they wanted and they got it. Somebody else had given me the same kind of break with Current Contents when we named it. So, anyhow, encyclopedism has always been kind of an interesting project. I wasn't aware, fully conscious aware, of various people who have written and done research on that: Paul Otlet and La Fontaine. I was just reading about this the other day; somebody has done that sort of research. Boyd Rayward, I think he's one of the people who has done a sort of biographical account of his work. People forget how old some of these ideas are. When I was at Oak Ridge I told them that it was Henri La Fontaine got the Nobel Prize for, peace prize, for his work with the International Institute of Bibliography. He and Otlet developed the system that was supposed to, bring peace and humanity to the world. It was part of the League of Nations. Otlet did not share in that prize, but I think he was recognised when he got the award that he was a contributor. They were, I guess, early predecessors to the idea that HG Wells had put forth, the world of the encyclopedia, and then all these other people picked up on it. So many people, like the guy in Canada, Goodman, he was an encyclopaedist guy. Then our friend Manfred Kochen. Manfred was a wonderful guy, and he was obsessed with that too, I think, the world encyclopaedia idea. There were so many different people. Then there was the other encyclopaedist work, Neurath - remember that?

Hans Neurath.

Hans Neurath had this idea of the unified encyclopaedia- I forget what they called it.

Index to science or unified encyclopaedia.

We used to write about some of those things. We cited some of that work long ago. So, all of these things are now coming to fruition with this, Wikipedia. Now I understand the founder of Wikipedia is going off to do, now they want to have more of a peer reviewed kind of Wikipedia because Wikipedia has got so much junk in it that people are upset about the fact that it is not really authenticated. I don't know how much you've... I know that when they first started that thing I put in a couple of paragraphs about citation indexing, and the last time I looked at it I couldn't recognise what I had written; so many different people had made changes in it, which took away what I thought was the thrust, the important stuff.

The Atlas of Science

When I made the move, when I decided to go with JPT, I did realise what their game plan was going to be. When they came in The Atlas of Science was just being launched more or less as Research Reviews. Sandy Grimwade was the editor and we were getting these reviews written. If we had held out for 5 years I think it would have been a very useful, profitable set of review articles that would be issued based on the concept that we'd evolved of identifying the emerging fields and disciplines of science through co-citation analysis. Well, naturally in the first year or two... it wasn't an instant success, nor was SCI. If they had been here when the SCI started they would have killed it before it got off the ground. So, they killed that product as they then killed The Scientist. In the case of The Scientist they may have made a wise financial decision, but that was an entirely different kind of a story, because of the difficulty we had that time when we had that FBI problem with the guy that planted the... was it Microsoft stuff where they said? Word Perfect.

Word Perfect? I didn't even know how to use this stuff! I probably paid the largest price for Word Perfect than anybody in the world just to get them off our backs. So

anyhow, The Atlas of Science, now you hear people talking about it like it's a natural thing to do, but... especially when I saw the work that they were doing down at Oak Ridge on it with this huge mapping thing. You ought to go down there and see that sometime, that control, command and control map... unbelievable. That thing... it's bigger than this wall, huge thing, I don't suppose the average user is going to be able to afford that, even with HD TV. it's probably like a 30 foot HD TV screen. But I guess people buy them. They have them outside the tennis matches, they have these huge screens, I don't know if they are HD, but they have them.

A dynamic Atlas of Science, meaning that it would generate the usual co-citation clustering maps, but you would re-cluster the file based on every week's accumulation of data, let's say, and then you would generate a new map every week, and then you could project the image of that map as a moving target so that you could see how new fields were emerging, in a very visual, dynamic way. Whereas now you've got a kind of extrapolate visually from one year to the other, you can cluster every year but the changes are significant because of the way in which the clustering occurs, whereas on a, on a week-by-week basis you probably wouldn't, it would be not as sensitive to the changes, right. So, that's an experiment that still ought to be done. I think it would be fascinating to see it. Hasn't the memory changed in such a way that you could do that in such a way?

Yes. I'm pretty sure we could do it.

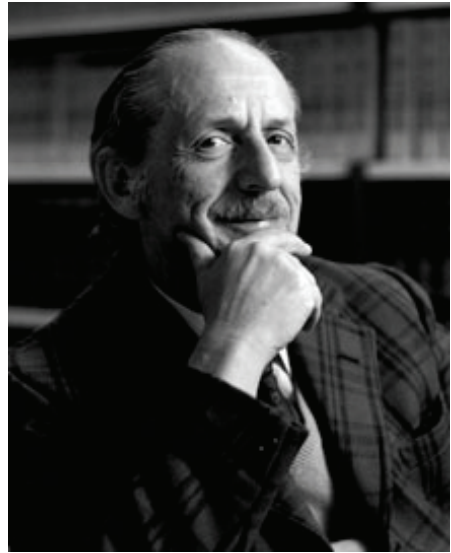
Can that be done on a PC with multiple gigabytes of memory?

Probably.

I think people would love that, and it's something we've always wanted to do. That's the kind of visual display we should have had for that thing we had in New York; not this other stuff. They had that exhibit, by the way, down at Oak Ridge at the science museum.

Very static.

Your picture is up on the wall, by the way, which I was very proud of. So, that was



another interesting project that we didn't quite complete, and something that needs to still be done, I think; it could be very useful. I think we ought to get over the hump of people's resistance to using that sort of thing.

Algorithmic and Historiographic Analysis

After that we did writing the history of DNA with citation data, We always talked about the idea of being able to do that in a kind of automatic, semi-automatic, algorithmic fashion. About 5 years ago I was talking to Sacha Pudovkin about that project, and I don't remember the exact sequence, he probably remembers it better than I do, we decided why don't we... why don't we now try it because things have changed, we have the computer power and so forth, and so we began to do the programmes for doing that. As a result of that work... and then we brought in Vladimir Istomin, his former student from Siberia, who was at the Washington State University at the time. So, he started to do all the programming. He was a very clever programmer. That's how we got into his site, doing these algorithmic historiographic analyses. The software is now going to be available; I believe next week is the launch. Because they had done the final touches on, whatever they have to do for a commercial programme. I think ISI is going to help the marketing of it, so we'll see where

that goes. But basically people are going to have to learn a little about modulating that kind of information because it doesn't work out as simply as you would like. Some of the things are very straightforward. I mean, you just do a typical search of a subject, not a complete subject, let's say, download 1,000 references from WOS, and you identify right away that sample, the 50 most important papers in the field based on the frequency analysis and so forth, and you get a nice looking little map, depending on how complex the field is. But some of the time you think you're going to be able to prove a point and it doesn't work out quite as well because there's this variation in the way that people cite the literature that doesn't always show you the micro links. I think in a macro sense it works perfectly well because the citation frequencies are so large that it doesn't matter whether you make small mistakes. But if you got something that was linked by one or two references, you could easily lose it. If you want to display more than 200 nodes at a time it gets to be difficult on that limited screen. That's why I mentioned that huge screen because, you know, you get a big screen and you can do a lot more with it and see some of the connections that you would otherwise miss. Because whenever I go back and try to trace the history of something I learn something new every time I look in the file. It turns out that what the software now does, in comparison with what it first did, we used to separate what we called inter and outer references, now it just generates one complete citation index for the file right away, and you can separate them, and you can either have both or all, and then you get a timeline right away; there is a time line with a subject. Depending... what happens is you get enough people who have written on a subject collectively they produce a pretty comprehensive history in the field, I mean it's all there. If these people aren't going to give you the information, who will? It's sort of like a given thing, unless you're going into some obscure subject that nobody's ever really written about, and in that case you wouldn't find any references anyhow. So, I think that ought to prove to be of interest to a lot of people when it gets to be more widely used. But I've always encountered

resistance to this sort of thing by trained historians; they don't trust that stuff, it's got to be subjective, the old fashioned way of doing it. You've got to read it all first and then you produce the analysis. It's like when I was at the Welch Medical Library I used to go to the lectures by Oswei Temkin, a very famous historian of medicine who was located there, and one time somebody went to one of his lectures and asked a question which involved... making implicitly... the answer that Temkin gave was, 'The answer to your question is if it was there I would have read it'. Meaning he had read everything ever written on that subject, in any language you could think of, including six or seven languages. That's not the way it is today. I don't know if that would have been true if Henry Sigerist or others like him ever meet me, or whatever. Bob Morton was a kind of guy who read a lot, and his recollection was incredible too, memory. Anyhow you would hope that ultimately when all this stuff gets put up on the web and it's in books and everywhere else is completely visual, we'll be able to get some very interesting historical analyses, but I guess we're going to have to modify all the different software to take advantage of all the full text that will be available. Because right now we're dependent on databases like WOS or Scopus or Google or whatever, but when you do the full text you are going to have to have different software written to take advantage of all that stuff, and then do your mapping. So, there's a lot more to be done yet.

Future of Open Access and Retractions in Citations

It will be curious to see how much you really find out when books... full text of books are available, but maybe less than a lot of people assume you will find out. But it will be interesting, I'm sure. Whether you will ever overcome the objections of people like MacRoberts I don't know. They always said that...

You don't cite your influences.

He must have passed away or something because I never hear anything more about them. They sort of withered away, I don't know what happened to them. Their field wasn't scientometrics, their field was really...

Botany.

Botany. It's too bad that we don't know them, probably someone will have written a good obituary if they did die. They were a couple I believe. Did you ever talk to them?

Never.

He was very strange. But I think they may have gotten somewhere, grants or something; I'm not sure. Anyhow the field is so big now that there are issues that we talked about many years ago, and now people need to re-review. I just got a request to referee a paper which apparently is about the degree to which retractions are reflected in citations. In other words, if people retract their results ... do other people find out about it, to the extent that they stop citing the paper and all about the retraction? A lot of people don't even know that WOS does a pretty good job in recording those retractions. I think it was me and Marie McVeigh had to recently publish a rebuttal to something where they weren't even aware... it was in the *Annals of Internal Medicine*, and I think they published the letter we wrote pointing out that ISI has been processing retractions as corrections. It's a question of K coding; instead of calling them erratum, in some of the journals. We index all of those corrections as one code. But they're all there. They do a pretty good job of that when they find them. But that doesn't mean that people are going to find them, unless the two are linked. That was actually when you think about it, the original basis for starting out the SCI, When I wrote that paper in '55 I quoted... I think it was Professor Stanley or some from Johns Hopkins, about the fact that people publish errors and they go on being cited, that unwittingly people keep on citing the same errors over and over. Well, that's because they didn't have citation indexes to help them. Now, if you have a citation index, unless it's automatically linked, there's no guarantee that you're going to look for those corrections. In the early days you had to teach people that; you couldn't expect them to find the corrections, although any citation could potentially be thought of as a correction. Why are you citing some earlier work? Hopefully you have improved on it or done something different with it. But if it was a real serious

error and it was known, it doesn't have to be a formal correction, someone just has to say well, so-and-so was wrong or I disagree with it. I remember when Professor Czapski came to visit, he always reminded us of a paper which he wrote once which put a stop to a field. Everybody had been, citing some earlier work, which it turns out was completely wrong, and he then did this experiment which proved it was wrong, and then presumably citations to that earlier work stopped. But I never checked up on that. A lot of other people might have made the same claim.

Duplication in Research and a Citator for Patents

They did a study at ASLIB in the UK, back in the '60s, and established that, they made an estimate that researchers reported that 25% of their work was unwitting duplication. Now, I had to go back and find out what the definition of that was, but this came up last week, in the USA, is that true anymore? To what extent is research unwittingly duplicated today? I don't know. If you go on the Mertonian principle that all the simultaneous discoveries that go on, it's some sort of an axiomatic thing. I don't know to what extent, but I would think that it would be less unwitting duplication today; there might be a lot of deliberate, more deliberate duplication. The question is, if it's deliberate, is it moral if they don't cite what they know already exists? Now, if you're applying for a patent, then you know that you've not actually violated the law. When you've signed your application, you're testifying the fact that you think that this is original, and you've done your research. But I have always said that it ought to be a matter of... a standard of procedure when people publish, or apply, for patents, that they should certify what they have done in the way of verifying that this is an original piece of work. Now, the way things work today, sometimes you could find out if something was reported before easier, by just going up on some bulletin board, if you're willing to expose your idea. But, you know, you take a chance. If you say, an original idea, then somebody else grabs it! Well, it's tough. But if you're just trying to establish whether

something ever was done before... I'm always amazed at how quickly people give the answers to, do you? You know, that, like that Chem Info website... it's amazing how quick I get answers on some of that stuff. On the other hand, people are always willing to say there's plenty of examples of research that's duplicated. When I applied for a patent on HistCite, I got the patent, but when the application was filed in Europe, the same identical application, one of the examiners cited one of my papers against me! I thought that was the ultimate irony. But that was because they didn't really understand the concept. Not totally without rationale, that, to say, well, you talked about something like that 30 or 40 years ago; that's not the same thing as saying, actually, solve the problem. Right? So, that was a crazy kind of thing that the... patents were one of the earliest things that we studied in the way of the citation, the value of citation indexing. Back in the early days, I actually published one of my first papers, in a journal at the Patent Office Society. Even before that paper, which was about 1956, I think, Arthur Seidel – did you ever meet my friend, Arthur Seidel? – who's still practising law, by the way, he still goes in. He's now moved to a retirement community, but he's still - because his wife was tired of taking care of the house – he still goes into the office. He's, an incredible guy. He's a walking encyclopaedia. He published a letter in the journal of the Patent Office Society in 1949, on the idea of doing a citator for patents. Of course, the patent office never did anything about it at that time. But that's how old the idea is. I think that I picked up that letter of his and cited it in my paper later.

Citing in Patents and Papers

Marge Couraine was the librarian of, at Merck, I think it was Merck she worked for. She gave me access to a collection of 5,000 patents. She and a group of students, I think, coded all this stuff with me so that we could get a large enough sample to prove the point. In the paper, I had some examples of what you could find by doing a citation-based search versus using a classification system. But the patent office showed that the patents were classified all over the place by individual patent examiners, which... and would have

missed various things for a variety of reasons. One of the objections to the use of citations at the time was that some of the examiners cite patents not for technical subject matter reasons, but for legalistic reasons, which brings in, completely irrelevant subject matter. I mean, you could cite patents that have nothing to do with one another, in terms of what's being invented for some legalistic reason. I don't know enough about patent law to give you an example, but that was what was part of the problem.

Do, do you think that scientists cite previous papers, in its quasi-legalistic way, to get proper credit to prior authors?

Do inventors cite...?

No, I mean scientists. I mean, is there an analogy between the way people cite in a patent, the way a scientist would cite in a paper?

I think it's verifiable, that the era of biotechnology, that a lot of the biotechnology patents are read just like papers. The whole flavour of the description of the invention and what went before, and so forth. Essentially, it's sort of like, it's almost like a patent application. In a sense, that's the way it ought to be. If somebody's invented something, he's willing to tell the whole history of the development of that particular field, there are some places where it might not be relevant, but certainly, if it's something like molecular biology, you want to be able, because, I think it permits them to make a very narrow claim. The narrower claim you make, the easier it is to get it patented. What the patent examiner is really doing, in many cases, I think, is that he's preventing you from getting such a broad patent, that it prevents anybody else from entering the field, where you have'n't really... not necessarily covered all the fields. So that's part of the process of how much you disclose, and how well you search the background to the field.

Did you see a connection between the, people citing practices and the Mertonian norms of science?

Yeah, I think that, the success of the Citation Index does not depend on perfect human behaviour. I mean, that's what MacRoberts

would have you believe. Okay? The fact that people don't cite all the influences, or whatever they don't cite all of, okay? The collective literature, and especially at the speed of which it moves these days, resolves that problem, I think. Now, there can always be exceptions. But I think, generally speaking, people tend to cite sufficient literature, but there are a lot of people who are just either lazy or ignorant of what's gone before. I don't think it's a question, therefore, of morality, that they deliberately didn't cite something for whatever reason, or other. It's not like the case of Jim Watson and Avery-McLeod, right? Where he eventually admitted that he should have cited it, but how often that goes on, I don't know.

Well, it could have been a mistake, or just an oversight?

They didn't make a mistake. They knew about that work, and as John Maddox said in a subsequent article, somewhere, or interview, that if he had been the editor of *Nature* at that time, he would not have allowed them to publish it without citing Avery-McLeod.

So, why do you think they didn't cite him? Was it some kind of priority issue there, or...?

Why do I think...? That's a good question. Yeah, I think that, he doesn't make too many bones about the fact that they were, out to get the, be first. They weren't going to let anybody else's priorities get in their way. Now, I don't know if that was true for Crick, but it certainly was true for Watson. He still, to this day, I heard him give this talk, he still claims that, Rosalind Franklin knew nothing about DNA and she ridiculed it.

Errors and How They Affect Retrieval

A guy like Josh Lederberg who was an early proponent of open access type of publication, un-refereed publication; so if you want to stake your reputation on publishing junk, without it being refereed, great! It's your chance, if you're willing to take that chance, fine. So let the chips fall where they may. But, sooner or later, somebody else is going to call you out. That's one thing I have found: if people can find a mistake in your work, they'll be very happy to point it out. Right? Look, how much

junk was published about all the errors, the alleged errors in the literature, with all that group in Amsterdam taking, and eventually it proved to be 7%, was it. What's the definition of an error? When I said, what's... it doesn't matter how many errors there are, how does it affect retrieval? It's not how many errors there are, but how does it affect things? So, if somebody misspells your name, did it mean complete loss of the information? When, in fact, there are ways that we are correcting those kinds of variations, and so on. So the statistics of variations doesn't prove anything. Do you know the work of Chaudhry and Simpkins? These fellows are, I think physicists or electrical engineers, or some computer guys. They've published in JASIS recently, a paper that errors were being perpetuated, because people were... and what it turned out— I wish I had documented it at the time, I should have been more careful about that... I checked what their statements were, and what they didn't know, that if you look up a citation in the SCI, just because the variant that ISI uses occurs in the list of, let's say, 50 citations, that's not the way they occurred in the actual article, because we unify them. So if there's an error in one citation, it doesn't mean that it's repeated 50 times. They make that assumption, that the errors that were repeated were dupes of the authors repeating the error in the actual paper, rather than ISI unifying them under that. So, if you don't know the intricacies of the way the indexes are put together, you can jump to these false conclusions. But since then, I don't know whether they've corrected their work or not, but I see they're still at it. I just don't have the time and the energy to go through all the math these guys put up.

The Mathematics in Papers Today

Joel Hildebrand was a guy, who was one of the centenarian chemists, he lived over 100. I took freshman chemistry from Joel Hildebrand, he was the lecturer. Years later, he wrote to me what he found out about the Citation Index and he wanted to know if I could use citation indexing to prove that the physical chemistry literature had become too mathematical. People have these ideas; they're looking for some way to, solve, answer

to some question. I never could help him, but he had this, he thought maybe it would work that way. But, I think, that in a lot of fields, that happens, that they get so... I don't know if the word is, obsessed, or carried away with the mathematics that's behind all these things, that they lose the forest for the trees. I find so many papers today that I can't even understand the math at all. You know, it's just beyond me. Whether that's good or bad, or not, I don't know, but that's not the kind of measurement we were looking for when we first started out. Have you seen that, some of these papers? Well hopefully someone understands them. Some of the papers that are appearing in these journals, I don't know how they can read them.

H-indexes and Impact Factors

There are certain discoveries that have a certain kind of simplistic beauty about them. I think that H-index is not really telling you anything that we didn't already know, by using citation frequencies. It's just that it's a more convenient little number for people to use. It happened because when WOS started to publish the citation frequency ranking for... that made it possible. Before that, nobody could do H-indexes without a huge effort. It would never even have occurred to them, probably.

It's a short cut.

Yeah. So, it wasn't, we were looking at citation records for people for decades and recording how many were cited above a certain frequency and so forth. But didn't have this little nice little number. It's like a new impact factor, right? For authors. That's what people like – something quick and dirty they can do and... because now, a lot of people have pointed out that there's flaws in some of these H-indexes.

Well, they don't have to put together the whole bibliography; they can just verify the first 20 or whatever papers, and that's it. They don't have to go through the whole thing.

Right. So, I, I keep getting feedback from people who expect me to do something, or recommend something about what should or shouldn't be done with impact factors. It's like I'm supposed to, maybe what, come

out and say they should be banned? Don't use them for anything. So, you have nothing else to do? I've just got another invitation to write a letter, an editorial about impact factors, in a journal called *Industrial Health*. I'm going to send it to you and Marie to see if you want to write it. You could spend the rest of your life writing, writing articles on impact factors.

Create a new industry.

So, when I tell these people now, I say, look, if you want an article, go up on my website, pick any article I've written and reprint it. Do anything you want, I'm not going to write another one to say the same thing over and over again. But apparently, that's the way the thing works. I don't follow the rule. Hans Selye, in order to sell his idea, the general adaptation syndrome, he told 'I made no bones about it – I published the same ideas in any journal that would accept the paper from me', and he was successful because it became a very highly-cited and used concept. So that's the kind of thing you have to do. I mean, getting across a scientific idea is not just having a good idea, but selling it. So, we're a little past, I'm long past the stage of wanting to sell the Citation Index, okay? That's what we had the impact factor for. Other people now want to make an industry out of it.

Communicating Science

I was just reading, starting to read an article about, have you heard about, it's in the latest issue of *The Scientist*, which probably is the largest issue of *The Scientist* ever published. It's happened, but... they're talking about the... communicating science to the public and what succeeds and what doesn't succeed. Talking about framing science and referring, do you know about some Nobel prize winning work by [Kahneman and] Tversky? Yeah, I don't know anything about that.

The economist or statistician?

Apparently, if you don't, you've got to frame the issue in terms of the various contexts of the person's interest, you know. If you talk in the abstract terms, science doesn't appeal. Science has to compete with too many other competitive media that people are interested in. That's why they're not interested in science,

because it's not, I'd say, as much fun, as other things might be. So, getting across scientific messages is not an easy thing to do because it takes a little effort to understand science. On the other hand, if you can demonstrate economic impact, in a sense, I didn't even know that this stuff I've been trying to do about demonstrating the economic impact of biomedical research is what they're talking about framing people's interest in science. Maybe that's part of the problem with selling information systems, too; you've got to demonstrate the context in which it's helpful. I don't know whether there's a message there for getting people interested in the field of information, so when you asked me, how did I get interested in information – I don't know what happened. But as far as making it interesting for the newer generations, then maybe all these quiz shows do it for us. I don't know, whether you're good at 20 Questions, or Trivia or Trivial Pursuits, or other things. I don't know whether that gets people interested in the information at all, because the people that correct one another are always looking for ways to interest people and getting into the field. We were talking about that down in Tennessee. I don't know what I can tell them. How do you get people interested in, getting into this field? They're competing with other things.

Getting Your Kids into Science and the Role of Class in Science

I wasn't successful... at getting my kids interested in science. Maybe it's a kind of field that the only interest they've got to discover it for themselves. I mean, the ones that need to discover it, will do it. The others, may not. Although, there are plenty of people who are scientists who have managed to bring their kids up with a similar interest in fact, even in the same specific field. You look at Dan Koshland who just died. He's got a highly qualified son who's also a biochemist Dan... Doug is his son's name. Interesting homograph, by the way, DE Koshland; one's Dan, one's Douglas. But his son only uses a D to differentiate himself, whereas his father always said DE. That clearly had nothing to do with money or anything like that – that was just curiosity.

He was basically, a very wealthy... was born into a very wealthy family; Levi Strauss, I think was their... though, of course, science used to be the interest of the upper class. Think of all the scientists who came from the upper middle class and the, even the ruling class, in England and elsewhere. It's rather interesting that in the early days some of you couldn't afford to be a scientist if you were poor. So that's an interesting part of the history of science in the 17th, 18th Century. Although I don't know, somebody like Benjamin Franklin wasn't wealthy, clearly. But he managed to make that transition from being a printer's apprentice to becoming a founder of the American Philosophical Society.

Information Science; Grants and Awards

It's only been a fairly recent thing that even people in the other sciences have begun to recognise that information is important. But National Academy, for example, doesn't have a section on Information Science, or anything close to it.

They have a History of Science, don't they?

Section? In the National Academy?

They might. I don't know.

I think that economists get in there and sociologists, I guess qualify. I don't think they have too many. Who do you know that's in National Academy that's not one of the true, you know, one of the traditional type scientists?

I think Tom Kuhn was there.

Was he a member of the National Academy? In what section?

I don't know.

That's the point. But the American Academy, on the other hand, when they elected me, they put me in the Communications section. So, like in AAAS, they have a section team which includes communication, information and so forth. But it was always thrown together with computing in the early days; it's not, communication and information Science is more than that, because computers invade everything. Although computer science is a perfectly legitimate separate field today. It used to be thought of as Computer and Information Sciences at Penn. CIS is the

designation. Even my e-mail address is still at cis.upenn.edu.

Did you say your Citation Classics as being a public understanding of science type of...?

Oh! I think that those are, many of them, if they're written well, are extremely. We always try to get them to do that. It wasn't always easy to get scientists to write a non-technical interpretation of why a paper was highly cited. But when they are written that way, they are very interesting for any large number of educated people. I think that it could easily be done that way. If you were to pursue it further, given more space, I mean, we were limited to one page; it was less than 500 words, as I recall. It's very, difficult to explain all the ramifications of a paper. You know, one of the things that I always had a big beef with the NIH, and it wasn't until quite recently that they began to try to do something about it. I remember when Varmus was the Head of NIH. I asked him about this at an open meeting of the Research! America why don't you get scientists to write summaries of their research? Perhaps that would explain to the public the significance of their work. He insisted that they were doing it already, and I had absolutely no evidence. The people who were in charge of their communication group, I remember, sort of, paid lip-service to doing it. But I think, Zerhuni has done more about that. He's tried to get that done, but not any... I haven't seen this done in any systematic way. I think that just for the sake of convincing congressmen, I mean, if you wouldn't have that... remember the old, who was the senator who used to give...?

Proxmire.

Proxmire Prize, for what did he call it? Without an adequate explanation, people could just take the title of a grant and misinterpret the meaning of it, the significance. But, even the most technical...

Golden Fleece, was the word. Was that it?

Yeah, Golden Fleece – that was it. Exactly. The most technical subject could be explained in terms of what its relationship is to other things. Now, maybe mapping would help people but, I don't know that every grant that's given has that justification. I think that a lot of them are going to say,

well, I'm doing this just for the curiosity? There ought to be some room for that, but it's interesting to the scientific community to support it. Or, this particular guy is very bright and he should be allowed to do what he wants. Now, that's not just a grant; that's like what do you call the award – the MacArthur award – they give people money just because they're bright – let them do what they want with it. But that's not what the government's about; they're supposed to be supporting research that helps people in one way or another. So, since it is believed that investing in biomedical research produces greater wealth – in quotation marks – in a form of what, longer life, better life, healthier life. You can put a value on that, which recently, a couple of guys that won the Research! America Award were, his name was Murphy and the other fellow from Chicago, they placed a value on life, and there's definitely a very significant return on that research if it produces a longer life, or healthier life and so on. Reduced, I would expect, reduced expenditure for pain - improved pain. So, I think that, that's why I joined, ISI was one of the early members of Research! America, and then later, they dropped out. Now they're back in Thomson now a member of Research! America, and The Scientist is a member. I set up this award when they elected me to the Board, 3 or 4 years ago, and then Mary Woolley, I think does a terrific job as President, but the other is an Economic Impact award, which is now the fifth award that's going to be given out, the fifth or sixth coming up, in fact, in a week or so. I'm going down there to attend it next week. They usually have that, they're going to have a lunche for the award-winners and then the Board meets. So, what I said was, maybe they need to do something like that in the field of information. You get more interest in this subject, this... presumably, more information and education leads to better improved economic benefit. What can they do in the Third World? They say that health and education are the two significant factors in bringing self-sufficiency in the Third World, and not the kind of help that the World Bank and other people have tried to do. They should focus on education and health.

Hit Lists and Baseball Cards for Science

I've always encouraged editors and journals that they should try and do something about identifying their, the citation classics in their field. When JAMA ran that series of articles, milestone papers, they called them and it turned out to be that many of them, but not all of them, are highly-cited classics. I think, ISI could do it, and do it not only for highly-cited papers, but highly-cited books too. Very viable, if you think about, what the public library wanted to know which were the most cited books and they aren't always going to be the ones that would necessarily point to what should be kept. But I can't imagine that a citation classic book would not be kept on the shelf for quite a while. Apart from determining that something is a classic otherwise. A hot paper, kind of, sounds temporary, doesn't it? I mean, it should be because it's hot for a while. How long, does it remain hot forever? A classic, I guess, eventually becomes, a classic is important whether it's useful anymore; it's interesting historically, it was a classic, and therefore it's something you should know about as an important, historical event in science; something that gets cited and is often used, clearly ought to be of interest to a lot of people. I think we have a capacity to read the titles of thousands of classics, if we keep at it long enough. That's why Current Contents, every week, you were getting a list of highly-cited papers. Well, how long does it take you to read that list? It tells you something about what's going on, of all the things you could read. So lists of hits, hit lists are very popular, especially in music, so why not science? Something else I think that project that I always wanted to encourage in advertising – somebody ought to do more about putting out the equivalent of baseball cards for scientists. You can get card decks of scientists, so you could get kids trading in Nobel Prize winners, I'll trade you two Joshua Lederbergs for three Albert Einsteins. That ought not to be so difficult to do; I think it would be a great thing to promote the product, some product. It's the kind of thing that Ellis Rubinstein would probably like to do for the New York Academy of Sciences. He's turned out to be quite a promoter for the Academy.

The Future; Information Nirvana

We talked about information Nirvana, I suppose, everything that's ever been published, if not everything ever written, will be available electronically, if they get around to it, scanning everything. Then, who knows? But it certainly would seem that the size of the memories, and the cost of the memories, is so incredibly cheap. It's still going, there's Mooers' Law, it's still operating under the Mooers of Intel. There's not a lot from Calvin Mooers, but I think that probably it'll happen in the next 20 years, that you'll be able to have the entire library of Congress at your fingertips. Truly, you know. What you do with it at that point, I don't know. But I know that I still, don't have an iPhone, or Blackberry, or whatever it is, and when I'm in my car, I don't have access to the internet, because I'm too cheap to pay \$40 a month, just to access the internet in your car? You should be able to, right? Because there's a lot of things that come up and I want to answer a question sometimes, like whether it's, during a conversation, or during a crossword puzzle, or whatever it is, you get something that comes up, and you say, for God's sake, why can't I get access to it without all the difficulty? Some people do it, if you pay for it. If you pay enough, you can get it.

Economic Issues Around Open Access

How are we going to solve the economic issues that surround open access?

Well, that's a problem that's been around for a long time. I think I've talked about that before that it was a problem that basically came up when JD Bernal proposed the central repository idea for reprints. The traditionalists will be opposed to it. Probably some kind of hybrid will eventually evolve, but I see now that Sigma Xi just put out an announcement about the idea of the current president, or somebody, wrote to me as a member of Sigma Xi – membership at large – I'm not a member of a Chapter because I didn't get elected to Sigma Xi as part of a Chapter when I was at school or anything like that. I just, became a life time member of Sigma Xi. Now they want to talk about a Sigma Xi journal of multi-disciplinary research, which would be their brand of,

Nature or Science. They all along, missed the boat on things they could have done with their membership; they were relying on the American Scientist – it's a nice little magazine, but it doesn't evoke peer view, if I can put it that way. Whereas getting your stuff in Science and Nature, it does. Now they want to do this multi-disciplinary journal, and whether they succeed with it or not, I don't know. But it's not necessarily an open access one. Basically, the idea is to have things available free of charge to the public. That's what they mean by really open access. It's that people can get at it free. Whether people can get at things free or not, it's going to happen in the next 10 or 20 years, I don't know. I cannot easily see the publishing industry ceding multi-billion dollars in profits that they're making out of, so called, value addition that they do. Open Access means you replace that with what, volunteer work? There's a new word they have, you know, this term they use for things like Wikipedia it's called, community authorship.

Collaborative?

Collaborative, or there's another word. For work like, Wikipedia, I think that they, the arguments for and against it, we talked about that, a thing like Wikipedia and so on. It's not properly authenticated; so unless you get things that are authenticated, they're not going to be accepted, and you won't get them authenticated unless they get people paid for doing the work that's required. You can't just get it out of volunteers. I don't know. It depends. Within the professional society, you get an awful lot done by volunteers, if you think about it.

They're going to list the author names, aren't they?

Are they going to what?

The authors' names... that was one of the proposals...

In Wikipedia? Well, I thought in the beginning they were doing that, but I could be wrong. I don't mind having my name put on something if I've written it, or proved it, or whatever. But if you get a collaborative authorship - that's the word - collaborative authorship; it's when you do an article where everybody approves it one way or another.

Citations as Currency

The Mertonian philosophy is basically that science is a communal or communist type activity, you know; it's something you do for the community and you get paid with, your reward is citation. Recognition is the reward, not necessarily true monetary compensation. There's people now who are rediscovering Bob's idea that citation is a form of currency. Was he the first one who talked about it that way? What's your recollection? Citation as a form of currency?

He, and maybe Warren Hagstrom.

Hagstrom. Right. There's some people now who are circulating the idea of citation as a financial reward type of idea. It's an economic activity for, you know, the more citations are going to get you, say tenure; it'll get you a higher salary, when, in reality, in East Europe and other places where they implemented these systems based on citations brought you more salary and so forth, well, that's an economic reward. I certainly didn't think of that as a... I believed that citation was an emotional compensation; you got a great thrill out of having your work cited, or quoted. Nothing wrong with that. I mean, that's something we, just as long as you don't go overboard and get obsessive about it. That's what we want people to see, that kind of recognition without hurting other people. But, we know about all kinds of abuses that can exist and do exist, but they don't really account for that much in the way science devolves because lousy ideas eventually just fall by the, don't get cited. Negatives that don't get you too far.

Big Science and How Things are Changing

Big science is here to stay. Where are you, how're you going to do things like the Manhattan Project, or NASA, or the Human Genome, or other things like that without big science? It doesn't mean that there won't be room for original ideas that individuals have. It's hard for me to imagine that stopping. It's hard to say whether, how much an individual need to have expensive equipment or other things that permit you to make new, interesting discoveries. I don't know what that one main fact would be,

but I suspect you'd be able to, with the access to knowledge being what it's going to be, whether free, or slightly paid, I can't imagine it being all that expensive to come up with information, so originality will certainly come up.

The technological infrastructure now is so vastly improved. I mean, the methodologies, the techniques which we see...

To what extent is it going to be – what's the word I want? – you know, I can use a TV set by turning it on and doing very simple things, but some of the complexities of using some of the stuff on the Internet, and just using my PC, it seems to me, it's getting to be, in some senses, overwhelming. I mean, I, maybe it's because I'm, my generation, it's gone beyond me. But I just find there's so many new twists and turns that you've got to learn. But, the only solution for me, as I've just said, I call my assistant in and I say, solve this problem for me, I don't want to have to figure out why this thing is telling me it won't do it, or it will do it. Or ISI's email system rejects this message and won't let you do that. I mean, it's just unbelievable, the things that you've got to keep track of, when other people don't seem to mind that. It's unbelievable the number of, and then if you change anything, you're going to go from Windows, what is the number I have on Windows 99 or 98, and I go to Vista, and then you find out you can't use this, if you've gone through the norm. It's just... all that is maddening. I just find it, because I just don't want to have to learn all that in order to use it. So they're going to make it, you know, with your car, you don't have to really learn how the damn computer in the car works, right? But you've got to be able to afford to take it somewhere to somebody who can figure it out. But just the number of things that happen on a PC are incredible. So, it's not surprising that people have made a big business out of, what do they call it? The geeks? There's a huge success of a new business called The Geek Squad. I know who to call to get help, but if I didn't know these people, I would have to go to something like that. You'd probably wind up having your computer serviced by some guy in India who's doing it. As a matter

of fact, when we were calling, I went to visit a friend who's PC got disconnected from... she couldn't get on her AOL account, and she didn't know how to deal with it, because her computer is hooked up to her telephone line, so in order to get the AOL, her phone's got to work, okay? Now, if she calls, she can't use her phone to call AOL, because she can't get onto AOL. So, I said can't you find a friend who's got a cell phone to call AOL? So, when I got down there, and went to visit – this is an old, friend of mine; her name's Anne Geary. I don't know if you've heard me talk about her – we got there, we called, we called AOL on the phone, and we were talking to some guy in India for over an hour! He couldn't figure out what was wrong, because what was happening is that it turned out that they assume that the modem in the computer is not working, and therefore not linking to AOL. Now, how could you, how can any ordinary person figure that out? There's nothing on the damn computer that says your modem isn't working; you've got to be pretty technically savvy to figure out what the hell to do. So, they call in The Geek Squad, the guys who... you're better off... her computer's 10 years old, so I said, go to the damn, the, not Circuit City, but the place where you buy, I think she got it through some place called Computer World, or whatever it was. Go down there and get this damn thing, and trade it in; they'll just junk it. And get yourself another computer; it'll be cheaper than trying to fix your modem on that one, you know. That's another subject – we're living in a throw-away world, right? So, and she'll probably, for one-fifth of what you paid for the original computer get a 10GB computer now, for less than the \$500, something like that. But all that technological stuff is why I can't, I tried to get my sister to come on email. She refuses, she will not try to get, to have her own personal computer that we can email. Even though she worked in an office and knew how to use a PC in the office, but she doesn't want to have to deal with the problems of email and, getting her own computer and being helped out. I did manage to get her to accept the answering machine, so when you call her phone. I also bought her a cellphone.

Just gave it to her. Now she uses it. So, that's what you've got to do sometimes to get them information systems, you've got to just stuff it down their throats.

ASCA Selective Dissemination System

We had the very first commercial SCI service; there's no question about it. And that, most of that, the credit for that should go to Irv Sher. Okay? Now, here's a system that we came up with in one year of the launch of the SCI. That paper, we published on ASCA in the *American Behavioural Scientist*. The ASCA system was announced in, '65 or '66? It's a story I always like to tell about the ASCA system. We, Joan Shook – did you know Joan Shook? – she died. She must have died more than 20 years ago. But Joan was our advertising manager and Joan created this advertisement which looked like a Rorschach test? What it was, was a one page ad in *Science* magazine. In the ad, it had this very abstract picture of a guy or a woman or man, with their eyes closed; you could see this first one. It said, 'Close your eyes. Imagine an information retrieval system designed just for you'. Then it said, 'Open your eyes. ASCA!' Now, I was told that this advertisement pulled the greatest number of inquiries in the history of *Science* magazine. We got, I think, if I remember, in those days it was a lot, like 3,000 enquiries, or something like that. So, we sent out, people wrote in and they even entered subscriptions, and we sent out a response to the subscription. We had the application form for ASCA, you had to fill out your profile. Well, a certain number of people answered it, but after quite a while, weeks went by, and these people were not sending us their profiles. Irv and I decided, we've got to do something about this because, at this rate, the thing is going to collapse. So we decided, we'd take a couple of dozen answers ourselves, the enquiries response, and call these people and see what the heck was going on. Well, it turns out that a substantial number of these people, and these are research scientists, mind you, not, lay people either, people had literally interpreted that ad meant that they did not have to do a darn thing, that we would know exactly what to send them. They didn't have

to fill out the profile. So I said, 'Irv, we're going to have to fill out the profiles for them'. It was like turning a motor. When we started the SciMate system, we had to teach people how to use a, whatever they called them those days, a computer, right? They didn't really even know. So, what we decided to do, which, later on, made a lot of sense, is we looked up, we knew that practically all of the responders were authors of papers, right? Otherwise, why are they using ASCA? Maybe somebody in industrial intelligence department would get it, or marketing product, but generally, they were all authors. We looked up the guys' most recent papers and we created the profiles based upon their reprints. Both the titles of the papers and the addresses, and what they cited. Do you know, we created dozens and dozens of these profiles, started sending ASCA to these people, and there were some people who did not... and even though ASCA had, on every weekly ASCA report... remember you had a place where you could change your profile, you could add to it, you could delete – there were people who used that identical profile for ten years. Not one change. In a certain sense, I can understand it, because I created a profile for myself, that I continued to use, with ASCA, with very few changes, I occasionally work in an additional name, or something, I used it for 35 years. When ISI asked to discontinue my ASCA report, I was one very unhappy cookie. I mean, they put this personal ASCA, they have now, but it doesn't do anything what the old one did. I mean, there are so many different things that are missing from it, including just the mere identification of what you're, what caused the hit. You don't get it. Like, sometimes you do and sometimes you don't. So, it never, if we had to live off of the ASCA system, we'd have been dead in the water a long time ago even though people do use, now the profile system. You can get a profile off of WOS; you can get a profile off of Google; you can get profiles in the early days, they called them push-pull systems. Do you remember that? Here we had this service, and that was a terrific service, and I wanted to give that service to every academic at Penn. Where, 30 years ago, when Dick Degenario was the

librarian, I offered that service to them. They absolutely refused to accept it because, first of all, and I said we'll give you help with doing profiling, they said, they didn't want to be inundated with requests for photocopies of papers. They didn't have enough staff to deal with it. So, to this day, I don't know of any place that really has a universal personal learning service, that they systematically give to every member of staff.

What Scientists Want

It's a very interesting phenomenon, but I'm sure there are people who are getting profiles of one kind or another, but it's nothing like that. So you never know what, people want ... when you ask people what they wanted, this is what they said they wanted. But when it came, when push came to shove, basic research scientists don't know what they want. Because somebody said, 'If I knew what I wanted, I would have made the discovery'. It's a kind of an interesting conundrum, where you try and supply them with something that, would be the ideal document, that would solve the problem that they're working on. Well, who wants that? It's like the, we used to talk about, do scientists really want to do literature searches? Effectively, when I used to, when, we used to talk about it this way, I said to a librarian, go into a library, and a guy is saying, do you think the average scientist comes to you, and he's happy when you come up with papers that anticipate his ideas? He's looking to you, for you to prove that he, that it's a novel idea, not that it's unoriginal. It's the same thing with a patent. What's the motivation for getting somebody to do a search for prior art, when he knows that if he does this search completely right, he won't get the patent. Right? The last thing I want to know is that it's not an idea, an original idea. Same thing when people were doing searches on compounds. They weren't really all that well-motivated. So, you've got to force them to do it. That's part of the dilemma that we face in using information for discovery processes. So... you eventually solve it. But people like Josh Lederberg always pointed out later on that if you're a mature scientist, eventually you get over that. His talent would be, and people like him, in framing questions

for which you give them partial answers. I mean, I'm interested in what people cite – if they cite my work, it's about citation and indexing work, or it's about impact factor, or it's about this, but it's not necessarily the answer to some fundamental question I'm trying to investigate. If it turns out to be the case, well, great. If you're working in a field of genetics, of course, if somebody answers some fundamental question, well, that's what you give people prizes for. But you don't get upset by the fact that somebody like Jim Watson, identified the double helix; you go on from there and then you go to the next step, right? So, it's your attitude, being mature about how you use and await discoveries. There's plenty more to be discovered after your great idea is proven not to be so original anymore. It's a good idea, but it isn't original. That's what makes science go around. I see that, I was recently asked about a paper that Tibor Braun has got, somebody submitted a paper, I think, about doing an analysis of words that are sound like, new or novel or creative. Irv Sher and I did a study years ago, how often people use the word 'new'. Every scientist publishes something; he thinks it's new and novel. There was a lot of discussion among editor's list as to whether they should ever even let people use the word 'novel' in a title. Should you have to say it? If it's novel, that's why we're accepting your paper. But some people... a novel method for doing this, or a new compound for doing that. So, I don't know how much that's changed or not, but that, that's what motivates people in one... that priority of discovery is not just for patents.

Summing Up My Career

People who have tried to give me credit. I was never really all that much of a theoretician, although I would have liked to have done more, discovered more, if I can put it that way, laws. Some people, when you talk about Garfield's Law, or Law of Concentration, that was the, versus Bradford's Law of Scattering. People like Steve Bensman understand the difference, but not too many other people ever think about that. The thing about Garfield's constant is not a constant.

It's a ratio, okay?

But it was kind of a useful way of expressing certain ideas. Have you looked at that recently? About, it's still going up. I think it's a function, the size of the literature; it's going keep on going up. How often, and what the average number of citations per year, to what... the average cited item is cited, what, two and a half, three times a year? Something like that.

Two point something. Or maybe it's three now.

It's going up. And so, what else? Do we have any Small's laws? We've got... so, what can I say? I like to think that it's been useful. And that it will carry on. I think that information, science is an interesting career—information engineering and so on. So are other things... what else can you say? You like to think that you spend your life doing things that help improve the condition of man. There was a... VI Lenin was a one who had this, there was a quotation that used to be used all the time that, you know, when people said, what did you do... 'As long as I live my life so I can say that it helped in the liberation of mankind'. How do you interpret the,

liberation of mankind? It depends on where you are, and how tied up you are. If I was living in Tsarist Russia, I would understand what the liberation of mankind meant. Here, I don't know what we're going to be liberated from, but... so, I think that, the challenges will remain and we'll... will always be, I hope it will always be an interesting and useful question that people will keep on answering. There will never be an end to the number of new questions that can be asked. So the unanswered questions of science. We used to call that TUQOS – remember that? That was another one of the projects that we never finished. We were going to do a project which would formulate the TUQOS, the unanswered questions of science, and keep on putting out, like a Guinness's Book of unanswered questions of science.

Notes and References

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About Web of Stories

Web of Stories offers the chance to listen to some of the greatest people of our time telling their life stories. Web of Stories began as an archive of life stories told by some of the great scientists of our time. As the number of stories grew, it became obvious that some were on related topics and a web was slowly created of connected stories. After a while Web of stories also invited famous people outside the field of science to tell their life stories. The aim of Web of stories is to provide an archive of stories from people who have influenced our world. Imagine, in a hundred years' time, future generations being able to watch people like Stan Lee, Doris Lessing, James Watson or Eugene Garfield telling stories about their lives and their achievements.

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Inspiration and Strength

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