THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

EUGENE GARFIELD

Transcript of an Interview
Conducted by

Arnold Thackray and Jeffrey L. Sturchio

at the

Institute for Scientific Information
Philadelphia, Pennsylvania

on

16 November 1987

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY
Oral History Program

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(Revised 20 February 1989)

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EUGENE GARFIELD

1925 Born in New York City on 16 September

Education

1948 B.S., chemistry, Columbia University
1954 M.S., library science, Columbia University
1961 Ph.D., structural linguistics, University of Pennsylvania

Professional Experience

1949-1950 Research Chemist, Evans Research and Development Corporation
1950-1951 Chemist, Columbia University
1951-1953 Staff member, machine indexing project, Johns Hopkins University
1954-1960 President, Eugene Garfield Associates
1960- President and Chief Executive Officer, Institute for Scientific Information
1974- Adjunct Professor, Computer and Information Sciences, University of Pennsylvania

Honors

1953-1954 Grolier Society Fellow, Columbia University
1975 Award of Merit, American Society for Information Scientists
1977 Book of the Year Award, American Society for Information Scientists
1977 Hall of Fame Award, Information Industry Association
1977 Herman Skolnik Award, Division of Chemical Information, American Chemical Society
1983 Patterson-Crane Award, Dayton and Columbus Sections, American Chemical Society
1983 Chemical Notation Association Award
1983 John Price Wetherill Medal, Franklin Institute
1984 Derek de Solla Price Memorial Medal, Scientometrics
1988 Doctor Honoris Causa, Urije Universiteit, Brussels, Belgium

ABSTRACT

In this interview Eugene Garfield begins with his early years in New York, including his family background and early education. This is followed by his interest in the West, his first jobs in Colorado and California, and a brief military career before his medical discharge. Garfield continues by discussing his undergraduate degree in chemistry, how his first ACS meeting and the Division of Chemical Literature influenced his life's work, and his participation in the Welch Medical Library project. The central portion of the interview focuses on the origin and development of the Institute for Scientific Information and its products, including how the major publications began. Garfield also describes his doctoral studies at the University of Pennsylvania, and the difficulties in having his thesis approved. The interview concludes with a discussion of his patents for a copying device, an analysis of the successes of ISI, and speculation on ISI's future activities.

INTERVIEWERS

Arnold Thackray majored in the physical sciences before turning to the history of science, receiving a Ph.D. from Cambridge University in 1966. He has held appointments at Oxford, Cambridge, Harvard, the Institute for Advanced Study, the Center for Advanced Study in the Behavioral Sciences, and the Hebrew University of Jerusalem. In 1983 he received the Dexter Award from the American Chemical Society for outstanding contributions to the history of chemistry. He is Director of the Beckman Center for the History of Chemistry.

Jeffrey L. Sturchio received an A.B. in history from Princeton University and a Ph.D. in the history and sociology of science from the University of Pennsylvania. He was Associate Director of the Beckman Center for the History of Chemistry from 1984 to 1988, and has held teaching appointments at the New Jersey Institute of Technology, Rutgers University, and the University of Pennsylvania as well as a fellowship at the Smithsonian National Museum of American History. After a sojourn on the senior staff of the AT&T Archives, Dr, Sturchio joined Merck & Co., Inc. as Corporate Archivist in June 1989.

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LOCATION: Institute for Scientific Information, Philadelphia, PA

DATE: 16 November 1987

THACKRAY: Gene, can you tell us something about your family background and very early years?

GARFIELD: I was born in the Bronx. My mother and father were children of immigrants. They were both born on the lower East Side in Manhattan. My sister Sylvia is two years older than I am. I am the second child. My mother was married at the age of fifteen and she was eighteen when I was born. By that time, however, my mother was not living with my father. He left her when she was pregnant.

THACKRAY: What were your mother's and father's names?

GARFIELD: My mother was Edith (nee Wolf). My father was Henry Garfinkle. My sister and I lived alone with my mother who worked at various jobs. In 1925 I was the first child in what was called the "First Ladies Jewish Nursery" in the East Bronx. It was on Fulton Avenue, not far from Crotona Park. I believe it is still standing, but not our tenement. Not too long ago somebody actually sent me a photograph of the street that I lived on (St. Paul's Place). It is unbelievable to see some of the changes, but most of those tenement buildings are still there, although across the street, the projects have come in. Even though I only stayed there until the age of five or six, I can still remember that neighborhood as vividly as yesterday.

My mother worked during the day, and I went to this nursery. She got a little bit of help from her brother who was not yet a budding clothing manufacturer. He was only a few years older than she was. With the encouragement of my uncle we moved to the West Bronx, which was a vastly improved neighborhood. I don't know how you would describe the East Bronx today. I don't think it was like a ghetto. The tenements or the slums weren't anything like they are today. At least it doesn't seem that way. Maybe it was because so many people lived that way. In any case we went to a more modern neighborhood in the West Bronx called Highbridge.

In the East Bronx we attended the public school. Although it was Public School Number 2, it was originally Number 1. I don't know if that means Public School Number 1 in the Bronx or in the City or what. When we moved to the West Bronx we attended Public School Number 11. We always walked to school. It was located three or four blocks from where I lived on Woody Crest Avenue. I've written about this somewhere; there was a branch of the New York Public Library on the same street (1). I think it was called the Woody Crest Branch but I'm not sure. It was where Woody Crest and Shakespeare Avenue came together at kind of an angle. That was very significant in my life because I used to spend a lot of time in that library. It was like a haven for me.

As I grew up we were poor and I tried to earn money doing little odd jobs. That was not unusual. My mother got very angry when she found out once that I worked for a quarter for a whole day in a local laundry helping run a press and a mangle machine. A mangle is what they called it. I also delivered orders for the corner grocery store. My mother always bought on credit until her next check or pay.

I was very much under the influence of my mother's five brothers. The youngest is only six years older than I. (In fact, he visited me here in Philadelphia about a month ago. It was the first time he had ever been here.) On the other hand, my oldest uncle was kind of a father figure in the family, although my grandfather was alive. I had very little contact with my biological father. The first time I saw him was when I was five years old. I once visited his mother when I was a few years old. I remember being very frightened of that experience.

As I indicated, my uncle became the father figure in my family. In particular, three of my uncles were radicals, involved in labor organizing and socialist-communist politics. There were two sides to the family. Two uncles were capitalists and they were always opposed to that. So there was a lot of turmoil going on. They cultivated my interest in science, classical music--in fact almost everything. Atheism was part of it. I was discouraged from going to any kind of religious school, so I did not go. Most Jewish kids of that age would have been Bar Mitzvahed, but I was not.

The next male influence came into my life when my mother married a second time. She married an Italian man by the name of Ernest Garofano. Eventually my half brother Ralph was born. He is about twelve years younger than I am. After their marriage we moved across the street to a bigger apartment. My stepfather significantly influenced my life. He was not an intellectual. Indeed, I don't think he finished grammar school. He was simply a very good, loyal, loving, and attentive person. He was very kind and assumed the role that my biological father had not. When he died I wrote an essay about him which expressed my gratitude even though I realize he often had a negative influence on my mother.

I finished grammar school at about the age of thirteen, a year ahead of the usual age. I attended Peter Stuyvesant High School for the first year. I had achieved high grades on the Regents' exams. I always had good grades in math. I used to take the elevated "subway" all the way from 167th Street down to 14th Street at Union Square. It was quite a long ride, going back and forth every day. During certain seasons, I could also work for my uncle. His firm was located at 37th Street. At the same time I had the ignominious task of visiting my father's brother Louis every week in order to pick up our support check. His wine and liquor store was located in Greenwich Village. I could walk from Stuyvesant on the East Side to the West Side in about fifteen minutes. Later on I would have to do this every Friday even though it involved a trip from 210th Street where De Witt Clinton High School was located.

THACKRAY: This was from your father?

GARFIELD: Yes. He did not condescend to put the check in the mail. That was too much trouble. So I developed a relatively close relationship with my Uncle Louis, a man whom I continue to see occasionally. He ran a liquor store in Greenwich Village. From the age of about nine years on I had to make a weekly trek down there by subway. During the summers I worked for my uncle in the garment center. This is a very hectic time of the year for that seasonal business because they are getting ready for the winter season. It was strange to be handling fur-trimmed coats in that sweltering heat, and there was no air conditioning.

I took clarinet lessons at Stuyvesant High School. I remember buying my first clarinet somewhere down on the lower East Side. When I started working in the summer for my Uncle Sam, my mother's oldest brother, I made thirty cents an hour. That was the minimum wage. One day I said to him, "I'd like to get a raise." And he said, "Why?" And I said, "Well, I want to buy a clarinet." He said, "I'm not going to give you a raise, but as your uncle I'll buy you a clarinet." [laughter] I think it cost about $15.00. The brand was Peddler. I think they were made in Elkhart, Indiana which is famous for musical instruments. It was there I learned to concentrate regardless of the noise around me. There were dozens of other students practicing in the balcony of the auditorium. To this day my wife cannot understand how I can work and read regardless of the activities in the house. Indeed, I prefer to have my work space in the family area.

For a combination of reasons, I transferred schools. I didn't particularly like the extra travel, but I also wanted to study a second language. In the program at Stuyvesant you had to take more science and less of the other humanities. I wasn't too excited about biology, and I was very clumsy in the lab. So I went to De Witt Clinton High School in the Bronx, which is at the other end of the city. I graduated from Clinton, but not without

some problems. I was not a superstar student by any stretch of the imagination, although I did well in math and languages.

In 1949 my uncle Nathan and his wife Dorothea decided to drive to Miami Beach, Florida. In spite of the fact that I would miss school I was allowed to go along on this trip. As a consequence I subsequently failed one or two courses in history and another subject. I had to make up the credits in summer school. I enjoyed this and had to walk over the bridge to Washington Heights every day. Failing also put me in a more pragmatic frame of mind. So I took as many easy courses as possible. You could take courses for credit in typing, office practices like printing and multigraphing, and bookkeeping. All have proven very useful to me in my career and life. I became a supertypist, and I've used that skill many times in my life. My instinct for printer's ink was not accidental. Eventually I did graduate. My best mentor in High School proved to be my senior English teacher--Mr. Wilmer Stine. I "adopted" him as my hero. He had been a journalist and even interviewed Jack London. Later on I would visit him in 1951, only to be disappointed that he didn't want me intruding into his retirement.

I always had this hankering to go West. I read Zane Grey stories a lot. I had an overly romantic notion about life in the West. I applied to the University of Arizona and the University of Colorado in that order. I was accepted at Boulder. I remember the train ride in the summer of 1942. Before I registered as a chemical engineering major I had a short job in a construction camp in Pando, Colorado, near Leadville. That chapter in my life was significant but can wait until another time. I was a laborer and helped construct the Army camp in which I would one day be a soldier.

THACKRAY: Why did you select chemical engineering?

GARFIELD: That's a very difficult thing for me to explain. I don't know why I chose chemical engineering as a major. I considered going to the Colorado School of Mines. I don't remember if I actually applied. My interest in mining was influenced by the literature my uncles gave me about the Wobblies and the old labor movement. Wanting to be a mining engineer was somehow more romantic and macho in those days. I learned later in life that working in construction, unless in the mines, could be hell. At one point I worked as a laborer in Leadville, Colorado.

My uncle Bernard Wolf had a chemistry set. I never had one myself. Oddly enough, though he had a great aptitude for science, he wound up getting a business degree. That was a great disappointment to me. None of my other four uncles graduated from college. In college, as an engineering major, I had to deal with courses like mechanical drawing. I never found that kind of thing very interesting. Even in high school I didn't

have much aptitude for it.

I met my first wife in Boulder. Faye [Byron] was seven years older than me. She was a physics major. I first saw her in the chemistry lecture hall. I didn't know that she would be introduced to me later by another student. I was invited to a party at her house.

My uncle Hy had a friend who was a union organizer. His name was Carl Campbell. Carl eventually became a professor of economics somewhere in the Midwest. He was a very interesting man. He was an albino. I went out to Colorado after graduating from school. My uncle said, "Go see Carl. He'll take care of you during the summer." As a consequence of that, he got me a job down in Pando, Colorado, not far from Leadville. Carl got me a job as a laborer in a huge construction camp. The government was building an Army ski troop camp. It eventually became Camp Hale. I stayed there for only a couple of weeks and then I got sick. Eventually I went back to Denver. It was quite a harrowing experience for me, never having known that kind of physical labor. The altitude was over 10,000 feet. We worked twelve hours a day and the temperature extremes were awful. But I survived.

I went on to Boulder, but I remained in school for only six months or so--one semester. (There are a lot of details left out of this.) My father [biological] had agreed to pay for my college tuition and when I said I was quitting he was very upset about that. I was having personal problems. I was in love and then I wasn't. I wanted to get away. The war was on. I got this kind of patriotic fever but I wasn't quite ready for the Army. I was seventeen when I went to college so I wasn't quite eighteen. My wife's sisters were in California, so I went to work in the shipyards at Sausalito and was trained as a welder. Eventually I went back for a while to work in Denver in an inland "shipyard." I knew that I was approaching draft age. At the inland shipyard they pre-fabricated sections of the Liberty ships. I worked there as a welder. I had a friend from Boulder, Richard Aspinall, who was the son of a Congressman. We worked together there. Eventually he joined the Merchant Marine as a radio operator.

I went back to New York and joined the Merchant Marine. But before I went to the Merchant Marine Academy, the draft board wrote me. I was a young and naive. When the draft notice came I thought I had to respond instantly. Like a darn fool, I went there and they drafted me before I could start at the Merchant Marine school. I remember the induction center at Camp Upton, New York. I wound up in the Infantry. During basic training I found out how my typing knowledge could help me. The top sergeant offered me the job of company clerk. He also liked the food parcels my mother used to send. But I turned down the offer.

After basic training you could volunteer for the paratroops or for the ski troops, that is, the mountain troops. I volunteered for the 10th Mountain Division primarily so that I could get back to Colorado see my girl friend. They accepted me and I went back to Camp Hale, the same place that I had helped to construct less than two years back. I wasn't in that outfit very long. The 10th Division was moved to Texas for jungle training against the Japanese in Burma. At Camp Swift, Austin, Texas I applied for Officers' Training School. I was transferred from Texas to Officers' Training School in Fort Benning, Georgia. I was there for about thirteen weeks when the Battle of the Bulge began. I was transferred to the Port of Embarkation in Maryland.

I never did finish Officer Training School. When I got to Fort George Meade I was sent to the hospital and diagnosed as having ulcers. I will never forget the Army Major, that is, the hospital doctor. He asked my mother to come down and visit me. The doctor said to my mother, "Mrs. G., I have some sad news to tell you. I am going to have to let your son out of the Army." [laughter] My mother started crying. He thought that she was really a superpatriot. But it was already April of 1945, so the war in Europe was practically over. But everyone was still concerned about the Japanese.

When I was discharged from the Army I went back to summer school immediately and started planning for college. I went to Columbia University that summer and studied French. Then I applied to school. By this time, and during the time I was in the Army, my wife, Faye, was already at Berkeley. She had graduated in Physics at Boulder. She was working at the Lawrence Radiation Lab and had switched from physics to medical physics. Eventually she became a premed. When I arrived in California, we were both premedical students when we were married. She was a whiz student who got straight As, with minimal studying. I struggled through most of the courses. My grades were average.

We were at Berkeley for two years. I took my first chemistry course with Joel Hildebrand. As a matter of fact, as I described somewhere, I remember Professor Hildebrand very well (2). In fact, we had some correspondence long after I graduated. He wrote to me once about the the Science Citation Index (SCI) and wanted to know if I could help him use it to uncover what he thought was the over-mathematization of chemistry. I don't think I was very helpful to him on that. I took all the usual undergraduate premed courses. I was not a particularly good student. My oldest son, Stefan, was born at the Stanford University Hospital (in January 1947) which was then located in San Francisco.

We then came East because my wife was accepted at Temple University Medical School. When I got to New York we moved into an apartment my mother had had. My wife had previously announced that she was leaving me to marry a close friend of ours. They are still married, God bless them. They were obviously meant for each other. He was a physicist that she had met out there during

the war. I've never known to this day why she married me when all along she was in love with him. But people do these things. She met him in 1944 or 1945 at Berkeley. I guess it was in the lab. He is a very nice guy and was a protege of [J. Robert] Oppenheimer. He eventually got his Ph.D. under him at Princeton. I don't remember all the details. It was a very, very difficult time and as a consequence of that I obtained the custody of our child. I guess Faye and Marvin felt it was unpatriotic and couldn't at first confront me. Loyalty does strange things. Sometimes the painful truth is necessary.

THACKRAY: That was very unusual for that time, wasn't it?

GARFIELD: Yes. Although actually she probably preferred it that way, that is, my having custody. She was going to medical school, and I'm not sure how she would have managed. In fact, I had a stepson Michael. She had been married twice before. She was a very unusual woman. Faye is seven years older than I am. She sent Michael, age five, to live with her mother in Colorado. She made a deal with her mother. Grandma was happily willing to help support her through school as long as she agreed to leave the child there to live with her. She would probably have done the same with my son.

That's how I got interested in child care. I could not get my infant son into a child care facility in New York. The rules and regulations were quite sexist. If you had <u>any</u> female in your family, able or willing to take care of your child, the city wasn't going to allow you to bring him to a child care center. I first tried a private child care facility, but they were awful people. Later I hired a nanny while I was going to school at Columbia. I drove a cab at night. I had a one-room studio on 114th Street.

[END OF TAPE, SIDE 1]

GARFIELD: At one stage I moved to Brooklyn with a family. I couldn't believe it but they gave my child different food than theirs. When I came home he would be freezing and wet. I went through different periods where I hired a nanny for the baby, and driving a cab. I was getting the G.I. Bill money at the same time so I could continue with my studies. Then I moved in with a young divorcee who had a child. We shared the expenses of a maid and the apartment on Madison Avenue. Eventually that did not work out too well either. Her husband was gay and his friends harassed me. Eventually, in my senior year my son went to live with my sister in the Bronx. I would have preferred he live with my mother, but she wasn't feeling well enough to do it. My sister had a very strange husband. He eventually went berserk and when my son Stefan was about seven, several years later, he came to live with me in South Jersey.

GARFIELD: Nevertheless, I graduated from college in February of 1949. By then I was a chemistry major. I had switched from premed to chemistry for a simple reason. My occupational objective was to get a degree in biochemistry. I was studying under Public Law 16. It was a special provision of the G.I. Bill for disabled veterans. Under that program you had to have a very specific vocational objective. I could not finish medical school in the time that was allotted to me. So my objective was a Master's degree in biochemistry. In fact, in 1949 I was accepted at Penn in physiological chemistry. Within a few weeks I suffered incredible pain in my leg and back. I was diagnosed as having a herniated disc. I was admitted to Temple University Hospital under Dr. Michael Scott. As a consequence of that episode I had to drop out of school. I went back to New York and eventually got different jobs. In those days it was very difficult to get a job as a chemist. For a while I worked as a sales correspondent for La Salle University, which was really a mail order correspondence school.

THACKRAY: Were you selling their courses?

GARFIELD: It was my typing again. I was really a glorified clerk. I processed the inquiry coupons that people clipped out of Popular Science Magazine or a similar publication. I would get all the basic information needed. I would look them up in the phone book and put together the information for the salesmen. Then the sales people would get on the phone and convince these people that they needed to buy the course, or they would arrange an appointment to go see them. Today they call it telemarketing. As part of that deal I got to take a free course in stenography. I not only learned Gregg shorthand but also steno typing. But I didn't complete these courses.

Although I applied at several chemical companies I still couldn't get a job as a chemist. I don't know whether it was discrimination, but in those days there were really strong racial and religious quotas. Even five years later I can remember it was rather unusual for Jews to get hired at Smith, Kline & French. People may forget that nowadays but it was very much the case back when I was going to college. That's why I empathized with blacks and other minorities. But I didn't let it get me down. Eventually I got a job working in a small company called Evans Research and Development Corporation. It was a consulting firm that did contract research. I was hired as a junior chemist, a euphemism for technician.

THACKRAY: So at his juncture you really identified yourself as a chemist?

GARFIELD: I had a degree in chemistry. I went to Evans after the sales correspondence job. One day they brought me in for a conference with a client. I was asked to take notes. They didn't know, of course, that I had some flair for taking notes. When I turned in the typewritten report they were overwhelmed by the fact that I could digest everything that was said. So that abstracting aptitude must have been there. I had been with them for three or four months when I was offered a job as a laboratory assistant to Professor Louis P. Hammett at Columbia. It was not a coincidence that my cousin was his Ph.D. student.

THACKRAY: What was his name?

GARFIELD: Sidney Bernhard. He was my first cousin. Sidney was a brilliant scientist. Today he is a professor of molecular biology at the University of Oregon. He knew that I was interested in getting back into school somehow. I needed this lab assistantship and he recommended that I be hired. So I signed up as a graduate student again. However, I soon found out that I certainly was not well suited for physical chemistry. But, it was there in that lab that I first got exposed to the chemical literature. Louis P. Hammett was the editor of the McGraw-Hill Book Series. He had a fabulous library. I used to do some literature searching because he had a full set of Chemical Abstracts (CA) there.

The department had a closet full of chemicals that had been synthesized by generations of students at Columbia. There were thousands of compounds. We were doing acid-base catalysis. One of the sponsors was Rohm and Haas. Rohm and Haas was producing all these ion-exchange resins that were used for catalytic beds. I had to make a series of organic compounds--esters. I saw this closet and found out that there were dozens of them already there. All I had to do was purify them. I didn't have to make them. So I made considerable progress in creating an index for this file of compounds. However, eventually after several explosions in the lab and other incidents, Dr. Hammett realized that I was not really going to make it as a physical chemist, at least according to his standards. So we decided I was going to look for other work. He was very nice and helpful about it.

THACKRAY: You had a publication in the Journal of the American Chemical Society with Professor Hammett (3).

GARFIELD: I think they were very generous to include me as an author. I did the lab work and understood what was going on, but I don't think that I contributed any other significant ideas to this project. Sidney Bernhard was the graduate student of Professor Hammett. But you are right, it was my first paper. My closet discovery, however, did speed things up quite a bit. Professor Hammett never knew about that because I didn't want him

to know about my lab inadequacies.

I told Professor Hammett that I was going to look for other work. Somebody told me that I should combine chemistry with all my knowledge of typing and stenography. I applied for a job and was accepted to be secretary to the Director of Research at the Ethyl Corporation in Detroit. I thought it was great that I would be able to combine a secretarial and science degree. I think it was the end of 1950 or the beginning of 1951 that I went to the American Chemical Society Diamond Jubilee Meeting. I had never been to an ACS meeting before. I guess that was where I was interviewed for the job at Ethyl. While I was there I stumbled into the Division of Chemical Literature.

It soon became apparent to me that there were people who actually made a living doing that kind of work. Jim Perry was chairing the session. James Perry was one of the pioneers in my field. He had just published the book Scientific Russian; A Textbook for Classes and Self-Study (4). He was about to publish a book with Bob Casey of the Schaeffer Pen Company on Punched Cards: Their Application to Science and Industry (5). I guess Jim was Chairman of the Division of Chemical Literature at that time. After he got through the session and came down off the platform I walked up to him and said, "Can you tell me how I get a job in this racket?" I couldn't believe that anyone got paid for such work. I would have done it for nothing. So he smiled. He was a giant of a man. He is long since gone. I still have contact with his wife. Ruth Perry is a very nice lady. She lives in Tucson, Arizona. Jim was, I think, a chemical engineer by training. He said, "OK. I'll be in touch with you."

He had this project at MIT, and I think Madeline M. Berry and Alan Kent were working with him. Alan Kent eventually went on to become the Dean of the School of Library and Information Science at the University of Pittsburgh. Madeline Berry is still involved part-time in the field. She worked at the National Science Foundation as one of the grant administrators. Perry said to me, "Yes. I think we could find a job for somebody on this project." Jim Perry arranged to come back several weeks later and met me at my mother's house in the Bronx. I'll never forget it because she fixed him this fantastic meal and he loved Jewish cooking. He then presented me with a copy of his new book on punched cards and said, "OK, we're going to hire you."

He called me up a few weeks later and said, "Can you hang on for a few more weeks?" By then I had to turn down the job in Detroit. He then called me up and said, "Well, things have changed. Would you be interested in a job in Baltimore? We can't pull it off in Boston, but there is a job in Baltimore."

The next thing I knew I was down at Johns Hopkins University being interviewed by Sanford V. Larkey, Director of the Welch Medical Library. He was also director of the Machine Indexing Project sponsored by the Army Medical Library. I didn't know until I got hired that I really wasn't working for Jim Perry. Jim was a friend of San Larkey's. Maybe out of kindness, instead of just turning me down, when he didn't have a job, he figured that I was the right person for the replacement needed on this project. So I worked with Williamina Himwich, a brain physiologist. We eventually became very good friends. I still see her occasionally, although she's long been retired. She was the scientist on this project that had started two or three years before I got there.

THACKRAY: What kind of project was it?

GARFIELD: It was a project which has been described in great length in the literature (6). It was sponsored by the then Army Medical Library which eventually became the National Library of Medicine. They were studying new ways of dealing with the medical literature. The methods they were using were hopelessly out of date. The Index-Catalogue was years behind. They had just started the Current List of Medical Literature and it was having growth problems.

All sorts of machine methods of searching the literature were being considered. When I came to the project they had done very little with actual machine methods. They had done something with coding. They had studied the abstracting and indexing services coverage of medical literature. When I got there we got hot and heavy into the problem of subject heading analysis. Together we developed the Subject Heading Authority List (SHAL) which served as the prototype for the current MeSH, or Medical Subject Headings. I take some gratification in realizing MeSH goes back to that project. I found out after I left the project that Dr. Larkey hired me because Professor Hammett had told him that I was "a very hard working, dedicated individual but not a particularly original thinker." This was exactly what Dr. Larkey wanted. [laughter] Poor San, he didn't really get what he bargained for. He got too much.

My mother had given me some money for graduation and I had given all that to my sister to take care of my son. I split up with the woman who was supposed to become my second wife because she didn't want to go to Baltimore unless we got married. I wasn't ready to get married again. So I went there and became a bachelor again. I lived in Baltimore and went to work for the project. They needed a chemist on this project, so the first assignment I got was working with enzyme and chemical nomenclature. I have always said that practically everything I have ever done at ISI is in some way or another attributable to the thought processes that went on in this basic research.

I met all kinds of interesting people who were a stimulus to a lot of things that I've done. Chauncey D. Leake was Chairman of what they called the Committee of Honorary Consultants. The Committee consisted of many of the outstanding figures in the field at the time. For a young guy who didn't know anything about the field, it was an incredible experience. There was Verner W. Clapp, the Chief Assistant Librarian of Congress; Mortimer Taube, also with the Library of Congress, who started his own business, Documentation, Inc.; Brad Rodgers, who had just been appointed as the new Director of The National Library of Medicine; Seymour Taine, Editor of Index Medicus; and Estelle Brodman, who became quite a respected historian of medicine herself, even though she was basically a medical librarian. She combined them in a way that no one I know ever has. David Kronick also combined the talents of a librarian and scholar. The last named three were not on the Committee. Others included Ralph Shaw and Gene Scott.

San Larkey was a dilettante. While he was alive I wasn't able to say too much about him. He was a man who had problems with alcohol. He came in at nine o'clock every day and would engage me in conversation about his wartime experiences and his interest in Elizabethan medicine. In the afternoon we worked on subject heading analysis. He loved that and we would talk about the origins of words. We worked on the WHO classification, assigned all the subject headings to appropriate categories. There is a whole literature on this (7). Then at five o'clock he would go home and at five after five o'clock I would begin research. Eventually we got an IBM 101 Statistical Sorting machine. I started playing around with it and eventually invented various systems for searching the literature stored on IBM (Hollerith) punched cards. However, there were a whole series of investigations we did at the project. Helen Field was a medical librarian on the team but she eventually left to marry Giles Rich of the U.S. Patent Court.

THACKRAY: Unlike the Columbia lab and the chemical company, you had really found your metier.

GARFIELD: Yes. I didn't mind working 24 hours a day. When I first came to Baltimore Dr. Larkey introduced me to someone who got me a very nice room near Charles Street, which is really a very nice section of Baltimore. However, I wanted to play the saxophone. The landlady objected to that so eventually I had to move out of there. I can tell some funny stories about Dr. Larkey. For example, I was evicted because there was a Republican judge (Laukitis). Larkey came down to defend me. When the judge saw Larkey, whose wife was a Democratic National Committeewoman, he immediately said, "Young man, get out in 24 hours!"

I decided to move someplace where nobody would bother me. I got a room on the corner of Mt. Royal and Charles St., on top of a drug store. It was a huge room, basically the size of this office but with higher ceilings. The landlord was a retired pharmacist who collected parakeets. He had another room the size of mine with 100 cages full of these parakeets. They were all illegal at the time. There were laws against the birds to prevent them spreading psittacosis. But he let me play the saxophone anytime I wanted to. One time when I had gone away for the weekend Dr. Larkey came looking for me. When he saw where I was living he was terribly upset. He said, "This is no place for a respectable young man to live, in the red light district." In fact, the lady downstairs from me was a stripper.

He said, "Why don't you get yourself a room right near Johns Hopkins? He found me a room right over the drug store opposite the Johns Hopkins Hospital emergency entrance. I tell you that was an experience. I had an apartment that was the size of a closet. But, I was within a stone's throw, literally, of the Welch Library. As a consequence I worked there literally day and night.

One of the culminating experiences was the public symposium that we had agreed to sponsor in March of 1953. San Larkey was supposed to give a talk, as was I and a new fellow on the project--John Whittock. As it turned out, both of them copped out. Larkey claimed illness and so did Jack. I had to give all the papers. It was a one-day symposium and 300 people came. It was called "The First Symposium on Machine Methods in Scientific Documentation." There is a brief write-up on it by Marjorie Hyslop in a journal called Special Libraries (8). The vice-president of the university, Lowell J. Reed, was our opening speaker. He was the one who used the expression, "Man is going to be drowned in a flood of paper." One of the newspapers had sent a reporter, and when this hit the news wire services it got national coverage.

We started getting letters from all over the country asking for information on this symposium. One of the letters that I got was from William C. Adair, a retired vice-president of Shepard's Citations in Colorado. He told me that Shepard's Citations had originally been in New York, and he had been approached by an engineering society about the idea of organizing literature using their methods. I didn't know what Shepard's was so I went down to the Enoch Pratt Free Library and went into the reference room. I found Shepard's Citations and I literally screamed, "Eureka."

I had been trying to devise a system around review articles which Chauncey Leake had been pushing me to do. He kept saying, "Review articles are extremely important to scientists. Study them carefully. Find out why they are so successful." I had done a primitive kind of linguistic analysis of reviews. Essentially if you parse a review article, each sentence becomes an indexing statement. (Recently Henry Small has just reversed it. In citation indexing you go into a citing article and pull

out a citing sentence.) I was taking the review article and analyzing each sentence and tagging it with the article it had cited. When I saw the Shepard's Citations I found the methodology that I needed for linking all these things, for indexing all these cited references that were cited in the review.

So I began a correspondence with Mr. Adair. However, I had to leave the project within a few months. I was literally fired. Dr. Larkey terminated my employment because he was angry as could be with me for a lot of reasons, including, he claimed, my refusal to stop publishing something called Contents In Advance. I had started Contents in Advance the year before. It was a compilation of the contents pages of all the library and documentation journals.

THACKRAY: The year before was 1952?

GARFIELD: Yes. It was 1952.

[END OF TAPE, SIDE 2]

GARFIELD: I couldn't see why we couldn't do that. That was one of the disagreements. In fact, the project was being terminated by the Army Medical Library. They weren't going to renew the contract. The net effect was that I didn't get my vacation pay. We parted and he was hostile to me until the very end. He never had a pleasant word to say to me until he died. Larkey was very weird about publishing. I could never get him to agree that something was ready for publication. When I left I submitted several manuscripts that I had written for publication. In one case, a paper that I eventually got published in the Journal of Documentation (9), he actually got the university attorney after me to try and prevent my publishing the paper. It was an innocuous paper on "The Preparation of Subject Heading Lists by Punched-Card Techniques." It certainly did not turn out to be a classic, but it was one of the first papers I ever published.

All of my contact with Mr. Adair came after I left the Welch project. A couple of friends had advised me that if I wanted to stay in the field, it would be a good idea if I got a library degree. I had been mixing with all these librarians but I didn't have the professional qualifications. So I attended the Columbia Library School during the summer. I don't remember how I scrounged up the extra money to pay for the summer session but I did. I found out that I could get a Master's Degree in one year. Luckily Dean Carl White had observed me and told me about the Grolier Society Fellowship which was sponsored by the Encyclopedia Americana. I applied for this fellowship and I got it. The main reason that I got it was on account of my references. I listed the Director of The National Agricultural

Library, The Director of the Army Medical Library, and the Chief Assistant Librarian of Congress.

THACKRAY: All of whom you knew from the Welch project.

GARFIELD: Yes. Ralph Shaw was one, and Brad Rodgers was another.

THACKRAY: The fellowship was obviously very important.

GARFIELD: It made quite a difference because I couldn't afford the program without it.

THACKRAY: Weren't you getting a little discouraged before that?

GARFIELD: At one point, I didn't know what I was going to do. Several people at the National Science Foundation had told me I should apply for a grant. Well, they weren't giving grants to unaffiliated individuals. That was a lot of nonsense. I still resent that. Why should you have to be affiliated with a university in order to qualify to get a grant? It is all part of this bureaucratic notion that people steal. If you are an individual who got a $5,000 grant, let's say, God knows what you would do with the money. You might spend it to eat.

I graduated as one of the top students in the class at Columbia. But during that year, even before I graduated, I published my major paper on citation indexing (10).

I was the youngest associate editor of American Documentation. Jesse Schera had asked me to become an editor and I would say that was quite an honor. As an editor of American Documentation I suggested to Adair that he should write an article on citation indexes (11). After he wrote his paper I said I would do my own paper.

THACKRAY: Was American Documentation a comparatively new journal?

GARFIELD: Well, relatively. It was the Journal of the American Documentation Institute. The American Documentation Institute became ASIS, an organization that is just celebrating its 50th anniversary. It was not always an individual membership group. The year I joined was the first year you could join as an individual.

In the summer of 1955, I drove out with my son Stefan to visit Mr. Adair in Colorado Springs. So know you know how I eventually published my most important paper--"Citation Indexes for Science."

While I was at Columbia I decided to continue my doctoral work in the field of mechanical translation because all during that time I had followed that field. A very close friend of mine, Casimir Borkowski, was working with Leon Dostert at Georgetown University. At the time he was the leading exponent of mechanical translation.

THACKRAY: What kind of mechanical translation?

GARFIELD: It was the mechanical translation of Russian and foreign languages. I was going to work at Georgetown. Casimir Borkowski was already working for Dostert. However, by the time that I had finished my work at Columbia, several things had happened. First of all, I had tried to put together a Ph.D. program at Columbia. I didn't want to do it on just any subject. There were no professors who really understood this field. So we had to put together what they called an interdisciplinary committee. I got three or four professors to agree to serve on this committee but I could never get them to meet. One of them was Professor George Kimball, a chemical physicist who was interested in computers. My topic was going to be "Machine Methods of Scientific Documentation - the Application of Computers." I couldn't swing it. I couldn't get them to meet.

In the meantime, I got a call from Ted Herdegen at Smith, Kline & French. He had met me down at the Welch project. It was just about the time that Thorazine had really burst on the scene and SmithKline was just inundated with information that they had to have processed. Ted said to me, "Why don't you come down?" Ted was a really sweet man and convinced me to come and work there temporarily for six months. He said, "You can earn a little money and after your six months here you can go back to Georgetown University and continue your work." So I came down here in the summer of 1954.

I guess the reason for the discrepancies in these dates is the time lag in the submission. It took a helluva long time for papers to get published in those days. Interestingly enough, the person who helped me edit the paper in Science, even though it didn't get published until 1955, was H. Bentley Glass, a professor of genetics at Johns Hopkins. I guess he was one of the editors or referees for Science. He went over the manuscript and really helped improve it. Eventually we got it published, although it was hit and miss as to where it would be published.

I was desperate for money at that time and I had to make a decision. So I went to work for Smith, Kline & French, and that's how I got to Philadelphia.

While I was at Columbia, there were a number of things that I had worked on. I had worked on the idea of an encyclopedia. I did a term paper on that and that's the origins of the Atlas of Science. I really wanted to do a different kind of encyclopedia as well. During that time I also did the paper published in the Journal of the Patent Office Society which came about the same time (12). I had several of the students help me, including Marge Courain from Merck. I think she is still with Merck but in a different part of the company. I continued to publish Contents in Advance, even in library school. I did that with the help of Ann McCann and some other library school students.

THACKRAY: What was the circulation of that publication? Who was it going to?

GARFIELD: It was going to library schools and librarians. It was very small and within a year or so I had to fold it. A subscription cost only six dollars a year but I couldn't maintain it anymore. I turned it over to Ann. She tried to publish it for a little while but eventually it went the way of all under-financed ideas that are a little ahead of their time. Upon reflection it would have been a natural for any library school to have produced it.

THACKRAY: That was in 1954 or 1955?

GARFIELD: Yes. We published on 8 x 11 size paper but photographically reduced four contents pages down to a single page. Most library journals were relatively small then.

THACKRAY: Do any of those samples exist?

GARFIELD: I don't know if we have one here or not. I haven't seen one. I'm sure you could find one in some library catalogue somewhere. We could look it up in the Library of Congress.

THACKRAY: How long did you stay at Smith, Kline & French?

GARFIELD: I came to work at SmithKline as a consultant, working here four days a week. I stayed at the Broadwood Hotel at Broad and Vine. It is now a health club. I could walk to work. Several years later I was to learn that I had been misinformed. I could not deduct my travel expenses as a tax deduction, even though I was living in New York. The Internal Revenue Service said that my principal residence was Philadelphia. If I worked four days a week (or for a certain

period of time) that's where I lived. As far as they were concerned, I kept my apartment in New York for convenience.

By the end of December of that year, my sister called me and told me that my brother-in-law had gone berserk--that I should come to take my son to live with me. I then rented a house across the road from Ted Herdegen, who lived four miles south of Mullica Hill, New Jersey. For $20 a month I rented a house on an asparagus farm, and indeed my son Stefan came to live with me. By then he was seven years old. He went to school below Mullica Hill, in Harrisonville Township. I drove to work every day with Ted Herdegen. His sister took care of my son after school. We lived down there until the following summer.

In the summer of 1955 I drove out to Colorado to visit Mr. Adair. On the way back we stopped in Chicago. We visited Winnie, my previous girl friend. I met her in 1948 when I was still at Columbia, the year after I was divorced. We decided to get married. We first lived in Woodbury, New Jersey for a few months and then we moved to Thorofare, New Jersey where we rented a log cabin. We had a big log "cabin" and a small log cabin that was used as a chicken coop. I converted the chicken coop into my office. All this time I was still a consultant to SmithKline. Winifred obtained a job as a secretary at the Franklin Institute. Eventually we had three children. SmithKline wanted me to work full-time. I made the decision that I was not going to be happy working full-time for them, but I kept on as a part-time consultant for a number of years.

THACKRAY: Was that as much as a couple of days a week?

GARFIELD: Yes, it was a couple of days a week. The rest of the time I had other clients. I did a major study for Biological Abstracts. It was called Biological Abstracts in an Era of Automation (13). That was with G. Miles Conrad. He is now long since dead. I even had a consulting job for the Encyclopedia Americana. My ideas on medicine indexing were too advanced for them in those days.

I had decided to do a Current Contents of Management and Social Science. Just as I had announced that, Bell Laboratories heard about it. They had been planning to do something similar. So I made a contract with them to produce my publication, which was called Management's DocuMation Preview. I put a separate cover on 500 copies for them which they called Survey of Current Management Literature. That contract lasted, actually, for about eight years. The year of that contract was a very, very momentous one because that was how I was introduced to Household Finance Corporation.

THACKRAY: Was this in 1955?

GARFIELD: I got this Honorary Membership Card in 1958. There is an article about how I got my money (14).

Instead of printing these things as I had been doing on a Xerox machine, I had to use a regular commercial printer. I couldn't do such a large quantity by Xerox. The printer wouldn't print my material unless I paid him in advance. The Bell Labs contract was for twice monthly and it was about $15,000 over a year's time. But Bell Labs wouldn't pay me anything up front. I had to finance the first issues. So I brought their purchase order to the bank. I first went to bank in Woodbury and then I went to banks all over Philadelphia. They would not advance me any money on that purchase order. They said it was not an account receivable, which is true. And as one put it, "You could walk out in the street tomorrow and get hit by a car, and then where would we be?" I didn't know how to answer that in those days. I don't know why they never heard of insurance. I was really up against it and I didn't know what to do.

I went back a second time to the Woodbury Trust Company. Mr. Fowler, a very nice man, said to me, "Listen, there is no way this bank is going to lend you money. But if you go down the street to Household Finance they will lend you $500 on your signature." I couldn't believe it. (If you grew up in the depression, going to a finance company was like being in the hands of the devil.) I went down there, and sure enough the guy asked me the usual questions and I signed for 500 bucks. I found out some very interesting things about financing which I have described in this article, but the essence of it is this: A bank will lend you money at 6% per annum (in those days), which is the equivalent of one-half percent per month. But they will also make a service charge on it. They will also say that you must keep the money for at least a month. You are paying for a month minimum. Well, if you are borrowing $500, that means you are paying $2.50 for the first month, but a service charge might add another $5 or $10. That's closer to 2% or 3% per month. Household Finance charges you 18% per annum. But there is no service charge. One and one half percent a month on $500 is $7.50. Furthermore, if you wanted to, you could return the money in two or three days or weeks or whatever.

I borrowed the $500 and got the material printed, but I had to wait an agonizingly long time to get paid. Bell Labs didn't deliberately do it, but somehow there was a screw up and I did not get a check for quite some time. Eventually I went back to the Household Finance Company and I said, "Here's your money." And he said to me, "Are you kidding? You're going to pay me off so soon?" I said, "What are you talking about. I thought you would be tickled to death that I was paying my debts." He said, "That's not the way to make money." [laughter]

So I said, "OK. I'll tell you what we'll do. I'll pay this one off. But the next time I print I'll be back." I came back to him, because the expenses started coming in again and I needed the $500, plus another $500 because I now was faced with the promotional costs. So I said, "Listen, I'm going to borrow this $500 from you, but I'll make a deal with you. How do I get $1,000?" He said, "$500 is the limit. There's a state limit." I said, "That's too bad." They didn't have computers in those days, and he said, "That's just this branch. They don't know from one branch to the next what they are doing. If you go down to the Camden office you can borrow another $500." Well, I started going around to various offices. Eventually that's how I got this Gold Card and how I financed my business. Of course, eventually I got to the point where I didn't need it. But as long as I was not overly ambitious with the borrowing, I was able to manage.

Here is how Current Contents got its name. Through this management publication contract with Bell Labs, I met a public relations man named Harry Brager. I think I met him through a printer in Washington. Harry Brager and this printer came to me and said, "We'll give you $5,000 to be partners on this publication. We'll start the mailing as a subscription publication. This name, Management's DocuMation Preview, has to be the worst title anyone could ever imagine." Eventually I had to agree. Brager said, "Why don't you give it some kind of a simple name?" We started talking, and it became Current Contents. That's what it is. That's Current Contents. So I made this deal with them. They even gave me an office at 3 Thomas Circle in Washington.

They then said something which I couldn't believe they were going to do. They wanted to take the entire $5,000 and spend it on one mailing. But they were going to bankroll this thing. If it really flew, then they would put in more--$5,000, $10,000. They would keep on recycling it. The idea was that if you got a 1% or 2% response on subscriptions at $25.00 a year (which was the price), then you could justify more money for more direct mail. Then you would take that cash and put it into more mailings. I couldn't believe it. They insisted on using a mailing list consisting of the highest paid individuals in the United States. In other words, they assumed that anybody with an income over a certain amount of money could afford it and would want it. I told these guys, "You've got to be crazy." They insisted on doing it. Well, it bombed. They got a total of maybe twenty subscriptions. I'll never forget the first one that came in. It was from an executive at Alan Wood Steel Company. After it bombed they backed out and said, "It's yours." That's how Current Contents got its name.

Well, I kept on trying. At one point we actually discontinued that publication and eventually we resumed it under the name of Current Contents of Behavioral and Social Sciences. In the meantime, that's when Miles Laboratories came along. A friend of mine, Charlotte Studer, who subsequently married a man

named Mitchell, was the librarian at Miles Labs. She said to me, "I'd like to do something with the medical journals for my people." So I started producing Xerox copies of the contents pages of about 100 journals for her in my chicken coop. Then eventually, when other drug companies bought in, I made a subscription policy that you had to buy 25 subscriptions for $1,500, the way I had done with her. She had bought perhaps 100. That's how it got started.

It wasn't until later on that it became a subscription situation where you could buy one copy. That happened not out of any brilliant design on my part, but because of Dr. Jacob Gershon-Cohen, a very famous radiologist in Philadelphia, who invented thermography.

[END OF TAPE, SIDE 3]

GARFIELD: He called me up and said, "Mr. Garfield, I've seen a copy of this thing that you are sending around to some of the drug companies. I could really make use of it in my work. I have a policy of never accepting gifts from drug companies. Why don't you sell me a subscription. Mail every issue to me." And I said, "I don't know. I have no price. What do you think it is worth?" Cohen replied, "About $2 a copy. $100 a year." That sounded like manna from heaven to me and I said, "Absolutely. You've got it." That was how I decided to set the price at $100 a year for one subscription.

THACKRAY: When was that?

GARFIELD: I'm not sure. Maybe it was a year or so after I started.

THACKRAY: That's when it really started to be a commercial success.

GARFIELD: Well, I wasn't promoting it yet. Then I got another phone call from Harold P. Rusch, the Director of The McArdle Memorial Lab at the University of Wisconsin. Dr. Rusch said, "I've been seeing this thing from the drug companies and you know, we could use it for our labs. We have a lot of trouble keeping up with the literature. Why don't you let us buy it." I thought for a moment and said, "We usually sell 25 copies." He said, "That's OK. We could use 25." And I said, "It's $1,500 a year." He said, "That seems a little steep. Why don't you give us an academic discount?" And I said, "What does that sound like to you?" He said, "50%." "So for $750, you've got it." And that's how we began bulk subscriptions. Bulk sales were very heavily promoted in the beginning. Later on, it was designed

more for individuals. That's why I don't know the exact year we started offering it. We later priced it at $50.00 a year for an individual academic subscription.

THACKRAY: At what moment did you hire somebody full-time?

GARFIELD: In the early years my wife did the paste-up. She had done similar work previously so she knew how to do it. She also had worked at the Franklin Institute as a secretary, but when the babies came she worked out in the chicken coop with me. So we put the thing together. Then I rented an office at 1523 Spring Garden Street, across the street from SmithKline. The building is still there, and I'm sure someone still lives in it. I took the third floor of one of those tenements and made that my office. I hired Claire Schultz to work for me part-time. She was a librarian from Merck. She was pregnant and needed some part-time work. Then I moved downstairs and occupied a little bigger space and hired my first full-time employee. It was either Beverly Bartolomeo, who still works for me, or a woman named Sylvia Shapiro who was my secretary. They were the first two employees.

At about that time I also hired a part-time fellow who was just getting his Master's Degree in Marketing at Penn State. That was Marvin Schiller, who was the first Vice-President of my company. Marv stayed with me for several years and then left under unpleasant circumstances. He was part-time and did the first direct mail campaign on Current Contents/Life Sciences.

THACKRAY: Did he leave for a split-off group like the phenomenon in the computer industry?

GARFIELD: What happened was not a very pleasant thing. I don't remember the exact year. After Current Contents was going we used to say, "Gee, won't it be some fantastic day when we have 5,000 subscribers."

I got a letter from Josh Lederberg in 1958, the year he got the Nobel Prize. That letter has got to be a classic by itself. In it he said, "For lack of a citation index I cannot tell what happened to your article in Science." That was how my correspondence began with people in the genetics community. Eventually I met the head of the genetics study section of NIH and they had a team of people visit me. As a result we agreed to produce the Genetics Citation Index. I'll get a copy of it if you've never seen one. That happened in the early 60s. We produced 1,000 copies of the Genetics Citation Index which were distributed free to geneticists. In order to produce it, we had to produce the 1961 SCI.

The NIH, under pressure from Congressman [Lawrence H.] Fountain, passed a rule that they would never again give grants to a private company. (Congressman Fountain had found a private company somewhere that absconded with some funds.) We already were halfway through the project at the time, so they transferred our grant to the National Science Foundation and made it into a contract. We were given a $50,000 a year contract for three years. When the end of the contract came, I said to NSF, "I have all this data. Why don't you let us publish the full SCI and distribute it." They said, "Nothing doing."

So we had to make a decision as to whether to publish it or not. We made a decision to publish and to launch the ongoing service. We sold the first Index at $500 and launched a quarterly. We soon found out that this was not an idea that was going to sweep the world overnight. As a consequence, our finances got to be very precarious. One of our advisors mistakenly forecast that we would run out of cash at a certain time. As a result, we were introduced to financiers in New York and a group bought a minority interest in the company. They acquired about 20% of the shares of the company as a result of that. They loaned us $500,000 as a convertible debenture which subsequently was all paid off. That was why I said the SCI almost bankrupt us even though we were doing well on Current Contents. Over the years it gradually caught on. That half a million dollars didn't do the trick. It was the Current Contents successes that did the trick, although I suppose the money did help. Anyway, that's how I got my first minority shareholders.

THACKRAY: ISI itself incorporated in 1960.

GARFIELD: I incorporated under the name of Eugene Garfield Associates. There is an interesting story there because in 1956 in Jersey my company was called DocuMation Inc. I changed it to Eugene Garfield Associates in Philadelphia and its subtitle was Information Engineers. However, a society for professional engineers or some engineering group in the state wrote me a letter claiming it was illegal to call yourself an engineer unless you were an accredited graduate of an engineering school.

By the time 1960 rolled around, I had anyhow decided that we were going to emulate a multidisciplinary organization like VINITI [Soviet All-Union Institute of Scientific and Technical Information] in Moscow. I said, "We'll do with two dozen people what they do with 2,000." So I decided to call it the Institute for Scientific Information. Part of the motivation was that we were operating in a non-profit world. The attitudes towards private companies in our field was just abominable. The name removed some of the stigma for some people. To this day, many people believe ISI is non-profit. Well, you know the old story about that. It's non-profit but we didn't plan it that way. That's the opposite of these big non-profits which are for profit but they didn't plan it that way either.

THACKRAY: It's interesting to see the corporate organization dimension. ISI was incorporated and wholly privately held in 1960. Then you had these outside finance people come in and you paid them back.

GARFIELD: Yes. They came in about 1964.

THACKRAY: Would the company have gone public at some time?

GARFIELD: We tried to go public one time. The day that we went public the market hit rock bottom for that year or decade. It was like the market crashed, or it was a recession type of thing. There was a second attempt and the underwriters backed out. And after a while, when the company became sufficiently self-supporting, the question was, "Why go public?" What was the reason now? I could think of some good reasons why we might have done so, but that's where we are. Maybe it will happen some day in the future.

THACKRAY: How did Index Chemicus come into this development?

GARFIELD: Because of my close links with the pharmaceutical industry, I remained consultant to SmithKline for many years. Back in 1955 we all were very unhappy with Chemical Abstracts. Newman Bortnick, Max Gordon, myself, and some other people put together a report for the local ACS Section, criticizing Chemical Abstracts and giving them some suggestions as to what they ought to do. Eventually they moved into the modern world especially when Dale Baker and Fred Tate came on the scene. Fred almost came to work for me after he was at Wyeth.

In 1957 I obtained a contract with a pharmaceutical manufacturer's association for about $15,000 to index the current steroid literature, in cooperation with the United States Patent Office. We were to code their indexing sheets and turn them over to the Patent Office in order to facilitate punch card searching of those patents. We did it very well. It was a growing field, and we indexed tens of thousands of steroids. In the process of doing that I got to see that there were interesting possibilities that could make Chemical Abstracts current.

This is all part of my thinking on my doctoral work, which was going on at the same time all these other things were going on. In 1955, I started my Ph.D. at Penn under Zelig Harris. I had been introduced to him by my friend Casmir Borkowski who had come from up here from Georgetown. He was among the group of students including Noam Chomsky, working under Harris. Cas introduced me to Zelig Harris. I, in turn, introduced Zelig Harris to information retrieval which eventually got him a half a million dollar grant from the National Science Foundation. He

recognized potential. After I told him my story about how I couldn't get my Ph.D. at Columbia, we agreed that I would work at Penn on my Ph.D. We decided on a program and how many credits I'd have to take. He let me transfer 30 credits. By the way, I had not only done my library school work, I had other graduate credits I had taken. I really had 60 credits. They gave me 30 credits and I agreed to pay for 30 credits for my Ph.D.

You know that whole story. My topic was "An Algorithm for Translating Chemical Names to Molecular Formulas." I told Harris that this was something people thought could not be done and he was not competent to judge the chemical significance of it. Professor Alan Day of the Chemistry Department knew me through Max Gordon, who had been one of his students. Max Gordon was at SmithKline. Alan agreed to be the co-shepherd of my Ph.D. and give the linguistics people the input to say, "Well, chemically, this is OK." That's how I got my Ph.D. at Penn.

I had been very chemically oriented all during this time. One of the problems that SmithKline and all the drug companies had was the fact that Chemical Abstracts indexes were three to five years late. While working on the Patent Office project I realized that people were going about all this in a completely absurd way. They were first identifying the chemicals and then they were going through a laborious process of assigning Geneva Systematic Nomenclature to the compounds. It was taking them forever. Besides, they never did have a fire under them so they didn't know what it meant to get it out. They were very nice guys. You know, I almost went to work as the Director of Research at Chemical Abstracts. I was interviewed for the job.

THACKRAY: When was that?

GARFIELD: Back in the early 50s. Karl Heumann went to work there. I was interviewed by E. J. Crane and Charles Bernier, the editor. We had known each other through the Welch project. I knew all these people. As a matter of fact, Fred Tate, who became Director of Research, was out at Wyeth. He and I almost went into business together. He decided to go there rather than ISI. In any event, I told my friends at SmithKline, "Listen, I have just figured out that I can produce for you a molecular formula index for the current chemical literature. It will cost you $25,000 a year."

Glenn Ullyott said, "You're absolutely out of your mind." Glenn is a Chemical Abstracts fan from way back. He's very loyal and I don't blame him. In a sense I should thank him. When he said I couldn't do it, it really made me mad. It would have been OK if he just said, "We can't afford it." But when he said I couldn't do it I said, "I'll show you." And then I said, "I'll give you your last chance. You can have an exclusive because I'm your consultant. I would not do it for anybody else if you want it done. Otherwise, what I am going to do is ask you and twelve

other companies to each give me $2,000." Then I called up all the people I had gotten to know through the PMA project and said, "Listen. I've been coding these things for a year and a half now and I think I know what it takes to index these compounds. I will produce a molecular formula index for you."

By the time the committee got through with it, they wanted structural diagrams. You've seen the first issue. It's quite different than it is today, but it is a helluva lot different than what I had proposed. On that basis, I said, "OK. If you want it that way, then you have to agree that I publish this thing and you each pay $2,000. Anybody else can buy it for $500. And you're committed for two years." That's how it got started. Believe it or not, Index Chemicus is still going. I can't believe it lasted all these years. In spite of everything that Chemical Abstracts does, these people still buy it. And they pay $3,000 a year for it.

It's not all that profitable. It has had its ups and downs. In the early days of Index Chemicus, four of my vice-presidents came to me and said, "We think this thing is just going to be a continual drain of money. We'll never make any profit on it. We want to kill it." I was opposed to it. I said, "We'll find ways to make it more efficient, do whatever we have to do, but I don't want to renege on it. I think it has a future." Then they said, "OK. Either you resign as president or we're all resigning together." So I called their bluff. Those four vice-presidents quit and formed another company. Eventually they went out of business.

THACKRAY: Did they stick with the field over the long haul?

GARFIELD: They were in the information field, but they never came up with a product that flew. In one case, they started a product that was so similar to what I was already doing, Current Contents, that I sued them. It fell apart. They didn't get the financing they were expecting. One of those people was the guy who came to work for me half-time. I found out about very deceptive behavior that people engage in. He lied to me. The day he left my company he said he was not part of the rebel group. He lied until he got his shares of stock. I had given him an option which eventually he sold out. We bought his shares years later. That's one of the trials and tribulations that you have in business.

Not everybody goes along with you. A few of the original people that invested in my company came down from New York within days of that incident. They were in a fit. They said to me, "You told us you had a team. How can you manage this whole company by yourself?" I said to them, "You know, when I was hiring people like my half-time marketing director, do you think I was hiring Whiz Kids?"

What you have to learn to deal with in American business is how to deal with mediocrity. You don't hire geniuses and keep them all very long. Even if you give them equity, which they had. I probably made the mistake of not giving other people equity in the company in the early days. But they had it. People want to remain comfortable very quickly in the game. They don't want to go on and take the risks. I guess there are times when people think that I am too much of a crapshooter. Sometimes even I agree. [laughter]

THACKRAY: Let me go back to our previous thought. At what moment was ISI essentially full-time? At what moment did have the realization that this was it, and that this was going to be your home?

GARFIELD: I was working for SmithKline a couple of days a week. I had my first son, and then we later had three additional children. Subsequently, much later on, one of those children died. At that time I had three, Stef and Josh and Laura. Thea was born last. She was the youngest. I had a very stiff schedule.

Many people who used to work in this company said to me, "You must have a lousy relationship with your children, because you work so hard." Yet I knew people who left here with the excuse that they had to go home, but when they went home they didn't spend any time with their kids. Home was just an excuse to leave. They paid no more attention to their kids than the man in the moon. My children knew that I was there at breakfast every morning. I came home Wednesday night, and Friday night I brought home the Chinese food. I worked on Saturday, but Sunday was absolutely devoted to nothing but the kids. When I was with my children, they knew I was with them. I wasn't anywhere else, even in my head. I hope the same thing applies with my son Alexander today.

So I was working eighteen hours a day or whatever it took. You do what you have to do. It wasn't very long after IC began that I stopped doing consulting. I was already selling them Index Chemicus and that started in 1960. Ted Herdegen died in 1960. The first issue was dedicated to him. He was my real boss over there. He had been ill for quite a while. My relationship on an active basis was terminated by then, and essentially it was soon terminated with all other clients.

Remember, I didn't get my Ph.D. until 1961. That was a funny story, because Zelig Harris procrastinated. He decided to go to Europe and Israel in 1960. In 1961 I was supposed to graduate with my Ph.D. Before his departure he tells me that Professor Henry Hiz was given the job of being my Ph.D. monitor.

[END OF TAPE, SIDE 4]

GARFIELD: For the next six months he made my life miserable. I submitted my thesis. It was ten pages long. I had proven that you could do what nobody else had ever believed possible. I don't think you or any reasonable person would demand that of a Ph.D. student. I said to Zelig Harris, "If my algorithm doesn't work, I'll do a different thesis topic." It was in the algorithm that I proved that we could use a computer to convert a chemical name directly into a molecular formula. Nobody had realized that you could "compute" them. Everyone had always drawn a structural diagram first and then computed the molecular formula from the graph. That's why it took CA so damn long to index compounds. So I proved my point, and wrote the thesis up.

As a matter of fact, unknown to them, a couple of months later I even submitted a preliminary communication to Nature which was published (15). It was titled "Chemico-Linguistics: Computer Translation of Chemical Nomenclature." It was just a very short item. I wasn't even sure that if he found out about it he would have used it as prior publication and prevented me from getting my doctorate.

Anyhow, Henry Hiz said, "This will never do. You can't have a ten-page dissertation." I then proceeded to write a 100 page document which turned out to be a transformational analysis of Geneva nomenclature. I reprinted it and it is included in my essays. If you really want to see it, it is all there (16). When I came in with my manuscript he would look at it, and he would make some minor changes in it. Now in those days, before there were word processors, before Xeroxes were known to universities, I would make all his trivial little changes in this manuscript back at my office. I had two secretaries working with me and I had the Xerox machine. He couldn't believe it. I would be back the next day with a completely "retyped" manuscript. I was using what they called flexi-writers in those days and I had the whole thing on tape.

Finally, it got to be absurd. I said to him, "This is ridiculous. You aren't going to make a decision." He admitted to me that Harris had left him holding the bag and nobody was going to make a decision about even sending my recommendation back to him. I got on the telephone and I called Zelig Harris in Rome. I said to him, "Zelig, you either put a cable in the works tonight informing them to give me my Ph.D. or I am going to come over there to Rome and drag you out of bed myself." I told him about the bargain he had made, and that he had really screwed me over. And when the cable came, two days later, Professor [Henry M.] Hoenigswald called me in and said, "You're going to get your Ph.D." And that's how I got my Ph.D. in 1961. They were going to push me another year. That was what I was doing in addition to all the other things. That was another diversion for me.

STURCHIO: Before we move on, I noticed that you filed for a patent in 1958 on a copying and reproducing device. You have a couple of patents on that (17). Can you tell us a little bit about that?

GARFIELD: Yes. It's called a "Copywriter." There is an article in Current Contents about it (18). Was it filed in 1958 or was it issued in 1958?

STURCHIO: It was filed in 1958 and issued in 1962.

GARFIELD: There were several patents issued. Almost all of them are obsolete now. The 1962 patent would be obsolete in 1979. You can go out and buy some versions of a Copywriter now. There are such things. In fact, I was just reading about one that apparently is an OCR [optical character recognition] version, and there are some others. Maybe there are some applications pending which we could file on if we choose to, but I decided to give up on that.

I had believed from the earliest times that what we needed was what Verner Clapp called a scholar's camera. The problem with copying machines is that you have to take the document to the machine. My idea was to bring the machine to the document.

I'll tell you how I got this idea originally. (This was how I met Jack Rabinow, who is probably America's most prolific inventor and engineer. I don't know if he will ever come into your orbit, but he sure should. He is a fantastic guy.) I had wide ranging interests in information techniques. During my days at Columbia, I had been involved in a "Committee for Sensory Devices and Aids to the Blind." The Veterans Administration had a fellow by the name of Eugene Murphy as the head of their research department in New York. He was a marvelous man, and an invalid. I used to go to these meetings. Under contract RCA had developed a device which was a primitive reading aid for the blind. This wand device would scan the text and generate an oscillating signal. It would generate something like a bird call. Believe it or not, you could literally learn those signals and translate them into the letters of the alphabet. Some of the blind people tested actually could learn to read that way but it would never work. Blind people are extremely sensitive to the idea of losing their hearing, and by constantly listening to those bird calls they began to lose some of their high frequency hearing ability. So they had to abandon that approach.

But in that device I saw the opportunity for a copying machine. Instead of producing the sound, I said, "Let's take that signal and translate it into a copy." I literally produced models. I even went down to the V.A. and they let me work there. We put the teledeltos electrosensitive paper into the circuit. Eventually I even got a grant for $5,000 from the Council on

Library Resources. Verner Clapp wanted it. The long and short of this whole thing was that the idea was just too far ahead of the technology. If you want to see them, I have the original models somewhere in a closet. They are included in that article I published in Current Contents. It is still not exactly the way I visualized it. The Japanese thing that you can buy now is very cumbersome and it doesn't quite work the way it should. Eventually they will solve the problems, but it does make copies. There is another version now that gives you a typewriter type of output with a platen. There is one that you can scan this way [vertically], and the Japanese use it to copy. If you have to copy all those ideographic characters, that can save a lot of time. That's how I got into that. I originally had visualized that we could copy structural diagrams with this selective copier. That was another motivation. We still copy diagrams a lot here, but by camera. Now you could use a page scanner like Omnipage to project the image onto the screen of a microcomputer.

We still have the Corporation, Selective Information Devices. What I failed to do there was to turn it over to someone else. I tried to do this alone at various stages. I tried to get a separate company formed, and to get a business manager to get venture capital for it. That's what it needed. I guess I was never a good enough manager to realize that I had to get people like that to run a venture like that. I couldn't just do it part-time. It was just another diversion that I should have turned over to somebody else. Maybe they were lucky because the technology just wasn't there for twenty years. Now, you have all these semiconductors, microprocessors, and whatever they can perform amazing tasks with miniature circuits.

THACKRAY: How much of your own time and energy has gone into the sales side of the organization?

GARFIELD: A lot. I always think that I have been a consummate salesman. I was always very eager to talk to people about our products, and pursue leads. I was a supersalesman. That was something I didn't leave entirely. I still have a temptation to get on the phone and sell somebody our products. And I always tended to engage in almost constant dialogue with the customers. In talking to them I seem to be able to even help them figure out what they wanted. Then, if there were enough others who wanted the same service or feature I'd give it to them. That's what I did with the drug companies. I got twelve of them together and said, "Look, you can't all have your way. You have to compromise and we'll give you a product that all of you can use." It was a shared product. Everybody wants personalized software, but you can't afford it. That's what it was all about. I got tired of doing the same thing over and over again for the same people. Why not maybe a product that everybody can use?

Well, you still see problems with that today. That's how we got ASCA (Automatic Science Citation Alert]. We had the first commercial SDI service. It's still going. It's a vastly under-utilized service. Maybe it will come into its heyday someday. In a sense it already has with the on-line networks because you can get SDI from the on-line service. I know librarians who couldn't recognize that I had been providing that service twenty years ago. They say, "We do it on-line now." They spend a lot more for it on-line than they do with our firm. That's another thing you find. The cheapest price is not always the best. You can't deliver the best service. I think that is one of the things that was wrong with ASCA. We shouldn't have made it so cheap. We should have made it better, made it easier and even included the original documents.

THACKRAY: In the early 60s you have Current Contents, Index Chemicus and Science Citation Index coming into their own. As you look back over the past 25 years of those publications, what are the peaks that stand out in their development?

GARFIELD: Well, you may not look at it as a different product, but the decision to publish cumulations was a major risk decision. It turned out to be the most important financial decision we ever made.

THACKRAY: Was that because more libraries wanted them?

GARFIELD: It just means an additional revenue that we can get and it proves the utility of it. It makes it less cumbersome to use. Then we also went back and did this for 1955 to 1964. We went back and did back years. That was unheard of. I'm very proud of that. I think that will be a major tool to the historian of science. Now we're almost finished with 1945 to 1954. Who would have thought that anybody would pay $25,000 for that set of books? That's what it had to be, and I knew that it had to be. I could have taken a bath for it. But somehow, instinctively, I just thought $25,000 was about it. There will only be about a couple of hundred libraries that will ever buy it anyhow. That's what it takes.

Hopefully all those libraries will continue to go with the thing until we go back to 1900 if we can. Every time we do it, we realize that there are projects for which people find great use for it. Once it is out, then historians and others will see utility for it. Even to this day, we still would like to answer the question, "Who was citing Avery, MacCleod and McCarty in those intervening years (19)?" Now we'll have answers to things like that. It is amazing how many things that are still important today are traceable to that period. What about the postwar years, or by the same token, how many were prewar years? It goes on and on and never stops.

THACKRAY: Would this go back to the Royal Society's Catalogue of Scientific Papers (20)?

GARFIELD: Yes. Every once in a while, you say, "My God. Somebody thought about that in 1870. I wonder what was going on in those few years?" When you think the way I do, in citation terms, you have to have that index for every year where there is a possible citation. The trouble is that as you get back to 1900, the citation practices really get sloppy so you have to do real textual analysis. The journals didn't have the formalized citations so you have to do what we do now with the social science texts.

STURCHIO: Do you mean in going back to the early years?

GARFIELD: No. We haven't gone back to the early period yet in the social sciences. We've got from 1966 onward. We're producing 1956 through 1966 now. I don't know when we'll ever go back and do the arts and humanities. There is the place where you really could use it, but they could least afford it. We're lucky if we get them to buy it now.

THACKRAY: That might be something NEH might support one day.

GARFIELD: We sure have the facility for doing it now on our own. I hope that we could cost it out so that the typical customer could afford it. I don't know. It is very labor intensive work.

THACKRAY: I should think NEH might really be interested in that.

GARFIELD: They might if somebody like you suggested it. I've always had that conflict of interest. When I go to these foundations, they have to interpret what I want as being in my self-interest, and in a certain sense it is. If somebody else was able to come up with a deal, I would gladly do those back year things for them at cost, just to get them done.

THACKRAY: I think when the science departments are further along it may be appropriate since we need it more.

GARFIELD: How much further along, I don't know. I think the compact disc will change things because there will be a lot of pressure on us to put that on compact disc. I think you could put that whole darn thing (Arts & Humanities Citation Index) on two discs. That makes it so much more accessible to so many more people. I think the individual professor of humanities could

conceivably have the arts and humanities right on his own desk and that's the way he should have it. You know, it's like having the Oxford English Dictionary. It's the same thing. These tools should be the scholar's tools. That was my ultimate hope.

THACKRAY: What's changed about your own work as ISI has grown?

GARFIELD: I used to be far more heavily involved in day to day work than I am now. Over the years I have let my interest in writing take over a lot of my time, although I have developed a particular editorial team and style that makes it fairly efficient. I've trained a lot of good writers who left me. That is similar to the kind of thing that happens to a newspaper. I feel far more removed from things than I used to.

I recently had a sad experience with bringing in a chief operating officer who just did not understand the business and he had some other personal problems as well. If that had worked out right, that would have given me more time to devote to R & D and editorial writing. That's basically what I'm interested in, as well as new product development. There are so many things that could be done with this data base that just haven't been done. That doesn't mean that they would necessarily be profitable, but they would be useful. A lot of people who are willing to pay for it on a cost basis can get a lot of interesting stuff out of this.

For example, I did study the 1,000 most cited authors. I did that as an editorial project. It cost us an unbelievable amount of money. We spent over $100,000 to do that series. I can't easily get a grant to do such studies. There's the data and I would gladly do that study. I have been waiting for three years to do just the follow-up for the 400 new names. I've got the names, but I just can't afford the cost of untangling the homographs because I don't want to publish erroneous data. So, I've thought of setting up some sort of nonprofit foundation that would do that. Again, that takes time. You have to be able to hire somebody who can apply for the grants. That's something we should have paid more attention to. We've had ups and downs. I guess I have had a lot of people come and go here who are not necessarily as loyal to ISI as I expected. And maybe I was unreasonable in expecting them to stick with it. But a lot of other people are using our data for all kinds of purposes. If we can I would like to see this International Science Indicators Project get off the ground, but that's taken a long time too.

THACKRAY: Let me take you over to The Scientist for a minute or two, because like so many other things you've done, the germ of it goes way back.

GARFIELD: The newspaper idea goes back so far that it includes a proposal I made to the National Science Foundation. It was a formal proposal to publish the Science Citation Index in a newspaper format. Derek Price refers to that in one of his letters which I have right here.

In a letter of March 8, 1963 he says, "Dear Gene: As regards the science newspaper, I see we have rather different ideas. My own notion is for something that looks very much like the Wall Street Journal, but substituting mimeographic indexes for the Stock Market Prices and having a page for each of the chief sciences written in the style of fairly medium news with the addition of a more newsy column for the experts and other readers. And of course, with the front page of the daily devoted to the main social and political news and gory details of all the breakthroughs. If you have not done so, would you send a copy of your proposal to Ed G. McAlis." That goes back quite a ways. What is missing here is a copy of my proposal. I'll have to get it out. What else can I tell you about the newspaper and its start. The story about it was told, in a condensed form, in Current Comments (21).

THACKRAY: I think we really want to conclude this interview, not by concluding the history, because the history is only in the middle, but by asking you to look forward. What do you think your own preoccupations are going to be in the next several years?

GARFIELD: Well, you may have heard from other people that when an organization gets beyond a magic number of perhaps 400 or 500 people, it takes on a different kind of life and it takes a different kind of management to run it. I think that one way or another, ISI will have to get linked in with probably more capital, more technology, and people with vast resources. We're dealing with satellite communications and all sorts of things. I think that unless we are extremely lucky, it will be difficult to finance all these new technological developments, such as CD-ROM. It will cost us a great deal to do this. Either we will have to go back and become a small company, so to speak, that stagnates and hangs on for the rest of my life, or it will become significantly big. It wouldn't be surprising to me if by one series of transformations or another this went from a $40 million company to a $100 million dollar company in five years or something like that.

Where that leaves me is really not clear. But one way or another, I am sure I will have a role in it. I will still be involved in the idea stage and the editorial slant. The newspaper has to have a kind of integrity that I want even though it changes. If it has to become like a general yellow dog kind of thing, I'd just as soon get rid of it. I never thought it would be so difficult to look for people that would produce an editorial product that does satisfy my desires without having to

sit and manage it every minute. As a publisher, it is a different kind of role for me and I have to decide how much time I am going to give to that and have to sacrifice other things to do it. Ellis Rubinstein is a marvelous editor and we can have a superb editorial product. All it now needs is the business management to gain advertising and circulation on a large scale.

You know, I could just devote all my time to Current Contents. I could improve it in many ways but I haven't had time to spend on it. Moneys that we spent on other things could have gone into making it much better. But I had the fun of doing the other things. SCI has all kinds of dimensions to it that need to be expanded. I hope that Chemical Abstracts and others will put SCI up on their network. I wanted to do a Chemistry Citation Index. The whole thing really ought not to be separated. How you get all these organizations together is hard. Eventually it has to happen. Even Index Chemicus. As the others get better, does it become redundant? Its funny that drug companies still think it is necessary. In a certain sense I think that CA still hasn't been able to do what is necessary and maybe we could just join them together. I don't know.

I think I don't really want to spend much of the rest of my time doing things that are really not necessary, and that other people could do just as well. The Atlas of Science, of course, is the next big project (22). I think, as a matter of fact, from the start of things, that it is a harbinger of things to come. It has almost infinite possibilities. We're only planning twelve sections right now, but it could easily be 100. It is difficult to make people realize that a series of review journals can be collectively an Atlas. As a result we have to modify the title we've given to pure research review journals.

[END OF TAPE, SIDE 5]

THACKRAY: In retrospect it seems as if you had an almost ideal education for what you turned out to do. What would you do differently about that background if you were doing it over?

GARFIELD: It's hard to know. If people had let me go in the course I had originally tried to do, I would have stayed at Hopkins. Dr. Larkey would have kept me on and I would have my Ph.D. in the history of science. I started out there and I took courses at Hopkins. I was fascinated by the history of science. Maybe, I was inherently an engineer all along. I mean, the fact that I went into engineering in the beginning--to build a better mousetrap? Maybe that was in me. I have made some contributions to the theory of my field but I don't think of myself as that much of a theoretician. You need time and temperment for that.

I once had an invention called "radio retrieval" which I think you would eventually see as the antecedent of the parallel processor. Some other time I can tell you about it. It was so outrageous an idea that I was afraid to tell anybody about it. Parallel processing turns out to be a fantastic field. It's not the education. It's hard for me to say.

I sort of like teaching. I did teach at Penn regularly for quite a few years. I didn't have enough time to do it right. I always felt a little ashamed in front of the students. I always thought they were looking at me like I was covering up the gaps in my knowledge. It was a strange feeling. I don't know how teachers get over that these days. Every day that goes by they get more and more obsolete. But I guess you just have to learn to slough it off. Maybe it was the kind of people I was getting there.

You see, I wanted to teach at the Annenberg School and George Gerber, bless his soul, wanted me to submit a proposal there to do a program in scientific communication retrieval. How can I possibly have done that in the time that I have available? I was offered the Deanship at Drexel years ago. Can you believe the guy wanted me to leave my company. I told him, "One of the best, most respected people I know is a chairman of a department at Stanford and is Vice-President of Research at Syntex." That was Carl Djerassi. And I said, "Isn't it enough for you that I was willing to take on the assignment of dean of the goddamn school, you want me to give up my company? I'd be crazy?" So it never happened. I mean a lot of people are interested in what you can do. And I might not do the other as well. Everybody has his limitations.

THACKRAY: I'll tell you what we're going to do, Gene. We're going to come back in 25 years and check with you on those intervening years.

GARFIELD: That's a very nice thought. I appreciate that thought. I'd be very happy to confirm that.

THACKRAY: It's a date.

GARFIELD: It's a very pleasant way to end the day. I'll tell that to my wife.

THACKRAY: Thank you.

[END OF TAPE, SIDE 6]

NOTES

1. E. Garfield, "Of Beggars, Bagladies, and Bums," Essays of an Information Scientist (Philadelphia: ISI Press, 1983), Vol. 5, pp. 395-405; Garfield, "To Remember My Mother," Ibid. (Philadelphia: ISI Press, 1977), Vol. 2, pp. 535-536; Garfield, "To Remember My Father," Ibid. (Philadelphia: ISI Press, 1980), Vol. 3, pp. 422-423.

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3. Sidney A. Bernhard, Eugene Garfield, and Louis P. Hammett, "Specific Effects in Acid Catalysis by Ion Exchange Resins. III. Some Observations on the Effect of Polyvalent Cations," Journal of the American Chemical Society, 76 (1954): 991-992.

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