Henry Small - Supplement to oral history interview

Family: My paternal grandfather was school principal in the Chicago area, two uncles were high school physics teachers in Chicago, and my father was a history/civics teacher in my high school. My mother was an elementary school teacher, and later in life taught at the Latin School in Chicago. Her father was a farmer and brick layer who owned a farm in Southern Illinois where we spent our summers.

1955-1958 I attended Amundsen High School in Chicago where my father taught. I had to be a model student and stay out of trouble. I did well but the school was not challenging. I did poorly in an aptitude test and was advised to go into sales. I was president of my senior class. I thought I wanted to be a scientist and studied chemistry and physics books (e.g. by Geoege Gamov) on my own and had a chemistry set. I kept notebooks of ideas, and thought about the causes of physics problems. I was intrigued by the idea that atoms were tiny solar systems, and the solar system was an atom in a larger world. I knew most of introductory chemistry by the time I got to high school. My father got me interested in the civil war. He had a record of his grandfather's service. I took piano lessons for a few years but was not very good. I preferred to think about music. My first essay in high school was on the concept of time. My father took me to John Crerar Library in downtown Chicago to do the research. I recall enjoying doing the research which I guess set my course early.

Another formative experience was going to IIT for a career day with a friend of mine. We had both won awards for drafting; he got first prize and I got second. At IIT we learned about careers in science and engineering. I also got a prize at a science fair for a very simple experiment on what determines the pitch of a vibrating string (I played guitar in a folk group). Mine was the only experiment that worked so I got a prize. My drafting skills came in handy in my first job at A.C. Nielsen lettering market research charts.

1959 – 1963 I was an undergrad at Univ. of Illinois majoring in chemistry, but interested in the humanities. I had an Illinois State Assembly Scholarship through the efforts of my father which paid the tuition. I didn't have the ambition to aspire to a better school. All my friends were going to U of I for engineering, but aside from chemistry I didn't know what I wanted to study but couldn't take a chemistry course until my second year because they were full. I was attracted to the humanities (not engineering), and theories about everything from poetry to music to science. I took courses in literature, philosophy and psychology. I had an English composition class with Stanley Elkin (later a famous author). His theory of art was creating connections. This might have influenced my thinking later on.

I studied German and did so well that I took a trip to Germany in my sophomore year. I worked at a brick factory for a month and then toured around Germany in a VW with another student. We ran out of gas in East Berlin and had to push the car through the Brandenburg gate. There were cheers from a big crowd on the western side. They were demonstrating the erection of the Berlin wall.

Much to the surprise of my high school teachers I was Phi Beta Kappa in my junior year (1962). My major was in chemistry but I had taken so many liberal arts courses that I was eligible for a

BA degree. I really enjoyed biology and should have continued in it. Illinois was so big I tried to find a biochemistry professor to discuss that option but I couldn't find him. I thought about possibly going into psychopharmacology.

In my senior year I attended a lecture by Charles Osgood in the psychology department at Illinois who talked about his citation map of psychology journals. This was my first exposure to bibliometrics, but I don't recall being that interested at the time. I was impressed by Lajaren Hiller who went from chemistry to music and wrote computer music. (Later when I studied electronic music in NYC with Steve Reich I wrote Hiller a letter about my ideas on how to write music with computers.)

1963-1967 I was a teaching assistant in chemistry at the Univ. of Wisconsin, Madison (1963-65 in chemistry dept. and 1966-68 in history of science dept.). I taught freshman honors chemistry for 5 years, 1963-67, and gave a seminar on noble gas compounds which were discovered around that time. My research project was to get the vacuum ultraviolet spectrum of titanium tetrachloride. It was used by my major professor Richard Fenske to do what was called semi-empirical molecular orbital theory, but I found the math like a black box. So I decided to get my MA in inorganic chemistry in 1966, and transfer into the history of science. I had taken a history of chemistry course from Aaron Ihde who wrote a text book on the history of chemistry. Bob Siegfried, the dept. chair in history of science, had a set of chemistry journals in his office. I recall asking why we couldn't just do history with the published literature. He said I needed to study diaries and unpublished manuscripts as well. But that idea stuck with me.

I had a University of Wisconsin fellowship in history of science from 1967-68. My PhD thesis was on the helium atom in the old quantum theory. It was a detailed history of a failure in the old quantum theory, what Kuhn called as an anomaly, which led, in part, to the new quantum mechanics in the mid-1920s. My major professor in the history of science, Erwin Hiebert, took a new job at Harvard at that time. When I was at Wisconsin I regret not meeting Warren Hagstrom who was a sociology professor.

In my dissertation on the helium atom in the old quantum theory I came up with a theory of how science develops based on my detailed internal history. I later worked that into a paper I presented at the annual history of science meeting in 1969. I quantified the degree of "certainty" in various assumptions of the atomic theory at the time, and showed that scientists tried varying the least certain assumptions first to get the theory to work. Bridging between facts and theories, also suggested a pictorial representation of problem solving. Gerald Holton was interested in my thesis and wanted to publish it as a book, but the deal with the publisher fell through.

1969-1972 Before I completed my dissertation I took a job in NYC at the Center for the History and Philosophy of Physics and the Niels Bohr Library located at the American Institute for Physics (335 45th street, NYC). At CHPP I had the position of a Research Associate from 1969-1970, and then acting director from 1971-1972, when Charles Weiner, its director, took a sabbatical at the Niels Bohr Institute in Copenhagen. Weiner was interested in émigré physicists who came to America in the 1930s. At CHPP I worked on an NSF funded project on the historical development of nuclear physic as a research field. I was mainly interested in the conceptual and technical development of the field which had also been my approach to the helium problem. My challenge was how to study the development of a research field. At AIP I completed my dissertation on the helium atom and took my oral exams in 1971 in Madison. I had completed my written and oral exams in both chemistry and history of science, and qualified for a joint PhD.

In 1969 I participated in a combined philosophy of science and history of science meeting in Minneapolis, Minn. This is the first scholarly meeting I had attended. A hot topic at the time was how philosophers and historians of science could work together, whether philosophers have any use for historical evidence or how historians could use philosophy. I was thrilled to be a commentator on one of the papers, and my comment was published the proceedings. I discussed how scientists selected the theoretical assumptions they needed to modify in order to make a theory work when faced with an anomaly based on my dissertation. This comment was my first "publication" however short, and gave me confidence to continue in academic work.

At CHPP I worked on oral history interviews and preservation of papers of famous physicists. One assignment was to retrieve Robert Oppenheimer's reprints from the basement of the Institute for Advanced Study in Princeton. But my main job was to work on an NSF grant to document the history of nuclear physics. I needed a way to lay out the history of the field. I recalled my comment to the professor at Wisconsin about using the scientific literature to do history. There was a small library of journals and printed indexes at the AIP Niels Bohr Library, and they had a complete run of Science Abstracts (later Physics Abstracts). Also they had one of the biggest collections of books in the history of science. This is where I met my wife Lois who also worked at the AIP for the American Physical Society as executive secretary. We were married in 1971 before I took the job at ISI.

At AIP I got to know Sam Schimovitch who worked on their classification scheme for the physics literature. AIP was the primary publisher of many physics journals. I also knew Art Herschman who headed the information division of AIP. Eventually he told me about ISI and the citation index. Sam had developed an iterative algorithm using citation data from AIP base on Mike Kessler's idea of bibliographic coupling. This was very influential in my work on co-citation because it showed me you could get compact clusters using citation data. I used to see Sam in the nearby engineering library pouring over the SCI and working on his algorithm.

I began coding entries from Physics Abstracts for the 1920s and 1930s on 3x5 index cards, and also coded all nuclear physics articles in the Physical Review. From Physics Abstracts I coded the indexing terms assigned to nuclear physics articles. From the articles I coded authors, author addresses, key words, and a sampling of cited references. I tried every way I could think of manipulating and counting these data elements, especially their joint usage, including co-occurring words, co-occurring index headings, author collaboration, and eventually co-occurring cited articles and cited authors. (I called these "pairings" but did not use the term co-citation until I went to ISI). Pairing references didn't work very well because I had collected only the first five or so references for each paper. I could show how new index headings or key words linked up over time, and how different branches of nuclear physics connected. This convinced me I was on the right track.

My report on nuclear physics in the Physical Review was the source of a lot of what I did later in my career. My aim was to create a kind of network diagram of nuclear physics in each year so I could show how the field was changing as a series of cross sectional structures a la Kuhn. I hoped to see new topics coming in and old ones leaving. As I read more in the bibliometrics literature I realized most of the earlier work had been concerned with rankings of various kinds but there was very little work on networks. There was Garfield's pioneering work on historiographs, Derek Price's network study, and of course Mike Kessler's work on coupling. So my focus became what I called "mapping" based on co-occurrence pairings of various descriptors. This seemed to be a wide open area. Kessler had avoided citations because they implied evaluation. I recall walking past the AIP computers on my way to the library thinking that maybe the Fortran I learned in grad school would come in handy someday in automating this sort of analysis. One of my influences at the time was Arthur Koestler's book "The Act of Creation" which showed how new ideas were unexpected comings together. I wrote up the part of this report dealing with co-occurring classification heading as a journal article and submitted it to ISIS, the lead history of science journal.

In 1969-1970 I made my first presentations at a history of science society meetings. These were on "Niels Bohr and the helium atom" in 1969, and on "nuclear physics in the physical review" in 1970. In the first paper I quantified the degree of "certainty" in various assumptions of the atomic theory. I recall that paper creating some consternation due to my use of quantification. In my nuclear physics paper I ventured into bibliometrics and mapping the discipline. Historian were not too keen on this either.

I was fortunate to have a few interactions with Kuhn. My major professor Hiebert who did a stint at the Institute for Advanced Study, invited me to have dinner with Kuhn in the Princeton barracks. I found out that Kuhn was thinking about doing a book on the helium failure, the topic of my dissertation. If this had happened my PhD would have been in jeopardy. I have always been thankful to him for not doing this. For the quantum history project with John Heilbron and Paul Forman, Kuhn had collected Bohr's notes on the helium problem which I made extensive use of in my research. They were housed in the American Philosophical Society Library in Philadelphia. I also met Kuhn in NYC because he was on the advisory board for the Niels Bohr Library and CHPP. He showed up once a day early for the meeting. He was in my office with his feet up on my desk. He was a very powerful personality but somehow vulnerable and highly sensitive to criticism.

At the same time I was working at AIP I was studying electronic music at the New School with Steve Reich. Steve was an inspiration to me because he and other minimalist composers trying to forge a new musical style and break with tradition. They also faced a lot of criticism. I had to decide whether to opt for performing at the Kitchen, a new music venue in NYC, or take the job in Philly. I decided to do the latter.

About this time I found out that the NSF grant was coming to an end and I needed to look for a job. I decided I wanted to work with a big bibliographic data base (where the data was already on magnetic tape) and wrote nearly every Abstracting and Indexing service asking if they wanted to hire a historian. I used a list of A&I services in a report by K.D. Carroll called "survey of scientific-technical tape services" which was an AIP report. I contacted Chemical Abstracts,

Biological Abstracts, Psych Abstracts, etc. I got a reply from Dale Baker at Chem Abstracts, saying he was interested in the history of chemistry but they didn't have a position. He sent me a graph. I also applied for jobs at history of science programs in universities, but the job market for academics was tight. So I looked to industry.

Art Herschman knew I was interested in bibliometrics and history of science and suggested I write Mort Malin at ISI. He knew Mort was looking for someone to help him get government contracts. In my letter to Mort dated Oct. 11, 1971 I said I was interested in identifying new growth points in science using bibliographic data. This is an allusion to a paper by Jack Meadows. The most promising approach, I said, was the "monitoring of key word or citation clusters", and I sent him a copy of my report and paper on nuclear physics. My letter suggested a specific product which would provide statistical information on current growth points in science for science planners and administrators.

I got an immediate reply from Mort who came to NYC to meet me and we had coffee in Grand Central Station. It turned out he had a degree in history as well. Later on Gene came to NYC and we met briefly. A memo from Gene to Mort Malin from Nov.23, 1971 describes our first meeting. We met at Penn Station, his train just arriving from Philly. He described me as a "pleasant person" but someone with no corporate experience. I recall saying something like I was just an ordinary guy, and he said they don't hire ordinary people. I had sent Gene my report on the bibliometrics of nuclear physics. I promised I would send him an outline of what I could do at ISI or a possible NSF proposal. I later went to Philly and met Gene at a AAAS meeting where he made me a job offer.

1972: In 1972 ISI was located at 325 Chestnut St, Philly. My office overlooked Carpenter's Hall in the Independence Hall historic section. In March I started at ISI as a Research Associate reporting to Mort Malin. Mort had worked at NSF in the information division.

Arriving in Philly I was pleasantly surprised at the welcome I received. Gene was a strong personality but open to my ideas and interested in using his index to study the history of science. My wife and I got to stay in the company apartment in Society Hill Towers until we could find a place of our own. I was excited that I would have access to a big database on science and a real job. The first thing I did was write a detailed memo for Mort and Gene on what I wanted to do. I recall that I first wanted to look into co-word patterns because that seemed to work well in my nuclear physics study, and ISI didn't have a classification scheme. They did have something called the Permuterm subject index, but words turned out to be difficult to use due to their multiple meanings in different fields. Then I tried the citation index. I picked a well-known physicist Murray Gellmann and found a couple highly cited papers in the SCI. I looked at the citing papers for other references that were also highly cited. It didn't take long before I was able to develop a network of highly cited and co-cited papers around Gellmann. This convinced me that there were strong patterns of joint citation in the SCI that involved key authors and papers and that these expressed stable subject matter relationships. I immediately dropped the co-word work and concentrated on citations.

This work became the basis of my first paper on co-citation, which I started writing a couple of months after my arrival. I wrote the paper up and walked it over to Art Elias at Biosis who was

then the editor of JASIS. Gene sent the paper out to some of his contacts and I got back some critical comments, but I resisted making substantive changes. One criticism was that I did not use a proper clustering algorithm, a defect that on worked on correcting for my second paper. Both Belver and Gene also suggested changes. I was lucky that this was my first published paper because it gradually had a big impact. It also gave me some status at ISI, and was sort of a rite of passage.

After the paper was published Tony Cawkell brought to our attention a paper by Irene Marshakova from Moscow. He had it translated from the Russian and it had a lot of the same ideas. Later in my career I was able to meet Irene on a couple of occasions at ISI.

A few years later I was browsing in an old conference proceeding volume that I found in Gene's office. It was from the International Conference on Scientific Information from 1959. I saw to my surprise a quotation from by Joshua Bar Hillel about something he called "co-quotation". I tracked down the paper by Hillel in American Documentation from 1957 which had the title "A Logician's reaction to recent theorizing on information search systems". I wrote to him but he had suffered a stroke and died soon thereafter. Hillel had also hit on the idea of co-citation but he did not think it would work. Years later Don Swanson suggested I write a paper noting Bar Hillel's priority and I cited Bar Hillel work in some of my later papers.

Around this time I got the news that the paper I had submitted to ISIS had been rejected. It used a methodology which was alien to them. One of the reviewers was my major professor in the history of science. He said I had reduced history to "statistical bare bones". This was very discouraging, and eventually I found a much more congenial audience among information scientists. Eventually I left history of science and became an information scientist. This experience shows that you have to dress your radical ideas in conventional clothing if you want them to be accepted.

1972: (April) I met Belver Griffith, professor of information science at Drexel Univ., as he was getting into an elevator at the ISI building. We said hello just as the elevator door was closing. I heard his tidewater Virginia accent say that I should get in touch and come to his office at Drexel. Belver had been involved with ISI in coming up with research ideas. And there was a published conference proceedings from around 1970 which he and Derek Price had papers in. My collaboration and friendship with Belver was important because he was a senior researcher, knew a lot about research methods, and had a big network of colleagues, including Derek Price, Nick Mullins, Susan Crawford, Diana Crane, etc. Many were social network researchers. ISI was a sort a nerve center for meeting people in sociology and information science, just as CHPP was for the history of science.

1972: (May) First mention of "co-citation" in my lab notebook. At AIP I had referred to these as "pairings".

1972: (May 15) I made my first trip to Drexel to see Belver. I thought his name was "Belva". Belver's office had dirty socks, squash racquets, and sailing paraphernalia strewn about. We discussed writing a paper and then adjourned to the Green Tree tavern next door to the Rush

Building for a couple of beers. This became our pattern -- few minutes of research and then off to the bar.

1972: (June) I wrote up a grant proposal for NSF. The proposal was called "mapping science" and was submitted to Sidney Passman at NSF in July. This grant supported a lot of the early cocitation research at ISI and Drexel. It was unusual for NSF to support work at a for-profit company. Outside consultants were Belver Griffith, Derek Price, Frank Carmone (an expert on MDSCAL from Drexel), and Joseph Kruskal from Bell Labs. I recall Passman saying that did not want us to engage in what he called "recriminative" number crunching by which I think he meant invidious comparisons of individuals.

1972: One major hurdle was to automate creation of a complete file of co-cited papers for a citation index file. I referred to this as a "pci" or permuted citation index. I came up with the idea of sorting the citation index in source item order and permuting the highly cited references in a given source. Then by sorting this permuted file I could determine how often each pair of references was cited together. A programmer at ISI named Ed Cline implemented this procedure. Cited reference pair files were delivered to Drexel programmers Sandy Dey and Judy Stonehill who wrote a single-link clustering routine which worked well due to the high precision of the co-cited pairs. Belver introduced me to multidimensional scaling which we used for mapping the papers in two dimensions. I met with him almost once a week during 1972. Back at ISI I worked with programmers to scale up the algorithms to cluster full annual SCIs. Later on the clustering was moved to the ISI mainframe computer which had a higher processing capacity. A few years later, Lou Holmes took over programming. Lou and I would sometimes put in all-nighters in the ISI computer room running annual SCI clusters so it wouldn't interfere with ISI's daily production.

1972: Another idea I had was a method to link clusters together to form higher level maps using the residual co-citations first the lower level. Belver was sailing on the Chesapeake Bay and I called him on his ship-to-shore radio to discuss this idea.

1972: (Dec) Belver and I visited Bell Labs to see Conyers Herring, who was involved in National Academy of Science reviews of physics, and Joe Kruskal, the inventor of the MDSCAL. Kruskal was very interested in our work on co-citation and wrote a long memo outlining a number of ideas on how it could be mathematically modeled.

1972: Ira Yermish (Gene's PhD student at Univ. Penn where Gene was an adjunct professor) was writing an experimental "on-line" citation network graphics system which ran on Penn's Univac computer and was written in Univac machine language. Ira used a Tektronics terminal with a vector display to draw citation networks, the first system of its kind.

1972: (June) Made a trip to Washington, DC with Mort Malin to see Bob Brainard who was running the Science Indicators operation at NSF which was using the ISI database. The contract had been given to CHI under Fran Narin rather than to ISI and Mort wanted to get the contract for ISI. We never succeeded in doing this but eventually started our own indicator series and products. Malin and I stayed in the Holiday Inn next door to the Watergate and in retrospect

realized that the Watergate break-in had been going on at about the same time. We also had meetings at NIH. This was the first of many trips I made to DC.

1973: I attended my first ASIS meeting and gave a paper on the relationship between citation indexing and word indexing by connecting cited references to title words of the citing papers. This was an attempt to reintroduce language content, but a better way turned out to be using citation contexts (see below). This was the first of many talks I gave at ASIS meetings. I always got a friendly reception at ASIS in contrast to my receptions at HSS and 4S meetings where there was hostility to citation analysis and quantitative methods.

1973: I met Derek Price for the first time at Drexel. Belver invited him often to give talks to the students and faculty. He was noted for his "back of the envelope" calculations. He also made frequent visits to ISI to see Gene and see what he could salvage from what he called the "cutting room floor", by which he meant discarded printouts from which he could make various bibliometric estimates.

1973: Belver and I submitted the two-part article to the journal Science Studies (later named the Journal for Social Studies of Science). Part 1 was on the method for creating specialty clusters, and part 2 on the micro- and macro-structure of specialties.

1974: Made a presentation to the local ASIS chapter on April 16, 1974. Carol Fenichel was the chairperson. I recall showing a cartoon of a Leonardo Da Vinci-like character throwing a contraption out of a window to see if it would fly. That was how I felt about my co-citation work. Later on I made a presentation to a group at the Franklin Institute and was on their awards committee.

1974: I attended a meeting at the Univ. of Illinois at the invitation of Martha Williams. It was their Allerton conference on systems theory, and in addition to Martha I met Gerry Salton for the first time. This was my first trip back to Illinois since my undergrad days.

1974: I explored explicit geometric models of co- and tri-citation with the help of my brother-inlaw Julian Noble, a physics professor at the University of Virginia. He wrote a program to find the volume of intersection of two or three Gaussian hills for my paper which allowed me to compare the volumes to co- and tri-citation counts. I got the idea of modeling the citations to a paper as a Gaussian hill from Joe Kruskal.

1974-1979: I became a Senior Fellow of the Dept. of History and Philosophy of Science at the Univ. Penn. where Arnold Thackray was dept. chair. I attended many of their weekly colloquia for a number of years and later worked on a history of chemistry project with Arnold.

1974: (Sept.) Belver and I interviewed Barry Blumberg at Fox Chase prompted by our cocitation cluster on the topic. Blumberg was working on a flow chart for his discovery of the socalled Australia antigen (the hepatitis virus). We also had meetings with Ron Breiger from Harvard, a student of Harrison White, the quantitative sociologist, regarding how to analyze roles in social networks. 1974: I began work on case studies of 29 specialties, collagen being one of them. Interviews were conducted and a questionnaire survey. Collagen provided an example of a micro-revolutions. This was part of my effort to show that the clusters were meaningful constructs for the scientist who were active in these areas and that I had identified the key players and ideas for each specialty.

1974: Also began a study of highly cited papers in chemistry where I systematically collected citation contexts and found that many papers were cited using the same language which led to my paper on concept symbols. This project also looked at how the highly cited papers had been received by requesting referee reports from authors. In some cases I found that papers were not initially well received. This study was important because I needed a better understanding of the unit of analysis I had selected for mapping.

1974: Attended a conference at the Center for Advanced Study in the Behavioral Sciences in Stanford, Ca. on the evaluation of science indicators. I participated along with Gene, and we submitted a paper for the book Toward a Metric of Science. The conference and book was intended as a critique of the newly released NSF Science Indicators report, the first edition of which appeared in 1972. The conference was sponsored by the Social Science Research Council and, we later reconvened at their office in Washington, DC in 1976, where I substituted for Gene. In this group I got to meet some important social scientists including Robert Merton, Harriet Zuckerman, Jonathan and Stephen Cole, Yehuda Elkana a historian from Israel, O.D. Duncan, and Zvi Griliches an economist. I don't think this high power group had much success in influencing the NSF Science Indicator program however. I recall explaining co-citation to Merton as a straddling of the shoulders of giants.

1974: I attended a joint HSS/4S conference in Berkeley, CA. It dealt broadly with approaches to science studies, historical and sociological and was very international. Our first papers on science mapping had just been published. David Edge gave a talk and criticized our work. I asked him to explain what was wrong with the methodology or assumptions but he could not reply except to say that he was interested in the behavior of individual scientists. The new constructivist sociology was emerging in Edinburgh and Paris in the work of Barry Barnes, David Bloor, Edge, Mike Mulkay, Bruno Latour in France, and Karen Knorr-Cetina in Germany. These approaches would later be called social constructivism or the strong program. This became the dominant approach in sociology. Constructivists believed that scientific knowledge was socially constructed, and scientific findings had no basis in reality. They traced belief systems to power or authority. Naturally, citation analysis and quantitative evidence was rejected because it claimed to offer a "true" and objective picture.

Obviously this created a rift between what we were doing in citation analysis and bibliometrics using quantitative methods, and what the 4S/HSS groups were doing. This would eventually result in our forming of our own society in 1993 called ISSI. ISSI became the focus of citation studies and 4S the more qualitative approaches based on lab studies and constructivism. Ironically, Bruno asked me for citation index data for his anthropological study of the Salk Institute which I provided by photocopying pages of the SCI. He visited ISI around May of 1977

to collect more data. He later used these data in his books Laboratory Life and Science in Action. He described citation practice using a warfare metaphor.

1975: (March) A workshop conference on citation data in the study of science, sponsored by NSF, was held in Elk Ridge, Maryland. It brought together leading researchers in citation analysis, including Nick Mullins, Gene Garfield, Derek Price, Belver Griffith, Stew Gillmore, Lowell Hargens, Fran Narin, etc. I presented on co-citation mapping. Gene brought his saxophone and practiced in his room at the conference center. Nick Mullins, a basketball fan, was preoccupied with watching March madness on TV. We also presented work on a comprehensive bibliography of citation analysis under the same NSF grant. We updated this regularly starting in 1974. Janet Stanley, Ming Ivory, and others in my group contributed to this effort. This bibliography was distributed in printout form for many years. This project gave me a better grounding in the literature of this field and a firmer sense of its history.

1975: Steve Aaronson's article "The Footnotes of Science" was published in the IBM journal Mosaic. This article reproduced our maps illustrating how biomedical research had changed from 1972 to 1973.

1975: Nicholas Wade wrote a Science editorial titled "Citation Analysis: a new tool for science administrators" which included our first annual maps of science using 1973 and 1974 data. These were the first maps based on annual cumulations of SCI data. Prior to this maps had been based on quarterly SCI files. The layouts were diagrammatic rather than analytical. Strong connections were shown as lines but the layout was not rationally derived as it was in later work using multidimensional scaling or force directed placement.

1975: 4S was founded and in 1976 the first meeting was held at Cornell Univ. My paper was coauthored with Diana Crane. I had met with her at the Institute for Advanced Study in Princeton where she was on sabbatical. Our paper was on specialties in the social sciences based on a cluster analysis of the Social Sciences Citation Index (SSCI) which was a separate index at the time. Belver and I had been working on a similar paper but he was unable to complete work on it, so I collaborated with Diana. (The paper with Belver was eventually published as a report by the Royal Institute of Technology in Stockholm. Another example of work with Belver that was not completed was the case study of Australia antigen based on interviews including the one with Nobel Prize winner Barry Blumberg.) At the Cornell meeting, Bruno Latour presented his revolutionary paper on the anthropology of science. There was a big argument between Derek Price and Everett Mendelsohn of Harvard, a historian of science. Following this meeting there was more friction between the quantitative and qualitative factions of 4S.

1975: Attended ACS meetings, one in Chicago and another in NYC. In Chicago I gave a paper to the local ACS chapter on "Refereeing the referees". Bob Buntrock was chair of the local chapter. The NYC presentation was organized by Bonnie Lawlor head of the ISI chemistry division. Even though there was not much interest in bibliometric in the ACS, I presented there on a number of occasions.

1975: (Dec) I was invited by Bill Goffman to give a talk at Case Western Reserve. We also discussed my taking an academic position at Case Western. Bill was a retired airline pilot and

mathematician. I turned down this and other job offers because I felt that my research depended on having access to ISI data. Therefore I did not leave ISI (later Thomson Reuters) until 2010 when I went to SciTech Strategies where they had access to Elsevier's Scopus database and also Elsevier full text.

1975: Gave a presentation at ASIS on a model for scientific specialties that evolved out of my case studies of 29 clusters. I represented the specialty as a network of highly cited and co-cited papers that changed over time.

1970s: Attended several meetings of the Classification Society where I got to meet Gerry Salton. This group was active in finding new clustering algorithms. They were mainly mathematicians and computer scientists.

1976: Worked on a study that was presented at the Classification Society and published in the journal International Classification after a meeting with the editor. I wanted to show changes in a map of science from one year to the next using arrows to show movement. Canonical correlation was a good methodology for comparing pairs of configurations. This was a prelude to my trying to animate such changing structures using a PC animation program (see below).

1977: Promoted to Director of grant and contract research.

1977: I prepared an inventory of government grant or contract supported work I had obtained in my five years at ISI. It included the original NSF mapping contract 1973-75; the study of highly cited papers in chemistry and peer review 1973-74; a citation analysis conference and bibliography in 1975; and a study of cluster validity 1976-77. I had published 13 papers and 4 reports. My citation count was 31.

1977: David Edge's "Why I am Not a co-citationist" was published in the 4S Newsletter. Later he published a full length critique of quantitative methods in the history of science in the journal History of Science. I wrote him a long letter to which he did not reply.

1977-1981: I became editor of the 4S newsletter in 1977. My associate editors were Jerry Gaston (then at SIU near the farm in Illinois) and Henrika (Ricky) Kuklick (at the Univ. Penn, history of science dept.). I was a member of the 4S council from 1979-1981.

1977: The second annual meeting of 4S was held in Boston, Mass. (Dec). My paper was on a comparative study of 10 specialties with Janet Stanley as co-author. It was based in part on interviews with researchers at NIH such as Carl Piez at NIDR and others for my specialty validation study.

1977: Had contacts with Hargens and Mullins regarding cluster data for a study they were doing. Sam Schiminovich of AIP also obtained data from ISI to identify key papers in various fields using his algorithm; I had my first contact with Elliot Siegel at NLM where we did contract work; I sent cluster data on plate tectonics to Warren Hagstrom, Jay Stewart, and Paul Allison who wrote a paper on it using a hill visualization, putting my name on the paper. I published my paper on cited documents as concept symbols based on a study for NSF of highly cited papers in

chemistry and a peer review survey. A preliminary version was presented at an ACS meeting in San Francisco in 1976. Some considered this paper to show my advocacy of social constructivism but I meant only that interpretations of the significance of papers could vary but often converged to consensus.

1978: I traveled to Europe with Mort Malin, first visiting Sweden. We met the information staff at the Royal Institute of Technology library who were running an SDI service using ISI data and visited various bibliometric centers. Also went to Budapest to visit Tibor Braun and his staff at the Hungarian Academy of Science and made a presentation to the Academy. We travelled with Tibor to a remote area to find Mort's Jewish ancestors. Hungary at the time was under Soviet control and a communist ran the Academy as well. Finally we visited to CNRS in Paris.

1978: Derek made a presentation at Drexel and afterwards discussed his idea of mapping both cited and citing papers in a grid-like structure -- a torn fishing net as he described it. He proposed that this could be the basis of the "war room" of science. Belver, Derek and I took this idea to the information division of NSF to see if they would fund it, but they declined.

1978: Gave a talk at Temple Univ. in Philly following an invitation from Kate McCain who was then librarian in the biosciences library. I think this was the first time we met.

1978: Ira Yermish, who had developed the experimental citation analysis system, started his own company, and became consultant to ISI.

1978: At some point Gene decided to include annual research front assignments of current papers in the on-line products such as Sci-search and other on-line files such as ISI's Biomed on-line product. They were also included in a print product.

1979: Moved to the new ISI building at 3501 Market Street in University City, near Drexel and Univ. of Penn.

1979: Bob Coward, a recent addition to the research staff at ISI with a degree from MIT in science policy, organized a conference with NSF funding to explore policy applications of the SCI. My paper was on possibilities of analysis using various ways of combining data elements: authors, journals, countries, institutions, references, title words, etc. similar to the possibilities I explored for the physics literature at CHPP.

1979: Gene and I attended a meeting held in a Rockefeller Institute villa in Lake Lugano, Italy. Ken Warren from the Rockefeller Foundation and William Goffman organized the meeting. The meeting was most memorable for it beautiful setting and great food.

1979: My presentation at the ASIS meeting was on citation contexts. My goal was to systematically study the knowledge content of specialties represented by co-citation clusters.

1970s: Was promoted to Director, grant and contract research. The department included Janet Stanley (a bibliographer), Bob Coward (1978), Ed Greenlee, Jim Shuster, and Gabe Pinski. Gabe had worked for CHI before coming to ISI and had developed the influence weight for

journals, an alternative to impact factor, which turned out to be a precursor of Google's page rank.

1979: Susan Cozzens joined the research staff at ISI. Susan was a Merton PhD student at Columbia and used opiate receptor clusters in her dissertation which as a case of a multiple discovery. Martha Dean also started work for us around this time as did Ed Nadel, a sociologist, and Elliot Noma, a statistician who later went to work on Walls Street. The plan was to gear up our contract research capability with policy, statistical and sociology expertise.

1979: Nicholas Kefalides' lab that specialized in collagen and basement membrane research was across Market St. from the new ISI building. Following Latour, I proposed to undertake a participant observation study of his lab but failed to get the project funded by NSF.

1979: Mike Mulkay and Nigel Gilbert were invited to stay in the ISI apartment in Society Hills Towers during their stay in Philly. They were doing interviews for their book Opening Pandora's Box. I took them over to the apartment but found that the lock had been changed, and had to call a locksmith to open it.

1980: Published papers in Journal of Documentation and Scientometrics on how citation contexts for a co-citation cluster could be used to define the paradigm for a topic such as recombinant-DNA. In these studies I used co-citation contexts for the first time.

1980: Wrote a report to NSF on using cluster strings (a sequence of annual clusters that had common highly cited papers) to detect emergent areas in science.

1980: The third world science conference was held at ISI organized by Mike Moravsick. Many scholars from around the world attended.

1980: Was invited by Nick Mullins to make presentation at Virginia Tech in Blacksberg, Va. I also attended a meeting in Mexico City (UNAM) with Nick, and other social network practitioners include Linton Freeman who had invented various network metrics. I recall arguing that we should keep the focus on science rather than range across all types of social networks. Mullins always pointed out that we can never know the total social network using the then available survey techniques. The best approximation we had of the total social network of science was the citation index. Nick died of cancer a few years later.

1980: I submitted a proposal to NSF to compile a citation index for physics covering the years 1920-1929 so we could study the emergence of quantum physics in the mid-1920. We put together a journal list and recruited grad students from Univ. of Pennsylvania and Drexel to code the articles. The coding sheets were then keyed at one of ISI's data entry facilities in New Jersey. The index was published in book form and was the object of several analyses and papers. A conference on the Historical Applications of Citation Data was held in the conference room at ISI. I chaired the meeting and presented a paper on how the index was compiled. I reported that the advent of the new quantum theory in the mid-1920 was marked by papers cited more heavily in the year they were published than in any subsequent year, an immediacy that is rarely seen in citation analysis. There were about 30 participants including historians, sociologists and

information scientists. (Paul Forman, John Heilbron, Spencer Weart, Fred Kochen, Ives Gingras, Belver Griffith, Carl Hufbauer, Lewis Pyenson, Howard White., etc.) Kochen became a supporter of our work but unfortunately passed away of a heart attack a few years later.

1981: I was promoted to Director of Corporate Research.

1981: The journal Scientometrics was launched. This journal became the most important publishing outlet for me besides JASIS and the Social Studies of Science.

1981: The first edition of the ISI Atlas of Science was published consisting of specialty maps and mini-reviews. Two more similar volumes followed, and then an attempt was made to turn it into a review journal with Sandy Grimwade as editor. The new owners of ISI eventually killed the product.

1981: Published a paper on the relationship of information science to the social sciences. I presented this work at a small meeting in Washington, DC where I met Karen Spark Jones.

1982: Wrote my only review article on classification schemes for citation, and content analysis of citation contexts, which I called "citation context analysis."

1983: Derek Price died. Last saw him at a conference and we had dinner. He told me about his theory that science was mainly driven by advances in technology in contrast to Kuhn. Later I attended a memorial for Derek held at his home in New Haven, Conn.

1984: Ken Warren of the Rockefeller foundation organized a conference at the New York Public Library on the topic of selectivity in information systems. My paper took the scientific paper through its life stages by analogy to the book by Lewis Thomas called The Lives of the Cell.

1984: Gene Garfield was awarded the first Derek J. de solla Price Medal in a ceremony at ISI led by Mike Moravsick who was a physicist and bibliometrician at the Univ. Oregon. Mike had done studies on 3rd world science and also citation classification. Originally called the Price Memorial Award, it was the brain child of the founder and managing editor of Scientometrics, Tibor Braun. The winner was selected by the editorial board by a voting process. Some years later Mike died suddenly and Gene and I wrote a memorial article for Mike in Scientometrics.

1984: (Nov) Joint 4S/HSS meeting was held in Ghent, Belgium, hometown of George Sarton, the founding father of the history of science. Nick Mullins was 4S president at the time. There were memorable talks by Robert Merton, and Joseph Needham. The local organizer was Mark De Mey who would write book called the Cognitive Paradigm, and reproduce some of our cocitation maps. Mort Malin attended and we met Ton Van Raan and Henk Moed from CWTS Leiden for the first time. My paper was an early version of my specialty narrative paper. I later returned to Univ. Ghent and Brussels for a citation analysis in the humanities meeting arranged by Mark DeMey.

1985: Published a pair of papers with co-workers Ed Greenlee and Ed Sweeney introducing some innovations in our clustering methods, including the use of fractional citation counts,

variable level clustering, and the creation of hierarchies of clusters based on a new method of linking cluster by co-citation. This became our standard methodology for several years. These modifications were aimed at making the clustering system more representative across fields and more comprehensive. Eventually my plans for optimizing clustering thresholds was described in a 2009 paper called "critical thresholds for co-citation clusters" but it was never made operational. It seemed apparent that cluster formation could be modeled as a "critical" phenomenon" as in condensed matter physics.

1985: Wrote a joint paper with Gene on a mapping of science where we overlaid country and disciplinary concentrations. We called this the geography of science. This paper was reprinted in the front matter of the SCI and SSCI for many years.

1985: Took a European trip with Gene visiting Zurich, Lausanne and Paris. Gene was giving lectures in different universities. In Zurich we accidentally met someone on the street who recognized him from his picture in Current Contents and had read all of his editorials. In Paris we bumped into Toni Carbo and David Bearman outside our hotel. This was around the time when Gene's son Alexander was born and there were numerous trans-Atlantic calls.

1985: George Vladutz joined the research dept. around 1985. He was a chemical information scientist who Gene helped get to the U.S. Like all Russians, George was very political, and cooked up plots and schemes targeting ISI management.

1986: I demoed our first computerized science mapping system to the then lieutenant governor of Pennsylvania, William W. Scranton. Gene, Martin Kenney and George Vladutz attended the demo. This was a system for display and searching of cluster maps in color on an IBM-PC.

1987: (Dec.) Received the Derek J. de Solla Price Medal from the journal Scientometrics. Ceremony was held in Amsterdam in connection with the EASST workshop. Presentation was by Loet Leydesdorff followed a talk by Ton Van Raan. The award dinner took place in a canalside restaurant. My co-awardee was the Russian dissident Nalimov who had authored a wellknown book with Mulchenko called Naukometrie in 1969, which translates to scientometrcs. I recall a discussion on whether bibliometrics was becoming a business.

1987: I received the JASIS best paper award at meeting in Boston which was the ASIS 50th anniversary. I met some of the information science pioneers. My paper in JASIS was on what I called the synthesis of specialty narratives which used citation contexts. I considered this to be one of my most important papers but it was rarely cited. It had more to do with what is known today as text summarization.

1987: Visualization using a triangulation method was implemented. This was used to create very fast two dimensional displays as an alternative to MDSCAL. Triangulation only took a limited number of links into consideration. This was later replaced by force placement algorithms.

1988: Was named a AAAS fellow, Section T Information, computing and communication.

1988: Worked on a joint ISI/PRC project aimed at designing a clustering and mapping system for use by the intelligence community. It was taken to the point of a systems design, but the implementation was not funded. The design, however, was useful in later work.

1988: Attended a meeting at the Ciba foundation in London. Had discussions with Tibor Braun about our separate national indicator data sets, both based on SCI data. ISI had decided to enter the indicator business because both NSF and the Hungarian Academy were generating their own indicator series. Later on CWTS under Ton von Raan and OST France under Reme Barre also launched their own versions, with ISI leasing data to them. This became an international comparison game.

1988: Attended a BA meeting in Oxford, England, presenting paper on mapping. I got into a disagreement with Diana Hicks, who had requested and received research front data from me, and without informing me, published a critical article on it.

1988: I attended a symposium on science communication organized by Everett Rogers of the Annenberg School of communications at USC which was sponsored by the EPA. My paper was on a feedback model for citation, which consisted of three stages: reception, translation and transmission. I got to meeting many people including Christine Borgman from UCLA and William Paisley. Christine was working on a book of collected papers titled Scholarly Communication and Bibliometrics which was published in 1990. We spent some time before the conference discussing the book. I had sent her a computer animation of how macro-maps of disciplines changed over a series of years which she showed to her class at UCLA. I had created the animation on my Amiga computer. My paper for her book (co-authored with Ed Greenlee) was on AIDS which was a major medical concern at that time with no cure in sight. The cluster analysis showed that the clinical and viral research had separated into two distinct strings and were not as integrated as they probably should have been.

1989: First issue of the Science Watch newsletter was published in July 1989. Its byline was "tracking trends and performance in basic research". Initially David Pendlebury was editor and later Chris King. David had previously worked in Gene's editorial department and I was fortunate that he joined our research group (he later became known for his Nobel Prize predictions). SW appeared every two months starting in 1990. It featured country and institution rankings, research fronts, hot papers, interviews with highly cited scientists, and analyses of topics such as cold fusion. Later we hired freelance science writers as contributors for specific sections. Eventually the newsletter went on-line.

1990: Gave a presentation at a HSS meeting in New Orleans at session devoted to Paul Wouter's book The Citation Culture which was an excellent history of ISI. However, I objected to his assertion that Garfield had invented the "citation", although he clearly invented the citation index.

1992: My 1973 paper on co-citation had become cited enough to be selected as a Citation Classic by the ISI editorial department. My commentary appeared in Current Contents as "Cogitations on Co-citations".

1992: Thomson Corporation, a Canadian company, acquired ISI from JPT publishing (Ted Cross, Joe Palazzolo and Paul Neuthaler). Ted Cross had gone birding expedition to Siberia with George Vladutz from my department. George passed away from cancer a few years later.

1992: The SCI-Map PC visualization software was implemented by Susan Ramee, a programmer in the research group. Later on a PC package called Sci-viz was also launched to visualize maps of science with additional analytical capabilities. Susan also came up with the concept of The Evaluation Station. The evaluation station idea represented our early thinking on a bibliometric software interface for the full SCI database. Jim Shuster later developed a PC interface which offered many of the same functions for a smaller, predefined dataset.

1993: Attended a meeting in Japan entitled Workshop on Information Resources for Japanese Studies in the U.S. My presentation was on the SSCI and A&HCI coverage of what was considered "Japan studies". I got to meet some of our Japanese colleagues and customers including Masamitsu Negishi and Hitoshi Inoue, an important figure in information science in Japan. The Japanese were very hospitable. I had met Naotake Ito at a previous meeting and he was kind enough to show me around Tokyo. It was clear that Japan was going to be important in the field of scientometrics in coming years.

1993: Up to 1993 I had attended most of the 4S conferences, some of the HSS conferences and many ASIS conferences. A series of conferences had been started by Leo Egghe and Ron Rousseau, from Belgium, which met every other year at various places around the world. I had not attended the first three conferences but did attend the fourth in East Berlin in 1993. Technically this was the 4th International conference on Bibliometrics, Informetrics and Scientometrics, organized by Hildrun Kretschmer. After the conference a number of us met and discussed forming a society. Leo I think wanted the name Informetrics, and representatives from the Hungarian Academy of Sciences under Tibor Braun including Andress Schubert wanted Scientometrics. So it became the International Society for Scientometrics and Informetrics (ISSI). I had expressed my exasperation with 4S as one reason for forming the society. We would continue to meet every other year and the local organizer would be president of the society for the two year period. Kees LePair was an important figure at the meeting. He directed a government science foundation in the Netherlands and agreed to file the necessary articles of incorporation for the society in Holland and open a bank account. Berlin was a good meeting. I recall meeting Don Swanson there for the first time as well as Michel Zitt and Bluna Peritz, although Bluma had visited ISI previously. Garfield agreed to fund a dissertation scholarship award and Tibor agree that the Price Medal would be awarded at the meeting. My paper at the Berlin meeting was on the SCI-Map system. I think everyone was excited about the new society except Hildrun, the organizer of the Berlin conference, who ended up organizing a separate conference that would focus on collaboration studies called Collnet.

1994: Hired Harry Rothman, a business school professor in Bristol, England, as our European sale rep. I participated in another BA meeting there and spent time with Harry. We did a science policy report using the SciMap which was published as a SPSG paper. SPSG, the science policy support group, was run by John Ziman who also attended the meeting but was not fond of bibliometrics. Gene was at the BA meeting. I recall giving an interview for BBC radio perhaps at this BA meeting.

1995: The ASIS meeting was held in Pittsburgh. I met Bob Korfhage who was at the Univ. Pittsburgh. Also around that time met Mark Rorvig (Univ. Texas). Together with Korfhage we attended a SIG-IR meeting in Zurich, Switzerland where there was a special session on visualization systems. There was an informal competition to see whose system could visualize the most documents. Both Mark and Bob passed away a few years later. I enjoyed Mark's company very much and he stimulated interest in visualization techniques. One of his projects was to build a "scientarium" which would show maps of science, just as a planetarium showed the stars.

1995: The first official ISSI meeting was held at Rosary College in Chicago organized by my friend Mike Koenig. I had gotten to know Mike when he worked at ISI and we co-authored a paper on journal clustering using bibliographic coupling. He later took academic positions at Rosary, Columbia, and most recently LIU. He also had worked in drug and other companies. He produced a nice printed proceedings for the Rosary meeting. Belver was having health problems at the conference and one time I had to find a chair he could sit on to make his presentation due to dizzy spells. Ton Van Raan was awarded the Price Medal at this meeting. My paper was on how to use SQL and relational databases to do bibliometric operations and I demoed our bibiometric interface that ran on a PC with a custom data base. At that time I was gearing up our departmental production system on an HP workstation running Oracle which I convinced Bill Schlegel (then CEO) we needed instead of relying on the main frame. We had gotten some flack running our data manipulations on the ISI main frame and the computer dept. would blame us when our jobs interfered with their production runs. The workstation proved not to be such a great production machine but it was a good platform for research and development.

1995: Presented a paper at ASIS that introduced longitudinal coupling, a third neglected form of coupling of papers besides co-citation and bibliographic coupling. I showed how the three forms and direct citation could be combined into a single measure.

1997: The ISSI meeting was held at the Hebrew University in Jerusalem organized by Bluma Peritz. This was an exciting meeting because there were terrorist attacks at that time and security was very tight. I was almost hit by a falling ceiling ornament while sitting in a restaurant with Sylvan Katz. I also witnessed the blowing up of a bag of groceries by the security forces. Bluma had to recruit me at the last minute to make a speech awarding the Price Medal to Ben Martin and John Irvine because Belver was too sick to attend the meeting. It was awarded at the banquet which was held in a beautiful outdoor restaurant. We also got to see the antiquities museum and the old city. My paper was on a generalized framework for creating large scale maps of science, which involved nesting co-citation clusters in a hierarchy that you could zoom into.

1997: Belver Griffith received the Derek J. de Solla Price Medal. I wrote a celebratory article on Belver for the journal Scientometrics.

1998: Gene and I had lunch with E.O. Wilson at the Free Library of Philly prior to a talk he gave at the Library. I had written a paper inspired by Wilson's book Consilience, which derived from the philosophical views of the 19th century English philosopher William Whewell.

1998: Began a collaboration with Sandia National Lab in Albuquerque, New Mexico with Chuck Meyers and others who had developed a visualization system called VxInsight. This was a virtual reality system running on a high end workstation. David Pendlebury and I gave a briefing at Sandia and met the developers. They arranged for us to have a machine for our use at ISI and provided a demo database derived from ISI data. Ultimately, they wanted to sell the system to ISI but this fell through. I got to meet Kevin Boyack who was then working for Sandia as an engineer but was interested in bibliometrics and mapping. Eventually he left Sandia and went to work with Dick Klavans at SciTech Strategies where I now work.

1998: At the ASIS meeting in Pittsburgh I received the ASIS award of merit. It was presented by the president of ASIS Michael Buckland. My talk summed up my research preoccupations up to that point. I convinced Gene to sit on the stage next to me as token of my appreciation. I felt bad that I could not share the limelight with Belver. Howard White commented that I had a lot of good one-liners. I said the reason my wife Lois could not be there was that she was at home counting citations.

At the paper session, I gave a presentation of our PC system called SCI-Viz which could display, navigate and search map of science based on about 20,000 highly cited papers. Maps were represented by linked circles, each circle containing a map at a lower level, i.e., hierarchically organized. Associative trails could be browsed as in manner of the Memex system proposed by Vannevar Bush.

1998: Published a paper that showed how ordination methods combined with clustering methods could be used to map science in a unified framework and visualized in manageable chunks.

1999: At the ASIS meeting in Washington, DC I gave a talk on "Knowledge from citation networks" in a special session organized by Jian Qin. I discussed Don Swanson's "undiscovered public knowledge" idea by looking at the proximity of topics on a map that later joined up and solved a problem. My example was from AIDS research. Gene and I had published a letter to the editor of JASIS on this earlier. Don's work was influential for me because the idea that bibliometrics could assist in making scientific discoveries was a novel idea. In practice, however, I don't think it has had much impact on the practice of science.

1999: Belver Griffith died. I gave talk at his memorial service and published a eulogy entitled "Belver and Henry" describing our close friendship and collaborative relationship. Unfortunately it was haunted by his demons and at some point by my desire to work on my own.

1999: We were funded by Unilever to provide a list of hot research fronts relevant to their general interests and to participate in a workshop on strategic research planning at retreat in rural England. At their company headquarters near Liverpool I gave demoed Sci-Viz, our newly developed PC interface for visualization using a data set on asthma.

1999: Published a paper on science visualization and the Sci-Viz system in JASIS. The term visualization was then supplanting the term mapping.

1990s: ISI got involved with the Connection Machine at MIT, an early parallel processor. We went to company headquarters in Boston. Later NRL bought a machine and tested it. George Vladutz and I were involved in the joint experiment and briefed NRL on how the machine could be used for clustering large data sets that took advantage of parallel processing. Difficulties were encountered, however, due to the lack of an operating system for the machine.

1990: I worked on a contract for OTA on bibliometric measures of basic research. The most notable feature of this project for me was that I had a chance to work with Daryl Chubin, then on the staff of the OTA, who I knew from the old 4S days. Daryl had moved to DC and become a science administrator.

1999: I became interested in creating pathways through science to demonstrate the unity of science and the nature of interdisciplinary links. This was to counter several papers which had been published on how dis-unified science was which were not, however, empirically based. In total I published three papers on this topic: in Library Trends in 1999, in Garfield's festschrift in 2000, and in the book Discourse Synthesis in 2001.

2000: Launched the on-line bibliometric product ESI (Essential Science Indicators), and the associated open websites called In-cites, Special Topics and ScienceWatch. The websites were linked to the subscription product. Spyder Schafer; Jennifer Minick, and Doug Benson worked on websites.

2000: Employees in the research group were David Pendlebury, Elizabeth Aversa (our DC sales rep), Nancy Bayers, George Vladutz, Jim Shuster, and Jessie Stephenson.

2001: Was named Chief Scientist at ISI. Leslie Singer was CEO at the time.

2001: Was named an Honorary Fellow of the National Federation of Abstracting and Information Services (NFAIS).

2001: Attended the ISSI meeting in Australia (the 8th) located near Sydney at the Univ. of New South Wales. Mari Davis was president of the society and Connie Wilson was the local organizer. I gave the keynote address at the conference, and also had to give an impromptu explanation of the ISI impact factor for journals, which was controversial at the time.

2002: An attempt was made to revive the idea of the Atlas of Science product. The product proposal was ultimately rejected by Mike Tansey, then CEO.

2003: Robert Merton died. I later wrote an article in tribute to him titled "On the shoulders of Robert Merton". He had been very supportive of my work. In this article I tried to work out a scheme whereby constructivist sociology could be brought into the same framework as Mertonian normative sociology, but norms were inconsistent with constructivism.

2003: Thomson buys out JPT (then later buys Reuters).

2003: A news item was published in Science on our ESI special topics which featured interviews with scientists. The ESI-related web sites were a source of great satisfaction because they created much good will toward us in the scientific community. They were inspired by Gene Garfield's Citation Classic Commentaries that had appeared in Current Contents for many years, and gave authors of highly cited papers a chance to discuss the background and implications of their work. The ESI web sites were designed to continue this tradition.

2003: I became the first elected president of ISSI. The first meeting after my presidency was in Beijing, China in 2003 (August) during the SARS scare. The ISSI meeting in Beijing was held at the Chinese Academy of Science. I met Xiaolin Zhang, executive director of the National Library of the Chinese Academy of Science. The meeting featured an opening lecture by a Chinese communist official in Chinese. I gave paper on emerging specialties, and other talks at organizations around Beijing organized by our sales office in Beijing.

2003: Participated in a meeting in Belgium (Ghent and Brussels) organized by Marc De Mey (author of the Cognitive Paradigm) on evaluation in the arts and humanities. My paper included a map of musical compositions of Igor Stravinsky and also co-citation maps of Greek philosophers. Howard White had provided me with some author co-citation maps based on artist's names, derived from A&HCI data. The meeting was held at the Belgium Academy of Science. I also gave a presentation to the university administration on citation evaluation.

2003: Introduced maps of science into our open ESI websites. The maps were of emerging research fronts or other selected areas, and also maps at higher levels showing specialties or discipline views. Maps were coded in HTML with limited hypertext interactivity and links to interviews.

2003: Wrote a retrospective on my career at the request of Chaomei Chen for a special issue of JASIST.

2004: The advent of Scopus from Elsevier to compete with the TR Web of Science.

2004: Organized the first joint ISSI/ASIST session. The topic was collaboration and coauthorship in science.

2004: (Nov) I made a second trip to China, and gave presentations to various groups in Beijing, including one at the Library of the Chinese Academy of Science attended by many students. I was closely questioned on why I used co-citation instead of co-word data, which was apparently preferred in China. I was traveling with Keith MacGregor then CEO. We also visited Japan where I got very sick, and Keith had to give my talk at an awards ceremony for scientists.

2005: Attended the ISSI meeting in Stockholm, Sweden, where I was president. My paper was on tracking growth areas in science and I used ESI research front data for the first time. We built cluster strings by comparing successive bimonthly periods and used the new strings for a specific time period as an indicator of emergence. The highlight of the conference was the combination banquet and night-time boat ride on the water-ways around Stockholm. This was one of the few meetings attended by Tibor Braun and his wife. I attempted to talk Tibor into making

Scientometrics the official journal of ISSI but was unsuccessful. I still think this is one of the weaknesses of the society.

2005: I organized the first 4S/ISSI joint session at a 4S meeting in Pasadena, Ca. on the topic of science mapping. Katy Borner, Kevin Boyack, Chaomei Chen and I presented.

2006: Attended an STI conference in Leuven, Belgium hosted by Koen Debackerer and Wolfgang Glanzel from the Catholic University of Leuven, and organized by the Leiden's CWTS group. My paper was based on a content analysis of responses by highly cited scientists to a survey question for ESI interviews on the social and political implications research. This was the first time I had written about how science may affect social and political issues. It was the second in a series of papers based on results of this web survey used for the ESI interviews, the first being on why authors think their papers are highly cited. In that paper I looked at the various reasons authors gave for their high citation rates. I realized later that I should have cited a similar study published earlier that used the Classic Commentaries as a data source.

2007: At the ISSI meeting in Madrid, Spain I was outgoing president, and the incoming president was Ron Rousseau. My paper was on the emerging research area of thin film organic transistors. My co-author was a Wharton School PhD student Phin Upham. Informal discussions with Michel Zitt of OST France and INRA led to our paper on a so-called citing side approach to citation count normalization (as opposed to the cited side approach based on journal categories). This new metric was called the Audience Factor, and was based in part on the idea of fractional citation counting which I had introduced in the 1980s.

2008: An ESI based map of physics was published in the journal Physics World.

2010: I published a paper showing how links between disciplines in science can be based on analogies between different subject matters.

2010: My paper published in the festschrift for Ton von Raan was on the origins of referencing in scientific writing going back to the writings of Aristotle and up through the 20th century. I was able to draw on my knowledge of the history of science to give some interesting examples of referencing and non-referencing practice.

2010: Joined staff of SciTech Strategies to work with Dick Klavans and Kevin Boyack. This was only the third employer in my career. I was interested in the opportunity to work with the combination of Elsevier full text and Scopus citation data. I wanted to see if citation contexts could enhance citation analysis, and how citation contexts could be used for knowledge representation.

2011: Published a paper on how the technique of sentiment analysis can be applied to citation context data and used to enhance the understanding of maps of science.

2011: I made a presentation at a meeting in Santa Fe for Elsevier. My talk was on citation context analysis using Elsevier full text.

2011: Made a trip to Israel with Gali Halevi for an Elsevier conference. We were hosted by Judit Bar-Ilan and visited Bluma Peritz in Jerusalem.

2012: Attended the ISSI meeting in Vienna. Presented a paper co-authored with Kevin Boyack and Dick Klavans on a method for identifying emerging topics in science using newly derived clusters based on direct citations (rather than co-citation). The method had been invented by researchers at CWTS. The paper was expanded and published in Research Policy in 2014.

2014: In a paper for the Blaise Cronin festschrift I discuss how evolutionary theories on altruism can be applied to understanding citation practice.

Future: I hope to complete work on a case study of citation contexts for a single co-citation cluster.