



Deeds and Dreams of Eugene Garfield

I S T V A N H A R G I T T A I

Dr. Eugene Garfield (b. 1925) is President and Editor-in-Chief of *The Scientist*, a biweekly professional newspaper, which he founded, and Chairman Emeritus of the Institute for Scientific Information (ISI), Philadelphia. He is probably best known for Current Contents (CC) and Science Citation Index (SCI). Dr. Garfield instituted an information revolution in scientific research. He received a B.S. in chemistry in 1949 and an M.S. in library science in 1954, both from Columbia University, and a Ph.D. in structural linguistics from the University of Pennsylvania in 1961. He was President of Eugene Garfield Associates from 1954 to 1960 and President and CEO of the Institute for Scientific Information from 1960 to 1992. He has published numerous books and articles on scientific information retrieval and related topics. We recorded our conversation in Dr. Garfield's home in Bryn Mawr, Pennsylvania, on March 7, 1999.

ISTVAN HARGITTAI (IH): You introduced *Science Citation Index* and changed the way scientists are employed, professors at universities are given tenure, and research journals are judged for their quality. This is heavy stuff. The Sputnik in 1957 had a tremendous impact on American science. Is there anything comparable to your impact worldwide?

EUGENE GARFIELD (EG): Thanks for the pleasant hyperbole but if there is any truth to the statement, I'm not acutely conscious of it. Of course, CC and SCI are widely used, but I don't hear people say much about it. If you use SCI especially for evaluative purposes, you don't advertise it. If the SCI is used in tenure evaluations, hopefully it is done intelligently. I described this in an essay on faculty evaluation [1], one of my most popular. Undoubtedly, this use of citation analysis is due to the paucity of objective data for such evaluations. I

can't imagine how you would evaluate the impact of my work. How would you measure it? The Internet is having an impact but how would you measure it? When we talk about intellectual impact, it is very subjective-economic impact is another thing.

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

the most respected journals do not hesitate to use impact factors in their advertising.

IH: In *The Chemical Intelligencer*, there were a couple of papers comparing the impact factors of the *Journal of the American Chemical Society* and *Angewandte Chemie* [2,3]. It was alleged that the impact factor of *Angewandte Chemie* was over-inflated because it is published in the original German and in an English translation edition. The people at *Angewandte Chemie* were rather unhappy about these papers because they thought that *Angewandte Chemie* should have a higher impact factor for the very reason that it carries reviews in addition to research papers.

EG: I think these allegations are overstated. The analysis is not as simple as it is made to seem. There is some inflation in the impact factor due to dual citation of both editions. But the journal's self-citation only represents about 10–15% of the citations that it receives. Undoubtedly, these disputes indicate that there is more citation consciousness among editors and publishers today.

In the studies that I did in the past, citation analysis "exposed" the political nature of East European science academies-many academicians were administrators, not world-class scientists. That was true also in some other European countries. In Italy, the SCI was like salvation to some scientists even though it did not immediately correct the unfair allocation of credit and resources. It called attention to the disparities in funding and publication. There are still many politically based science decisions-who gets tenure, who gets research funding, money, and so forth. The Italians started using the SCI data over 20 years ago, not only to measure citation impact but

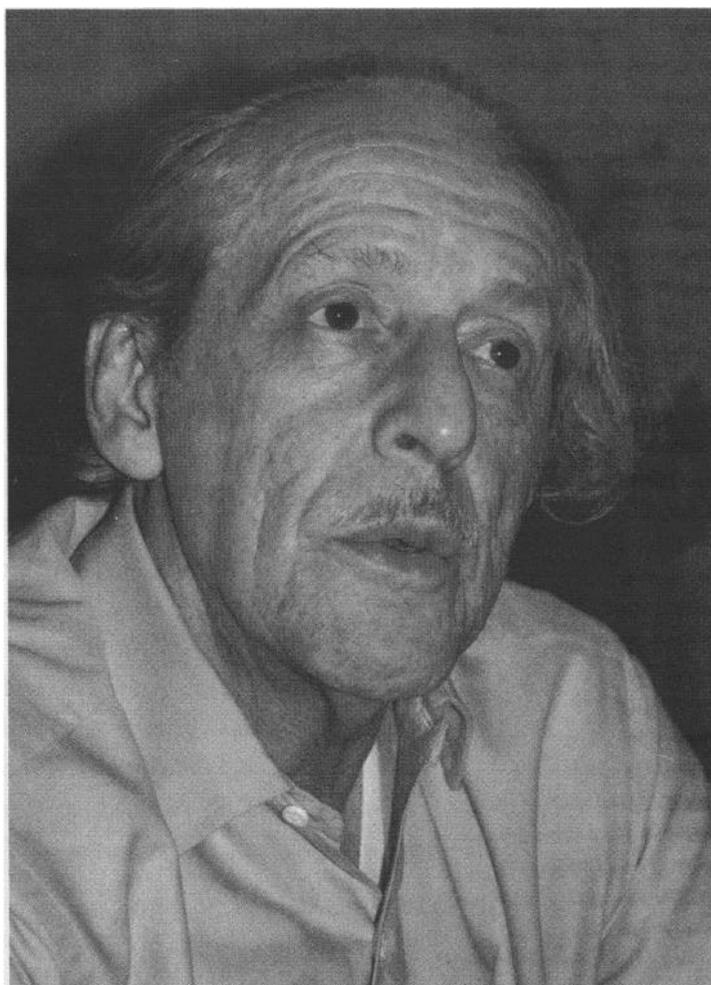
Eugene Garfield during
the conversation.
(Photo by I. Hargittai.)

simply to determine if particular grantees had published any papers in peer-reviewed journals. The younger scientists resent the power of the old guard, who continue to get the money. The younger ones publish in reputable journals and do significant research. So there is no doubt that SCI had some effect, in particular in Europe. In those days, I don't think we had that much of a problem in the United States. There may not have been enough money available, but, in general, our peer-review system is not nearly as politicized. The use of citation data in Italy led to the publication of an interesting monograph [4].

I'm curious as to of what effect *SCI* had in Israel. Gideon Czapski, a Professor of Chemistry at Hebrew University, has made an extensive citation analysis of Israeli science, especially in chemistry [5]. Nevertheless, he likes to point out that one of his papers [6] is rarely cited because it disproved a theory that was investigated heavily. There is no need to continue citing the proof that a theory is wrong. Falsification in science is also important. However, I don't think he disagrees with the idea that citation frequency is associated with creativity, but it is always important to note that there are exceptions. Some important discoveries are not matched with high citation. And false ones, like cold fusion, may be cited heavily but they are the exception. In general, Nobel-class work is accompanied by significantly higher citation, as we demonstrated over 30 years ago. And every Nobelist has published one or more *Citation Classics*.

EH: You have brought many of your ideas to fruition. Have there been any that did not happen?

EG: Sure. When I sold ISI, new management almost immedi-

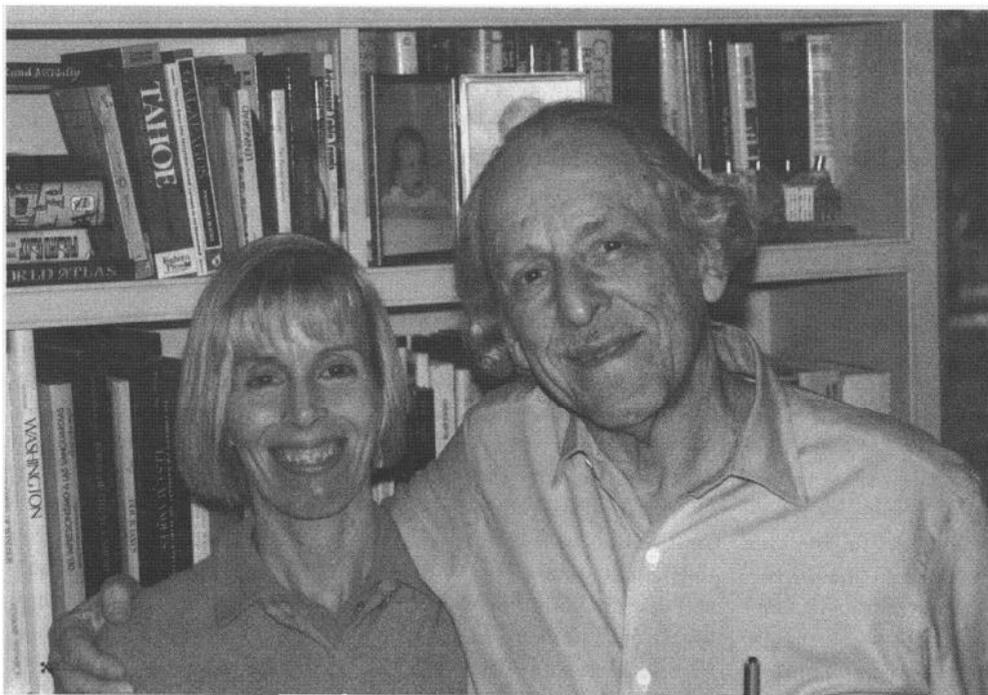


ately emasculated certain projects. We had started *The Atlas of Science* and later changed its name to *Research Reviews*. We used the results of our global co-citation analyses to identify the newly emerging research fronts that needed to be reviewed. We published several volumes. JPT, the new partners, killed it because it still was not profitable, and it might have taken five years for it to break even. It would have been an encyclopedic treatment of current science. There are, of course, plenty of review articles published. The *Current Opinion* series, published by Vitek Tracz, came out later. He is a brilliant Polish-Russian-Israeli scientist who now lives in London. His company, Current Science, also is located in Philadelphia. He understood the mapping concept very well but, to my knowledge, neither

he nor anyone else has used co-citation mapping to produce an international encyclopedia of science. Systematic examination of the literature is necessary to identify what is *not* being reviewed.

I'm on the Board of Directors of the nonprofit *Annual Reviews*, which produces about 30 annual review volumes in print and online each year. They have not used citation data as yet. Their methods for choosing topics are purely subjective—not that that is bad, just different. Their editorial boards are top-notch. Derek Price used to say that for every 50 new papers in each field, you need a review, which then becomes the paper that people cite as a surrogate for those references. One could do an interesting historical mapping based on the network of review papers.

So, returning to your original



Catheryn and Eugene Garfield
in their home.

(Photo by I.Hargittai.)

question, there are a lot of things I wanted to do that have not happened. I wanted to publish a constantly current dictionary of science. What could be a better source of new terms than the ISI database? There is constant input of new terminology. The nomenclature from indexing services is not systematically being exploited to compile dictionaries. Libraries spend a lot of energy compiling thesauri. Most of those terms eventually do get into dictionaries, but it could take many years.

I would also like to see the algorithm finished for creating historiographs. The *Citation Index* is a gold mine for the history of science. Mapping all the key references for a given topic, you should be able to graphically portray the development of a field. My brother, Ralph Garner, wrote a graph theoretical description of such networks [7]. And there has been some recent work done on visualizing citation networks [8].

I find it very frustrating that so many scientists are ignorant of what they could do with information retrieval systems. I think it is important not only to be literature-minded, but to de-

velop citation consciousness. I'm not sure how you teach this. It requires indoctrination by informed mentors.

I also wanted to use ISI Press to launch a systematic series of scientific biographies. It would have been an extension of our *Citation Classics* series. We published 4000 of those in *Current Contents*, and 2000 were reprinted in a series of books called *Contemporary Classics in Science* [ISI Press: Philadelphia, 1986].

We could have easily published many more thousands of *Citation Classics*. And there are always more recent ones to be covered. A systematic series of biographies could include not only most-cited authors and members of the academies. Josh Lederberg was a strong supporter of this idea. The National Academy of Sciences publishes their *Memoirs*, but they appear only after members die. In addition to monographic autobiographies and biographies, I thought a journal of scientific biography would also be an interesting project.

EH: Early on, you had a meeting with J. D. Bernal. He was very much concerned with the ways of science publishing. He con-

sidered the unit of scientific publication the article, not the journal. How much impact did Bernal have on you?

EG: On the Internet we now have a preprint depository in physics and other topics. That was essentially what Bernal had in mind. He gave a paper in 1958 at the Information Conference on Scientific Information in Washington. That's where I met him for the first time.

My uncles were Marxists. One of them gave me Bernal's book *The Social Function of Science* in 1939 when I was 14 years old. It may have had some influence on me. But it was not until 1951 that I realized that he was involved in the "science of science" movement, the predecessor of scientometrics and science policy studies. He was involved in the 1947 Royal Society Scientific Information Conference. The *Proceedings* volume was my bible when I worked at Johns Hopkins from 1951 to 1953. My interest, however, was in information retrieval, not in research evaluation. Bernal was a Nobel-class scientist who might have received more recognition for his science if he had not been so openly leftist. His politics undoubtedly affected his influence. In 1962, when we had the first experimental printouts of SCI from the *Genetics Citation Index*, I sent samples to him, Robert K. Merton, and Derek deSolla Price. He responded very positively as did Bob and Derek.

Looking back, was there anything in your family background that steered you in the direction of your future career?

EG: There were political discussions with my uncles but not much science. Only one of my uncles finished college. At first, I attended a science high school, Stuyvesant, but I left for a variety of reasons. I had no real mentors there and

throughout high school. We lived in the Bronx, and Stuyvesant High School was a long subway ride to lower downtown Manhattan. And I wanted to study more foreign languages. So I transferred to DeWitt Clinton High School in the Bronx. Except for math, I was not a good student in high school. My grades were not exceptional. I still was interested in science and I wonder what might have happened if I had stayed at Stuyvesant. My regret is that I didn't encounter a scientist or teacher there who could have steered me in the right direction. And Stuyvesant was very competitive.

In my last year at Clinton, I met an English teacher and former journalist, Wilmer T. Stone, who gave me some direction. Almost 10 years after I graduated, I visited him in Maryland, where he had retired, but he really didn't want to be bothered. Of course, I was not his child, just one of many students to whom he had described his experiences as a free-lance journalist interviewing Jack London, among others. He taught in high school because of the depression.

When I was an undergraduate in college, none of my professors had a significant impact. At 17, I started out in chemical engineering at the University of Colorado, but it was wartime, so I left soon for San Francisco, worked in a shipyard, and eventually was drafted even though I had been accepted for the merchant marine. After the war, I returned to Berkeley. Classes were huge but I did encounter famous chemists like Joel Hildebrand and Melvin Calvin. But I was a premed student at that time and switched to chemistry later.

IH: What did your parents do?

EC: My mother was a housewife. My father became a successful newspaper-magazine distributor, but I never lived with my father. My parents separated before I was born and shortly afterward were divorced, when my sister Sylvia was 2 years old. I was 5 years old when I saw my father for the first time. And then, again, four or five years later. Our relationship is a long and sad story. My mother's oldest brother became my surrogate but absentee father. My uncle helped support us, but he never was there in person. The

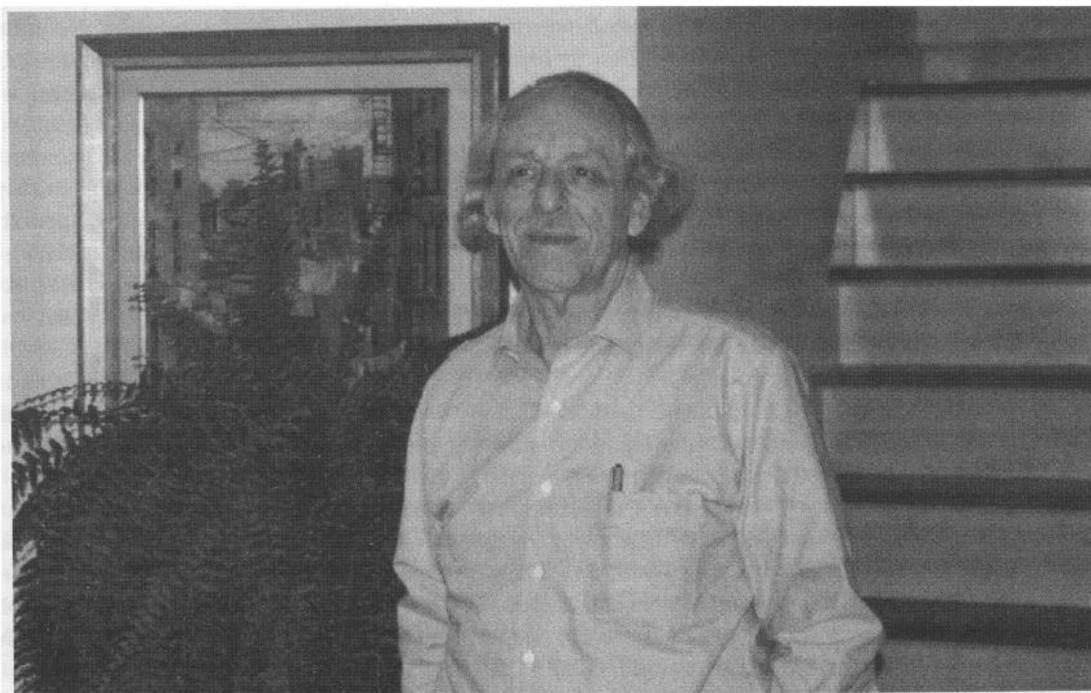
only time we would see him was at my grandmother's house on Friday night. He was a successful ladies' coat and suit manufacturer.

On my mother's side, they were Lithuanian Jews. I'm not sure about my father's parents. I once heard that they came from Galicia. Garfield is not my original name. It was the name of my father's firm, the Garfield News Company. My father opposed my changing my name but my uncle forced the issue since they had had a long, bitter rivalry.

My stepfather was a butcher and later drove his own taxicab. He was an Italian immigrant, so we were a nondenominational family. I was never bar mitzvahed. My half-brother Ralph was born when I was 12. IH: There is a lot of change going on in journal publishing: the American Chemical Society is bringing out new journals and the European chemical societies are consolidating their national journals.

EC: Science is still growing so there's more capacity for journal growth. Inevitably, there is twiggling of journals to accommodate new fields.

IH: You have written about the



*Eugene Garfield relaxing after the interview.
(Photo by M. Hargittai.)*

connection between publishing, impact, and the Nobel Prize.

EG: It became an interesting game. But I never tried to predict who would win the prize. It was more relevant to suggest the fields that might be recognized. We might have predicted a prize for nitric oxide. Certainly, among those names would be Salvador Moncada. Moncada was certainly among the most-cited authors. Nobel prizewinners are almost invariably well cited. The Nobel Committee didn't include him, and it has created a lot of controversy. I'm not suggesting that the committees should select on the basis of citation analysis, but they should be aware of the most cited scientists for each field considered. The same thing happened to Moncada for the Lasker Award. Something odd is going on there. I find it very strange that members of many lesser prize committees prefer to choose Nobel laureates. Why not pick someone who hasn't been so visibly recognized? I have fought this battle many times. Most award committees like to play it safe. I think awards should go to people for whom the award would make a significant career difference. Why give a lesser prize to Nobel laureates? They've already had the highest recognition. But there will always be exceptions even to that generalization.

IH: Speaking about publishing, sometimes people complain that they cannot get their message through.

EG: Hans Selye said that to get his general adaptation syndrome accepted, he published everywhere and over and over. He didn't care if he repeated his message. But take an opposite example, Eiji Osawa in Japan, who had the basic idea of what later became known as buckminsterfullerene. Did he do what he should've done to get

across his message? The question is, to what extent does a scientist sell his ideas? The word "sell" is not usually used, but that's part of it. Scientists all have to get their ideas across to fellow scientists. Consider the *SCI*, for instance. It didn't happen just by itself. Long after I published my 1955 paper in *Science*, I had to publish dozens of articles and give hundreds of talks. I became a propaganda machine. Merton described this very well in his Foreword to my book *Citation Indexing* [9]. It is the same with scientific ideas. I'm awfully curious to know what was missing in the Moncada affair.

In the case of the Japanese, their problem often is that they don't learn to speak English well. So they are at a disadvantage at conferences. At least in the past, the Japanese authorities didn't insist that scientists learn to speak and write English, from an early age. If Japan wants to have its fair share of recognized scientists, they have to emphasize good linguistic skills. A lot of good work in Japan is probably underappreciated because they are so timid about promoting their ideas, especially to authoritative figures.

IH: Would you care to single out what you consider to be the most important thing you have done?

EG: To many people, *Current Contents* had the most pervasive influence. *Current Contents* is a ridiculously simple idea. Curiously, there has never been a scholarly article written about *Current Contents*. But it is still the model that has been adopted and copied. Its simplicity is what made it so successful. You say that I have a strong influence on science. Well, for a 25-year period I had a captive audience worldwide. The readership was larger than that of *Science* or *Nature*. The number of printed copies was as high as 40,000, but the aver-

age readership was tenfold that number. Some copies were read by hundreds of people in Eastern Europe and China, where they also copied it. When I went to Eastern Europe and elsewhere, people respected me because I did not attempt to criticize their political systems. I used citation data to demonstrate the relative strengths of their science. I didn't have to tell them what they knew. Rather, I provided them a window on the rest of the world. Since *Current Contents* was just a bibliographical tool, the Russian censors did not touch my essays. They allowed my essays to go through. Of course, I was proselytizing about citation indexing and not capitalism. Many people still think that I'm writing those weekly essays. Recently, I met a senior scientist who said that he loved my essays and read them every week! I wrote the last one six years ago. Maybe he's thinking of my occasional editorials in *The Scientist*. In fact, many people don't know that *The Scientist* has not been an ISI publication for 10 years.

IH: Do you have any children?

EG: I have a 52-year-old son, Stefan, who is a crane operator. My second son, Joshua, 40 years old, graduated in marine biology but is now a computer scientist. Both live in Florida. I had two daughters, Laura and Thea. Laura is 41, but I don't hear from her. My younger daughter committed suicide 20 years ago. I have a stepdaughter, Cornelia, who lives in Philadelphia and we visit regularly. From my third marriage, I have a 14-year-old son, Alexander Merton, who is a violinist and a good student in math and science. My wife, Catheryn, originally taught biology. Then she got an information science degree and worked at ISI as a lecturer. Eventually, she became Vice President but left ISI after

we sold the company to Thomson. We have been married for 16 years.

IH: How would you formulate the lessons to be learned from your career?

EG: Too often, people are afraid of failure. They worry that they cannot manage financially. Money never drove me; it came to me. Nevertheless, if I had worried about money, I might never have achieved financial success. I don't know what accounts for this quality of persistence. My mother never stopped until she finished the task at hand. You learn a certain doggedness. I grew up working. When I was 9, I was delivering orders in a grocery store and worked in a laundry for hours just to earn a quarter. Later, I went to work for my uncles. I delivered orders in my Uncle Lou's liquor store. Then I worked in the garment district after school and summers. Maybe that was another reason that I didn't do that well in high school. I certainly enjoyed the work but realized I didn't want to remain in the garment business, much as my uncle Sam would have liked me to.

IH: How did your Ph.D. happen to be in structural linguistics?

EG: I got my B.S. in chemistry from Columbia. I had a good friend who was working on mechanical translation of Russian at Georgetown University. I was supposed to join him there. However, I was broke and had to support my son. I got sidetracked by some people from Smith Kline and French. I met them when I was at Johns Hopkins. They offered me a consulting job to set up a punch card system on thorazine. That's why I came to Philadelphia in 1954. My friend Casimir Borkowski later came to Philadelphia to get his Ph.D. under Zelig Harris, the chairman of structural linguistics at Penn. Noam Chomsky was

graduating that year. I introduced Zelig Harris to the field of information retrieval. Within a few months, he had a half a million dollar grant from the National Science Foundation.

I had started my doctoral program at Columbia but couldn't get my interdisciplinary committee to meet. So I left and subsequently made a deal with Professor Harris. He agreed that I could transfer my credits and do my dissertation in chemical linguistics. My task was to create an algorithm for translating chemical nomenclature into molecular formulas using a computer. Today it seems ridiculously simple but in those days it seemed impossible. That's how I got my degree in the linguistics department. It was as much a chemistry topic as structural linguistics. Allan Day, Chair of Chemistry, was a great help to me.

Later on, I taught at Penn in the electrical engineering school. I gave a course in information retrieval for computer and information science graduate students. Many of them worked on Department of Defense contracts involving information retrieval.

IH: Do you keep track of citations of your own work?

EG: I have received a weekly printout for over 30 years, which lists every paper that cites my work. Because of my essays in *Current Contents*, I am probably the most self-cited person in the world. There are still quite a few papers published that cite my papers and books, but lately impact factors are very popular. I've posted almost all my work full-text on my web site [http://165.123.33.33/eugene_garfield] and that's a good place to end.

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