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The Sociology of Science and the Garfield Effect: Happy Accidents, Unanticipated Developments and Unexploited Potentials

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The sociology of science and the garfield effect: happy accidents, unpredictable developments and unexploited potentials.

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This paper is a stock-taking and an homage. The stock-taking was prompted by an invitation to assess Eugene Garfield's influence on the sociology of science at a conference celebrating his life[1]. It led me to reflect on Garfield's contributions as a whole, rather than piecemeal, and this, in turn, led to the homage. The latter was not my intention at the outset.

Gene Garfield was a friend of long standing-a half century in fact[2]. We worked together, we corresponded and, as the record will show, we occasionally disagreed (Wouters, 1999). Through it all, we shared life's ups and downs without interruption. Thus, it seems appropriate to treat this occasion as an analysis-cum-memoir. I will abandon formalities and call Gene Garfield, Gene. I will also call Robert Merton, Bob. He was a key actor in this story and, after all, he was my partner and husband. Similarly, I will abandon formalities when referring to friends and colleagues with whom I've also been on a first-name basis for years.

As it happens, no fewer than three other accounts of my assigned subject are already in print. The first, by Bob Merton, appeared in 1979 in his book, *The Sociology of Science: An Episodic Memoir,* under the typically Mertonian title, "The Garfield Input" (Merton, 1979). The second, a lengthier version, by Bob, was published in *The Web of Knowledge*, the *Festschrift*, presented to Gene in 2000 (Merton, 2000), and the third, also in that *Festschrift*, is by Jonathan R. Cole, "A Short History of the Use of Citations as a Measure of the Impact of Scientific and Scholarly Work" (Cole, 2000). Since the most recent of those accounts is now 17 years old and since much has happened in and to the sociology of science, a new stock-taking, may be in order.

[1]"Commemoration and Celebration of the Life of Eugene Garfield, 1925–2017," Philadelphia: September 15–16, 2017, sponsored by Clarivate Analytics.

[2]I joined Gene Garfield on the Board of Annual Reviews Inc., the scientific publisher, in 1974. We saw each other at meetings every year for 43 years, give or take. Quite separately, Gene and my husband, Bob Merton, were also friends; they not only liked each other and worked together, but they also admired one another. This made for a long term and tight three-way bond.

My comments divide into five parts.

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MEETING GENE GARFIELD

I believe I met Gene in the winter of 1964 or possibly 1965 – I was close to finishing my dissertation at the time. It began as an evaluation of the then popular (but now antiquated) claim that major discoveries were the work of "great men of science[3]" who worked alone, and the complementary claim that collaborative research (pejoratively labeled "team research") was inevitably routine and of little importance. I thought otherwise. My skepticism was based on impressions I'd gleaned from research I had done, as Bob's assistant, on authorship of papers in scientific journals. It seemed numerous important papers were the product of collaborations and thus, that a systematic study was in order of how major discoveries and those of lesser significance had been made. Since I had no means of identifying major discoveries, I decided to focus on those which had been honored by Nobel prizes and on the work practices of Nobel prizewinners, and to compare them with those of a sample of scientists of varying degrees of accomplishment. As it turned out, I collected a lot of data on trends in multiple authorship in the sciences, social sciences and the humanities, a lot of data on life course patterns of collaboration of individual scientists and I interviewed four-fifths of the then living Nobel laureates in science living in the United States[4]. I did not count citations. No database of citations existed at the time, as far as I knew. Indeed, I didn't have any idea of what a *Science Citation Index* might be.

Quite early Gene believed that citation indexes would be invaluable to scientists in identifying the antecedents of their work. But he also thought they might prove useful to historians and sociologists of science.

As Gene reconstructed many years later how he came to think that citation indexes might have uses well beyond those in the sciences, he wrote,

> "How did it all begin? As I scanned the *New Scientist* for 2 November 1961, I came across an article on the 'Role of Genius in Scientific Advance' (Merton, 1961a). I remember being particularly struck by the fact that the author was a professor at Columbia University from which I had received both my BSc and MS degrees (Garfield, 2004)."

Gene immediately saw that Citation Indexing might be relevant to the evidence the *New Scientist* paper presented, namely that independent duplicative discoveries (those Bob called multiple independent discoveries) were exceedingly frequent in science and that great scientists were themselves involved in a number of such multiple discoveries. As a consequence, many of the advances made by great scientists would likely have been made by others or the reverse, one is that many scientists were unnecessary since the work they did would be done in any event by great scientists[5]. At the same time, duplication in science is not without positive outcomes since it assures that the work involved will in fact be completed and, in the process, be confirmed. Gene believed quite the opposite. "A stated goal of the Science Citation Index (SCI) was to prevent or identify unwitting duplication of scientific discoveries (Garfield, 1955b)."

Academic networks turn out, of course, to be exceedingly effective in transferring custom-tailored information. Gene may have had ties to Columbia, once having been a student there, but far more important in this case, he knew the physicist and then provost of the University, Polykarp Kusch, who served at the time as an advisor at Gene's Institute for Scientific Information (hereafter ISI). Gene must have discussed the *New Scientist* paper with Kusch since he credits Kusch with introducing him to sociological research on science and to having suggested that he write to Bob Merton[6].

Gene did so in 1962, asking in a letter whether a Science Citation Index might be useful in sociology. Some weeks later, Bob responded that Gene's letter and the materials that came

[3]This terminology was widely used. There were of course few women of science at the time. Indeed, the standard directory of American scientists titled, "American Men of Science," did, in fact, include a few women of science. Few found this problematic.

[4]The study involved analysis of the extent of collaborative behavior of scientists over the course of their careers, the authorship of papers in their bibliographies, tracking the kinds of research they pursued and how it squared with comparable data on the careers of other scientists. Almost from the beginning, I did interviews with scientists in order to try out questions I thought needed to be asked, for example on how collaborations began and ended, how ideas developed in social interaction, how authorship was determined, the character of collaborations between status equals and between apprentice-scientists and their "masters," and when and how conflicts over priority and recognition emerged. *Nobel Laureates: Sociological Studies of Scientific Collaboration*. Unpublished Ph.D. Dissertation. New York, NY: Columbia University, 1965.

[5]Rather than claiming that great scientists to be "superfluous," Merton held that they could be thought of substitutes for the numerous other scientists with whom they shared multiple discoveries. He went on to examine instances of those who had participated in numerous "multiples" and proposed a not altogether serious measure of the number of researchers for whom they were "stand-ins," those he called "men of many multiples" (Merton, 1961a,b). This was the paper Gene Garfield referred to and which is cited earlier.

[6]It was never clear whether Gene, when he turned to Bob Merton for advice, knew of Bob's standing in sociology, indeed in the social sciences, at the time or only knew, via Polykarp Kusch, that Bob had had a long-term interest in the history and sociology of science.

along with it, made it clear to him that such an index would be "a rich source for the sociologist" and that he (Bob) needed precisely the kind of material Gene's citation index could provide for the work he was doing on multiple independent discoveries. Gene could not have known that Bob already had observed that citations were far more than bibliographic indicators. He saw them as playing a major role in science, serving he believed to be both as incentives to scientists to do the hard work scientific research requires and as rewards for it[7].

THE EVOLVING SOCIOLOGY OF SCIENCE: MY PUTATIVE ASSIGNMENT

A Trio of Happy Accidents

One can never know of course whether what appear to be accidental convergences in the thinking of three quite different scholars are all that accidental. In this case, I think the fact that Gene Garfield, Bob Merton and Derek Price addressed related but not overlapping problems in the history and sociology of science was mostly accidental. I am hard put to identify the evidence for their being affected at the same time by some intellectual force or "spirit of the age" leading them to take up the similar problems. Nor were their later research agendas significantly affected by what the others were doing although they did keep in touch. The relevant biographical details suggest that Bob and Derek independently reestablished their earlier interests in science and its workings at more or less the same time[8]. That Gene Garfield's innovative thinking about the need for a citation index in science (not a subject index) began in the early to mid-1950s[9] also seems quite unrelated to the Derek Price's and Bob Merton's thinking at the time. Price had published his distinctive papers on "Quantitative Measures of the Development of Science" and "The Exponential Curve of Science[10]" a year before "Priorities in Scientific Discovery" was published in 1957[11]. But the two had very different perspectives on what needed studying and how those studies should be done. And at this time, we know that Gene had not yet encountered Merton's work. And while Gene knew about Derek Price's work quite early and resonated to it, the directions of Gene's thinking were not explicitly influenced by Derek's agenda. I do not remember Bob commenting on when he first encountered Derek Price and his writings, and Bob's work on priorities contain no Pricean stamp. It does seem likely that he'd heard about Derek by the time he published *Science Since Babylon* in 1961 and may well have read it. Bob surely had read *Little Science Big Science* either before or shortly after it was published[12]. He was not only an avid reader but was well-tuned into the small network of scholars doing historical and sociological work on science.

The contributions these three made drew attention to a set of related questions each approached from markedly different perspectives. Those perspectives remained different in substance and style over the years, but the three came to know one another, to like one another, and were attuned to what the others were thinking about and doing. Each of them was an intellectual cosmopolitan. Each attracted different audiences to the study of how science worked. Each was an original. Their work was complementary. There are no indications I know of that they viewed one another as competitors. As a trio, they were highly influential on then prevailing thinking about the nature of the scientific enterprise[13].

The Sociology of Science: How It Was Then

Fast forward to 1965. Gene arranged for a presentation about citation indexing to be given in New York and invited Bob to attend. Bob had other pressing things that needed doing at the time and asked me to turn up and report in on what I'd learned. Downtown I went and was introduced to the wonders of citation indexing. I think I was taken by the possibilities of citation analysis but I surely had no clear understanding of its potential uses in the sociology of science. I returned to Columbia and reported on what I'd heard to Bob and to Jonathan and

[7]And Gene was unlikely to have known known that Bob was an assiduous "footnoter" himself and that his notes were influential in and of themselves. There are numerous instances that other authors have used Bob's footnotes in writing their own since they contain (or do not contain) certain telling details. For example, in those rare cases when Bob's references were incomplete, the references of those who followed his lead were also incomplete. Not situations of plagiarism, these are instead indicators of the extent to which others relied on the precision of Mertonian scholarship and, perhaps, also of their thinking it unnecessary to track down for themselves just what the original sources said.

[8]Bob Merton once argued that science would remain an area of conspicuous neglect in sociology until science itself came to be defined as a social problem. "Foreword," *Science and the Social Order*, New York, NY: Collier Books, 1952, 1–20 see especially 18–19. But while this prediction proved more or less correct, it is a stretch to ascribe the three happy accidents to science seeming to be particularly problematic at the time.

[9]See for example, Garfield (1979). In the Preface, Garfield notes "This book is, in a sense, the biography of an idea. The idea is the one of indexing the literature of science by the material cited by that literature. The idea was turned into reality in 1963 by the first annual edition of the *Science Citation Index*." However, as he notes on page xi, he had begun writing about citation indexing and its potential uses as early as 1955.

[10]Price's earliest work deals with physics and the history of science (Price, 1951, 1956a,b). These papers anticipated Price (1961), (Paperbound, 1962, Enlarged edition, 1975). See also his influential (Price, 1963). Among the original and influential concepts and models Price developed in addition are the exponential growth of science and the half-life of scientific literature as well as the formulation of Price's Law, namely that 25% of scientific authors are responsible for 75% of quantitative studies published papers (Price, 1963). Price was also responsible for conceiving of networks of citations between scientific papers. Price (1965) including for example, the discovery that both the in- and out-degrees of a citation network have power-law distributions, making this the first published example of a scale-free network. Price produced a mathematical theory of the growth of citation networks, based on what would now be called a preferential attachment process (Price, 1976), an idea closely related to Merton's Matthew Effect and Garfield's observations on the skewness of distributions of papers by individuals, and of citations to papers and journals.

[11]Merton's sociological studies of science began much earlier of course, in a series of papers published from 1935 onward including, for example Merton (1935), and the publication of his doctoral dissertation Merton (1938). Merton's writing on science and technology continued with interruptions through 1957 when he returned with greater focus to the sociology of science with Merton (1957).

[12]Merton's "copy of Little Science Big Science" is unfortunately undated but must have been sent to him by Derek as it is inscribed "To Bob Merton on whose shoulders etc.! Derek de S. Price." I assume—without evidence—Price sent Merton a copy of the book shortly after it was published. Price's allusion to Merton's "shoulders," refers to Merton's (1965), published 2 years after *Little Science Big Science*, but the manuscript had been circulating for some years before it was published.

[13]As it happens Merton and Garfield (1986) jointly authored the introduction to the revised edition of Price (1986). It is a telling and affectionate account of Price's intellectual power and his idiosyncrasies: his love of dramatic statements, vivid locutions and impulse to quantify.

Stephen Cole, who were both members of the Sociology of Science seminar which Bob had started and that he and I later co-taught[14].

The Coles and I had, of course, read Bob's classic paper on the norms of science (published in 1942)[15] and his study of the importance of priority in scientific discovery (Merton, 1957). We were convinced that scientific work was hard, that scientists sought evidence that their work mattered to knowledgeable peers, and that they took citations (or their absence) as evidence this was so[16]. Not citing relevant prior work constituted a violation of the norms. And last, we thought a central question to be answered was how well the resources and rewards scientists received for their work reflected the extent to which they had contributed to scientific knowledge and if they did not, what could account for the disparity between them.

These ideas had been explored in Bob's publications: on "Singletons and Multiples in Scientific Discovery," in 1961, and in "Resistance to the Systematic Study of Multiple Independent Discovery in Science," (Merton, 1963)[17]. In both, he laid out the significance of scientists' drive to gain recognition from their peers, vividly exemplified in strategic cases of multiple independent discoveries, made-to-order occasions for priority disputes[18]. But while these ideas could be illustrated using historical and contemporary accounts by scientists, there were no satisfactory means at hand to measure how much given scientists had contributed. We could count authors' papers to measure their productivity (and count we did, despite our belief that mere counts were inadequate indicators) and we could inventory the awards they received (and inventory we did despite our knowing that awards had their limitations as a measure). We knew both were unsatisfactory indicators of what we really wanted to measure which was the influence of scientific work of individuals and collectivities.

[14]By this time, I had earned my degree and had been appointed an assistant professor of Sociology at Columbia.

[15]Published originally in 1942 (Merton, 1942) and reprinted in Merton (1973). Merton laid out the "ethos of science," which includes the prescription that truth claims be judged only on their scientific merits, not on social attributes of religion, politics or the prestige or standing of those who had made them (*Universalism*); that scientists are required to make their work public—to communicate it and make it available to the community (*Communism*); that in evaluating truth claims, no expected benefits other than deserved peer recognition should affect the scientists who make such claims or those who accept or reject them. (*Disinterestedness*), and finally that every truth claim must evoke skepticism and be subject to social arrangements established in science for this purpose (*Organized Skepticism*).

[16]Later attacks on the Mertonian norms so central to the Constructivist position were still to come. I can only report that there was ample evidence in the interviews I did with Nobel laureates, focused, in part, on questions of conflict over credit for research contributions, that demonstrated their commitment to abiding by the norms, though not necessarily conforming to them on any and all occasions. As I've noted they commented often on getting more credit than perhaps they should have, an indicator of their having a fine-tuned sense of the proper exchange rate that should hold between credit and contributions to science (Zuckerman, 1977).

[17]Reprinted in Merton (1979) as Chapters 16 and 17, 343–382.

[18]In the first tranche of volumes drawing on Merton's views about how science operated, Warren Hagstrom emphasized the importance of the reward structure in science and the complementary role it played in exerting social control on scientists in the three disciplines he studied (Hagstrom, 1965).

Meanwhile, in 1962 and 1963, Irving Sher and others at ISI were busy doing research on the distribution of citations scientists received, on how well they squared with other indicators of scientific importance, and how skewed the number of citations were in samples of papers, journals, and scientists. Their work demonstrated over and over that a small number of authors, papers and journals garnered the lion's share of citations. This is just one example of the meshing of Gene's research, Bob's theorizing and Derek Price's models–all three focused on the skewness of scientific contributions and the skewness of rewards, both indicating the presence of processes of cumulative advantage[19].

My having met Gene and learning about citation indexing subsequently proved far reaching for the Coles and for me. Jonathan's piece in the *Festschrift* for Gene, published in 2000, recounts of how eagerly both Coles took to the possibilities of using citation analysis to study a variety of important problems in the sociology of science including but not limited to the extent to which the importance of scientists' contributions, (or, for them, the "quality" of scientific papers scientists wrote as gauged by the number of citations they received), were in accord with the prestige and number of rewards scientist collected. (Cole, 2000) Aiming at building up an empirical picture of the how the reward system worked, they began by examining the correlations between citations and other indicators of the "quality" of scientific contributions. They went on to show the striking connections between citations and published productivity, scientists' visibility to others in their fields, and scientists' judgments of the work others did[20]. They used citation counts or "quality," the concept-plus-word, to signify scientific importance more consistently than many others, including Gene. He was intent on citations being thought of as indicators not of value or importance or significance but of "influence," no more and no less than that. The Coles also probed the validity of the Ortega Hypothesis[21] which held that scientific development depends on contributions by many members of the scientific community rather than resulting from research by a small number of elite contributors. Based on detailed citation analysis of papers in physics, they concluded that the data did not support the Ortega Hypothesis. They found that significant and highly cited contributions did not cite the work of many less cited scientists but instead referred to a limited number of other highly cited contributions, thus leading to questions about the utility of the scale and structure of scientific activity then in existence.

[19](Price, 1976) Price labeled these *preferential attachment distributions*, probably following Yule, and the term continues to be used in statistics, network analysis and scientometrics, especially in citation analysis. A preferential attachment process is one of a class of processes (including citation dynamics) in which some quantity, typically some form of wealth or credit, is distributed among a number of individuals or objects according to how much they already have, so that those who are already wealthy receive more than any others.

[20]Many though not all of these investigations were summarized in Cole and Cole (1973).

[21]Cole and Cole (1972). This research set off a flurry of attempts to test the validity of the Coles' findings that largely but not entirely agreed with their conclusions while also setting off still more research on whether the philosopher, Ortega y Gasset had actually proposed the hypothesis attributed to him.

In a study commissioned by the National Academy of Sciences, they extended their analysis to the operation of peer review drawing on data on assessments of proposals scientists submitted for financial support for their research. One central question they raised was whether assessments by independent peer reviewers exhibited high levels of consensus about the merits of proposals they were judging. This was important not only because consensus was taken as fundamental to the operation of science by such respected observers as John Ziman and Michael Polanyi but also because many scientists were convinced that consensus was close to being universal in scientific judgements[22]. Examining the scores reviewers gave the proposals they were assigned to rate, the Coles and Leonard Rubin found that agreement among reviewers was far from uniform. To be sure, small shares of proposals received very high scores and very low scores–from all the reviewers who read them. But the majority of proposals fell into a "gray" area in which reviewers failed to agree on whether proposers should receive support, raising questions about whether consensus was really as pervasive as claimed, at least at in this early stage of assessment[23]. These findings raised questions about the extent to which consensus held at every phase of evaluation or indeed may not be as characteristic of science as so many believed. If indeed consensus was so central to science, it may characterize evaluation only after research had been completed[24].

In the early phases of the sociology of science, at least in the United States and in the 1940s and 1950s, a mixed group of sociologists, historians of science, scientists, philosophers of science and students of science policy published books and papers relevant to sociological problems[25]. But it was only from the 1960s onward, that a new and young cadre of those who did the sociology of science were mostly trained as sociologists. They studied science as a social enterprise and the behavior of scientists from a distinctly social perspective (much like other sociologists studied religion or politics or the family). The effects of social structure on how scientists went about their work and of general sociological processes such as social stratification became the subject of research on science and scientists. These studies focused on the social origins of scientists, the development of their careers, the organization of scientific activities, the contexts in which science was pursued (in government, industry and the academy), and the extent and kinds of scientific work that appeared in the published literature[26].

This newest cadre of mostly young researchers, a number of whom were well-trained statisticians and methodologists, found citation analysis a remarkably effective new tool for measuring scientists' productivity and the influence of scientific work, both significant in assessing theories of how science worked. They included Paul Allison, John Stewart, Lowell Hargens, Scott Long, Robert McGinnis, and Barbara Reskin. All sought to answer questions about the effects of various aspects of social structure, how well the distribution of rewards, squared with the importance of scientists' contributions. If it turned out the amount and kinds of rewards scientists received were not closely related to the influence of work, as theoretical accounts said they should be, these researchers aimed to identify other determinants that affected who got rewards, of what kind and when[27]. In doing so, the research agenda grew to include the effects on career success of particularistic characteristics of scientists, those that were unrelated to the merits of the work scientists contributed. Prime examples of particularism included, for example, having important scientists as sponsors who facilitated the career development of their protégés, being located in a high prestige department which also facilitated career development, holding an appointment in a leading university, or being a man rather than a woman. Much of this work demonstrated, when care was taken to compare only those with the same level of accomplishment, that the allocation of the rewards did not strictly adhere to the norms, especially in the early phases of the scientific career before young people had assembled independent records of contribution. Thus, one lesson to be drawn from these studies was that the allocation of rewards did not accord as neatly with the norms as the theoretical accounts of science should have led us to expect. At the same time, sociologists have learned

[22] The idea that consensus is a central feature of science is found in the writings of observers as different as Michael Polanyi, Thomas Kuhn, and John Ziman. Indeed, Ziman makes consensus central: "the goal of science is a consensus of rational opinion over the widest possible field." *Reliable Knowledge the Exploration of the Growth of Belief in Science*. Originally published 1978 Reprinted 1996. Cambridge: Cambridge University Press (Ziman, 1978). Warren Hagstrom's study (1965), based on interviews with scientists in a variety of disciplines, also emphasized the importance of consensus in science and portrayed the community as one based on a system of exchanges in which contributions to knowledge were expected by their contributors to evoke recognition.

[23] To no one's surprise, the most highly rated proposals received funding and those with the lowest ratings were rejected but that left a significant number of proposals for which the ratings alone did not give unambiguous directions to funders.

[24] Cole et al. (1978), Their results were subject to intensive review by statistically sophisticated members of the National Academy who were skeptical about the findings that violated their beliefs about how science really operated. Ultimately, they gained permission to publish but not without a great deal of disagreement about whether the data supported their interpretation.

[25] That is in papers appearing in bibliographies of work in the sociology of science. See Cole and Zuckerman (1975), for a historical and sociological analysis of the specialty in its early incarnation.

[26] A separate set of studies treated scientists and science done for practical purposes in government and industry and the disjunctions between the objectives of applied research and research of a more fundamental nature. (I do not, of course, wish to imply that fundamental and applied science ever mapped neatly on to research carried on in the academy on the one hand and in government and industry on the other. This is clearly not the case as the Bell Laboratories and the National Institutes of Health indicate. But for a time, sociologists of science explored differences between government and industrial science on the one hand and academic science, on the other. See Kornhauser (1962) and Marcson (1960, 1966).

A principal use of citation analysis, to the extent it was used at all, was in descriptive studies of the U.S. scientific enterprise appearing under the auspices of the Science and Engineering Indicators project of the National Science Foundation. These studies were intended to gauge how influential research carried out in these domains rather than in finding out how the social and cultural arrangements in these enterprises affected the science was produced.

[27] Empirical research on this and related questions produced a raft of papers, by these authors, separately and together, in the leading sociological journals, testimony to the importance of these matters for the study of social stratification as well as the sociology of science and their sense of the audiences they wished to address. See Hargens and Hagstrom (1967), Hargens and Farr (1973), Allison and Stewart (1974), Reskin (1976), Reskin and Hargens (1978), Long et al. (1979), Reskin (1979), Allison (1980), Long et al. (1980), Hargens and Hagstrom (1982), McGinnis et al. (1982), Allison and Long (1990), and Long et al. (1993).

long ago, that conformity to norms is a complicated matter. Behavior does not always conform to normative expectations and yet, at the same time, this does not mean that the norms are not accepted as legitimate. That particularism was part of the evaluation and reward systems and affected scientists early in their careers was important because the evidence indicated that early career success had much to do with success later on. Thus, another lesson to be drawn from these studies is that important feedback effects were observable in the operation of the reward system (a lesson not given much attention at the time but one that would later become significant in thinking about how the reward system in science operated). And still another lesson these studies taught was that citation analysis yielded results needing further explanation, not least that the reasons for disparities in citation counts among groups of scientists such as men and women and to those working in institutions of varying degrees of prestige[28].

At the same time, other questions about the social structure of scientific work were being pursued, these treating the connections between changes in scientific knowledge and the structural contexts such as specialties and sub-specialties, invisible colleges, and schools of thought in which scientists did their work and communicated it to others. It is striking that most of the inquiries by sociologists dealing with social structure either eschewed citation analysis altogether or made marginal use of it. This was the case in one of the early and still persuasive account of the development of radio astronomy, as a specialty, and its close linkages to the research agenda pursued by radio astronomers. Edge and Mulkay's *Astronomy Transformed*[29] showed how close attention to changing ideas, methods and connections between individual participants in the radio astronomy community, using a nascent version of network analysis (Edge and Mulkay, 1976), could illuminate how a specialty developed. They did not however use citation analysis, indeed, even if they had had access to good citation data on astronomy when they were doing their research, they would not have done so. Edge had strong reservations about such evidence, being highly skeptical about using bibliometric data in general and it is clear, at least after the fact, that Mulkay would also not have thought adding citation data would have been helpful in their study.

Several other inquiries tapped into the classic problem in the sociology of knowledge: the linkages between the pursuit of knowledge and its transmission and the social structures in which these phenomena occur. Crane's (1972) early monograph on *Invisible Colleges* comes to mind especially, as does Mulkay et al. (1975), Mullins and Mullins' (1973) study of "theory groups" in sociology[30] along with Amsterdamska's (1987) historical study of schools of thought in the development of linguistics[31]. Drawing on acknowledgments of colleagues' advice as indicators of information flow Crane's principal questions were: how did specialties grow exponentially (as Derek Price proposed they did)? Did information, especially about innovative contributions, move along lines developed in social interactions among members of the same communities in which they were made? And did such information move across specialty lines, as citations to journals outside the discipline of the citing author might signal? It did. Her research concluded that collectivities like invisible colleges (or specialties) were central to the growth and diffusion of new contributions, innovative scientific ideas are not developed in isolated "ivory towers" but rather are the outcomes of vigorous interaction within groups and indeed these groups serve not only as contexts in which ideas form but also those that promote their diffusion, and finally, that sociologists should pay more attention to them than they had been doing. She was the only one of the trio to use citation analysis in research on specialty development.

As it turned out the most powerful demonstration of how specialties developed using citation analysis came not from sociologists of science but from Small and Griffith (1974) who, using Small's (1973) co-citation analysis and other citation indicators, showed how these data served as signals of the emergence of specialties often before the scientists involved would have perceived this was occurring (Small, 1977).

The *Science Citation Index* also proved exceptionally useful in probing aspects of what we called the cognitive (as against the social) structure of science. Cognitive structure consists of features of scientific knowledge that cut across substantive domains that affect scientists' careers, and their chances to contribute as well as the organization of scientific research. It includes, for example, the extent of codification of knowledge prevailing in different disciplines and specialties. It also includes how much or little consensus prevails on the validity of knowledge claims and in evaluation of the importance of contributions and the extent of diversity in the research agendas scientists pursue in disciplines and specialties.

As Merton and I put it, "codification refers to the consolidation of empirical knowledge into succinct and interdependent theoretical formulation," it affects the extent

[28] Reskin (1976, 1978) and Hargens et al. (1978). At much the same time, Jonathan R. Cole was laying out disparities in publication and citation rates of men and women in Cole (1979). He concluded that when judged by these conventional measures, women have contributed less to scientific knowledge than men and that together these variables account for much of the observed gender differences in rewards. He also reported that these disparities have been shrinking.

[29] Edge was trained as a physicist and moved into radio astronomy before taking over the directorship of the Science Studies Unit at the University of Edinburgh. Mulkay was a sociologist with decided views from the outset about the shortcomings of the sociology of science even in the beginnings of his career. Both will appear later in this account in the section on advent of Social Constructionism.

[30] Mullins and Mullins (1973) The Mullins' study examines stages of development of what they called "theory groups." The Mullins were primarily concerned with mapping social relations between members of theory groups rather than with the kind of intellectual influence citations might indicate, and they may have thought that citations were less good indicators of social networks than the sociometric information they used.

[31] I do not remember Amsterdamska and I ever discussing using citation analysis in studying schools of thought, in general, or in linguistics. But I suspect that the absence of data for the period of time relevant to her work (18th and 19th centuries), and linguistics is now highly technical and leans toward the sciences but once was mainly philologically inclined made this decision moot (Amsterdamska, 1987). Her later work shows no disinclination to use citations where appropriate but (Leydesdorff and Amsterdamska, 1990) argued that insufficient attention had been paid to the diverse functions citations serve and thus there is considerable ambiguity in what they signal despite scientists taking their meaning as more general than what citing authors intended. See also Luukkonen (1992).

to which "experience should count" in scientists' chances for making new and important contributions as well as other features of scientific accomplishment (Zuckerman and Merton, 1972). Put another way, codification is a way of expressing how tight the connections are between theory and empirical findings, how broad the scope was of explanations of phenomena and of the implications which could be drawn from them[32]. We thought that differing degrees of codification helped to explain why younger scientists were more likely to make major contributions in certain disciplines and specialties more often than they did in others. It seemed to us that the more codified knowledge was in disciplines or specialties, the more readily young people could acquire enough knowledge that they could make important contributions. We also thought that the extent of codification was likely to affect how readily the merits of new contributions could be judged and how rapidly new knowledge diffused. Evidence for these speculations is hard to come by, not because there is much difficulty in identifying when in scientists' careers they make important contributions but because measuring the extent of codification of knowledge has so far proved highly problematic.

Jonathan Cole and I also used citation analysis as a means of identifying various aspects of cognitive structure in our analysis of the growth of the sociology of science. For example, we wanted to know whether a research front was consolidating (as the "Price Index" would show), that is, did increasing shares of citations go to recent publications and if they did, when did this occur. We also used citation analysis to determine whether consensus converged over time, which publications were deemed influential, and who were considered the principal influentials in a field (Cole and Zuckerman, 1975). This last matter of convergence of citations is a problem Henry Small took up independently and solved far more elegantly than we had (Small, 1973, 1977).

Research also proceeded in several quarters on other aspects of cognitive structure, notably Tom Gieryn's study of variations in the size of scientists' research portfolios and their substantive diversity (for example how varied or tightly focused they were), and of the sources of new ideas (whether they came from the center or periphery), and the paths through which such ideas diffused. After leaving Columbia, he struck out on his own and turned his attention to quite different matters: on the means by which scientists create boundaries between science and non-science, on the architecture of laboratories, and more generally on the relations between place (most recently "Truth Spots") and their effects on the credibility of claims emerging from them (Gieryn, 1978, 2018; Gieryn and Hirsch, 1983).

It would be appropriate I suspect to apologize for mentioning my Columbia colleagues as often as I have. My doing so is a result of remembering their past work more vividly than other sociologists' contributions and paying more attention to their newer contributions as they appeared. Had I systematically inventoried the research done in the sociology of science over time, my account would have been more even-handed and therefore less biased. In self-defense, however, research coming out of Columbia was influential and of course, for a long time, the field was very small–there just weren't that many individuals working in the field so by dint of small numbers alone, it was hard not to make one's mark wherever one worked.

THE ADVENT OF A REVISIONIST SOCIOLOGY OF SCIENCE: AN UNANTICIPATED AND SIGNIFICANT DEVELOPMENT

Varieties of Constructionist Accounts of Science

Not long after the sociology of science had begun to acquire the conventional trappings of a new specialty in the mid-1970s (the establishment of new journals and professional societies, for two examples), an array of papers and books began to appear that were unlike others I and my colleagues had seen before. Mostly but not entirely originating from researchers in the United Kingdom, these publications were highly critical of Robert Merton and the work he had stimulated. They advocated the adoption of a research agenda that would be nothing less than an alternative kind of sociology of science. They sought to replace the institutional approach with relativist perspectives of several stripes. Variously termed as time passed as Social Studies of Science, Science Studies, Science and Technology Studies, the Sociology of Scientific Knowledge, and the Social Construction of Science (or Constructivism), the real challenge these publications declared was to demonstrate how society and culture determined the substance of knowledge claims scientists made. This new agenda was not to supplement the institutional approach, it was to be seen as the only way to understand how science worked. The criticisms to which constructionists gave the strongest weight concerned "deep flaws" in the institutional approach owing to its unexamined positivist assumptions and its reliance on the role of social norms played in regulating the behavior of scientists.

Michael Mulkay's dismissal of the Mertonian norms was among the first of these papers that aimed to reshape then current thinking about the scientific enterprise, indeed to demystify it and to replace it with a new depiction of how science really operated. He contended that Merton's account of science provided a "storybook image" of scientists' beliefs and behavior, one that reflected scientists' self-aggrandizing views of the collective enterprise and consisted mainly of rhetoric aimed at protecting the image they wished to preserve (Mulkay, 1976). More or less simultaneously, David Bloor was laying out the fundamentals of Constructivism in his Strong Programme for the Sociology of Science, a statement of full scale commitment to "radical relativism" (Bloor, 1976). A series of papers and monographs were soon to follow by Bloor and his colleagues in the Science Studies Unit at the University of Edinburgh then directed, as I noted, by David Edge, that included Barry Barnes, Stephen

[32]Zuckerman and Merton (1972) first published in 1972 and reprinted in Merton (1973). We also proposed that a number of aspects of the social structure of science, for example, the ages at which scientists make important contributions, is affected by the "codification" of scientific knowledge (Zuckerman and Merton, 1972, See p. 507 ff in 1973 reprint).

Shapin, Donald MacKenzie and Harry Collins and later G. Nigel Gilbert[33]. They were largely young, trained as philosophers or historians of science and some not only had had formal training in one or another science, but had, in fact also worked as scientists. The few coming out of sociology were not much interested in drawing on then established sociological ideas and were not strongly identified with the discipline as we knew it. Mertonian Sociology of Science and its reluctance to take on the relativist agenda they proposed was seen as "meek," even as "lack[ing] of nerve[34]" (Collin, 2010, p. 36).

All had large intellectual aspirations.

As it soon became clear, this was a full-scale rebellion. Indeed, they ceased paying attention to any particular lines of research but summarized all of it as coming from, as they termed it, the "North American" school. This was a misnomer, of course, in light of the fact that sociologists of science at work at the time were by no means all from the United States or Canada. But more important, that terminology had the consequence of effacing differences in existing perspectives among them and of erasing the very identities of everyone other than Merton.

While all those who subscribed to the constructionist position considered unalloyed positivism unacceptable and rejected the institutional perspective on science[35], what emerged was not a single alternative way of thinking others were expected to accept but instead a collection of quite different and sometimes at-odds constructivist depictions of science. Yet, despite this multiplicity of perspectives, the advent of constructivism ultimately undermined the institutional perspective. It made it seem naive, useless, even *infra dig*, to those who elected to study science after the constructivists came on the scene[36].

While there was no agreed upon research agenda other than focusing on the construction of scientific (and technological) knowledge, no one set of research procedures to be applied, and no exemplary or model contribution to be followed[37], there was then and continues to be a core of beliefs many share that is worth articulating. Philip Kitcher's "Four Dogmas" of Constructivism seems to me to capture its current mindset[38]. Kitcher, a philosopher of science, has been more sympathetic than most philosophers to Constructivist ideas, seeking to understand them instead of damning them forthwith. His "Four Dogmas" are: "(1) There is no truth save social acceptance; (2) no system of belief is constrained by reason or reality, and no system of beliefs is privileged; (3) there shall be no asymmetries in explanation of truth or falsehood, society or nature; and (4) honor must always be given to the "actors categories[39]" (Kitcher, 1998). Each of these present epistemological problems, as Kitcher notes. Most important, for him, "blanket constructivism, rejection of notions of reason, evidence, and truth,... make it impossible to sort out valuable science from insidious imitations (Kitcher, 1998)." This last is obviously greatly problematic for all but thoroughgoing relativists.

The following briefly summarizes several influential Constructivist research studies now defined as "classics." They illustrate differences in the approaches taken in the early days of constructivism[40]. Latour and Woolgar's ethnographic study, their much-admired and much-cited *Laboratory Life*, was based on observations of the daily activities and conversations of scientists working in Roger Guillemin's Salk Institute laboratory on what would come to be the discovery of the Thyrotropin Releasing Factor[41]. They treat what they saw and heard as a

[33] Shapin has gone on to do highly influential work in the history of science, MacKenzie to similarly influential research on financial economics, and Collins to rethink the role of replication in science and of expert knowledge in twenty-first century thought that has major implications about the trustworthiness of scientific claims.

[34] Merton's reluctance to adopt this agenda derived principally from his view that as modern science developed as a social institution, the knowledge claims its practitioners made grew out of internal developments, quite independent from its social milieu. However, it is always the case that with the general directions scientists take at a given time, their "foci of attention" are inevitably influenced by external pressures and individual preferences in science as is the pace of development in science which depends, among other external constraints, on resources available to get research done.

[35] Those who have read Merton's work will be skeptical about the charge that he was an unthinking positivist. While he did believe that nature existed independent of scientists' observations and constrained their findings, he held that multiple theories could explain the same observation, that what were understood as facts were inevitably connected to the theoretical context in which they had been investigated, that scientists were influenced by the social and cultural contexts in which they did their work, and took it as a given that scientists in deciding on the problems they chose to study were far from free of the influence of their cultural and intellectual histories. Still, he was known to say that he would prefer to fly on an airplane designed in accord with scientific definitions of aerodynamics than on a plane built as a social construction.

[36] I will have more to say about the current foci of attention in research by American sociologists later on.

[37] See my account of these differences in "The sociology of science," in Neil Smelser, ed. *The Handbook of Sociology*. Newbury Park, CA: Sage, Publications, 1988, 511–75 (Zuckerman, 1988). Were I ever to update that paper, I would give more attention to actor network theory as it has been promulgated by Michel Callon, Bruno Latour and John Law to the succession of changes in those adopting constructivist perspectives. Harry Collins has been especially sensitive to the importance of thinking about changes of this sort. See for an early example, Collins and Evans (2002).

[38] Kitcher's four dogmas bear a slight family resemblance to the four indispensable components of the Strong Programme developed by Bloor (1976). The following is the Bloor original account of the elements of the Strong Programme for the Sociology of Science.

Impartiality: it examines successful as well as unsuccessful knowledge claims

Symmetry: the same types of explanations are used for successful and unsuccessful knowledge claims alike

Reflexivity: it must be applicable to sociology itself

Causality: it examines the conditions (psychological, social, and cultural) that bring about claims to a certain kind of knowledge.

[39] Kitcher, places himself in the "marginalized middle" between the extremes of "realist-rationalism" (read positivism-old style) and the "socio-historical perspective" (read relativism new-style). He addresses current work in constructivist studies with a view to "integrate[ing] the best features of each extreme (Kitcher, 1998)." It may be useful to provide a bit of substance to Kitcher's tightly worded summary. Examples of the disagreements among constructionists are about the principal determinants of what scientists take to be true. Some focus on the role political or class interests play in shaping scientific claims. Others focus on how the presentation and defense of truth claims is a kind of military exercise in which force and power are mobilized in support the views being proposed while opposing ideas are undermined and discarded. Still others pay attention to of is science as a negotiation process in which conflicts are settled, at least for a time.

[40] Note that these studies were exemplars of constructivist inquiry at the time the constructivist turn occurred and in no way represent current constructivist inquiries.

[41] Latour and Woolgar (1979) That the Guillemin research would win the Nobel Prize in 1977 after Latour and Woolgar did their research and involved an intensely

case in point of how scientific "facts" are constructed in the laboratory rather than being reflections, however distant, of some external "natural" phenomenon. There is far more than this to Latour and Woolgar's depiction of science. As self-defined ethnographers, they viewed the scientists who were their research subjects in much the way that members of non-literate tribes have routinely been viewed. Observing their tribal subjects at work day after day led Latour and Woolgar to conclude that the evidence being collected was created by technically advanced research machinery in accord with complex research designs aimed at increasing facticity rather than being expressions of some exterior natural world which does not exist in any meaningful sense. Increasing facticity, they held, was central to shaping the statements scientists wished to claim were important "facts," facts that others would be likely to accept. Facts would then be assembled in such a way that their importance was established in published papers which would then earn credit from the relevant larger community. Acquiring credit would then improve scientists chances of securing new funding that would enable them to continue their work and allow the cycle of research and credit-seeking to begin again[42].

Karin Knorr Cetina's research resembles the Latour and Woolgar inquiry in important ways. An anthropologist by training, Knorr Cetina began her ethnographic research with some of the same reservations Latour and Woolgar had about nature's role in scientists' claims. Again, based on ethnographic observation of working high energy physicists and molecular biologists, she earmarked two important phases in the "manufacture" of knowledge, as she put it, first, in the role laboratory machinery played in manufacturing scientists' observations, and then in the successive painstaking editing of knowledge claims scientists undertook in preparing their work for publication. Multiple drafts of research reports clearly exhibited the process of refining what began as disorganized data assemblages into the classic scientific paper (Cetina, 1981)[43]. Her later work turned to the ways that larger social groups, those she called "Epistemic Cultures" (disciplinary-based collectivities) also construct the prevailing research agenda and the findings they produce (Cetina, 1999). Analysis of the operations of such macro-social units allowed her to enlarge her claim that scientific knowledge is constructed not simply in the laboratory but also in larger collectivities, which raises questions about the existence of anything like unified science[44].

Harry Collins, has long been occupied with problems associated with replicability in science, a central procedural attribute of science[45]. He has spent a career observing how scientists negotiated the status of controversial claims, particularly, but not only claims about the existence of gravity waves. In the absence of experimental evidence for a long period of time, physicists' conclusions about the existence of gravity waves were, at best, inferential. The succession of views about gravity waves, Collins held, were strategic examples of knowledge claims being what scientists had agreed they were, that is, such claims were crucial evidence of science being socially constructed. No mere scientific detail, Collins had chosen a significant line of inquiry in physics that began with Einstein's prediction in 1916 that gravity waves had to exist as an outcome of his general theory of relativity. Their existence and how to determine it continued to occupy physicists for a century or more, until 2015 when the first indications of the presence of gravity waves emerged in research using advanced observational techniques. Their existence was then decisively established and officially announced in 2016[46].

A third take on social constructivism is the emphasis G. Nigel Gilbert and Michael Mulkay have put on "Discourse Analysis." They claimed that focusing on of what scientists say will yield better, more telling, evidence of the fluid state of scientific knowledge claims than do observations of scientists' behavior[47].

Still different are the evolving versions of "actor-network theory" (hereafter A-NT) proposed by Bruno Latour, Michel Callon and John Law to explain how major and minor scientific changes in scientific and technological views are achieved through the networks of actions by humans, non-human research objects, and ideas (Latour, 1987, 2005; Law and Hassard, 1999)[48]. (Precisely what A-NT is and how it explains the substance of scientific and technological contributions is, unsurprisingly, a

competitive race for credit with Andrew Schally (who shared the prize with Guillemin) gives the account a secondary but not uninteresting twist.

[42]Little or no attention is given to the processes of evaluation set in motion once research findings are announced, made public and submitted for publication. As all scientists know, at this point, informal and formal judgments begin, focused on the credibility and worth of published contributions. These processes of evaluation are critical. They may be subject to social construction much as science coming out of laboratories is supposed to be, as Harry Collins has contended, but constructed or not, they are critical to understanding how knowledge claims are established.

[43]The latter highlights the dramatic differences existing between the cultures of research in molecular biology and high energy physics and their effects on the scientific knowledge these communities produce.

[44]Like Donald MacKenzie, Knorr Cetina has also turned her attention to the study of financial markets, treating global financial markets as "virtual societies." See Cetina and Preda (2004, 2014).

[45]Harry Collins originated the term and concept "the experimenters regress" as a means of conveying the impossibility of determining whether experimental evidence is validated by replication. He draws on the contention that theory and experiment are so intertwined such that assessing the usefulness of competing theories requires recourse to evidence but evidence, itself, is theory-based. Collins has concluded, as a consequence, that disputes about evidence can never be settled via experimental replication, See Collins (1992). More recently, he has concluded that replication must be preserved as a criterion for assessing experiments "Reproducibility of experiments: Experimenters' regress, statistical uncertainty principle, and the replication imperative" (Collins, 2016).

[46]Collins began his research on the contested state of gravity waves forty years ago. While the theoretical importance of gravity waves was unquestioned, their existence could not be directly observed and thus their status remained negotiable. It was not until a series of experiments by Cal Tech and MIT physicists on the LIGO project demonstrated their existence in 2016. Just a year later, this work was awarded the Nobel Prize in Physics in 2017.

[47]Mulkay et al. (1983) and Gilbert and Mulkay (1977). Discourse analysis rests on the variations in the accounts of the same phenomena scientist produce in different settings and the great range of variability in accounts of the same event by different participants, both being consistent with Mulkay's early emphasis on the rhetorical character of scientists' claims about their commitment to the norms of science. Note that this is the same G. Nigel Gilbert whose paper on citations being modes of persuasion (to be discussed shortly) is consistent with if not exactly the same as examining scientists' discourse.

[48]AN-T is said to be an amalgam of Semiotics, Garfinkel's Ethnomethodology, and Gabriel Tarde's sociology. Despite sharing the image and the word "network" in their titles, A-NT has little to do with network analysis as it is pursued by sociologists and political scientists.

matter of contention, as have been opinions about its analytic value although there are many who subscribe to its claims). Latour has since become less enthusiastic about the success of A-NT in light of the skepticism it has evoked in the general public about the reliability and validity of science of all kinds, and especially of climate science, whose rejection Latour had not anticipated and now finds worrisome (Latour, 2004).

I have not given sufficient attention to the now large literature analyzing technological innovations as responses to complex social and cultural influences, a development that has proved far less contentious than the notion of the social construction of science[49]. Nor have I given the rapidly growing research literature on finance and financial markets the due it merits as a "strategic research site," to use a Mertonian term, if I may. Donald Mackenzie has vividly portrayed the role theoretical financial economics has played in constructing market behavior and the behavior of traders, just the reverse of conventional accounts of how scientific descriptions should mirror behavior not determine it[50].

Nor have I mentioned the varying feminist accounts of science and the role gender plays not only in differential evaluation of the research contributions of men and women but also in problem choice, theory choice, unconscious selective observations of phenomena and selective interpretations. Not all feminist accounts of science rest on constructionist principles but those probing the construction of scientific knowledge are at least consistent with them[51].

I have mentioned but not called sufficient attention to constructivists' inclinations to dismiss what scientists say about their motives, interests, behavior and the consequences their acts, inclinations many scientists find unacceptable[52]. I have also not remarked on the hostile responses many scientists have had to constructive accounts of science or the Science Wars that erupted following the Sokal Hoax[53]. Efforts to reconcile differences in approach, whether they are between scientists and constructionists or between more sociologically inclined students of science and thoroughgoing constructionists[54], have not resulted in markedly less disagreement though efforts have been made to articulate more precisely what each side believes is at stake, and it does appear that when this occurs that the heat of the controversy is considerably reduced even though minds are not changed (Mermin, 1998)[55]. As I have noted, most scientists have continued to reject the constructionist view or to be indifferent to it, that is, those scientists who have bothered to pay attention at all.

Apart from the substantive changes. Constructivism brought to the sociology of science, its rise is important for our story because with it came an assault on the meaning of citations and how they worked, and thus, on citation analysis itself.

A Revisionist View of Citations

The advent of social constructionism has also been consequential for Gene Garfield's long-term influence on the sociology of science. Gene was unwavering in thinking that the value of citations to the scientific enterprise derived primarily from the evidence they provided for reconstructions authors provided of the antecedents of their research and thus of how much influence cited papers had[56]. He recognized of course but did not give much weight to the legitimating or undermining the role citations might play in assessing the merits of cited contributions. G. Nigel Gilbert's 1977 paper on "Referencing as Persuasion" contended that Gene's long held views were at odds with reality. Gilbert argued that citing authors' principal reasons for citing (or "referencing") was to persuade their readers of the novelty and validity of their claims, while seeking also to locate their work in the context of already established knowledge (Gilbert, 1977).

Viewed in this way, citations (of references) had little to do with recognizing earlier contributions and fell short, very short, of the role of establishing the kind of connections to prior contributions Gene believed they did. References from this

[49]See Mackenzie and Wajcman (1985) and Bijker et al. (1987). The first, a collection of articles, attests to the influence of society on technological design and includes a seminal article by Trevor Pinch and Wiebe Bijker, showing how the sociology of technology could proceed along the theoretical and methodological lines established by the sociology of scientific knowledge. Hughes, a historian, treats technological innovations not simply as individual inventions but as calling for the assembly of large-scale systems, for example, the complex of developments and institutions required to operate ICBMs and energy delivery.

[50]Mackenzie (2006, 2007, 2009), and a number of recent papers, for example, on high frequency trading. As noted earlier, Karin Knorr Cetina has also moved to the study of financial markets and, at the beginning of this work, it linked up with economic sociology which was not then and is not now wedded to constructionist principles.

[51]Among the most persuasive commentators of the complex connections between gender and science are Evelyn Fox Keller and Helen Longino, both concerned with epistemological issues in the development of knowledge. Fox Keller's interpretive biography of the geneticist Barbara McClintock (Keller, 1983), *A Feeling for the Organism: The Life and Work of Barbara McClintock*. Freeman: 1983 remains a classic. See also her more recent analysis of the gender and science (Keller, 1986) and her analysis of interconnections of nature and nurture book (Keller, 2010). *The Mirage of a Space Between Nature and Nurture,* Chapel Hill, NC, Duke University Press, 2010. See also Longino (2002), which she explores and attempts to reconcile the accounts of knowledge of philosophers and sociologists of science, and in her more recent studies. She compares approaches to research on aggression and sexuality from the standpoint of epistemology (Longino, 2013).

[52]Social scientists have long been aware of the problems of accepting at face value what research subjects say about themselves and the circumstances they confront but these are not sufficient grounds to reject everything that subjects say as misguided or self-serving.

[53]See for example, Gross and Levitt (1994), which includes essays by a number of scientist-critics of the social constructivists. See also the thoughtful and still relevant essay on the role nature plays in scientific inquiry by the physicist.

[54]Cole's (1992) is a rare attempt to articulate areas of disagreement between Constructivists and one sociologically-inclined student of science and to identify those that might be resolved or illuminated by recourse to research evidence. His work has not elicited much in the way of response.

[55]Labinger and Collins (2001), Essays by Steven Weinberg, Harry Collins, Steven Shapin, David Mermin and Peter Dear and others are informative and respectful.

[56]Garfield also believed that citations played multiple roles in the development of knowledge. Not only did they serve as intellectual histories of the work in which they appeared, they also permitted scientist-readers to see for themselves what the cited sources actually said. This last is an important check on the validity of citing authors' claims. While he understood that citations might also serve as rewards for cited authors, this was not, as I indicated earlier, his prime reason for thinking citation indexes contributed to the development of scientific knowledge. His focus on citations as links to the sources of prior investigations differed from but meshed neatly with Bob Merton's in the role of citations in identifying intellectual forebears of research and how they served as rewards for scientists.

perspective were instruments for shaping the contexts in which authors wished to locate their contributions, for identifying those they sought to impress, and served careerist ends.

Gilbert's assertions are not, in fact, as damaging to Gene's account of citation analysis as it might seem. As Susan Cozzens observed, Gilbert apparently accepted the notion that citations could establish the priority of citing authors, that is, they could serve as claims to intellectual property and might possibly also provide psychic income from their work being recognized in citations. And Norman Kaplan ascribed both of these functions to citations as far back as 1965 and saw both as being consistent with Mertonian norms[57]. Yet Gilbert also argued that citations could not be an important part of the reward system or, by extension, could not be experienced as rewards to cited authors, because, he reasoned, there was no systematic way authors could discover how often their work had been cited and by whom. (This was of course prior to the wide availability of digital citation indexes–the SCI, the SSCI and others such as Google Scholar. Those who wish to find who is citing this or that individual or this or that paper and how often, can now easily do so)[58].

Gilbert also reviewed the evidence on the skewing of citations toward those in authority and took this as still another reason to reject citation data as a source of information about influences on the papers containing them. Skewing of citations toward the publications of elites does raise questions about the motives of those inclined to cite them as a means of reinforcing their own standing[59]. Careful estimates do need to be made of whether cited authors actually contributed to the work that cited them. The same is true for Gilbert's "perfunctory citations" that raise questions about just how influential the cited work actually was, as does the frequency of negative citations that also call into question the "influence" of cited works. These patterns, Gilbert concluded, all point to the failure of citations to indicate antecedents of contributions or to serve as meaningful rewards[60]. Gilbert's paper has been cited just over 300 times over 40 years, and thus qualifies as a "landmark paper" in its field in the sense that Gene identified them[61] How Gilbert would now interpret this accumulation of citations to the paper he published so long ago is not self-evident. He might point to the persuasiveness of his analysis and the citations it contained or to these accumulated citations as indications of his priority and property rights to defining citations in a new way. One way or the other, the paper has been influential in the way he may have thought it would be.

As all these citations to Gilbert's paper suggest, it has become the occasion for extended commentary and some further research on citations. Some of the citing papers contain classifications of citations based on the functions they serve for individuals and the collective[62.] They also include studies of how scientists view citations and of their citation practices, and the uses to which citations have been put in different disciplines[63]. and still further efforts to identify the shortcomings of citations as signs of influence[64].

Later papers have also taken note of the problems created by what Gene termed "uncitedness" or instances in which prior research was not cited but should have been. "Uncitedness" obviously reduces the validity of citations as indicators of the intellectual lineages of scientific contributions, a problem Gene concluded was relatively small in scale, and therefore did not significantly bias the information contained in the huge corpus of citations included in his citation indexes (Garfield, 1979).

Bob Merton also understood the problems of validity introduced by uncitedness[65] but was more interested in it as strategic evidence for one of the ironies of the scientific life; that is, what he called the "obliteration of source by incorporation in the body of knowledge" or OBI[66]. OBI occurs when scientists responsible for important contributions fail to be cited because they have become so well-known that it is unnecessary (or considered amateurish) to cite them[67]. Not the outcome of scientist authors willfully depriving contributors of their due, OBI has the result of erasing the identity of the source of important contributions. It also reduces the number of citations to once highly cited works, reduces the skewing

[57]See Cozzens (1981) and Kaplan (1965), the first to think sociologically about what citations signified in science and what consequences they had. He was also the first, as far as I know, to call for a "theory of citation" and for research to be done on the various purposes citations serve, including their prime role in establishing intellectual property rights. He also understood that citations are elements in scientific communication and in legitimizing perspectives authors wished to advocate. Further he also recognized that scientists might overcite their own work and that of their friends and under-cite the work of others.

[58]Contrary to good citation practice that warns against giving credence to individual counts in the absence of suitable comparative information, some scientists religiously follow data on their own citation standing. I have even seen up-to-the minute individual scores included in *curriculum vitae* as a means of enhancing authorial status.

[59]The disjunction between individuals' intentions and the outcomes of their actions is often overlooked in analyses of behaviors and their implications. Scientists' intentions in citing particular works may be quite different from the outcomes of their doing so Zuckerman (1987).

[60]Latour (1987), portrays citations as an opportunity for citing authors to "do whatever you need to [do to] the former literature to render it as helpful as possible (p. 37)... where all deformations are fair" (p. 40. Latour's heated version of the role citations can play does not change the essence of Gilbert's analysis.

[61]David Pendlebury of Clarivate Analytics generously provided me with this datum. See Garfield's (1987) commentary on how landmark papers are selected (in the instance of the Journal of the American Medical Association which can stand in for other journals).

[62]For example, Leydesdorff and Amsterdamska (1990) and Luukkonen (1992). Both conclude that citations play multiple roles and that they operate both in the reward and in the communications systems of science.

[63]Hargens and Schuman (1990), reproduced in *Current Comments*, 3, 5–11, 1991. Baldi (1998)

[64]See MacRoberts and MacRoberts (1984, 2018) decades-long efforts to identify deficiencies of citation analysis, including one early publication on negative citations.

[65]Merton also found the phenomenon of "over citation" or adumbrationism a matter of some interest. Adumbrationism occurs when citing authors claim high prestige antecedents in order to enhance the status on the citing work or its author. *On the Shoulders of Giants*. New York, NY: Harcourt Brace, 1965.

[66]Merton introduced the concept of "obliteration by incorporation" in his *Social Theory and Social Structure* in 1949 (although the revised edition of 1968 is usually cited, 27–28, 35–37, in the enlarged edition). Ironically and not Gene Garfield's intention, some attribute OBI to Garfield who helped bring the OBI phenomenon to the attention of his readers (Garfield, 1975). The geneticist, Joshua Lederberg described OBI as occurring when a contributor has become so much a "household word" that everyone knows who has done the work and therefore it is unnecessary to cite it.

[67]One would not, for example, cite Isaac Newton in a contemporary physics paper, as the source of his work on optics.

of citations, and thereby limits the information citation data can convey about the most influential contributions in the literature.

Divided Accounts of Citation Behavior and Divided Views of Science

Two markedly different views about how science works by members of the larger communities of philosophers, historians, sociologists and bibliometricians are consistent with the conflicting perspectives on citations just described. One is a more or less realist view of science and the other, a more or less relativist view of science, that is, that nature whatever it is, has little or nothing to do with scientists' claims about it. These two accounts of science are, as I and others have noted, associated with how each depicts the behavior of scientists, what goes on in scientific inquiry, scientists' accounts of their work in the published literature, and the extent to which science is a competitive or cooperative enterprise. This last matter of the prevalence of competition and of cooperation in science is central to all views of what science is and what it attempts to do. It is the central question Henry Small addressed in a recent appraisal of realism and constructivism in science. The former emphasizes the cooperative and generous aspects of science and the latter its competitiveness and mean-spiritedness. He lays out the logical and evidentiary problems burdening each of these views and how they link up with what is known about citing practices, what scientists intend citations to do and how they are received by readers[68]. Relativist claims, Small holds, depict science as a fundamentally competitive activity, in which citations do, indeed, serve as means of persuasion, employed by scientists, in campaigns to make their views known, and accepted, "no-holds barred"[69]. By contrast, realists view science as essentially cooperative and that generosity in citing helps satisfy system needs for cooperation. He argues that referencing provides a model of "strong reciprocity, where generous citation is rewarded and non-citers are sanctioned" and that individuals' drives for maximizing their own credit at the expense of others are dampened by norms supporting "the cooperative mode of behavior (Small, 2016)." Small, is not naïve, he does not believe that scientists are by inclination, either generous or competitive. Rather, in pointing to current conversations in evolutionary biology about conditions under which both altruism and self-interest are at work, he sees a new way of thinking about norms in science that call for generosity in citing (as in other kinds of activity), that are ambiguous, in the sense that they can also serve individual self-interest while also being altruistic, depending on the circumstances. This seems to me to be consistent with available evidence on citing behavior and may also stimulate further thinking about citations and the larger questions about the character of science and its pursuit.

[68] Small (2016), Susan E. Cozzens comes to a somewhat different conclusion about the presence of both competitive and cooperative strains in science. Drawing on her study of the complex 4-way multiple independent discovery of the opiate receptor, she writes of the implicit commitment scientists have to maintaining the viability of the scientific community and the shared and continuing value they place on the common activity of scientific work. For her, "moral force" does not reside in norms but rather in the perception of shared interests and values, but she writes, that "This formulation is not a drastic departure from the statement that a social group "has" a set of values and norms.... [that is, reinforced] in the constant creative activity of evaluating actions..." Cozzens (1989). She also notes that scientists' views about what they say in public about allocation of credit for discovery and what they say in private can differ as do the perspectives of those who are personally involved and those whose principal concerns are to maintain sufficient peace for everyone's work to go on.

[69] Small notes that Latour's theory of citation "calls for a no-holds-barred approach to referencing." He goes on to observe that "in a norm governed publication world, misquoting or distorting a prior author's work would not be regarded with equanimity" and, in a realist mode, observes that such instances "are relatively rare."

PART IV–PARALLEL DEVELOPMENTS: OVERLAPPING PROBLEMS IN SCIENTOMETRICS AND THE SOCIOLOGY OF SCIENCE

I bring scientometrics into my story to make just two points: one is that scientometricians have been studying problems similar to those sociologists addressed well before Constructivism redirected the sociological agenda to studying the social determinants of scientific knowledge. The second point is that while scientometricians and sociologists may study the same phenomena, the objectives they have and the procedures they use differ considerably. Let me suggest just a few examples of those differences. Both lines of inquiry have traveled on different tracks, sometimes being closer to one another and at other times, quite remote.

Both have examined the development of scientific specialties and collaboration in science. I think particularly of Henry Small's co-citation analysis as a means of identifying nascent specialties well before they are recognized by participants as social realities. I also think of the joint work he and Belver Griffith did on specialty development by devising a method of mapping micro-and macro-clusters of literatures, as a means of identifying specialty formation (Small, 1973, 1977; Small and Griffith, 1974). Although neither Small nor Griffith were trained as sociologists (Small's degree was in the history of science and Griffith began as an experimental psychologist), both were interested in the connections between sociometrics and the sociology of science and were intent on identifying patterns exhibited in citation data on how specialties coalesce. This is quite different from sociological studies of specialty formation but the two are complementary and mutually reinforcing. Small's continuing contributions suggest that he is without making any fuss, simultaneously a scientometrician, a sociologist and a historian of science[70].

Two papers, one recent, illustrate how scientometricians have addressed collaboration in science and its consequences. One compared the productivity and influence of collaborative groups whose members represent the same or different nations and the same or different disciplines in order to shed light on which formations are the most effective scientifically (Bordons and Gómez, 2000). The other sought to assess the innovativeness of

[70] See for example, Small (2004), and, as noted, Small (2016).

research (in this instance on virus vaccines) produced by multi-disciplinary collaborative groups but added the variable of the strength of collaborators' ties to global networks of researchers (de Fonseca et al., 2016). Both treat the forms collaboration assumes as subjects of evaluation not as social phenomena to be described and understood.

Sociologists who have studied collaboration in science (I being one) have also aimed at assessing the relative productivity and innovativeness of scientific collaborations of various kinds, but their principal concern has been with how collaborations actually worked. For example, I was interested in how collaborating scientists dealt with the nearly inevitable conflict about who had contributed which ideas to the research, about how credit was to be allocated among them and the long-term prospects of scientists continuing to work together. These are real life problems for scientists who work together and they are settled in rather different ways among those of equal or different standing, in interdisciplinary as against single discipline research, and in groups of varying sizes[71].

At this reading, collaboration in science is not a subject of great interest among sociologists of science although I expect that scientometricians will continue to study it and also to continue research on other problems such as specialty development that was once addressed by sociologists.

Gene's influence on research on scientometrics has remained strong, as other papers in this issue of *Frontiers* demonstrate. Indeed, there are no signs of interest waning in citation analysis in scientometrics research despite sociologists of science having turned to other kinds of evidence as the field has been reshaped by constructivism.

WHITHER THE SOCIOLOGY OF SCIENCE?

Where Things Stand Now

In electing to describe the directions being taken now in research in the sociology of science, I have limited myself to inquiries in the United States and Canada and thus have substantially narrowed any conclusions I might draw[72]. This said, the first observation to be made is that a number of researchers who once were active in the 1970s and 1980s have turned to other subjects[73], although this is not true of all of them[74]. Second, as I noted earlier, ongoing criticism by Constructionists of the Mertonian institutional focus has led that kind of sociology of science to wane. Third, the task of identifying who is and is not a sociologist of science is complicated. Departmental affiliations are not all that helpful since teaching and research on science and technology has been hived off in some colleges and universities into separate interdisciplinary programs or departments of Science and Technology Studies while in others, it remains in departments of sociology and in still others is pursued in schools of public policy or environmental studies.

Constructivism, while moving to the fore, has led the sociology of science—or what is left of it—down a great many different paths. Current research, depending on one's preferences, can be described either as richly varied or quite unfocused. It appears to be taken as a given that current inquiries should focus on social and cultural influences on scientific knowledge and it also appears that this has resulted in the production of a number of small case studies, without the cases being chosen to shed light on a particular problem or much indication given on why they are important in light of larger objectives. The prevalence of case studies is quite consistent with Constructivism. It is striking that ethnographic or qualitative methods of research are now preferred over quantitative ones. The "qualitative turn" is also consistent, for example, with John Law's contention that ethnography reveals the actual "messiness" of social life and indeed that methods create the phenomena they are used to describe, just as the laboratory machinery cited in earlier case studies of science were said to construct the data they produced (Law, 2004).

Making claims about current work in the sociology of science and technology clearly calls for support by relevant evidence, at least if one is of the realist persuasion. As a first cut, I present evidence, collected in the style dubbed "quick and dirty research." This kind of inquiry is justified by its being an efficient way of finding out whether given phenomena are worthy of further inquiry without making large investments of time and effort. "Quick and dirty research" also has the benefit of producing some data which is better most of the time than having no data at all.

[71] These were some of the questions my study of Nobel prizewinners addressed. I drew on detailed analysis of their publication records (including even the name ordering practices they used over time and on studies they believed were likely to be important), the biography of their researches, responses to their work and interviews with them. Harriet Zuckerman, *Scientific Elite: Nobel Laureates in the United States*, New York The Free Press, 1977 and its expanded version reprinted by the Transaction Press in 1997 (Zuckerman, 1977). One of the more unexpected findings of my work was the pattern of "nobelesse oblige" in which Nobel laureates were prone to giving prime place of authorship to their younger colleagues – except when they published research they believed would have major consequences for their own careers.

[72] I hope I will be excused from treating developments in the U.K, Europe, Eastern and Central Europe and Latin America as well as Israel, India, China and Japan. A thorough review would of course, have to consider the research being done world-wide. Judging from the activity registered in communications from the International Sociological Association's RC23, much is going on.

[73] For example, Diana Crane moved on to studies of fashion, the art market and culture, Paul Allison turned his attention fully to advanced statistical analyses, and Barbara Reskin to labor markets and gender, while Jonathan Cole took on the Provostship of Columbia University while continuing his interest in the Sociology of Law. When I became the Senior Vice President at the Andrew W. Mellon Foundation, my nascent interest in the Sociology of Higher Education came to the fore as did a longstanding curiosity about the sociology of academic disciplines.

[74] Stephen Cole, Lowell Hargens, Scott Long, and Mary Frank Fox have continued to work on aspects of the sociology of science finding new problems to address with their considerable expertise while Tom Gieryn turned early to the establishment and maintenance of boundaries between science and other activities and how "place," including building structures and geographic locations affect the pursuit of research and truth seeking. He also served as Vice Provost for Faculty and Academic Affairs at Indiana University in Bloomington. Susan Cozzens is now Vice Provost for Graduate Education and Faculty Affairs at Georgia Institute of Technology and has broadened her research agenda to include science and technology policy. Peter Messeri is now a sociologist of medicine, focusing on the organization of health care systems, community interventions in healthcare, HIV and tobacco control.

I settled on doing a small-scale content analysis of the titles and précis of papers classified as being in the sections of Sociology of Science, Knowledge and Technology, (labeled SKAT), and in the Sociology of Technology presented at the Montreal meetings of the American Sociological Association (ASA) in 2017.

In all, the ASA meetings that year hosted 1,898 official registered sessions. Of these, one was allocated to SKAT and one to the Sociology of Technology. I examined the précis and titles of the papers given in these two standard convention sessions as well as the larger number defined as peer-reviewed short reports and round-table presentations listed in the convention program, as assigned to the SKAT and the Sociology of Technology sections. I quickly discovered that the term "science," for reasons that are far from clear, led to the inclusion of a papers with questionable relations to research on SKAT or Technology. For example, some of these dealt with the demography of sociology and sociologists and others with research methods, broadly conceived. Thus, I took the liberty of separating those which belonged from those that did not under the rubrics I had chosen. The number of papers misclassified as dealing with science was relatively small.

The subjects of the papers that were included were, to say the least, highly varied. Not having comparative data on the presentations at earlier ASA meetings, it was impossible to say whether the subject matters in SKAT sessions had or had not become more diverse, perhaps even diffuse. But the précis' of papers accepted for presentation showed little of the coherence one might expect if the sessions were intended to present research contributions on more or less the same subjects that would, in time, become cumulative[75]. I confess to not having examined the papers given in other sessions allocated to other conventional specialties. For example, did the subject matters treated in papers listed under such headings as the sociology of the family or religion or political or organizational sociology, have the same diffuseness I observed in the SKAT and Technology? It remains an open question whether this tendency is discipline-wide or specialty-specific.

Perhaps a few examples will illustrate the diversity of subjects covered by papers delivered in the SKAT session. These included papers on: "designer babies," a study based on 12 focus group discussions about anxieties generated by new genetic technologies; on gender in science which drew from 13 interviews with young women geologists about their perceptions of "dirty old geologists" and the respects in which older men in the field had gotten in the way of women's career development, while a third paper examined research on race that appeared in two important journals and examined how race was operationalized in these studies. These titles suggest, to say the least, a high degree of heterogeneity. The last comes closest to being an examination of race as a social category and therefore deals, in some measure, with the construction of scientific knowledge.

[75]The same is the case for the remarkable array of the subjects of short reports in both specialties although the wide-ranging subjects they cover has its own fascination. For example, under the rubric of technology were papers on "Games and their Consequences," "Music, Bitcoin and Digitization Information," "The Role of Information Technology and Increasing Equality," "Popular Culture," "Animals and Society," "Social Movements and Digital Media," and "Ecological Crises and Social Activism."

The subjects of papers treated in the main session on the Sociology of Technology were also varied. They explored gender-related phenomena, including the "trailing spouse" problem as it was experienced by partners of 38 MIT scientists who had migrated from home base. A second probed the nature of grant-supported research undertaken in universities as an exercise in connecting "mapping knowledge space," published productivity and the impact of research[76]. A third examined the sources of errors in the Fourth Assessment of Climate Change published by Intergovernmental Panel on Climate Change (IPCC), the leading agency responsible worldwide for assessing climate change. Here the author ascribed errors to "embeddedness" as a cause of social and cultural "holes" in the different social networks of scientific practice within the IPCC. The author concluded that network "holes" provided a better explanation of the presence of errors than the credentials of the authors or arguments by skeptics of climate change who aimed to undermine the narrative of the science of climate change[77].

If variety was the order of the day in subject matter, this was not so in the methods of research adopted in these papers. In line with my earlier comments, qualitative methods predominated while quantitative analyses, including network analysis and quantitative studies of productivity and citations were mostly absent. The heavy use of qualitative data and qualitative analysis is different from earlier research literature in the sociology of science and different too from the sociological literature in the major journals. It is impossible to determine from the data whether these papers show signs of being influenced by Constructivism; an analysis of their citations might shed some light on this question. And finally, it is also not clear that sociologists of science and technology have a sense of where they are going. If they do, that would be telling and if they do not, this would hold its own sociological interest[78].

Some Unexploited Potentials in the Sociology of Science

The waning interest in the citation analysis among sociologists of science does not imply that it is no longer able to shed light on interesting and important sociological problems. Such problems are what I had in mind in the last part of my title, "unexploited potentials." It was intended to signal there is still work to be done using citation data. Perhaps I should have amplified the

[76]I could not determine whether productivity or citation measures were used from the brief descriptions in the program.

[77]This paper, I assume, draws on Burt's analysis of structural holes and on later studies of cultural holes in networks but again the paper précis did not provide this level of detail (Burt, 1995).

[78]Meanwhile, Bob Merton's work remains alive in the current "mentalité" of sociology. One of the three plenary sessions at the 2017 meetings was devoted to the role played by "unanticipated consequences" of increasing social inequality. The concept-and-term, unanticipated consequences, was a theme he treated over his long career, one he introduced in 1936 and continued to explore in his last publication on serendipity. In his view, "UCs," as he liked to call them, present foundational problems in sociological analysis. The term-and-concept has become pervasive in almost all the social sciences and in the mass media, if not in general social discourse. Not surprisingly, Merton's name has been uncoupled from its use, just as the "obliteration by incorporation" phenomenon he proposed would have predicted would occur (Merton, 1968: p. 27–28; 35–37).

title to include "or partly exploited" potentials but doing so seems awkward and more precise than necessary. I mention just three classes of potential uses.

POTENTIALS RESULTING FROM COMBINING QUANTITATIVE CITATION DATA WITH QUALITATIVE AND HISTORICAL STUDIES OF SCIENCE.

Historians of science do not routinely study problems having obvious sociological implications. However, two recent papers by a chemist and historian of science, Jeffrey Seeman, meet this criterion. One addresses the positive effects errors have had in research in organic chemistry and the other, constituting a kind of riff on Merton's multiple independent discoveries, explores the existence of "Multiple Independent Errors," also in organic chemistry. The first treats the effects of erroneous claims made in the published literature. Once they are recognized as such, errors, Seeman found, not only motivate scientists to correct them but increase the number of scientists focused on correcting them, and thus, likely accelerates the speed with which the erroneous claims are put right (Seeman and Cantrill, 2016). Seeman's account of the "seminal" effects of error seems at odds with the conventional view that errors are distracting at best and at worst, create misunderstandings and deception (Seeman, 2018).

Seeman thinks errors in science are frequent but they tend to be overlooked and thus do minor damage to the scientific corpus. Still, the positive effects of errors led him to wonder whether Bob Merton's claims about the high frequency of multiple independent discoveries in science (discoveries made independently at more or less the same time)[79], might also mean that errors are also likely to occur in multiples (Seeman, 2018) and because they are multiples, are more likely to capture attention than their singleton counterparts[80].

In order to study how errors, identified as such, affect the foci of attention in a field and its pace of development, Seeman has assembled an inventory of Multiple Independent Errors, or MIEs, his being in organic chemistry, and serves as a counterpart to Merton and Barber's inventory of Multiple Independent Discoveries (or MIDs) that cover the sciences more generally (Merton, 1961a, 1963, 1968 reprinted in Merton, 1973).

Seeman rightly asks whether the study of MIEs could be instructive, especially if there are classes of errors that are repetitive. These might be errors deriving from making certain kinds of observations, from employing certain error-prone procedures, from using explanations known to be misleading, from omissions of relevant precedents, and from excessive commitment to certain theoretical positions. All of these tend to point scientists in directions that have proved useless in other instances. If identified in advance, some of these common causes of errors may be avoided. This seems all to the good.

Seeman had another rationale for examining MIEs: to determine whether a "multiplicity" of errors enhanced the visibility of the questions being addressed and then stimulated scientists to try to resolve them. Did MIEs in organic chemistry lead to greater collective focus on the problem at hand? Did anything like a "stampede" develop when it became evident that an error that mattered had been published and could be corrected? And, if not to a stampede of scientists seeking a correct solution, then at least to a tighter focus on a problem that ultimately might produce a satisfactory solution? Such responses are evident in the detailed historical accounts Seeman has assembled. But they might well benefit in addition from information drawing on citation and co-citation studies of the answers to questions such as how long after publication are MIEs and singleton errors first cited? Do more authors pick up the problems MIEs raise than singleton errors? How quickly are errors exposed in MIEs resolved as compared with singleton errors? Granted that assembling an inventory of MIEs and singleton errors of sufficient comparability (for example in difficulty and the effort needed to resolve them) will not be easy. But if Seeman is correct and errors are frequent, that should make the task of assembling comparable samples easier. Citation analysis of such an inventory of cases would complement historical and contemporary evidence on the mobilizing effects of MIEs and shed light on how often and under what conditions contributions that are defined as influential (that is, have been frequently cited) arise from the correction of errors. This last is not as speculative as it may seem since Seeman's study shows that some MIEs and the collective focusing effects they have had in organic chemistry have resulted in multiple independent discoveries of major significance. (As Seeman notes, such MIEs have even led to Nobel prizes). Seeman's analysis of MIEs promises to illuminate the ways in which errors can have their own salutary outcomes and additional citation analysis would bolster the case he could make.

SOCIAL NETWORK ANALYSIS AND CITATION ANALYSIS

Social Network Analysis (SNA hereafter) treats the structure of relationships or linkages existing between all sorts of social entities: persons, groups, organizations, nation states, even scholarly and scientific publications. SNA now has practitioners (and advocates) in all or nearly all of the social sciences[81]. It

[79] Multiple independent discoveries in science refer to that class of discoveries that are consequentially the same but have been produced independently by different scientists, often at approximately the same time but not always. Merton claims these are far more frequent than many assume and indeed that all discoveries are or could be multiples. See Merton's papers focused specifically on Multiples or Multiple Independent Discoveries, in Merton (1973: 343, 371–382, and 439–459).

[80] Merton (1961a) reprinted *The Sociology of Science*, Chicago, IL: University of Chicago Press, 1973, 343–70. Elinor Barber assisted Merton in a "methodical" study of multiple discoveries, for example how many were doublets, triplets, quadruplets quintuplets and even sextuplets. See 364–365.

[81] Freeman (2004). More precisely, as Freeman observes, "The relationships that social network analysts study are usually those that link individual human beings. But important social relationships may link social individuals that are not human, like ants or bees or deer or giraffes or apes. Or they may link actors that are not individuals at all. Network analysts often examine links among groups or organizations—even among nation-states or international alliances. The social network approach is grounded in the intuitive notion that the patterning of social ties in which actors are embedded has important consequences for those actors.

uses mathematical and statistical tools to study social processes such as the diffusion of ideas, the dispersion of popular music, art, and other consumables, the spread of diseases, the expansion of markets, and the effects of social network location on recruitment to political movements[82]. It is similar to network studies scientometricians have done but for their distinctive research objectives.

It requires no great imagination to guess that applying the sophisticated mathematical techniques of network analysis to citation data might be fruitful for studying social aspects of science. Indeed, at least one recent study demonstrates the value of doing so. Taking up the problem of consensus formation once again—but this time examining it from the perspective of the structures of citation networks—two network analysts, Uri Schwed and Peter Bearman, compared the temporal structures of citation networks in published research in medical science, specifically those on findings that were deemed contested or matters of dispute and those judged uncontested, that is, whose validity is agreed on. The research literatures whose citation networks they studied dealt with the carcinogenicity of smoking (now an "uncontested effect," or a matter of consensus) and with the carcinogenicity of drinking coffee (a "contested effect"). They found that internal divisions initially present in citation networks of research in the smoking case were reduced, a phenomenon Schwed and Bearman interpret as consensus formation—but in the coffee drinking case, internal divisions in citation networks remained over time, suggesting to the authors, the persistence of dissensus or lack of agreement in this research domain. Similar divisions in citation networks appeared, they report, in research on the carcinogenicity of cell phone use and the relationship of vaccine use on the development of autism, both areas in which basic findings remain contested (Schwed and Bearman, 2010). That different trajectories in the structure of citation networks might reveal variations in the extent of consensus in specific areas of scientific work should stimulate further studies of the sociology of science. Consensus formation has remained fundamental to thinking about science in the philosophy and the sociology of science for some time, as I observed earlier. The structure of networks of citations in science and other uses of network analysis might stimulate a return in the sociology of science to the study of consensus specifically and of cognitive structures, more generally. This kind of research is an example of the potentials in my title or more precisely, an example of a potential that has been realized—in one instance. Whatever is, is possible.

CITATION ANALYSIS AND THE SOCIOLOGY OF SCHOLARSHIP

Last, I note that citation analysis might contribute to the embryonic study of the Sociology of Scholarship. I hesitate to suggest that empirical research on the pursuit of scholarship in the Humanities and the Sciences might prove useful. So much heat has been generated over the last half century or more about the character of research in the Humanities and the Sciences and about their social and cultural importance that avoiding such matters might well be a reasonable course to take. However, issues of the relative value of these pursuits are not at all what I have in mind.

Nor do I have in mind anything like Gene Garfield's construction of a citation index for the Old Testament described back in 1955 (Garfield, 1955a). Instead, I wonder about the research potentials of the now four decades old *Arts and Humanities Citation Index (A&HCI)*. Might it be possible to learn something about the cognitive and social structures of the humanistic disciplines along the same lines as the *Science Citation Index* and the *Social Sciences Citation Index* have been used to study those fields? Probably not, or at least only with great caution. Based on existing evidence, Henk F. Moed has concluded that the coverage of the humanities literature in the A&HCI runs between poor and moderate. The principal shortcomings in the Index result from the importance in the humanities of books and conference proceedings and the fact that language differences and national barriers play a greater role in coverage than they do in the humanities than in the sciences (Moed, 2005). Working with flawed data is risky at best.

However, it may wise to step back and first determine whether the coverage of any of the humanistic disciplines is good enough to justify undertaking research. If so, then a preliminary foray may be in order to determine the existence of phenomena like those already the subject of research in the sciences. If the data in several humanistic disciplines are usable, the comparative research on citation patterns within the humanities could be done. The highly disparate character of disciplines in the humanities might itself make such comparisons worthy of investigation.

There are interesting hints in Price's now ancient data showing that the humanities, or as he called them "the nonsciences," differed in one important respect from the sciences. For example, as a group, they had a lower rate of "metabolism," by which Price meant that the journal literature in the humanities contained far fewer citations to recent literature than did the sciences, this as measured by Price's "immediacy index." These same data showed that a tendency to cite recent literature also differed among disciplines in the humanities, a matter Price did not pursue (Price, 1970). His data were collected some time before the A&HCI became available; they were the result of hand counts, were limited in their coverage of journals and the years they included and were crude to say the least. As Cole, Cole and Dietrich noted, Price did not take into account the fact that scholars in the humanities often study the very sources they also cite, thereby conflating citations to their research materials

Network analysts, then, seek to uncover various kinds of patterns and they try to determine the conditions under which those patterns arise and to discover their consequences," p. 2. "Modern social network analysis" is characterized by the following attributes "and together they define the field:

1. Social network analysis is motivated by a structural intuition based on ties linking social actors,
2. It is grounded in systematic empirical data,
3. It draws heavily on graphic imagery, and
4. It relies on the use of mathematical and/or computational models," p. 3.

[82] Podolny et al. (1996), Parallels to scientists' considerations of problem choice spring immediately to mind.

with to those whose influence they also recognize in citations. Consider one prosaic example, Milton scholars writing about *Paradise Lost* will cite it as having been published in 1664, and with that one reference greatly increase the average age of citations in their publications–not an indicator of the slow metabolism of research in English literature but because of the age of the materials they study (Cole et al., 1978, especially 225–226)[83]. To say that such features of humanistic inquiry need to be taken into account is evident but not very interesting if research on the age of citations is to be considered. One cannot but wonder whether the skewness of distributions of productivity and citations to individuals, journals, institutions and nations (for starters) is characteristic of the humanities in general or in particular disciplines, whether there are positive correlations between scholarly influence as assessed by citations and prestige or peer recognition humanists receive in the form of awards, appointments or other honors. Similarly, it is tempting to consider whether something like consensus exists in the humanities for example on the fruitfulness of new interpretations of evidence, both central to disciplines such as history, literature and art history. If it turns out that some humanistic disciplines share attributes of this kind with the sciences, their doing so raises questions about whether these attributes are the product of the culture of science, the nature of scientific knowledge or whether the academy and its operations have produced similarities not recognized heretofore.

Putting aside comparison with the sciences, I think more can be learned about citation practices of scholars in various disciplines in the humanities, as is demonstrated in the as yet unpublished and illuminating (to me) review of bibliometric or scientometrics research on the humanities by Franssen and Wouters (2017).

Judging from Gene's own writings, he expected citation data and the variety of techniques that have been developed for their analysis would be useful in research on the sociology of science. He could not have known exactly how this would be so but his expectations have proved correct. Some might think that his main contributions were the creation of citation indexes along with the analytic techniques he and his colleagues devised for their use. But this is not the case. Such achievements are significant but Gene had far larger ideas–about the nature of science, about the significance of the scientific record and about the transmission of ideas. His inventions and the instruments he developed have taken researchers into new territories, influenced how they thought about their work and how they actually did it. Gene was a student ***of*** intellectual influence in science who exercised intellectual influence ***on*** science[84]. Not quite a case of reflexivity but almost.

[83]Cole et al. (1978) report that when citations in two journals in Language and Literature are separated into those to materials on which scholars did their research (or "data citations," for example to John Milton) from other or "influence" citations, the average age of citation in the humanities literature dropped considerably but did not reach the levels usually found in the sciences.

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The author confirms being the sole contributor of this work and approved it for publication.

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[84]This description of Gene's double contribution is, I think, a riff on Joshua Lederberg's description of Gene as a student of the use of citations and a subject of many citations. Whatever the wording, I subscribe to Josh's formulation as well as my own.

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