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Normative shaping of scientific practice: The magic of Merton

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This slender piece does not conform to any established academic writing genre. It is neither an *in memoriam* nor properly speaking a reminiscence, for I never met Robert K. Merton (RKM). In any event, the Great Man's passing earlier this year, aged 92, has already generated a wave of heartfelt obituaries and accolades by those manifestly qualified to write such. Fulsome testimonials have appeared in the pages of the *New York Times* (February 25, 2003) and many other newspapers, as well as in the professional literature of sociology and related fields: see, for example, the March 2003 issue of *Footnotes* (31[3], pp. 1, 8-10), the newsletter of the American Sociological Association, of which, by the way, Merton had been president in 1957. Nor does my contribution to this special issue of *Scientometrics* aspire to be a conventional, scholarly paper, offering, say, a systematic critique of some aspect of Merton's structural-functionalist sociology of science, or an assessment of his many contributions to sociology in general. It is, rather, a collage – a word he, in fact, used in the title of what *may* have been his last, or at least one of his last, original papers, "On the Garfield Input to the Sociology of Science; A Retrospective Collage." [The originally intended topic, sidelined because of recurrent ill-health, was "The Partial Citation Phenomenon and the Matthew Effect: The Thomas Theorem as a Case in Point."] Merton's chapter appeared in a *Festschrift* honoring Eugene Garfield, edited by Helen Barsky Atkins and myself (MERTON, 2000). Lest it be thought that I am trying to misrepresent matters, I duly note that a compilation of Merton's writings on serendipity was published recently by in the near future by the Princeton University Press (MERTON & BARBER, 2004).

When I first contacted Robert Merton in March 1999 to ask if he would be willing to contribute to the Garfield *Festschrift*, I received by way of reply, as presumably many others had over the years, a copy of his 1982 Self-Emancipation Proclamation (SEP) which mandated the "declination of any and all invitations to contribute unwritten papers even in the limiting case when the editor of a *Festschrift* in honor of my oldest and closest friend invites me to contribute a piece to that altogether deserved honorific

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volume …" The SEP, to my relief, was immediately rescinded once the reason for my request was understood, and so began a year-long electronic exchange surrounding, but not limited to, the gestation of his meticulously crafted chapter in honor of his longstanding, entrepreneurial friend, Eugene Garfield, founder of the Institute for Scientific Information (ISI), whom he once described as having "a deep intuitive sense of the social, cultural, and cognitive structures latent in the practice of science" (MERTON, 1979, p. vii).

But I am getting ahead of myself. My very first encounter with Merton was in either 1981 or 1982, when I received a request for an off-print of an article I had published in, as I recall, the *Journal of Documentation*. As a doctoral student in the United Kingdom, I was tickled pink to think that something I had written had been noticed by the doyen of contemporary sociology on the other side of the Atlantic, someone, moreover, whose writings were central to my formative scholastic endeavors. In due course, I spun off a small book (an essay between shiny covers would be a more apt description) from my doctoral research into the norms of citation behavior. "The Citation Process" (CRONIN, 1984) is perhaps best described as a critical review of the literature that contrasted established normative accounts of science with emerging, relativistic analyses. To be sure, it shows its age and a certain lack of maturity, but it has been fairly widely cited over the years, by, among others, RKM. In "The Citation Process," I cite and quote liberally from six of Merton's publications, drawing on his normative framework (disinterestedness, universalism, etc.) to explain the consistency of citation practices within the communication system of science. Even if the rules of citation are not codified, scientists are presumed to have a tacit understanding of what is expected of them, just as seems to be the case with other forms of scholarly acknowledgment. In a subsequent book, "The Scholar's Courtesy" (CRONIN, 1994, p. 107), I show that "many scholars subscribe to the idea of a governing etiquette" and exhibit "a highly socialized approach to acknowledgment." Merton would, I imagine, put it somewhat differently, and certainly more elegantly. In "The Normative Structure of Science" (MERTON, 1973, p. 276) he asserts that there exists "a distinctive pattern of institutional control of a wide range of motives which characterizes the behavior of scientists" and goes on to say that "once the institution of science enjoins disinterested activity, it is to the interest of scientists to conform on pain of sanctions and, insofar as the norm has been internalized, on pain of psychological conflict." Disinterested activity is routinely exhibited in the citation behaviors of publishing scientists: there are rules (bestow credit where credit is due) and there are sanctions (for plagiary).

Microsociologists may decry the lack of attention to, or interest in, individual practice, materiality, grounded observation, and situated action, and also bristle at the way science is accorded "a privileged position as a rational, neutral knowledge domain" (VAN HOUSE, 2004, p. 7) in sweeping, functionalist interpretations of science, but for some of us Merton continues to offer a persuasive, if unfashionable, framework that

melds institutional with individual motivations. In his *Festschrift* contribution (MERTON, 2000, p. 439) he puts citation crisply in context: "It will have been noticed that the instrumental and symbolic signaling functions of citations are not simply a matter of individual motivation but depend upon the historically evolving character of science as a social institution. The very form of the scientific article as it has evolved over the last three centuries normatively requires authors to acknowledge on whose shoulders they stand…" Many others have no difficulty grasping the power of norms to shape and regulate collective practice. As FRANCK (1999, p. 53) explains: "Publication puts intellectual property at the disposal of the general public under the sole condition that its processing into the intellectual property of the user is credited by citation." Who among us, reader or author, is not at once aware of, and routinely directed by, that commandment?

Citations are much more than baubles dangling from the tails of scholarly texts; they testify to authors' common understanding of the bases on which recognition, rights, and rewards are dispensed and managed in contemporary science. Which is not to say that the citation process is devoid of subjectivity. The quiddites of individual authors' citation behaviors are just one of the factors contributing to the constitutional complexity (LEYDESDORFF, 1998) of modern citation. Of course, there are unknown and particularistic reasons why an author chooses to cite A and not B in a given situation, and, yes, there are cases where personal bias, collegial or ideological ties, and other extra-scientific factors affect the choice of citation; but, pragmatically, it is hard to argue with WHITE (1990, p. 91) who observes with telling parsimony that "[w]hen one sees that scores, hundreds, and even thousands of citations have accrued to a work, an author, a set of authors, it is difficult to believe that all of them are suspect. Why not believe that there *is* a norm in citing – a straightforward acknowledgment of related documents – and that the great majority of citations conform to it?" Hear! Hear! Why not simply accept, if I may paraphrase RYLE (1949), that there is a normative ghost in the scientific machine? That said, I am not unsympathetic to the interpretivist standpoint; in fact, in a recent study we showed how thick description could be used to expose some of the underlying socio-cognitive relationships that help explain citation practices at the micro level (CRONIN & SHAW, 2002). But, equally, I am unimpressed by the nihilistic consequences of extreme relativism. A pointillist approach is all very well, but there comes a point when the dots need to be connected, and the big picture revealed.

Fine-grained citational exegesis is, of course, is a labor-intensive activity, and well nigh impossible unless one has some considerable familiarity with the actors, their *oeuvres* and operating *milieux*, as well as the epistemic communities of which they are part, but the findings of such excavations do not necessarily vitiate the aggregationist approach, as the results of BALDI's research (1998) rather convincingly suggest. As it happens, I am indebted to Merton for drawing my attention to Baldi's seminal study.

In March 2000, he wrote asking if I or any of the contributors to the *Festschrift* had referenced "Normative Versus Social Constructivist Processes in the Allocation of Citations." We had not, as it happened, but I quickly inserted a reference in the volume's introductory section. This is an important and methodologically rigorous study. Baldi's aim (p. 835) was to test (a) the normative argument that "citations reflect the worth and content of citations (i.e., "what one says")", and (b) the social constructivist argument that "citations are allocated on the basis of an author's position within the stratification structure of science (i.e., "who one is")." He used a battery of variables (e.g., number of authors in citing article, cited article size, social ties between authors, gender of cited author, cited author institutional prestige) to determine the factors that influenced citation behavior. His conclusions (p. 843) are unambiguous: "Authors are most likely to cite articles that are relevant to their work in terms of subject, recency of knowledge, theoretical orientation, and seem to have little concern with the characteristics of the authors who write them. This finding suggests that, at least in the research area examined [astrophysics], one's position in the stratification structure of science is likely to be the result of the worth and usefulness of one's scientific contributions rather than the reverse, as social constructivists would have us believe." Merton was cautiously impressed by Baldi's study; "A considerable step forward in design and execution. And untainted by prior commitments to alternative paradigmatic ideas … Far too early for qualifying as even an empirical generalization."

Returning now to the "Citation Process," I also summarized the views of MITROFF (for the record, five of his publications are referenced in my bibliography), who has argued that Mertonian accounts of how science works do not give sufficient attention to "normative ambivalence" (e.g., MITROFF, 1977). For every norm (e.g., disinterestedness) there is a counter norm: self-seeking, ambition and chicanery are, as we know only too well, inescapable facts of scientific life, whether at the margins or worse. Science has its subjective side, and Mitroff challenged the empirical, regulative, and epistemic status of the popularly accepted positive norms, in the process expanding Merton's original quartet (and subsequent sextet) of norms to eleven. To oversimplify, the normative camp posits a world of rational action governed by a universalistic code of practice, while the interpretivist camp prefers to foreground what MITROFF (1977, p. 580) has called "the deep personal character of science."

Fast forward to January 2000 and an email from RKM: "I've re-read – actually, scanned – the ever-informative CITATION PROCESS and, as I had half remembered, did come upon your especially relevant chapters on "science as a social system" and "the need for a theory of citing." I was incidentally struck by your extensive treatment of Mitroff and HIS notion of "normative ambivalence." (Hardly a self-exemplifying priority claim on my part since Mitroff goes to some pains in his SUBJECTIVE SIDE OF SCIENCE to declare that I had long since introduced the concept of "sociological ambivalence" and of "counter norms" as inherent in social institutions.)" Merton

proceeds to take issue with Mitroff, specifically his "chutzpah in expropriating and misusing this central idea as well as that of "the storybook image of science" in broad daylight." According to an evidently irritated Merton, Mitroff "proceeds to ignore all this as he declares he has found counter-norms in the institution of science. He reiteratively mis-represents the 1942 paper on the social norms of science (which he repeatedly misdates as '49), and then caricatures or appropriates central ideas in the 1957 paper, "Priorities in scientific discovery."" Feisty stuff from a near nonagenerian.

But this late life capacity for robust argumentation continues to be leavened with generous dollops of wit, unfailing erudition and easy urbanity. In the course of a very few emails, RKM raced from "Dear Dean Cronin" through "Dear Blaise" to "My very dear Blaise." What took him beyond mere "tutoiment" (a practice that one could not bring oneself to reciprocate) was my invoking of the words "Insanabile scribendi cacoethes," a reference to something the manikin and the Great Man (Merlin, by the way, was Merton's stage name as a teenage magician) had in common, namely, the incurable writing disease. These three Latin words (the third of which I misspelled) signaled to RKM that I was a reader of his "prodigal brainchild" (a.k.a. OTSOG, or for those who prefer bibliographic exactitude, see MERTON (1965)) and one was thus embraced virtually, orthographic slovenliness notwithstanding. The aforementioned itch to publish clearly persisted, for even as he succumbed to illness, Merton doggedly declined to abandon his draft chapter, in January delivering the following touchingly wry request: "Could you let me know the outermost date for a manuscript before a deadline turns from metaphor to literalism?" Would that some of one's sub-stellar colleagues would take a leaf or two out of this grandee's book when it comes to fulfilling their self-inflicted commitments. Would that all of us could match the clarity and luster of his prose. Would that there were more giants of Mertonian proportions upon whose broad shoulders we manikins might stand (see MERTON (1965) for the sedulously constructed history of this aphorism).

Merton's stature as one of the preeminent sociologists of the twentieth century is undisputed, but less well grasped, perhaps, is the extent to which his ideas have percolated to the amorphous world of information science. In their co-citation analysis of information science for the period 1972–1995, WHITE & MCCAIN (1998, p. 339) generated mean co-citation counts for 120 authors in information science: Merton ranked fifth on this list, a clear indication of his persistent presence. In fairness, Merton was not the only sociologist or historian of science appearing on the White/McCain list (Thomas Kuhn also featured), but, by the same token, several of the other most frequently co-cited individuals were former students of RKM (e.g., Jonathan Cole), which merely underscores the depth of the Great Man's influence both inside and outside his native discipline.

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