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Chapter and Verse: From Variety to Uniformity in Scholarly Source Citation Practice

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Being provided with the source of a scholarly product (book, data, document, etc.) used in a published work is an important aspect of communication in the scholarly world because it allows the author's peers to verify the results presented therein. Until recently, inconsistency, idiosyncratic practices, and a lack of explicit standards have been the dominant characteristics of the history of supplying bibliographic information for a publication mentioned. This essay uses Robert K. Merton's (1910—2003) well-known paper "Science and the Social Order" (1938) as a case study to illustrate how "deficient" bibliographic referencing hampers the research process. The long-standing absence of norms regulating source referencing information was gradually reversed during the twentieth century, when sets of formalized and standardized rules were put into place and codified as the new millennium approached—especially with the advent of digital technology. This article examines the variegated ways in which bibliographic material was provided (or not) and suggests how a convergence of factors put an end to this "disorderly" bibliographic sourcing environment.

Keywords: bibliographic documentation; source referencing norms; scholarly communication; bibliographic practices; bibliographic standardization

Introduction: Source Referencing Diversity in Action

> You say eether and I say eyether,
> You say neether and I say nyther;
> Eether, eyether, neether, nyther,
> Let's call the whole thing off!
>
> —"Let's Call the Whole Thing Off," George and Ira Gershwin, ([1937] 1960, 81)

In today's scholarly world, providing sources for works used in one's intellectual production is a well-established and strictly regulated institutionalized practice. The expectation of those who review the research and, ultimately, of the reader is that these resources be fully documented so that they can be readily consulted if one wishes to do so. Sometimes, although the onus is on the writer, journals will check the author's references for accuracy and completeness before publishing a research paper. Depending on the publication, these sources are to be found in footnotes or endnotes or in the references (list or bibliography). These references are essentially of two kinds: data citation, the facts upon which researchers base their argumentation (i.e., evidence); and peer citation, previous works authors use to elaborate their conceptual framework (theory, methodology, analysis). In the latter case, it is, of course, a form of recognition and a facet of the scholarly world's reward system. It is an acknowledgement that one cannot produce a learned piece of work without the support, however implicit, of the community to which one belongs.[1] It is also a form of social control: It shields the author from accusations of plagiarism. In the former case, the source

[1] For the many functions of quoting see, for example, Ruth Finnegan's *Why Do We Quote? The Culture and History of Quotation* (2011).

can be of a quantitative nature, such as a sample survey (e.g., opinion poll) or any other instrument that yields numeric outputs, or it can be qualitative, a document such as a Supreme Court decision. It too has an oversight function: Its purpose is to prevent authors from making fraudulent claims. In modern scholarship, then, citations—assuming they are not used for any "decorative purpose" (Barzun and Graff 1957, 295)—are provided because of their utilitarian quality.

Peer citation—authors quoting authors—has a very long tradition. For example, Moses I. Finley (1912–1986), historian of Ancient Greece, tells us that Hero of Alexandria (ca. first century CE) quotes Strato (335–265 BCE) of Lampsacus in his treatise on pneumatics (Finley [1963] 1987, 125). Much has been written about why authors quote (Gilbert 1977; Finnegan 2011; Erikson and Erlandson 2014). However, there is a scarcity of research on the history of *how* authors cite other authors—the informational formatting of source references.[2] Scholarly citation practices have evolved over the centuries, and explicit citation and referencing norms have not always existed (aside, perhaps, for the Bible). When one delves into the *Essays* of Montaigne (1533–92), a book (three, in fact) replete with quotations if there ever was one, the reader will be hard-pressed to find any sources. Montaigne states, for example: "Cicero says that to philosophize is nothing else but to prepare for death" (1965, 56). But where does the Roman philosopher (106–43 BCE) say so? The inquisitive reader (who could also be an editor or translator of Montaigne's) will have to go on a search for the source of that statement.[3]

Yet the practice of providing a source was not totally unknown in those days. In the Geneva Bible (1560),[4] for instance, when the Old Testament is mentioned at Matthew 21:4-5 ("what was spoken by the prophet . . . "), it provides, in the margin, three exact references: Isaiah 62:11, Zacariah 9:9, and John 12:15 (Finnegan 2011, 85).[5] This is internal referencing (the citations send the reader to other parts of the book), but external referencing practices also existed in the sixteenth century. Ben Jonson (1572–1637), historian Robert J. Connors tells us, provided "specific editions, tomes, and pages" (1998, 11) when quoting the work of others. But this "citation note" (Connors 1998, 21) was an individual choice—and not always appreciated. Thus, in early eighteen-century France, one author (Jean-Baptiste Thiers, 1636–1703) chastised another (Jacques Boileau, 1635–1716) for doing just that: "What good," Thiers complained, "do these over meticulous and so affected citations accomplish other than to fatten his *History*, which would have been a considerably slimmer tome without all these minutiae?" (quoted in Grafton 1997, 220n59, my translation; see Grafton 1997, 142–47 for similar examples). In contrast, the footnotes used by David Hume (1711–76) contain "little publication information" (Connors 1998, 35).[6] But at about the same time, Edward Gibbon (1737–94), author of *The History of the Decline and Fall of the Roman Empire*, adopts the opposite practice: His references are "complete, including author, title, volume, place of publication, and section or page" (Connors 1998, 39).[7] A few decades later, the writings of the scientist Pierre Simon de Laplace (1749–1827) rarely include instances of even partial documentation (e.g., "Lagrange, in Volume VIII of the *Journal of the Polytechnic School*, has proven . . . "), let alone complete ones (Laplace [1818] 1898, 421, my translation).[8] Forty years after Laplace's death, Karl Marx (1818–83) in volume one of *Das Kapital* gives his readers documentation for the works he quotes that is nearly complete by current standards. For instance, in chapter thirty-one ("Genesis of the Industrial Capitalist") he provides the title of the work quoted, its author, the year when and the city where

[2] Anthony Grafton (1997) and Robert J. Connors (1998) provide much valuable information on citational practices, but it is incidental to their main topics, which are, for Grafton, the origin of the footnote and its uses (especially among historians), and, for Connors, the historical development of the citation system in the scholarly West. Some have examined jointly cited documents as a method to identify a shared "paradigm" among researchers (Small 1980), while others have analyzed the errors in reference citations, their likely causes, and "why they are not discovered and corrected before publication" (Sweetland 1989, 297; see also Agarwal et al. 2023).

[3] It is in *Tusculan Disputations*, Book I: On the Contempt of Death, XXX: "For the whole life of a philosopher, as the same wise man says, is a meditation on death" (Cicero (44 BCE) 1960, 87). The "same wise man" is Socrates, and the work from which this originates is Plato's *Phaedo*, a dialog about death, *inter alia*, and which Cicero does not cite. In it, the "gadfly" (Socrates) declares: "Those who rightly love wisdom [philosophers] are practising dying, and death to them is the least terrible thing in the world." He adds: "If you see a man fretting because he is to die, he was not really a philosopher" (Plato 1999, 561). For "gadfly," see "The Apology*"* (Plato 1999, 518).

[4] The Geneva Bible was one of the important translations of the Bible published in the sixteenth century. It was brought by the pilgrims to America and was "the favoured Bible of the Plymouth and Virginia settlers" (Finnegan 2011, 84).

[5] I owe this example to Finnegan. Her purpose is to discuss the origin of the use of quotation marks, but in doing so she provides the reproduction of a page in the Geneva Bible, which contains the references mentioned.

[6] Apparently, Hume started identifying his sources only after being reprimanded for failing to do so in his *History of England* (Grafton 1997, 103).

[7] Note that Connors, whose paper was published in 1998, says "complete" to describe Gibbon's citational behavior, but something is missing by today's requirements: the publisher's name.

[8] I was alerted to Laplace's citation practices by Stephen M. Stigler (1978). Laplace's referencing style is not much different from Montaigne's.

it was published, but never the name of the publisher (Marx [1867] 1912, 822–34).[9] There are many other such examples of inconsistent referencing in the history of scholarly publishing (see Grafton 1997, 29–31).

Nowadays, of course, journals, publishers, and professional associations (e.g., the American Psychological Association [APA], the Modern Language Association [MLA]) provide authors with numerous and detailed guidelines when it comes to referencing the materials used in their work. But these are more than indications or suggestions; they have now become institutionalized norms that are enforced and with which authors must comply if they want to be published. In addition, there is a cottage industry of how-to publications for writers (e.g., Shon 2019), which can be consulted by authors who need both to be socialized into this citational environment and to learn the social customs of the scholarly world. They are a sort of "manners" manuals that describe the proper referencing etiquette to follow. Individual idiosyncrasies, characteristic of the past, have been eliminated: Formalization and standardization reign.

But an established body of rules governing citation behavior is a recent creation. Just sixty years ago, a sociologist of science could write: "A preliminary search of the literature has failed to turn up very many explicit normative guides for citation practices, whether historical or contemporary" (Kaplan 1965, 179). The publishing environment that enabled the idiosyncratic referencing behavior to flourish persisted well into the twentieth century. This essay makes this plain in the pages that follow by revisiting a foundational paper in the sociology of science, Robert K. Merton's "Science and the Social Order" (1938), which is used as a case study.

The present study looks at a very specific aspect of an author's citation practice—that of providing information, accurate or not, for references used in one's work (paper, presentation, book, etc.). Its central claim is that the referencing environment has changed from one in which authors were left to adopt whatever approach they fancied (and in which editors and publishers seem to have had very little input) into one which constrains the scholar's sphere of discretionary action. Over the last hundred years or so, there has been a gradual shift from an unregulated source-citation context to one in which standards have been made explicit and are enforced. Although hardly scientific let alone scholarly, the English have a colloquialism that describes well the process I wish to explore: "got to get some discipline into the punters." Indeed, comparing the examples of referencing given above and the case study with how things are done today (more about this later) illustrates a turn from the undisciplined to a very controlled publishing environment in which citation practices are prescribed as opposed to being left up to the authors. My ambition here is simply to explore one feature of the wider system of scholarly communication that is considered by both practitioners and observers to be critical to the development of research. This essay is neither about the completeness of bibliographic references, nor their accuracy, nor their absence. Its purpose is to compare two eras: one we could call, with some slight exaggeration, the "Wild West" of source referencing and the other characterized by standardized and enforced rules—the age of "law and order."[10] The epigraph at the beginning of this section expresses what defined the first period: There was nearly as much diversity in the practice of source citation as there were authors.

A Case in Point: "Science and the Social Order"

I turn now to Merton's "Science and the Social Order" to illustrate both how inconsistent citation practices could be even within the same paper and how they create difficulties and roadblocks for later generations of researchers. It is ironic to use his work to contrast the citation sourcing ways of the past with today's strict norms. In this case study representative of bygone days, we shall see that referencing therein appears, from an early twenty-first-century perspective, somewhat slipshod—generally inconsistent, sometimes absent, occasionally complete but not often, and at times unreliable. The irony lies in the fact that Merton spent a considerable part of his later scholarly energy on tracing the precise source of various works and quotes in writings such as *On the Shoulders of Giants: A Shandean Postscript. The Post-Italianate Edition* (1993).

While working on a project exploring the interaction and interrelation between science and the State, I chose to revisit "Science and the Social Order," Merton's landmark paper on science and the nature of the political system in which it operates, published by the journal *Philosophy of Science* in 1938. This article, and others he wrote in the 1930s and '40s, are considered by many (e.g., Crothers 1987, 126; Barnes and Dolby 1970, 3) as the starting point of what was to become the sociological subfield of science studies. I decided to look up the sources he mentions to gain a better understanding of the concerns prevalent in the scientific community at the time.[11] I came across several referencing anomalies, what *Philosophy of Science* calls "inconsistencies" and "incompleteness" in its current referencing guidelines to authors (2025). First, there

[9] The reader might view this as nitpicking. There is undeniably sufficient source documentation there to find with ease the book of interest. But my goal is not to cast aspersions but to contrast the past with the present. Indeed, nowadays all major publishing guides (i.e., APA, Chicago, MLA, Harvard) recommend that the publisher be provided.

[10] For other standardizing trends in the publishing industry, i.e., "article format," see Wolfgang Kaltenbrunner et al. (2022).

[11] It is not the entire paper that I scrutinize here but two pages of particular interest to me.

was what seemed to be, ostensibly, an erroneous attribution and a non-existent reference. At one point in the article, Merton claims that Sir Josiah Stamp (1880–1941), among others, "proposed a moratorium on invention and discovery" (1938, 330). As a reference, he mentions a supposed "address" by Stamp to the British Association for the Advancement of Science (BAAS), given in Aberdeen, Scotland, on September 6, 1934 (Merton 1938, 330–31n24).[12]

Thus, I consulted the 1934 *Report* of the annual meeting, which indeed took place in Aberdeen from September 5 to September 12. I could only find Stamp's report as the General Treasurer of the Association (1934b, xxx–xxxi) and his participation, on Thursday, September 6 (the very same date given by Merton), in a discussion on "The need for a technique of economic change" (Stamp 1934a, 341–42), part of the transactions of Section F (Economic Science and Statistics, subsection F*, Department of Industrial Co-Operation) of the Association, but nothing as formal as an "address."[13] Merton provides no page number, let alone any documentation for Stamp's "address," which, I assumed (wrongly as it turns out), had it occurred, would have been included in the association's annual report. Was this a non-existent reference?

In search of this elusive "address," I consulted a book written by Stamp: *The Science of Social Adjustment*. In it he states that he "never suggested a 'moratorium' for scientific discovery" (1937, 59). Were words being put in his mouth? And was Merton perpetuating, unwittingly, this deception? Stamp identifies the origin of this attribution (false, according to him) to three sources. The first (Stamp 1937, 59n1) is a book (including page number 30) by the biologist Lancelot Hogben (1895–1975): *The Retreat from Reason*.[14] Indeed, therein Hogben writes: "When Sir Josiah Stamp appeals for a moratorium on scientific inventions . . ." (1937, 28). He does not provide a source for his claim. Also, Stamp is evidently quoting Hogben when he mentions the word "moratorium" inside inverted commas—since it is not used by the other two sources. The second reference (Stamp 1937, 59n1) is another book (including page number 31), one written by the demographer and statistician Enid Charles (1894–1972), *The Twilight of Parenthood*, in which she states: "Faced with this [current economic] crisis . . . Sir Josiah Stamp blames the inventions of the scientist" (1936, 31).[15] Charles, likewise, does not provide a source for her assertion. A third source of Stamp's alleged advocacy of a "moratorium" are "abbreviated press reports," which, he says, without providing sources, "have conveyed that impression" (Stamp 1937, 59). This leads me to speculate that the reason neither Hogben nor Charles provided a source for Stamp's proposed "moratorium" is because his views had been so widely reported in the press that a reference was thought to be unnecessary (more on that shortly).

Since Charles dates the introduction to her book in February of 1934 (1936, vi), it stands to reason that Stamp's "address," if indeed there was one, must have taken place earlier than both Merton's reference (the BAAS meeting in Aberdeen) and the date of Charles's introduction. Accordingly, I consulted the BAAS proceedings for the year 1933. That year the Association met in Leicester from September 6 to September 13. And, lo and behold, Stamp did make an "address," part of the meeting's "Evening Discourses," whose title is "Must Science Ruin Economic Progress?" (Stamp 1933, 578–83) and which took place on Friday, September 8. Was Merton's reference off by 363 days and 442 miles? Did he mistake the 1934 report for the 1933 one?

It is unlikely that Merton would have seen the British "abbreviated press reports," but surely he must have seen the report in *The New York Times*. The paper's science writer, Waldemar Kaempffert, was covering the 1933 BAAS's meeting. Despite Stamp's denial, this journalist, like others, interpreted his words to mean that he was "suggesting" a "curb on science" (Kaempffert 1933). Nowhere in Kaempffert's story does the word "moratorium" appear. Might Merton have seen the reference to Stamp in Enid Charles's book? That is doubtful. More likely he may have read the book published in the United States by the better known Hogben. And he is the one (and only one) who uses the word in question. Of course, this begs the issue. Since Hogben gives no source, we may ask him the same questions we did of Merton. Surely, Hogben would have had easy access to the proceedings of the 1933 BAAS meeting and to the press reports. So, perhaps, based on either or both sources, Hogben might have concluded that Stamp was calling for a "moratorium on scientific inventions." Thus, Merton may have picked up the terminology from Hogben—something, if he did, he does not acknowledge. Undoubtedly, he was perfectly capable of using that term based on his interpretation of Stamp's reported words. Accordingly, the worst that can be said here is that he provided, no doubt inadvertently, an erroneous (and very incomplete) reference. Is this a case of *errare humanum est*?

[12] Merton's exact words are: "Sir Josiah Stamp's suggestion may be found in his address to the British Association for the Advancement of Science, Aberdeen, 6 Sept. 1934" (1938, 331n24).

[13] There is no date associated with his treasurer's report. His remarks to Section F* of the BAAS make no allusion to a "moratorium on invention and discovery."

[14] The book is based on his Conway Hall Lecture given on May 20, 1936, published by Watts & Co. I am quoting the 1937 American edition.

[15] Originally published in 1934 as *The Twilight of Parenthood*, the book was republished in 1936 under a new title: *The Menace of Under-Population*. I am quoting from this edition.

Not at all. Further digging reveals that Stamp did in fact make an "address to the British Association for the Advancement of Science," as Merton stated (1938, 331). Indeed, *The New York Times* reported on September 7, 1934, "Stamps urges curb on inventions" (Kuhn 1934)—déjà vu all over again. Clearly, Stamp's talk did not make it into the proceedings of the 1934 meeting. The *Times* story indicates that Stamp's speech was given during "the meeting of the economics section" (Kuhn 1934, 7) of the BAAS.[16] It was reported also in other American press outlets such as *The Literary Digest* ("More Science or Less?" 1934) and *Time* magazine ("Advancement at Aberdeen" 1934). I can only speculate, yet again, that Merton saw the press reports and came to his conclusion based on them.

Other instances in Merton's paper that nowadays would, most likely, raise eyebrows are several floating quotes with no attribution. The first (the "embarrassing fecundity of technology") occurs near the top of page 331. One might think, since he is the last person mentioned in the same sentence (on the previous page, 330), that Stamp is the one who used the expression. But that is not the case. So, inquisitive readers are left to their own devices. Ironically, the near exact expression can be found in the foreword of a book entitled *The Frustration of Science*, a book Merton mentions in note twenty-four on page 331. The writer talks about "the embarrassing fecundity of modern technology" (Hall et al. [1935] 1968, 8). The book, a collection of essays, was originally published in 1935. I am quoting the 1968 reprint. It contains eight chapters, each written by a different author. The foreword was penned by "Professor Soddy" (Merton 1938, 331n25).[17] Why not provide a page number? He does so on other occasions. For instance, when mentioning personalities, other than Stamp, who had proposed a "moratorium" on scientific research, he lists the French politician Joseph Caillaux (1863–1944) and gives as the source for his assertion a book by John Strachey (1901–63), *The Coming Struggle For Power* (1935), and a page number (Merton 1938, 331n24). And his self-referencing is (nearly) impeccable: title of article, name of publication, volume number (although not the issue number), year, and page(s) (see Merton 1938, 331n24, 332n26).

Merton describes Soddy as one of the leaders of a "movement" among "English scientists" against the "'prostitution of scientific effort to war purposes'" (1938, 331). This last quote is also undocumented. So, again, the inquisitive reader must trace down its origin. It appears in an editorial of the British journal of science *Nature* entitled "War, Science and Citizenship" and dated May 9, 1936 (757).[18] The very same expression was used in a talk entitled "Science and Humane Values" given by Sir Richard Gregory (1864–1952), the journal's long-time editor (1919–39), in February 1940 (Gregory 1940).[19]

Another quotation without a specific source can be found in the same note: "'a new awareness of social responsibility among the rising generation of scientific workers'" (Merton 1938, 331n25). Merton prefaced this unsourced phrase by stating: "Presidential addresses at annual meetings of the British Association for the Advancement of Science, frequent editorials and letters in *Nature* attest to this movement" (1938, 331n25). Indeed, the quote is lifted from an editorial in that journal dated May 30, 1936 ("Science and Citizenship" 1936, 883). Why not say so? This is all the more remarkable since a few lines below, in the same note, he gives a precise reference ("*Nature*, 137, 1936, 829") for a letter, which he does not quote, "signed by twenty-two scientists of Cambridge University urging a program for dissociating science from warfare" (Merton 1938, 331n25). Merton provides most of the bibliographic information (see above) but omits the issue, the date, and the title (Bamford et al. 1936).

Discussion

None of the instances identified from this case study amounts to unethical research conduct. What they do illustrate is the inconsistent way source citation was practiced. All this occurs in one paper published in the first half of the twentieth century. Merton's citation behavior is a continuation of the unsystematic way in which bibliographic information was provided (or not) for many centuries, as described earlier. It also shows that both publishers and editors appear to have been indifferent to what we would consider today conduct unbecoming of a scholar.

Given today's prevailing scholarly publishing rules, it is surprising to see that the instances presented went undocumented. Indeed, by modern standards, the editorial team of the journal *Philosophy of Science*,

[16] It also quotes Stamp verbatim. But his words are nowhere to be found in his published remarks to the F* subsection of the BAAS (Stamp 1934a, 341–42).

[17] Frederick Soddy (1877–1956) was the (reserved) 1921 Nobel prize winner in chemistry (awarded in 1922) (Friedman 2001, 282). When the Nobel committee is unable to select an eligible candidate in a given year, the prize can be reserved until the following year. Merton indicates mistakenly that Soddy was the editor of the book ("ed. by F. Soddy") (1938, 331n24). There is no evidence to that effect.

[18] Also used a month later in another editorial in *Nature*, "The Protection of Scientific Freedom" (1936, 963).

[19] Gregory's talk, with others, was published later that year in a book entitled *The Deeper Causes of the War and Its Issues*, edited by Sydney Ernest Hooper of the British Institute of Philosophy. In it, Gregory states: "The association of science with war, and the prostitution of scientific effort to war purposes, cannot be condemned too strongly" (1940, 133).

which accepted Merton's paper, would be expected to check the quotes and references used by the authors they publish. But this is an anachronistic expectation. In those days, it would seem that the integrity and reliability of a scholar's work was taken for granted—a scholar's work is a scholar's bond (?). It may have been inconceivable, then, to scrutinize the humdrum tasks that an author must perform while writing a paper. Questioning a scholar's argumentation was one thing, and of course, most acceptable, but to look over a scholar's minute chores (such as providing "complete" references) might have been considered beyond the pale. Let us not forget that this was an era that believed in "the virtual absence of fraud in the annals of science" (Merton 1942, 124).[20]

Besides, Merton's 1934 BAAS reference, for one, exudes credibility: It tells us when and where the "address" took place. But the inquisitive reader is given no clue where to access it. Yet this lack of source citation does not seem to have bothered the journal's editors. Clearly this would not be the case today. By current standards, Merton would have received an email from the copy editor asking him to provide complete bibliographic information for Stamp's speech at the 1934 BAAS annual meeting. We now know, at least tentatively, that there does not seem to be a publication reference for it and that Merton, most likely, relied on press reports to describe Stamp's views. A more genuine reference on his part would have been to say that Stamp's "proposal" was conveyed in his talk to the economic section of the BAAS as reported by *The New York Times*. This would have prevented the inquisitive reader from spending an inordinate amount of time tracking down Stamp's 1934 "address." [21]

We should note that the problematic references identified in Merton's classic paper are used as primary data (i.e., evidence to back up a claim). In other words, the items he "fails" to document thoroughly are the raw material that supports his argumentation. They are not quotations that he relies on to build his conceptual framework. The distinction is important. The "neglectfulness" and inconsistencies identified earlier are minor peccadillos that in no way compromise the tenor of Merton's conclusions. Had Merton treated works used to elaborate his conceptual framework the same way he did in the instances mentioned earlier, he *might* have been chastised because the former requires prompt acknowledgement. Failing that will open the author to accusations of plagiarism, unless the words borrowed are enclosed within quotation marks and/or their originator(s) recognized—preferably both. Nowadays, ethics and copyright laws (and other means of enforcement) require authors to identify their sources, particularly when quoting directly from them. Indeed, as the seventeenth edition of *The Chicago Manual of Style* states, "source citations *must always* provide sufficient information either to lead readers directly to the sources consulted or, for materials that may not be readily available, to enable readers to positively identify them, regardless of whether the sources are published or unpublished or in printed or electronic form" (University of Chicago Press 2017, 743, emphasis added).

Yet Merton does not abandon his practice of inconsistent referencing even when it comes to authors he relies on for his analytic approach—although he never fails to acknowledge them. For example, he quotes "W. I. Thomas' sociological theorem" (1938, 331–32) but neglects to provide the publication and page where it can be found.[22] In a later publication he justifies his omission by stating that it "was merely a passing allusion that clearly required no citation" (1995, 408). Really? So, the rule he was applying was: no need to provide a bibliographic reference when a quote is "a passing allusion"? Yet, he cites the theorem in full.[23] Does that qualify as "a passing allusion"? Not according to the *MLA Handbook* (Modern Language Association 2021, 101–2)—not when you quote. Its current guidelines state that "documentation is required for any work

[20] It is difficult for today's researchers to fathom harboring such a creed in a world of unending retractions, data manipulation, plagiarism, and other acts of deviance in science. Are we so much more unethical than our forebears? Merton would have scoffed at such a suggestion. For him it is the structural constraints of institutionalized science that keep researchers on the straight and narrow (or fail to) irrespective of their "personal qualities" (1942, 124).

[21] For an experience very similar to the one just described, which took place in 1859, see Judson B. Gilbert's "Notes on Medical Bibliographic Citation" (1941). In it is reproduced a translated article by Dr. Aristide Verneuil in which he describes the time he "wasted" (six hours in all) to track down correct references in order for him to write a sentence of roughly six lines, which, he surmises, would most likely go unnoticed in his published research. I want to thank the anonymous reviewer for pointing me in the direction of James H. Sweetland's (1989) article, which contains the reference to Gilbert's paper. I noticed that Gilbert's citation of Verneuil's original story, in French, contains a wrong word in the title: "De" instead of "Du" and a misspelled word, "temp" instead of "temps." The *erratum* (?) or *corrigendum* (?) is reproduced in Sweetland's reference list (302)—perpetuation of error.

[22] There has been much controversy about the attribution of the "theorem" to William I. Thomas (1863–1947) alone despite the book (*The Child in America: Behavior Problems and Programs*, 1928) having a co-author: Dorothy Swaine Thomas (1899–1977). This discriminatory acknowledgement has been described as "institutionalized sexism" (Merton 1995, 381 and passim) although, for what it is worth, she did state that her contribution to the book was "only" the statistical analyses and insisted that "the concept of 'defining the situation' was strictly W.I.'s" (Merton 1995, 391). This is not the place to weigh in on the controversy, but I will make two remarks. First, today if a book has two (or more) authors, whatever is written therein is attributed to all authors—unless parts of the book are clearly demarcated by authorship or the work contains a statement describing the division of labor between authors. Second, a recent article (Singh and Lynch 2025, 329) states that D. S. Thomas was W. I. Thomas's spouse. Although that is true, it was not the case when the book was published in 1928.

[23] "If men define situations as real, they are real in their consequences" (Thomas and Thomas 1928, 572).

that you quote from or paraphrase" (2021, 101).[24] Was the "theorem" common knowledge? If so, why quote it? But, again, the editors of the journal do not seem to have been disturbed by this absence of documentation. And Merton repeated his practice of quoting without providing a source on two prior occasions: once with Weber and once with Pareto (1938, 327n18 and 329n22 respectively).

Most readers (past and present) are not interested in looking up Stamp's address or browsing *The Frustration of Science* or finding out who deplored the "prostitution of scientific effort to war purposes." They want to know what Merton says about the relationship between science and society at large, and what his views are about the influence of social factors on the practice of science. But for the small number of "dedicated readers" (Merton 1984, 1092) who are researching the period during which this paper was written and who want to use it as a kind of entry point into the protagonists' frame of mind, Merton does not make it easy for them. In fact, they are led on a wild goose chase of sorts—reminiscent of Dr. Verneuil's (see above). But we can hardly blame him; after all, the paper was published as is, which means that it was satisfactory to those who accepted it. Besides, other contemporary authors (e.g., Hogben and Charles), as we have seen, did the same. Clearly it was not an uncommon practice and, apparently, not one that violated any normative standard.

Nowadays, journal editors, thesis supervisors, and others will most likely ask authors to provide adequate documentation for any quotation, unless it is a well-known phrase that has become part of our collective memory.[25] For instance, when Max Weber (1864–1920) gave his lecture on "Science as a Vocation," he did not see fit to mention a source for (or to translate) the quote "Lasciate ogni speranza" ([1918] 1946, 134; "Forsake all hope," my translation). He did not even bother to say something like "as Dante warns," or "I could say, with Alighieri."[26] He simply assumed his audience, mostly academics, to be multilingual and acquainted with the classics of Western literature.[27] Marx does the same at the end of the *Preface* (1867) to volume one of *Das Kapital*, also quoting, or should I say deliberately altering, a quote from Dante's *Divina Commedia* (Purgatorio, Canto V: 13, Alighieri [1321] 1933, 172): "Segui il tuo corso, e lascia dir le genti" (Marx [1867] 1912, 16; "Follow your path and let people talk," my translation).[28] Less than two pages, out of thirty-one, was all it took Albert Einstein to introduce his theory of special relativity in a paper without any bibliographic references—these being unnecessary, we are told, because they were known to his readers, his peers (Bensman 1988, 461).[29]

Many journals nowadays require references to be tethered to a hyperlinked digital object identifier (DOI), which makes it easy to check the accuracy of the citation. Today, if things were left the way they are in Merton's published paper, it would elicit in all probability some expression of disapproval. The imperative to provide complete and accurate references is not only a way to assess the author's honesty, a mechanism of social control; it is also an act of courtesy, however implicit, done to one's peers (current and future).[30] In another paper of the same period, Merton made explicit what he believed were the norms of the "ethos of science" (1942, 116). One of the hallmarks of scholarly research in general and science in particular is unimpeded communication. One type of barrier is secrecy (Merton 1942, 122).[31] Such practice is generally frowned upon, although exceptions abound. For instance, to take an historical example, within the context of a

[24] It is interesting to note that Jacques Barzun, that master of writing etiquette, uses nearly the same expression in *The Modern Researcher*—"passing phrases" (1957, 318), which, we are told, are exempt of reference. Here I am deliberately quoting the first edition on the assumption that the mores of those days were similar to those at the time Merton published his paper (1938). Indeed, in the 2004 edition of *The Modern Researcher*, the "rule" has vanished (Barzun and Graff 2004, 266).

[25] Such as, for Americans, at least those who are (older) baseball fans, the "déjà vu" tautology encountered earlier.

[26] Not a verbatim quote but a pastiche of Weber (Mills and Gerth 1946, 12).

[27] Here Weber is talking about the chances of a young Jewish academic of obtaining his habilitation (a postdoctoral qualification to become a *Privatdozent*, a lecturer) in the German university system and what he would say to him: "Give up any hope"—quoting Dante's *Inferno* (Canto III: 9, Alighieri [1321] 1933, 12). It was not the first time Weber used the quote. He did so in his inaugural lecture at the University of Freiburg in 1894 (Mills and Gerth 1946, 35).

[28] That is Marx's modification of Virgil addressing Dante and saying, "Vien dietro a me, e lascia dir le genti" ("Follow me and let people talk," my translation).

[29] Bensman manages to write this twenty-eight-page essay on footnotes and the "crimes" committed in their name without a single reference. I suppose he is trying to make a point. But this lack of documentation becomes problematic when he makes statements such as "There is substantial evidence that many writers plagiarize the footnotes (and bibliographies) of others, that is, that they copy another's footnotes without having read the works to which they refer" (Bensman 1988, 456). Is there? The author does not reward the reader with a source of that "evidence." One can understand that it may be a bit delicate to "out" living researchers unless they have already been denounced for their "deviancy." But surely, this is not a novel phenomenon and past authors, who are no longer with us, could serve as the evidence.

[30] As *The Chicago Manual of Style* states, "ethics, copyright laws, and courtesy to readers require authors to identify the sources of direct quotations or paraphrases and of any facts or opinions not generally known or easily checked" (University of Chicago Press 2017, 743).

[31] "Secrecy" falls under (and is the "antithesis" of) the "institutional imperative" of what Merton calls "communism" (1942, 121–24): "The substantive findings of science are a product of social collaboration and are assigned to the community. They constitute a common heritage in which the equity of the individual producer is severely limited" (121). The expression was mostly abandoned due to its reviled political overtones and was replaced by more innocuous terms such as "communality" (e.g., Chubin 1988, 61).

for-profit economy, inventions are kept secret for financial gain, such as, in chemistry, the Haber-Bosch process (Friedman 2001, 104). Secrecy is the absence of communication. The other end of the continuum is open, accurate, and transparent exchange. Authors who practice it in effect provide their peers with a roadmap of their scholarly product, thus allowing the latter to retrace with ease the research steps of the former.[32] The irregularly sourced instances uncovered in Merton's 1938 paper fall somewhere in between these two extremes.

Nevertheless, it is as if Merton, in three of the cases mentioned earlier ("embarrassing fecundity," "prostitution of scientific effort" and "new awareness"), was the conduit for the "social mechanism" known as "obliteration by incorporation" (OBI)—an idea he himself originated and discussed decades later (1995, 407ff.).[33] This occurs when some product of the human intellect, say an invention in science, analysis of variance (ANOVA) for instance, is so deeply embedded in a disciplinary field (here, statistics) and so generally relied upon that practitioners do not think (nor is it expected of them) to reference the original source (book or article) or its author, Ronald A. Fisher (1890–1962) in *Statistical Methods for Research Workers* (1925). In many cases, researchers are totally ignorant of the origin of the work: It is an orphan of sorts because few, if any, know not only its progenitor(s) but even its birthplace, the book or article where it was first published. Or, if you are in the field of linguistics and write "deep structure" (Chomsky 1964, 10) as part of a sentence in a paper, you will be unlikely to provide a reference for it—it is so well known that it is simply taken for granted (Garfield 1977, 398). Katherine W. McCain notes that "in order to be achieved, OBI must result in total incorporation through elimination of the creator's name from the concept" (2014, 134). Thus, it is probable that neither ANOVA nor "deep structure" have been totally OBI'ed because there are still practicing statisticians and linguists that can associate these scientific constructs with their inventors.

The examples extracted from Merton's 1938 paper have all the appearances of having been subjected to OBI, or a variety of that phenomenon, since both the "works" from which the quotes originate and the authors are referentially absent—as if these items were deeply ingrained in the cognitive consciousness of the milieu Merton was describing. They are introduced as primary data and as such would seem to be legitimate candidates, just like any other sources, for complete referencing. The question is: Could these expressions have been "common knowledge" (Garfield 1996, 452)? Is this an instance similar to the examples given above (e.g., "lasciate," "deep structure," etc.), where the audience is thought by authors to be "so thoroughly familiar" with the sources of the items used that "preferring not to insult their readers' knowledgeability, . . . [they] no longer refer to the original source" (Merton 1983, vii)? Certainly, a case could be made in favor of that argument. Indeed, many of the quotes are from the journal *Nature*, which was widely read even by non-scientists ("Nature and Politics Between the Wars" 1969, 462). The issues ("embarrassing fecundity," etc.) mentioned in Merton's inadequately sourced quotations were so constantly discussed within the scientific community that there was no need to provide an exact reference. They were burning questions of the day among scientists—and beyond, as indicated by many press reports. Besides, to whom is this paper "Science and the Social Order" addressed? Merton's audience are his contemporary peers. They are the ones who will judge the value of his work. They are the ones whose plaudits he seeks. He is not writing for posterity—at least not as his primary target.[34] This is particularly true in the natural sciences where "puzzle-solving" is the preeminent preoccupation (Kuhn [1962] 2012, 35–42). But once the puzzle is solved and the solution incorporated into the body of accepted knowledge, no one in that community will look back. A scientist using Ohm's law in a paper published today will not cite the work where the relationship between voltage, current, and resistance was first described mathematically. And even though it is named after its formulator (eponym), the average scientist may have no idea who Georg Ohm (1789–1854) was.

The complaints expressed herein are those of a latter-day researcher (although read Dr. Verneuil's tribulations, as outlined above). As Garfield notes, "work that was widely known and cited a decade ago may not be familiar today" (1996, 452). Were these orphan quotes still common knowledge in 1968 (when Merton "enlarged" his *Social Theory and Social Structure* tome, which includes the 1938 paper), assuming they ever had been?[35] Not likely for scholars in their twenties. Young researchers then would have had the same difficulties we have today. They might have exclaimed in frustration: "Give us the tools and we will finish the

[32] Here I am referring to the final published product, which is by no means a description of the *actual* research process with its starts and stops, its backtracking and forward movements, etc.

[33] Merton first suggested this concept in the 1968 enlarged edition of his thick tome *Social Theory and Social Structure* (see pages 27–8 and especially 35).

[34] "In science, each of us knows that what he has accomplished will be antiquated in ten, twenty, fifty years" (Weber [1918] 1946, 138). Weber short-changed himself.

[35] The expansion of his *Social Theory and Social Structure* tome would have been a perfect opportunity to reveal, for a start, the origin of the undocumented quotes mentioned earlier. Of course, this also could have been done in 1957 for the revised edition. It was not.

job!" (Manchester and Reid 2012, 295).[36] In other words, provide references for these quotes so we can get on with our research. But even in the late 1930s, would they, and the issues they exemplified, have resonated with readers in other countries? In Britain, this might have been foremost on the minds of members of the scientific community and of scholars who study it (historians, philosophers, sociologists), but would that have been the case elsewhere, in Japan, for example, or, for that matter, in America (see Merton 1938, 331n25)? It does not take great powers of observation to realize that "common knowledge" is historically, socially, and culturally contingent.

But are we not upholding another "institutional imperative" of the Mertonian "ethos of science" when demanding that authors provide the complete source for any material they use? "Organized scepticism" is "the suspension of judgment until 'the facts are at hand'" (Merton 1942, 126). If we are not told the sources of quotes that pepper an article, do we have all the facts "at hand"? We must be able to verify. Unless and until we are given sufficient source reference information, we will have difficulties doing so.

Conclusion

Just like punctuation, mathematical operator symbols, quotation marks, or music notation, standardized formats for citations have not always been around. The few examples given earlier show that inconsistency (or diversity, depending on which way you look at it) is the central characteristic of the history of supplying such information—until recently, it has been a highly idiosyncratic practice. The anachronistic recriminations expressed herein demonstrate that expectations have changed in the past three-quarters of a century or so. It also shows that we write with a view to gaining the recognition—preferably positive—of our contemporary peers, i.e., individuals in the same field who tackle similar problems. Scholars crave their acceptance to build their reputation.[37] Merton makes that clear when he discusses OBI. One reason authors adopt such a practice, he states, is to avoid insulting their contemporary readers' knowledgeability (1983, vii). In other words, it is assumed that both the writer and the readers share a "common knowledge." This allows for a presumption of legitimacy and validity. There is no need to provide a source for the quote because "everybody" is familiar with its origin. The author's readers are people "in the know." As discussed earlier, "common knowledge" is narrowly but strategically circumscribed to those who matter. Besides, one would not want to alienate one's readers by denigrating their knowledgeability. This might affect how they view our work (see Thiers's reaction to Boileau's work, as cited above). And since they are an important factor in one's career path, doing so would be professionally counterproductive. Thus, it is not surprising that Merton, and others of that era, gave no thought to future (or contemporary but foreign) scholars—they would have to fend for themselves. Nor do we today.[38] What has changed, however, is the publishing infrastructure. In addition to institutionalized rules of source referencing, the electronic and digitized system within which publications are embedded dispenses researchers from "worrying" whether their readers will be able to find their quoted sources.

In the nearly nine decades since "Science and the Social Order," citation reference norms have been codified. What has taken place is a process of formalization and standardization in which citation behavior has gone from the arbitrary and idiosyncratic to the impersonal and institutionalized: in other words, from individualized diversity to depersonalized uniformity. It also has become an ethical imperative. "Source citations *must always* provide sufficient information . . . to lead readers directly to the sources consulted," *The Chicago Manual of Style* declares, and "to enable readers to positively identify them" (University of Chicago Press 2017, 743, emphasis added).[39] In the past, things were left to the author's discretion. Personal preferences appear to have been dominant while external constraints were mostly absent. Merton's citation practice in his 1938 paper exemplifies the way things were done then. One's predilections, no doubt informed by one's professional milieu, in which mimesis plays a large role, and commonly shared social norms have been replaced by external constraints characterized by a great deal of homogeneity. The process of standardization emerged in the late nineteenth century along with the professionalization of intellectual activities: "Precise citation comes with professionalization," Grafton tells us (1997, 30). It was completed near the end of the second millennium with the rise of the electronic age. The proliferation of handbooks for writers,

[36] Is this quote "common knowledge" in 2025? Certainly, for many who lived through World War II and its aftermath, but their numbers are fast dwindling. However, for today's average researcher, it would not be. Fortunately for them, they have access to the Internet and would quickly learn that wartime British Prime Minister Winston Churchill (1874–1965) made that pronouncement during a 1941 radio address.

[37] As Charles Darwin (1809–82) stated, "My love of natural science has been steady and ardent. This pure love has, however, been much aided by the ambition to be esteemed by my fellow naturalists" (1959, 141).

[38] However, as one anonymous reviewer pointed out, today, as a result of "the increased entanglement of disciplinary audiences," one's readership might not be limited to one's narrow specialty.

[39] The *MLA Handbook* makes the same point (Modern Language Association 2021, 95).

manuals of style and procedures, which have taken on a quasi-official quality (not to mention the detailed guidelines given to authors by publishers and their scholarly journals), is a testimony to the bureaucratization and policing of this practice. So does the fact that word processing software (e.g., Microsoft Word), a fundamental tool of electronic publishing, and reference management applications (e.g., Zotero) incorporate the rules writers must follow when documenting sources.[40] These are symbolic of the triumph of the standardized over the idiosyncratic.

What brought about this system is a subject matter worthy of study. How did the change from largely informal practices to an explicit grammar of rules occur? It is a topic that has yet to be tackled, at least sociologically. This neglected aspect of scholarly communication in general and of science communication in particular deserves to be explored. I can only offer here a few, incomplete, musings. There has been a convergence of factors. In addition to the ones already mentioned (professionalization and digital technology), the population explosion of both researchers and, correspondingly, of scholarly production and citations (Hyland and Jiang 2019); the proliferation of journals to distribute these goods; the advent of multidisciplinary projects and readerships; and the increased specialization in a multitude of fields of study both in the sciences and in the humanities have had undoubtedly a major influence in the normalization of source citation procedures. Also, and perhaps concurrently, the "rise" and awareness of "fraud and deceit" (Broad and Wade 1982; Freckelton 2016) in the scholarly world was a spur to put in place explicit guidelines for the authors to follow. To be noted is the fact that the production of the means of knowledge distribution and dissemination (e.g., journals, books) has transitioned, in the late twentieth century, from a material product to a mostly, if not exclusively, virtual one. In the late nineteenth century and early twentieth century, manuals of style were written for use primarily by printers; nowadays, they are designed for authors and editors (Connors 1998, 43). The shift from the former to the latter is best illustrated, we are told (Howell 1983, xi), by looking at the evolution of *The Chicago Manual of Style*, starting with its first edition in 1906. By the early 1980s, the section for printers disappeared while the portion for editors and authors followed the reverse path; it now represents the entire book. These changes have led to a major reconfiguration of the division of labor in the production of the means of knowledge distribution, which has imposed a greater burden of labor on the knowledge producers (authors) themselves at the expense of the manual workers previously involved in that process.

The practice of documenting sources has existed, in various inconsistent forms, since at least the seventeenth century. What is new here is its institutionalization, its transformation into a formalized and bureaucratized process that is now uniform and enforceable. It is no longer left to the individual researcher to decide whether it should be done. Unlike a nineteenth-century German philologist, Jacob Bernays (1824–81), today's scholar can no longer say, "This is not my way" (quoted in Grafton 1997, 109). As one sociologist puts it, "formalized rules make an organized pattern of social relations and conduct independent of particular human beings" (Blau [1964] 1986, 274). Precisely, the idiosyncratic and erratic way source referencing was practiced has been jettisoned in favor of a universal impersonal standard whose enforcement is embedded in the very tools used by every scholar to generate their final product. Given the combined power of the publisher's copy editors and the bibliographic functionality of reference management and word processing applications, source referencing norms are likely to be implemented fully. These norms inhibit authors, who in the past, as a group, have been inconsistent in their bibliographic sourcing behavior, from determining how they will format the citation information about the works they use in their research. In essence, the powers-that-be have monopolized the means of source referencing documentation. In this bureaucratized bibliographic environment, the norms cannot be transgressed—or if they are, the transgression is quickly reversed. They have become an institutionalized ethical imperative.

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[40] I hasten to add, to avoid any misunderstanding, that completeness does not imply accuracy. There is still room for error (see Agarwal et al. 2023). I can, for example, misspell an author's name when filling out the appropriate field provided by a word processing application—less likely if one uses a reference management program.

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