

Citation Analysis

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Introduction

This chapter presents a critical review of the theories that have formed and/or continue to form the basic assumptions underlying citation analysis. Unless stated otherwise, the term *citation* is used synonymously with the term *bibliographic reference*. *Citation analysis* is consequently taken to represent the analysis of bibliographic references, which form part of the apparatus of scholarly communication. Thus, studies of citations appearing in abstracting and indexing services, in subject bibliographies, or in lists or catalogs of the holding of libraries fall outside the scope of this chapter. The essence of this distinction was first noted by Martyn (1975, p. 290) who argued that “citation in the primary literature expressly states a connection between two documents, one which cites and the other which is cited, whereas citation in other listings does not usually imply any connection between documents other than that effected by the indexing machinery.” The two main foci of the chapter are citing behavior (or “citationology” [Garfield, 1998]) and symbolic characteristics of citations (i.e., how citations reflect the characteristics of science and scholarship. These topics, the distinction between which stems from Wouters (1999b), have attracted a great deal of attention from researchers in information science and other fields.

Knowledge about citing behavior and the symbolic characteristics of citations is essential in order to determine whether it makes sense to use citation analysis in various areas of application. As Zunde (1971) noted, citation analysis has three main applications:

1. Qualitative and quantitative evaluation of scientists, publications, and scientific institutions
2. Modeling of the historical development of science and technology
3. Information search and retrieval

Moreover, the introduction of two special citation-analytical techniques has paved the way for a fourth application: Knowledge organization based

on bibliographic coupling (Kessler, 1963) and co-citation analysis (Marshakova, 1973; Small, 1973).

The chapter contains three main sections. The first serves as a historical preface, reviewing briefly both the scientific tradition of citing and the recurring calls by scholars for a theory of citation. The second provides an overview of theories and studies of citing behavior. It considers, in turn, the widespread belief that citing is best understood as a psychological process, studies of citer motivations, the normative theory of citing, the social constructivist theory of citing, and contemporary theories of citing behavior based on evolutionary accounts of science and scholarship. The final section presents a critical analysis of Wouters's reflexive citation theory.

Historical Preface

This is the first *ARIST* chapter explicitly entitled *Citation Analysis*. However, previous *ARIST* chapters on bibliometrics (Borgman & Furner, 2002; Narin & Moll, 1977; White & McCain, 1989) and informetrics (Wilson, 2001) partly overlap with the topics addressed in the present chapter.

The Scientific Tradition of Citing

Scientific tradition requires that scientists, when documenting their own research, refer to earlier works that relate to the subject matter of their reported work. These bibliographic references are supposed to identify those earlier researchers whose concepts, theories, methods, equipment, and so on, inspired or were used by the author in the process of conducting and presenting his or her own research. Although this tradition is sometimes said to be as old as science itself (e.g., Price, 1963, p. 65), historians of science disagree about the origins of the reference. According to Grafton (1997), historians of science have variously placed the birth of the modern reference in the twelfth, seventeenth, eighteenth, or nineteenth century. Mustelin (1988), however, maintains that, prior to the sixteenth century, authors often duplicated the work of their predecessors without proper recognition. From the latter part of the sixteenth century, authors of scientific works strove to give their texts greater evidential weight by noting and referring to other sources. Among the earliest proponents of this practice were philologists and editors of texts, with historians and others following later. Nowadays, explicit references are believed to be essential in order to communicate "effectively" and "intelligently" about scientific and technical matters (Garfield, 1977, p. 8), and the act of citing is deemed to be "second nature" to anyone writing a scholarly or scientific paper (Kaplan, 1965, p. 179).

An important feature of citations is that each reference is an inscription (Latour & Woolgar, 1986, pp. 45–53) describing a certain text by a

standardized code. Although different publication manuals give different codes and many publishers and journals use their own standards, these manuals and standards usually instruct the author to write his or her references as a combination of author name, title, journal name or publisher, year of publication, and page numbers. References themselves are thus texts pointing to other texts (Wouters, 1998). This does not entail that the cited texts are always to be found where the citing texts say they are. Garfield (1990, p. 40) reviewed a number of studies dealing with bibliographic errors and concluded, that “to err bibliographically is human.” For instance, in a study of the incidence and variety of bibliographic errors in six medical journals, De Lacey, Record, and Wade (1985) found that almost a quarter of the references contained at least one mistake and 8 percent of these were judged serious enough to prevent retrieval of the article. Moed and Vriens (1989) examined discrepancies between 4,500 papers from five scientific journals and approximately 25,000 articles that cited these papers, finding that almost 10 percent of the citations in the cited reference dataset showed a discrepancy in either the title, the author name, or the page number. They concluded that one cause for the multiplication of errors seemed to be authors’ copying of erroneous references from other articles. Broadus (1983) came to the same conclusion in a study of a 1975 textbook on sociobiology that included among its references an erroneous reference to a 1964 article (one word was incorrectly substituted in the title). By examining 148 subsequent papers that cited both the book and the article, Broadus could see how many authors repeated the book’s mistaken reference. He found that 23 percent of the citing authors also listed the faulty title. A similar study by Simkin and Roychowdhury (2003) reported an almost 80-percent repetition of misprints.

Recurring Calls for a Theory

During the 1970s, claims such as Cawkell’s (1974, p. 123) that deductions can be made from a citation network without knowledge of its subject content appeared less frequently. Instead, sociologists, information scientists, and others began to recognize the need for a theory of citing that could explain why authors cite the way they do. Among the first contributors was Mulkay (1974), who argued that there had been no clear demonstration of the way in which citations reflect the process of scientific influence. The absence of such a demonstration led him to conclude that “in fact we know very little about who cites whom in science, and why” (Mulkay, 1974, p. 111). A few years later, Swanson (1977, p. 145) called for a “convenient and rapid method for discovering the *nature* of the relevance link which the citing author has established.” In 1981, three more authors raised this problem independently from different perspectives: Cozzens (1981) reviewed existing theories of citing from the perspective of sociology, Cronin (1981) called for a theory of citing from the perspective of information retrieval, and L. C. Smith (1981, p.

99) concluded that not enough was known about the citing behavior of authors and that such knowledge is essential in order to know whether it makes sense to use citation analysis for particular applications. Some years later, Zuckerman (1987) repeated the call for a theory of citing, despite her conclusion that such a call seemed redundant. In 1998, a whole issue of the journal *Scientometrics* was devoted to the discussion of, and a renewal of the call for, a theory of citing. Leydesdorff (1998) initiated this discussion with a paper entitled "Theories of Citation?" in which he argued that, although a variety of contexts for citation analysis had been proposed, a comprehensive theory had not been formulated.

Citing Behavior

As Kochen (1987, p. 54) has noted, "a paper that conforms to the norms of scholarly perfection would explicitly cite every past publication to which *it* owes an intellectual debt." This ideal has long been debated by information scientists and others, with discussion centering around two fundamental questions: (1) What makes authors cite/not cite their influences? and (2) To what extent is the ideal exemplified?

Citing as a Psychological Process

A number of commentators apparently share the belief that citing is best understood as a psychological or cognitive process and that, accordingly, theories of citing should be constructed from studies of individual citers conducted by interview techniques, thinking aloud methods, or the recording of behavioral patterns (see, for example, Case & Higgins, 2000). The focal point of this subsection is a specific theory about citing behavior proposed by Harter (1992, p. 614), who has argued that the act of citing is "a dynamic, complex, cognitive process."

Harter's (1992) starting point was a theory of the relevance of everyday speech utterances to listeners that had been proposed six years earlier by Sperber and Wilson (1986) in their book *Relevance: Communication and Cognition*. At the end of their book, Sperber and Wilson asserted that their theory was applicable not only to human speech, but to thought processes in general. Convinced that this conjecture was correct and drawing a number of its implications for information retrieval and bibliometric theory, Harter outlined his theory of citing:

Relevant references found by a researcher in an IR search (or in another way) cause cognitive change. As the research progresses, the references (and the knowledge found in them) have their effect on the conceptual framework for the work, the choice of problems and methods, and the interpretation of the results. Finally, when the research has been completed, those references that are especially relevant, or that have led

to especially relevant sources, will be incorporated into the list of references at the end of the published work that reports the results of the research. An author who includes particular citations in his list of references is announcing to readers the historical relevance of these citations to the research; at some point in the research or writing process the author found each reference relevant. (Harter, 1992, pp. 612–613)

Although Harter's theory has received some support from White and Wang's (1997) longitudinal study of citing behavior, the account of relevance upon which it is based has been shown to be quite problematic (see, for example, the critical reviews of Gibbs [1987a, 1987b], Hjørland [2000b], Mey & Talbot [1988], and Talbot [1997]), primarily because of its disregard of sociocultural issues. Sperber and Wilson (1986) viewed human beings as information processors with an inbuilt capacity to infer relevance: Assuming this capacity to be of fundamental importance, they constructed around it what they claimed to be a unified theory of cognition that could serve as a basis for studying human communication. Harter (1992, p. 604) claimed that Sperber and Wilson had understood human beings as being in command of a number of manifest assumptions, which are products of each individual's cognitive ability, cultural and social group identity, educational background, and physical environment. But this is actually not true. Mey and Talbot (1988) and Talbot (1997) have noted that considerations of cultural, social, and epistemological affiliation are absent from Sperber and Wilson's characterization of individuals' cognitive environments. In the second edition of their book, Sperber and Wilson (1995, p. 279) admitted that they had developed their theory without taking sociocultural issues into account and acknowledged that "the social character and context of communication are, of course, essential to the wider picture, to the study of which we hope relevance theory can contribute, and from which it stands greatly to benefit." Two years later, Sperber and Wilson (1997, p. 145) observed that most relevance-theoretic work had largely ignored aspects of communication discussed in the sociological literature, but argued that this is more "a reflection of a sound initial research strategy (which is likely to change as the field develops) than some silly anti-sociological bias."

Sperber and Wilson's (1986) view that differences between human beings derive solely from variations in physical environment and cognitive ability, makes it difficult to understand how a listener may infer a speaker's intentions. According to their theory, a listener can infer a speaker's intentions on the basis of knowledge of the speaker's cognitive environment because the knowledge manifest to different individuals is largely the same. However, as Talbot (1997, p. 447) has observed, reading *Relevance* leaves one with the impression that everyone lives in the same kind of white, middleclass, educated world:

While this may be true, to some extent, of the linguists and cognitive scientists comprising the authors' audience, it is a serious inadequate provision of social context for a study of either communication or cognition. ... Their ad hoc choice of unrelated facts known both to themselves and their readers for the potentially endless production of negative assumptions betrays an unsystematic approach. ... In the absence of any social element, with which to locate and specify kinds of knowledge that might be mutually accessible to different individuals, this is inevitable.

Harter (1992) must, perhaps unconsciously, have seen this. Why else would he have included *cultural and social group identity* along with cognitive ability, educational background, and physical environment in his definition of what an individual's manifest assumptions are? Viewed in this light, the title of his article and his conclusion that relevance is a theoretical concept of cognitive psychology are difficult to comprehend. On the other hand, Harter simply declared the significance of sociocultural issues without developing the point further, focusing instead on the technical aspects of how a phenomenon (a stimulus) such as a retrieved citation may cause cognitive changes in a user's mental state. He did ask why an item is initially found relevant and what criteria users employ when they select bibliographic items for subsequent citation in their published works (Harter, 1992, p. 614). But his narrow focus on cognitive issues prevented him from giving an adequate answer to these questions and so diminished the cogency of his theory of citing.

Motives for Citing

Baldi (1998, p. 831) has noted that, between 1965 and 1979, trying to capture the various motives for citing became a "cottage industry" that produced a number of schemes and typologies. This subsection highlights the most significant of these.

Moravcsik and Murugesan (1975) constructed a classification scheme and used it to categorize 706 references in 30 articles in the field of theoretical high-energy physics that had been published in the journal *Physical Review* during the period 1968–1972. Their study revealed, among other things, that 41 percent of the references were nonessential (*perfunctory*) and 14 percent were negative (*negational*). Shortly after Moravcsik and Murugesan's study, Chubin and Moitra (1975) published their results of a similar study, in which they had developed their own classification scheme and categorized the references from 43 physics articles published in the period 1968–1969. The results of their content analysis revealed that 20 percent of the references were nonessential and 5 percent were negative. However, Chubin and Moitra (1975, p. 426) criticized the content-analytical method as incapable of describing authors' actual motives for citing and suggested that future studies

should take a phenomenological approach focusing on “the private process by which authors choose references (i.e., when writing, do authors have an implicit set of categories which guide the kind and number of references they make?).” They also discussed how to conduct such an investigation, concluding that “direct questioning of authors about why they referenced who they did, and in what fashion, may be a beginning” (Chubin & Moitra, 1975, p. 426).

Brooks (1985) analyzed a number of theoretical models and isolated seven motives for citing posited by all of them. He then asked 26 researchers to complete a questionnaire regarding the motivations for their references in their recent articles. His survey found that persuasiveness was the most common purpose for citing and that only 2 percent of the references were negational. In a similar study, Cano (1989) asked a group of engineers to complete a questionnaire, based on Moravcsik and Murugesan’s (1975) classification scheme, concerning their motives for their references in 42 articles published in three different journals. She found that 26 percent of the references were deemed nonessential by the authors, and only 2 percent were negational. Shadish, Tolliver, Gray, and Sen Gupta (1995) conducted the first major investigation of social scientists’ motives for citing. Using a questionnaire that listed 28 potential purposes for citing, they surveyed researchers who had published articles in psychological journals. The results showed that psychologists rarely made use of negational references.

Unfortunately, all of these studies suffer from the same fundamental problem: They cannot clarify why a cited reference was found relevant to begin with. An individual is often partly unconscious of, or fails to recognize, his or her reasons for citing a particular source and not citing another. Thus, questioning an author about his or her motives for citing/not citing cannot reveal the actual reasons why an author has cited as he or she has done. This dilemma is a variant of *the relevance dilemma*. According to Hjørland (2000a, 2002) and Hjørland and Sejer Christensen (2002), there are important precepts to science that are so integral to the researcher’s life and culture that he or she is partially or wholly unaware of them. But the researcher’s lack of awareness of their influence does not render them unimportant. In fact, the opposite is true. It is important to recognize and understand what these precepts are, and how they affect the individual. In stressing the necessity of understanding the sociocultural environment of an individual in order to comprehend his or her relevance criteria, Hjørland (2000a; 2002) and Hjørland and Sejer Christensen (2002) break with the psychological understanding of relevance.

The Normative Theory of Citing

In his article “The Norms of Citation Behaviour,” which is usually held to be the first explicit account of citing as normative behavior, Kaplan (1965) argued that footnoting practices are passed on both by

word of mouth from professor to student and by examination of the varying practices of different journals. Ravetz (1971, pp. 256–257) likewise held citing to be governed by an etiquette based on purely informal, perhaps tacit and unselfconscious, craft knowledge. According to Kaplan (1965), the major function of footnoting practice was the reaffirmation of the underlying norms of scientific behavior. The normative theory of citing is based on the assumption that science is a normative institution governed by internal rewards and sanctions. It holds that scientists exchange information (in the form of publications) for recognition (in the form of awards and citations). This view suggests that citations are a way to acknowledge intellectual debts and, thus, are mostly influenced by the perceived worth, as well as the cognitive, methodological, or topical content of the cited articles (Baldi, 1998).

Early sociologists of science generally believed that consensus in science was governed by a particular scientific ethos, that is, a set of rules supposed to establish trust in, and guarantee the reliability of, the knowledge claims produced by scientists. This view was given its most succinct and influential formulation by the Merton (1942/1973), who defined the ethos of science in terms of four basic norms—communism, universalism, disinterestedness, and organized skepticism. Merton (p. 269) held these norms to be binding on the man of science. Although he never claimed that the ethos of science operates explicitly at all times, his remark that “it has become manifest that in each age there is a system of science that rests upon a set of assumptions, usually implicit and seldom questioned by most of the scientific workers of the time” (Merton, 1938/1973, p. 250) reveals a profound conviction that the ethos is always decisive. Merton and the early sociologists of science knew, of course, about many instances of scientists resisting scientific discoveries and disagreeing on the merits of particular findings. But sociologists such as Merton and Barber (1961) explained these deviations from the expected consensus by arguing that cultural factors occasionally serve as institutional and intellectual obstacles to scientists otherwise behaving as faithful disciples of the norms of science.

It is not my intention to attribute a “naïve, idealized, Arcadian image of scientists” to Merton, for I agree with his biographer (Sztompka, 1986, p. 56) that such ascription would be a mistake. Merton reworked and modified his sociology of science throughout his career and made numerous adjustments and corrections to it. For example, in 1963 he acknowledged “the often painful contrast between the actual behavior of scientists and the behavior ideally prescribed for them” (Merton, 1963/1973, p. 393). However, his earlier writings on the ethos of science and the extent to which scientists adhere to certain norms were not hedged with similar reservations. On the contrary, the “younger” Merton (1942/1973, p. 276) wrote about “the virtual absence of fraud in the annals of science” and even claimed that “deviant practices ... are extremely infrequent” (Merton, 1957/1973, p. 311). Indeed, the “older” Merton sometimes seemed to forget the reservations expressed in 1963.

Discussing why he did not come up with the idea of the citation index before Garfield, Merton (1977, p. 49) wrote that “all the substantive ingredients for invention of that tool were being observed back in 1942.” Merton (1977) apparently believed his 1942 theory of scientific practice to be a sound foundation for a citation index and, thus, for a theory of citing. Many have concurred with Merton and have attempted to construct a normative theory of citing based on the early sociology of science, including the work of the “younger” Merton. This theory posits that investigators cite those materials that have proved to be of value to them (i.e., communism). It also holds that scientists, when evaluating the work of others, are behaving universalistically—that is to say, their decisions about what to cite are not influenced by functionally irrelevant characteristics such as a scientific author’s sex, race, religion, or rank (i.e., universalism). Furthermore, it assumes that scientists are disinterested and do not seek to gain personal advantages by flattering others or citing themselves (i.e., disinterestedness). Moreover, it holds that scientists treat their own work with the same skepticism as the work of others (i.e., organized skepticism). These assumptions have motivated many citation analyses, as illustrated by L. C. Smith’s (1981, p. 87–89) list of basic assumptions underlying citation analysis in general:

1. Citation of a document implies use of that document by the citing author.
2. Citation of a document (author, journal, etc.) reflects the merit (quality, significance, impact) of that document (author, journal, etc.).
3. Citations are made to the best possible works.
4. A cited document is related in content to the citing document.
5. All citations are equal.

The second assumption in Smith’s list reflects the Mertonian norm of communism and its inherent principle that scientists should give credit where credit is due whenever they have made use of the work of others. In the foreword to Garfield’s (1979) book *Citation Indexing: Its Theory and Application in Science, Technology, and Humanities*, Merton (1979, p. viii) emphasized this particular responsibility of the academy by stating that

the anomalous character of intellectual property in science becoming fully established only by being openly given away (i.e., published) links up with the correlative moral as well as cognitive requirement for scientists to acknowledge their having made use of it. Citations and references thus operate within a jointly cognitive and moral framework. In their cognitive aspect, they are designed to provide the historical lineage of knowledge and to guide readers of new work to

sources they may want to check or draw upon for themselves. In their moral aspect, they are designed to repay intellectual debts in the only form in which this can be done: through open acknowledgment of them.

Merton was convinced that authors generally cite the materials that have proved of value to them because of the social control mechanisms of science. He addressed the issue again in 1995, claiming that

the process of socialization in the culture of science joins with such social arrangements as published and unpublished "peer review" that serve as agencies of social control which see to it, among other things, that authors generally abide by the norm of indicating their predecessors and sources. (Merton, 1995, p. 389)

Early Tests of the Normative Theory of Citing

According to the normative theory, failure to give credit where credit is due is unusual. For example, Cole and Cole (1972, p. 370) stated that "sometimes ... a crucial intellectual forebear to a paper is not cited. The omission is rarely due to direct malice on the part of the author but more often to oversight or lack of awareness. ... We can assume that omitted citations to less influential work are random in nature. ..." Garfield (1977, p. 7) took a similar view, declaring that "the vast majority of citations are accurate and the vast majority of papers do properly cite the earlier literature." However, in the very next sentence, Garfield admitted that this assertion had not been empirically substantiated: "Unfortunately, there has never been a definitive study of this assertion." The basic assumption of the normative theory of citing was not tested until the 1980s. The pioneers of this work were not adherents of the normative theory, but a group of skeptics including, among others, the biologists Michael and Barbara MacRoberts and the information scientist Terrence Brooks.

The MacRoberts published a number of articles in which they argued that citation analysis was an invalid tool for research evaluation (MacRoberts, 1997; MacRoberts & MacRoberts, 1984, 1986, 1987a, 1987b, 1988, 1989a, 1989b, 1996). In these articles, they challenged the basic assumption of the normative theory of citing—that scientists cite their influences. MacRoberts and MacRoberts (1986) tested this assumption by reading and analyzing 15 randomly selected papers in the history of genetics, a subject with which they claimed to be familiar. They found that from 0- (i.e., the paper had no references or footnotes) to 64-percent influence was captured in references and footnotes. After reconstructing the bibliographies of the papers in their sample, the MacRoberts were able to estimate that the 15 papers required some

719 references at a minimum to cover the influences manifested in them, whereas, in fact, they contained only 216—a coverage of 30 percent for the entire sample. A decade later, they claimed that this percentage typified all the fields with which they were familiar (botany, zoology, ethology, sociology, and psychology) and concluded that

if one wants to know what influence has gone into a particular bit of research, there is only one way to proceed: head for the lab bench, stick close to the scientist as he works and interacts with colleagues, examine his lab notebooks, pay close attention to what he reads, and consider carefully his cultural milieu. (MacRoberts & MacRoberts, 1996, p. 442)

Brooks (1985, 1986) mounted another challenge to the basic assumptions of the normative theory of citing in two papers reporting on a survey of 26 researchers at the University of Iowa who indicated their motivations for giving each reference in their recently published articles by rating seven motives for citing. According to Brooks, the survey results suggested that authors cite for many reasons, with the allotment of credit being the least important motivation for citation. Of the 900 references studied, Brooks (1985, p. 228) found that about 70 percent were multiply motivated, concluding that “no longer can we naively assume that authors cite only noteworthy pieces in a positive manner. Authors are revealed to be advocates of their own points of view who utilize previous literature in a calculated attempt to self-justify. However, as White (2004b, p. 98) has pointed out, the results of Brooks’s survey need to be assessed with caution, for one of the motives listed was “persuasiveness” and the respondents almost certainly understood “persuasion” to mean citing to “help build a case” not manipulative name-dropping or distortion of citees’ meanings.

The Social Constructivist Theory of Citing

This section examines the phenomenon of citation from a social constructivist perspective (e.g., Baldi, 1997, p. 17; MacRoberts & MacRoberts, 1996, p. 439; Small, 1998, p. 143; White, 2004b). At the outset, it is important to note that the label *social constructivists* has historically been applied to two distinct groups of scholars. Collin (1997) admits that both groups are commonly termed social constructivists, but reserves that name for the first of these, which includes Emile Durkheim, Peter Berger, Thomas Luckmann, Don Zimmerman, Melvin Pollner, Alasdair MacIntyre, and Peter Winch. These social constructivists concentrate on how social reality is produced by the cognitive efforts of ordinary social agents. On the other hand, members of the second group, whom Collin (1997, p. 13) terms *science constructivists*, focus specifically on scientific communities and scientific research. Because the theories propounded by this latter group form the focus of this section,

the term *social constructivists* is used here to refer to scientific constructivists such as Barry Barnes, David Bloor, Michel Callon, Harry Collins, Karin Knorr Cetina, Bruno Latour, and Steve Woolgar.

The social constructivists believe that scientific closure is the outcome of a negotiation process in which one party convinces the other by mere persuasion. In fact, Latour and Woolgar (1986, p. 69) have maintained that science is the art of persuasion. In their view, successful scientists are those who most skillfully manage to persuade others that they are not just being persuaded, that no mediations intercede between what is said and the truth (Latour & Woolgar, 1986, p. 70).

In the art of persuasion, no holds are barred. According to the social constructivists, the successful scientist makes use of many persuasive moves when reporting research. According to this view, when authors cite, they are marshalling earlier documents in such a way as to persuade readers of the goodness of their claims. Indeed, MacRoberts and MacRoberts (1987b, p. 294) have argued that persuasion, not a desire to give credit where credit is due, is the major motivation for citing. This position, which stands in direct opposition to the normative theory of citing, has been much influenced by Gilbert's (1977, p.116) article "Referencing as Persuasion," in which he claimed that

Authors preparing papers will tend to cite the "important and correct" papers, may cite "erroneous" papers in order to challenge them and will avoid citing the "trivial" and "irrelevant" ones. Indeed, respected papers may be cited in order to shine in their reflected glory even if they do not seem closely related to the substantive content of the report.

Latour (1987, pp. 33–34) speculated along the same lines, arguing that in order to put up a persuasive facade, authors engage in chicanery: "First, many references may be misquoted or wrong; second, many of the articles alluded to might have no bearing whatsoever on the claim and might be there just for display." Latour (1987, p. 33) did not consider such actions to be inconsequential. On the contrary, he maintained that if readers were to find out what is actually going on—e.g., the use of citations purely for display—the result would be "disastrous" for the authors. As White (2004b) has pointed out, the issue is not the ordinary claim that scientists and scholars write to persuade and use citations as a rhetorical resource. The persuasion hypothesis, rather, is the idea that persuasion in science and scholarship relies on manipulation indistinguishable from that used in commercial advertising. For example, MacRoberts and MacRoberts (1996, p. 441) have claimed that "papers are meant to sell a product," and Law and Williams (1982, p. 543) have likened scientists' choice of references to "packaging a product for market."

White (2004b) has recently presented a careful analysis of the persuasion hypothesis, arguing that it consists of two parts. The first has to do with what citers say about cited works or, more precisely, the contexts

in which they discuss them. He calls this part “persuasion by distortion,” noting that “citers often misrepresent the works they allude to, twisting their meaning for their own ends” (p. 96). The second has to do with the choice of the cited works themselves, regardless of what is being said about them. White (2004b, p. 96) calls this part “persuasion by name-dropping” and notes that it is more or less independent of context: “Citers disproportionately cite works by established authorities, so as to gain credibility by association.”

Empirical Tests of the Persuasion Hypothesis

Researchers have usually tested either the first or the second part of the persuasion hypothesis. One important exception is Baldi’s network-analytic study of normative versus social constructivist processes in the allocation of citations. Baldi (1997, 1998) studied articles about celestial masers, an area of astrophysics research, and discovered that “authors are most likely to cite articles that are relevant to their work in terms of subject, recency of knowledge, [and] theoretical orientation, and seem to have little concern with the characteristics of authors who wrote them” (Baldi, 1998, p. 843). However, both Small (1998) and Collins (2000) have questioned the adequacy of Baldi’s method.

According to the first part of the persuasion hypothesis (i.e., persuasion by distortion), citers often misrepresent the works to which they refer, twisting their meaning for their own ends. In the words of Latour (1987, p. 34): “Many of the articles alluded to might have no bearing whatsoever on the claim and might be there just for display.” The first part of the persuasion hypothesis is thus the negation of the normative assumption that “a cited document is related in content to the citing document” (Smith, 1981, p. 89). The normative theory assumes that references signal direct semantic relationships between the citing and the cited works. For instance, Garfield (1979, p. 3) maintained, after discussing the semantic problems of subject indexes, that the citation is a precise, unambiguous representation of a subject. However, the assumption has been tested only a few times in studies that have yielded contradictory results (Harter, Nisonger, & Weng, 1993; Peters, Braam, & Van Raan, 1995; Song & Galardi, 2001; Trivison, 1987). Thus, it is not possible to conclude to what extent cited and citing documents are semantically related. Cronin (1994) has noted that texts may be cited at different levels of granularity or aggregation. This, he argues, may influence similarity scores and explain why the degree of subject similarity between pairs of cited and citing documents is frequently found to be small.

According to the second part of the persuasion hypothesis (i.e., persuasion by name-dropping), authors disproportionately cite works by established authorities in order to gain credibility by association. As MacRoberts and MacRoberts (1996, pp. 440–441) have put it, “an author’s main objective is not to cite their influences but to present as authoritative an argument as possible.” However, at least three studies

have questioned the validity of this claim (Moed & Garfield, 2003; White, 2004b; Zuckerman, 1987).¹

Zuckerman (1987, p. 334) posed the question: If persuasion really were the major motivation to cite, would citation distributions look as they do? Basing her argument on data provided by Garfield (1985), she concluded that the answer was “plainly not.” Garfield (1985, p. 6) had presented a table illustrating the number of citations retrieved by items cited one or more times in the 1975–1979 cumulated *Science Citation Index (SCI)*. The table revealed that only 6.3 percent of the 10.6 million citations went to documents cited ten or more times during the five-year period. Zuckerman (1987) pointed to the low percentage as evidence against the plausibility of the persuasion hypothesis, which would predict a much higher percentage. Zuckerman (1987, p. 334) referred to Gilbert (1977, p. 113), one of the proponents of the persuasion hypothesis, who had stated that it is the papers seen as “important and correct” that “are selected because the author hopes that the referenced papers will be regarded as authoritative by the intended audience.” However, if one adopts a modest criterion of *authoritative papers* as those that have been cited at least ten times in five years (or twice annually), the persuasion hypothesis needs to be radically adjusted, for Garfield’s data do not support the social constructivist suggestion that an author’s main objective is not to cite his or her influences but to present as authoritative an argument as possible.

White (2004b) realized that if an author cites “a world figure” (e.g., Noam Chomsky or Thomas Kuhn), he or she might be accused of name-dropping no matter what works by these world figures are cited. Accordingly, it makes little sense to believe that cited authors’ levels of prestige and authority vary much from work to work. Instead of testing the persuasion hypothesis, as Zuckerman (1987) had done, by determining the percentage of citations received by authoritative papers, White realized that one could test the hypothesis by determining the percentage of citations received by authoritative authors. White’s test did not support the persuasion hypothesis, for he found that authors do not exclusively favor high-end names with authoritative reputations, but tend to cite at all levels over the entire scale of reputation. Indeed, his findings suggest, if anything, that authors tend slightly to favor low-end names. White (2004a) pointed out that if the persuasion hypothesis were correct, widespread and unwarranted citations of prestigious authors should be detectable in the writings of the social constructivists as well (e.g., Gilbert & Mulkay, 1984; Latour, 1987; Myers, 1990). “Yet,” he concluded, “I find no evidence of empty name-dropping on their part; in my reading, they play the citation game straight” (White, 2004a, p. 111).

Moed and Garfield (2003, p. 192) added yet another dimension to the critique of the persuasion hypothesis in a study seeking to answer the question “how does the relative frequency at which authors in a research field cite ‘authoritative’ documents in the reference lists in their papers vary with the number of references such papers contain?” They reasoned

that “if this proportion decreases as reference lists become shorter, it can be concluded that citing authoritative documents is less important than other types of citations, and is not a major motivation to cite” (p. 192). The authors analyzed the references cited in all source items denoted as “normal articles” that were included in the 2001 edition of the *SCI* on CD-ROM. The source papers were arranged by research fields, which were defined in terms of aggregates of journal categories. The authors focused on four such fields: molecular biology and biochemistry, physics and astronomy, applied physics and chemistry, and engineering. Their findings clearly suggest that authors in all four fields cited proportionally fewer “authoritative” documents as their bibliographies became shorter. In other words, when the authors displayed selective referencing behavior, references to “authoritative” documents dropped radically. From this, Moed and Garfield (2003, p. 195) drew the conclusion that “in this sense, persuasion is not the major motivation to cite.”

The Standard Account

A number of commentators have questioned whether a theory of citing is needed at all. Indeed, one has even suggested calling a halt to *theorizing* and returning to the standpoint of logical positivism: “I think the current state of our field calls for more empirical and practical work, and all this theorising should wait till a very large body—beyond a threshold—of empirical knowledge is built” (Arunachalam, 1998, p. 142). Many citation analysts seem to accept the standard account (Nicolaisen, 2004). Although readily granting that authors frequently do not give credit where credit is due, they claim that, *on average*, authors do give due credit by properly citing their inspirations and sources. This claim, they maintain, does not invalidate citation analyses. For example, Small (1987, p. 339) has argued that “the issue is not whether we can rely on reference lists in individual cases as complete sets of influences (we cannot), but rather whether references can be used statistically, in the aggregate, as an indicator of influence.” Nederhof and Van Raan (1987, p. 326) have put forth much the same argument:

If one looks at the references contained in one individual paper, many irregularities may be found, such as missing references to important papers, or to the work of authors which have made important contributions to the body of knowledge in a field. Thus, a seriously mistaken picture of the main influences in a particular field would be obtained when only one particular paper is used for this purpose.

Commentators have frequently suggested that the biases and deficiencies of individual citers are repaired to a tolerable degree by the combined activity of the many (e.g., White, 2001, p. 102). For example, Nederhof and Van Raan (1987, p. 326) have maintained that “even if all

papers would to a large extent (but not completely) cite in an arbitrary way, it would still be possible to detect valid patterns in the citations, if a sufficiently large number of papers would be sampled.”

White (1990, p. 91) has thus asked: “Why not believe that there is a norm of citing—straightforward acknowledgement of related documents—and that the great majority of citations conform to it?” The answer is, of course, that an existence claim or even an existence proof—Kurtz, Eichhorn, Accomazzi, Grant, Demleitner, Murray, et al. (2005, p. 116) claim to have proven that the normative theory of citing is true in the main—does not explain the phenomenon in question. For that, we need some kind of theory. A theory is normally conceived as a set of formally specified and interconnected general propositions that can be used for the successful explanation and prediction of some phenomenon (Geuss, 1998). Consequently, White’s belief or conviction does not qualify. Although it may help to predict citing behavior, it does not help to explain the phenomenon.

Evolutionary Accounts of Science and Scholarship

Kuhn (1962) concluded *The Structure of Scientific Revolutions* by proposing an evolutionary view of science. Ten years later, Toulmin (1972) argued that it is possible to produce a single analysis of selection processes that would be equally applicable to social, conceptual, and biological evolution. Others have subsequently taken up the challenge and sought to develop Kuhn’s and Toulmin’s suggestions. Prominent among these is David L. Hull, who, in his book *Science as a Process: An Evolutionary Account of the Social and Conceptual Development of Science* (Hull, 1988) and in a number of essays (Hull, 2001), has demonstrated that science and scientific behavior closely resemble selection processes normally studied in evolutionary biology.

Evolutionary theories of human behavior have also inspired research in information science. One example is Sandstrom’s work on scholarly communication as a socio-ecological system (Sandstrom, 1994, 1998, 1999, 2001, 2004; see also Cronin & Hert, 1995). Sandstrom’s view of scholarly communication derives its inspiration from the socio-ecological theory of optimal foraging, the basic assumption of which is that “organisms will behave as if they are optimizing some fitness-related currency or set of currencies” (Sandstrom, 1994, p. 417 [citing Kaplan & Hill, 1992]). According to optimal foraging theory, “a particular prey type will be included in the optimal diet only if its net energy return per unit handling time is greater than the average return rate (including search time) for all prey types of higher rank” (Sandstrom, 1994, p. 425 [citing E. A. Smith, 1983]). In other words, the foraging behavior of any organism is thought to be a balance between cost and benefit. If the cost of including a particular prey type within an organism’s diet exceeds the benefit of doing so, the organism simply will not include it. Sandstrom (1994, p. 428) has suggested that it is probable that scholars choose

among information resources according to the same basic principle: “Scholars (both as readers and writers) are likely to maximize their interaction with an array of resources offering higher returns in terms of handling and to minimize their efforts in procuring the obscure ones.” The socio-ecological theory of optimal foraging has been challenged for more than two decades. A number of anthropological studies have revealed that human foragers often violate the cost-benefit principle underlying the theory. The anthropologist Eric Alden Smith, one of the leading figures of optimal foraging theory in the 1980s, now concedes that the theory fails to explain much of human foraging behavior. In fact, he acknowledged the limitations of optimal foraging theory a year before Sandstrom’s first publication on the subject (Smith, 1993). Like many others, Smith now subscribes to the handicap principle, otherwise known as the theory of costly signaling (e.g., Bliege Bird, Smith, & Bird, 2001; Smith, Bliege Bird, & Bird, 2003).

Nicolaisen (2004) has recently outlined a theoretical explanation for citation behavior that draws its inspiration from models developed within the domain of evolutionary biology, especially the handicap principle developed by the Israeli biologist Amotz Zahavi.

The Handicap Principle

Nicolaisen (2004) likens references to *threat signals*. Although this may seem puzzling at first, it is not an entirely new idea. In fact, Latour (1987, p. 33) seems to have proposed the very same in his book *Science in Action*: “Attacking a paper heavy with footnotes means that the dissenter has to weaken each of the other papers, or will at least be *threatened* with having to do so” (italics added).

Although Latour and Nicolaisen apparently share the idea that references are threat signals, they do not agree on how authors may utilize them. According to Latour (1987, p. 33), an author “can turn a fact into a fiction or a fiction into a fact just by adding or subtracting references.” However, to pull off this trick, the author must know and exercise the right strategies. Latour examined two such strategies: *stacking* and *modalizing*. He maintained that the presence or absence of references in a scientific text signifies whether the text is serious and strong. In order to appear serious and strong, the author should cite a number of other documents—a practice that Latour (1987, p. 33) called “stacking masses of references.” Stacking can often be an effective means of transforming fiction into fact, for it requires that a potential reader read, or be acquainted with, the cited documents to be able to determine the strength and accuracy of the citing text—a difficult task to accomplish when many documents are cited. However, Latour (1987, p. 33) himself noted that stacking masses of references is not sufficient to appear serious and strong if the author is confronted with “a bold opponent.” Such an opponent might just trace all of the references and probe their degree of attachment to the author’s argument: “If the reader is courageous enough, the result may be disastrous for the author” (p. 33). Therefore,

another move is needed to pull off the trick. The author has to *modalize* the status of the cited documents—i.e., to modify or qualify the reference to make it more in keeping with the argument of the citing text.

Apparently, Latour believed that authors are free to do whatever they need to the earlier literature to render it as helpful as possible for their own arguments. This belief is founded on an understanding of scientific communication portrayed in Figure 1.3 in Latour's (1987, p. 38) *Science in Action*. This figure shows a citing author, her article, and its (modalized) references; other elements include authors of the cited documents, as well as an unidentified man and an "isolated reader." It is unclear what, precisely, Latour means by "isolated reader." However, for the sake of argument, let us suppose he means one who is unacquainted with the literature cited in the citing article. Now, if all readers were isolated readers like the one portrayed by Latour, it seems reasonable to assume that authors would be free to do whatever they needed to the earlier literature to align it as much as possible with their own arguments. However, as Nicolaisen (2004) has pointed out, this situation is highly unlikely and Latour's assumption seems rather naive. Most readers are not isolated in the sense given here. On the contrary, potential readers are generally well-read subject specialists possessing a broad knowledge of the literature and field covered by the text. This becomes evident if one reflects on the typical life cycle of what is currently the most common scientific or scholarly text type—the journal article. Most journal articles face two potential groups of readers: those who read them prior to publication and those who read them after publication. Potential readers prior to publication include, among others, editors and referees, who participate in the typical pre-publication peer review of academic journals. Among the potential post-publication readers are others working in the field and, perhaps most important, the authors who are cited in the article. Like the editors and referees, many of these potential post-publication readers possess expert knowledge of the field and its literature. Authors who bend the earlier literature to their own purposes and thus commit willful acts of deceit risk exposure as the cheaters they are by their potential readers. Honest authors, who cite their sources of information and inspiration properly, need not fear such exposure. The two types of authors may appeal to the same set of sources as backup for their arguments, but not at the same potential cost, which is much higher for the cheating authors than for the honest ones. Thus, the essential requirement allowing the handicap principle to work is in place.

As explained by its originator (Zahavi, 2003, p. 860), the handicap principle "suggests that if an individual is of high quality and its quality is not known, it [the individual] may benefit from investing a part of its advantage in advertising that quality, by taking on a handicap, in a way that inferior individuals would not be able to do, because for them the investment would be too high." According to this line of reasoning, costliness is essential to the evolution of honesty. Honest signals have

evolved because they take forms that require considerable cost to produce, a condition that would result in ineffective communication if the sender could not bear that cost. Zahavi (1975) referred to the costly signals as handicaps and his theory thus came to be known as *the handicap principle*. Shortly after formulating his theory, Zahavi found himself debating the logic of the handicap principle with mathematicians and theoreticians (Arnold, 1983; Davis & O'Donald, 1976; Kirkpatrick, 1986; Maynard Smith, 1976), who could not prove the handicap principle with genetic models and therefore rejected it. The simple argument of the handicap principle was deemed overly intuitive and the skeptics insisted that the development of mathematical models was necessary to demonstrate the feasibility of its operation in evolution. In 1990, the Oxford biologist Alan Grafen (1990a, 1990b) successfully formulated the required model and made the handicap principle acceptable to mathematically minded evolutionary biologists. He also demonstrated that signals need be honest only on average to be evolutionary stable. Zahavi (1987, p. 319) had already put forward this idea three years earlier, recognizing that deception may be possible, but only if there is a limit to the frequency of bluffing so that receivers, on average, benefit from trusting the signals:

I do not claim that cheating is never encountered in nature. Several types of mimicry seem to provide false information. It is interesting to note that in most cases mimicry is concerned with a third party mimicking a communication channel that has evolved due to the honest interaction of other parties. Such cheating can only exist when the toll it levies on that communication channel is kept within limits that render uncovering it too costly.

There are several well-documented examples of such a mixture of honest and deceptive signals in nature (Számádó, 2000). In all of these cases, the cheating exists because its incidence is low enough for receivers on average to benefit from the interaction.

The notion of honest signals as costly handicaps has gained considerable backing and interest in recent years (Johnstone, 1995). Moreover, the handicap principle has proved useful for unraveling an array of biological and anthropological puzzles, such as the extreme expenditures often involved in sexual advertisement, the evolutionary mystery of animal altruism, the workings of collaborative systems in the animal kingdom (Zahavi & Zahavi, 1997), human foraging (e.g., Bliege Bird, Smith, & Bird, 2001; Hawkes & Bliege Bird, 2002; Smith, Bliege Bird, & Bird, 2003), decoration of the human body (Zahavi & Zahavi, 1997), and the evolution of art (Zahavi & Zahavi, 1997). The handicap principle has also provided an explanation for threatening behavior. Zahavi and Zahavi (1997) noted that rivals rarely attack each other without initially signaling their intentions. Indeed, most of the time, they do not attack

at all. Instead, the conflict is typically solved by the exchange of threats. Zahavi and Zahavi pointed out that all living creatures that communicate in any way make use of threats. Resolving a conflict simply by threatening prevents the loss of time, energy, and the risk of injury or death. It is obvious what the winner gains from threatening rather than fighting, but why should threats alone make the other party back down? What convinces one of the rivals to give up without a fight? Maynard Smith and Parker (1976) proposed that if one is going to lose anyway, it is better to lose without being defeated in a fight. But how does one know that one is going to lose? What convinces one of the rivals that defeat is inevitable, or that the possible returns from winning are not worth the risk of fighting? Zahavi (1977) answered these questions by proposing that threats are reliable indicators of each rival's chance in a fight. Threat displays communicate reliable information about one's opponent's ability and willingness to fight. Assessing such information against one's own ability and willingness provides a good idea of one's chances in a fight. If the chances are slim, then one had better give up the fight and back off. Yet, how can threat displays work in this way? Why can the party who is most likely to win a fight threaten more effectively than the other? Zahavi (1977) proposed that, in order to function in this way, the threat itself must increase the risk that the threatening party will be attacked or will be at a disadvantage if attacked. An individual who is genuinely willing to fight and has confidence in his own abilities will accept such a risk, whereas another, who lacks the requisite strength or motivation, will find the stakes too high and thus be unwilling to threaten to the same extent. In Zahavi's (1977, p. 256) words:

The use of a threat signal which endangers the threatening individual, in correlation to the magnitude of the threat signal, deters fighters of poor quality from threatening too much. Only the high quality fighters may threaten without great harm to their potential as fighters.

Latour (1987) was surely right to argue that attacking a text full of references requires weakening the documents it cites. However, like the coiled body of an animal, the cited documents of a citing text are a sign of confidence (Nicolaisen, 2004). A stack of references is a handicap that only an honest author can afford. Like a jutting chin, it presents an inviting target for a bold opponent. Modalized references expose themselves like the vocalization of a bluffing rival. A skilled rival will detect the false note right away and know where to attack. The potential cost of such a move will often make the author reconsider his deceitful behavior. When the references are made in public, the stakes are raised even higher. Like a shouting human, references may have witnesses. Yet, only a confident author can afford to shout before the crowd. Authors who are uncertain of themselves will usually not risk the

potential loss of reputation. However, in keeping with Zahavi's theory, Nicolaisen (2004) does *not* propose that all references are honest, for there are enough cases of fraud and deceit in science and scholarship to falsify such a proposal. Rather, he suggests that the handicap principle ensures that citing authors honestly credit their inspirations and sources to a tolerable degree—enough to save the scientific communication system from collapsing.

The validity of the handicap principle has not been discussed much in the information science literature: the only example, other than Nicolaisen (2004), seems to be Kock and Davison (2003). However, Zahavi's theory has received ample discussion in the literature of evolutionary biology. A fairly recent critique of the handicap principle is that of Hagstrom (2002).

Symbolic Characteristics of Citations

Van der Veer Martens (2001, online) considers “the current ‘holy grail’ in scientometrics” to be the development of indicator theories rather than the development of theories of citing behavior. Indicator theorists are concerned with the symbolic characteristics of the citation and its indicative abilities. They seek to understand how citations reflect and represent science—not the reasons why authors cite: Small's (1978) pioneering theory about highly cited documents being “concept symbols” is an important example. One of the most fervent advocates of this line of research is Wouters, whose notion of a reflexive citation theory has been presented in his Ph.D. thesis (Wouters, 1999b) and in two articles (Wouters, 1998, 1999a).

Wouters's Reflexive Citation Theory

Wouters (1999b, p. 211) considers the quest for a citation theory that seeks to explain the citation by relating it to the citing behavior of the scientist to be “a dead end”; therefore, we must abandon this pursuit and, instead, focus our attention on the symbolic characteristics of the citation and its indicative abilities. He sees citations as indicators constituting a “formalized representation” of science that initially neglects meaning (Wouters, 1999b, p. 209). However, in order to interpret these formalized representations, one needs to attribute meaning to the self-same indicators. According to Wouters (1999b, p. 209), “this attribution of meaning can be postponed” and should be based not on the citing behavior of the citing scientists, but on how citations reflect the characteristics of science. Wouters (1999b, p. 213) entitles this “the reflexive citation theory.”

Wouters's theory rests on his interpretation of the *reference* and the *citation* as two different signs. Others have noted the technical difference between the two, but Wouters considers the difference to be fundamental:

The reference is completely defined by the citing text it belongs to and the cited text to which it points. In semiotic terms the reference is a sign—the elementary unit of a representational system with the cited text as its referent. ... The citation is the mirror image of the reference. ... By organizing the references not according to the texts they belong to, but according to the texts they point at—they become attributes of the cited instead of the original, citing text. Semiotically, the citing text is the referent of the citation. (Wouters, 1998, pp. 232–233)

Wouters thus sees the citation as a new sign—one that is different from the reference upon which it builds. Unlike the reference, the citation is dimensionless and meaningless (Wouters, 1999b, p. 209), and acquires meaning only at the hands of the citation analyst: in other words, the (ISI) indexer's desk, not the scientist's, is the birthplace of the citation. Studies of scientists' citing behavior, therefore, facilitate the explanation of patterns of references, not patterns of citations:

Since the citation and the reference have different referents and are actually each other's mirror image, it does not seem very wise to blur the distinction between them. This distinction has moreover the advantage that the quest for a citation theory in scientometrics and the sociology of science splits into two different, analytically independent research problems: the patterns in the citing behaviour of scientists, social scientists and scholars in the humanities on the one hand, and the theoretical foundation of citation analysis on the other. (Wouters, 1999b, p. 195)

Wouters's theory reflects the main idea of *informational semantics*, a family of theories seeking to provide a naturalistic and reductive explanation of the semantic and intentional properties of thought and language. Basically, the informational approach explains truth conditional content in terms of causal, nomic, or simply regular correlation between a representation (a signal) and a state of affairs (a situation). Signals may be reliably correlated with the situation and hence indicate that situation.

The central work of informational semantics is that of Dretske (1981). His point of departure was Shannon's (1948) theory of information, which provides a mathematical measure of the amount of information carried by a signal. Dretske (1981) supplemented Shannon's work with an account of the meaning that a signal carries. His idea was that a signal carries the meaning p if and only if the signal naturally means (that is, indicates) p , as is the case, for example, when smoke indicates fire. Thus, the main idea of informational semantics is to ignore the actual history of a signal and to focus on what the signal

reliably indicates. But informational semantics faces a major problem that has been noted by a number of commentators (e.g., Devitt, 1991, 1998; Godfrey-Smith, 1989, 1992; McLaughlin & Rey, 1998): It does not allow for error. Informational semantics cannot explain how a representation can acquire a determinate content and yet be false. This problem arises precisely because informational semantics holds that representation is a kind of correlation or causation. As Godfrey-Smith (1989) has asked, how can a representation be caused by, or be correlated with, a state of affairs that does not obtain?

Devitt (1991) provides a good example of the error problem. Occasionally, he says, we see a muddy zebra but misrepresent it by thinking "horse." So, some zebras are among the things that would cause "horse" signals. What "horse" is reliably correlated with is really the presence of horses, muddy zebras, odd cows, etcetera. Thus, it should refer to horses, muddy zebras, odd cows, and so on. To solve this problem, the informational semanticist claims that the circumstances in which muddy zebras and odd cows cause "horse" are not appropriate for fixing its reference—that is, a "horse" signal represents what such signals are caused by in normal circumstances. However, as Godfrey-Smith (1989) has noted, this solution raises another problem for informational semantics, the problem of providing a naturalistic account of *normal instances*.

The error problem is logically unsolvable and, thus, any theory of representation that depends on reliable causation is doomed. The reason is simply that certain recognition of *p* is impossible. Misrepresentation is common—*q*, *r*, *s* ... are often confused with *p*. For instance, in nature it is common for an organism to represent the presence of a predator when none is there. Thus, what it indicates is mostly not what it represents. According to Devitt (1991, p. 434), this situation is common in nature because it has an evolutionary payoff:

Consider the typical bird that is the prey of hawks. A high proportion of the time that it registers the presence of a predator it is wrong; it has responded to a harmless bird, a shadow, or whatever. These false positives do not matter to its survival. What matters is that it *avoid false negatives*; what matters is that it registers the hawk when there is one. The price the bird has to pay for that is frequently registering a hawk when there isn't one. What nature has selected is a *safe mechanism* not a *certain* one.

However, informational semantics seeks not to provide safe mechanisms, but to uncover signals that are reliably correlated with specific situations and hence indicate these situations. In other words, informational semantics attempts to establish certain mechanisms, but only safe ones are logically possible.

Wouters's reflexive citation theory suffers from the same unsolvable problem as informational semantics, for it cannot handle false positives, that is, citations that do not in fact indicate the situation they are spontaneously understood to indicate. Wouters himself does not attempt to determine the correlatives of citations, but the bibliometric literature is loaded with such attempts. Perhaps most notable is the attempt to show that citations are indicators of quality. This approach posits that citations and research quality go hand in hand and thus are linearly related. Studies of the predictive validity of citation analysis have consequently sought to demonstrate the existence of a linear relationship between research quality and citation counts.² However, most of these studies suffer from a number of problems (Nicolaisen, 2002). Their biggest problem is that they have often focused narrowly on the opposite extremes of citation distributions. Only a few studies have dealt with entire citation distributions. These have documented low to moderate degrees of linear correlation (e.g., Gottfredson, 1978; Schubert, Zsindely, Telcs, & Braun, 1984; Virgo, 1977; Wolfgang, Figlio, & Thornberry, 1978). Moreover, Bornstein's (1991) hypothesis about a J-shaped relationship between research quality and citation counts has recently received empirical confirmation (Nicolaisen, 2002). It thus seems justified to conclude that not all citations are indicators of quality. But what, then, do they indicate? Garfield (1979) tried to answer this question in his book *Citation Indexing*. In his view, citations do not indicate elegance, importance, quality, or significance. Rather, they are indicators of utility and impact.

A highly cited work is one that has been found useful by a relatively large number of people, or in a relatively large number of experiments. ... The citation count of a particular piece of scientific work does not necessarily say anything about its elegance or its relative importance to the advancement of science or society. ... The only responsible claim made for citation counts as an aid in evaluating individuals is that they provide a measure of the utility or impact of scientific work. They say nothing about the nature of the work, nothing about the reason for its utility or impact. (Garfield, 1979, p. 246)

What Garfield established was nothing other than a safe mechanism. A citation indicates that a cited work has been referred to, and used by, a citing work—nothing more, nothing less. He expressly avoided claiming any other correlation between citations and the world—a clever move in light of the error problem.

Conclusion

Garfield's introduction of the *SCI* in 1963 marked a very important stage in the history of information science. The unique possibility of

retrieving documents according to received citations represented a significant improvement on previous term-based retrieval techniques. Moreover, as Hjørland and Kyllèsbech Nielsen (2001, p. 257) have noted, citation-based retrieval has changed our understanding of the concept of subject relatedness and subject matter. However, as Small (2000, p. 451) has correctly observed, the *SCI* did not invent the citation, as Wouters seems to think, any more than the dictionary invented words. The citation is just the mirror image of the reference. Thus, if we are to understand the nature of the citation, we need to understand the nature of the reference. And if we are to understand the nature of the reference, we need a theory of citing that explains why authors cite the way they do. Ignoring the reference (i.e., ignoring the history of the citation) in order to understand the citation is logically impossible. Moed (2005, p. 216) seems to arrive at the same conclusion in his recent monograph on citation analysis: "Reference and citation theories, although analytically distinct, should be grounded in a notion of what scientists tend to express in their referencing practices."

The quest for a theory of citation that seeks to explain the citation by relating it to the citing behavior of the scientist is not a dead end. It is, on the contrary, the only way forward if we are to realize the full potential of citation analysis. A good deal of the previous research on citing behavior has provided only a few pieces for solving the citation puzzle. Studies have tended to rest on the assumption that citing is best understood as a psychological process and that theories of citing should be constructed from studies of individual citers that utilize interview techniques, thinking aloud methods, or the recording of behavioral patterns. This line of research has produced a number of classification schemes capturing various reasons for citing. But these are, as Baldi (1998, p. 832) and Cronin (1994, p. 537) have correctly observed, only of limited use.

In his recent book on academic writing and its rewards, Cronin (2005, p. 154) referred to a number of the studies dealt with here and concluded that "we are still left with a black-box explanation of citing behavior." This chapter has sought to make clear that, in order to explain such behavior, we must cease taking the individual's knowledge structures as our starting point. Rather, we should focus our attention on knowledge domains, disciplines, or other collective knowledge structures. Attempts to explain citation behavior should thus refrain from psychologizing the act of citing and instead recognize it as embedded within the sociocultural conventions of collectives.

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Endnotes

1. See also the studies by Stewart (1983) and Van Dalen and Henkens (2001), which report minor impact of author reputations on citation frequencies.
2. One of the reviewers commented that s/he was not aware of anyone having claimed that there is a linear relationship between research quality and citation counts and, thus, that a straw-man argument was possibly being set up here. This is, of course, not the case. The vast majority of studies of the predictive validity of citation counts have made use of linear regression analysis as a measure of the covariation between research quality and citation counts. A basic premise of this test is that the dependent mean of *Y* is assumed to be a linear function of the values of the independent variable. Thus, by employing this test, a number of analysts have implicitly assumed that there indeed exists a linear relationship between research quality and citation counts.

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