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Referencing as Cooperation or Competition

1 The Citation Process Revisited

Blaise Cronin's book *The Citation Process* (1984) gave the information science and bibliometrics communities their first major statement on citation theory. It framed the theoretical discussion around the two principal approaches to science studies at the time, the Mertonian normative view and the post-modernist social constructivist view. The book saw the conflict in sharp terms: between those who espouse a positivist, normative, and aggregationist view of citation, and those who see it as subtle, individualistic, and a product of research-in-practice, where norms carry little or no weight.

Rereading *The Citation Process* after a hiatus of 30 years recalled feelings of both excitement and depression. I was energized by these tentative theoretical steps, but disheartened by his suggestion that we should follow the lead of the social constructivist. As someone trained in science and the history of science, the constructivist view did not ring true. Perhaps I was stuck in my story-book version of science. In any event, the bibliometrics community ignored the new sociology and remained largely empirical and atheoretical.

In a later paper, Cronin (1998) revisited the problem of citation theory in a way more congenial to me, proposing a middle ground between the normative and constructivist approaches, and then later even seemed to return to a more normative position in the context of semiotic theory (2000).

This long interlude of three decades saw many shifts in the positions of major players in the contending camps, among these, the split between the bibliometricians and constructivists with the formation of ISSI in 1993 and the burgeoning of the 4S society. My purpose in revisiting this debate is to better understand the fundamental issues it raised regarding the nature of science, the shortcomings of both constructivist and normative theories, and to suggest some possible ways forward based on recent theories in evolutionary biology which give a rationale for cooperative behavior.

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2 Social Construction and Its Problems

One of the fundamental tenants of social construction is that science does not have direct access to the “reality” of the external world. Rather, that access is so thoroughly mediated by devices, presuppositions, and social constraints that our knowledge is not about the external world at all. Furthermore, what knowledge we do have is relative to the framework and circumstance in which it is created, and no one framework is more valid or closer to reality than any other. In Latour and Woolgar’s (1979) view, scientists in the lab are engaged in processes of inscriptions that lead to the creation of a “mythology.” Inscription devices in the lab act as black boxes allowing social interactions to dictate their output: “There thus occurs a transformation of the simple end product of inscription into the terms of the mythology which informs participants’ activities” (Latour & Woolgar, 1979, p. 63).

From the “strong program” of the Edinburgh school, we have the view that scientific knowledge is no different from any other belief system, including those of primitive cultures, and the notion that “belief systems cannot be objectively ranked in terms of their proximity to reality or their rationality” (Barnes, 1974, p. 154). Knorr-Cetina, in *The Manufacture of Knowledge* asks (1981, p. 2), asks: “Why should our interest-geared, instrumentally-generated world order mirror some inherent structure in nature?” She claims that the lab is an artificial and constructed environment, and this social environment and its instruments manufacture what we call science, which bears no necessary relationship to the world.

According to Latour and Woolgar, the ultimate aim of the lab is the production of papers. In writing papers the task is to persuade fellow scientists and the outside world that the mythology they have created in the lab is valid. It is impossible for facts and theories to be convincing on their own because, as myth, they can only be made convincing by rhetorical means.

Knorr-Cetina sees the scientific paper as yet a further construction with its own unique arguments and end products which bear little or no resemblance to what went on in the lab. In fact: “Compared with the work observed in the laboratory, the written paper is … a first complete perversion” (Knorr-Cetina, 1981, p. 132). One reason for this is that the paper does not reveal the more or less irrational and haphazard path that went on in the lab. The paper constructs a different “logic” and sequence of events for the benefit of various audiences. Knorr-Cetina’s view is echoed by the rhetorician Ken Hyland who states: “Texts … can never be regarded as accurate representations of what the world is like … Reality is constructed through processes that are essentially social and involve crafting texts in ways which will be persuasive to readers” (2009, p. 12).

The message is that we need to analyze the various rhetorical devices that scientists use to convince (i.e., mislead) readers into thinking that the writer is describing the physical world. As Swales puts it: "The art of the matter, as far as the creation of facts is concerned, lies in deceiving the reader into thinking that there is no rhetoric … that the facts are indeed speaking for themselves" (1990, p. 112).

To show how "facts" are created in the lab, Latour introduces the idea that there is a gradation in the so-called "facticity" of knowledge-claims. This is embedded in statements scientists make about these claims and is marked by the use of "modalities" which, to varying degrees, cast doubt on or boost the credibility of given assertions. Modalities are expressed by words or phrases such as "premature," "suggested," "reported," "first described," "assumed," "confirmed," etc. The goal of the lab is to move statements from low to high facticity by rhetorical persuasion such that all modalities disappear from the language. "Facticity" does not mean that some fact about the real world has been established, only that actors view it as such.

Latour introduces a theory of citation in *Science in Action*. In the citation context, the text surrounding the reference, modality words and phrases are deployed either to strengthen those who support the author or weaken the opposition. References are massed as opposing armies on a battlefield. In citation warfare all distortions of the meaning of the prior text is fair game: "… do whatever you need to the former literature to render it as helpful as possible …" (1987, p. 37) and "… all deformations are fair" (1987, p. 40). Referencing becomes part of the process of persuasion in which one mythical version of the world is promoted over another.

Constructivist theory is now under attack from within, in part, because it has been hijacked by reactionary groups that invent their own versions of science (Latour 2004; Collins, 2014). Nevertheless, it is useful to review some of the objections that have a bearing on a theory of citation.

One problem is that fraud or error in science would be indistinguishable from any other type of science. If facts are socially constructed, then error and fraud are as well. Failure to replicate or corroborate an author's result would carry no weight because these activities are not anchored in the real world. Replication or reproducibility is, however, the basis for the detection of fraud or error in science and is fundamental to the normative operation of science (Zuckerman, 1977; Hull, 1988, p. 435).

For constructivists there is no way of evaluating or comparing the relative merits of one group's scientific views over another group's because neither is based on reality. Each group can provide persuasive arguments for the validity of its beliefs. Kuhn (1962), however, rejected this extreme relativism and conjectured that there could be progress through scientific revolutions. Because of the increasing growth and specialization of science through a revolution "… both the list of problems solved by science and the precision of individual problem-solutions will grow and grow" (p. 169).

Constructivists rely on rhetorical persuasion (Gilbert, 1977) to convert non-believers to believers presumably because the evidence is not sufficiently compelling on its own. Rhetorical devices include hedging and purging personal motives (Swales, 1990, p. 112). However, if rhetorical persuasion is all that it takes to win converts, it is unlikely that Einstein's theory of relativity or Heisenberg's matrix mechanics would have gained many converts. Persuasion can also come about by presenting a coherent framework of theory and observations. In some cases, a new finding is compelling enough to bring about the emergence of a new research area (Small, Boyack, & Klavans, 2014). This is because rhetoric is weak compared to the perception of a new ordering of facts and theory (Cole, 1992, p. 47).

It could also be argued that the rigid form of the modern scientific paper (Swales, 1990, p. 134) militates against persuasive presentation. The conventional IMRD format (introduction, methods, results, discussion) forces papers to present their findings in a uniform manner which may facilitate their browsing, comparison, and registration in databases, but does not facilitate discursive argument (Zuckerman, 1977, p. 125). Unlike the earliest scientific reports published in journals such as the *Philosophical Transactions of the Royal Society*, the modern scientific paper is not designed to be a first-hand account of an author's observations in the lab and does not allow the "virtual witnessing" that Robert Boyle practiced (Shapin & Schaffer, 1985, p. 55).

Regarding Latour's views on referencing as a strengthening of allies and a weakening of enemies, there is little evidence that such a nuanced deploying of individual references has much effect on a paper's impact. Much more depends on the actual findings reported in the paper, on the data or theory it presents. Watson and Crick's 1953 paper on the structure of DNA had only six references, Darwin's *Origin of Species* relatively few, while Einstein's 1905 paper on special relativity contained no references at all. A careful deploying of modalized references is not enough to make a trivial paper convincing.

3 The Realist Alternative

The idealist position espoused by the constructivists can be contrasted with the realist position that we are observers of a world which we have access to through our senses (Leplin, 1984). We extend the power of our senses by inventing instruments and analytical methods that magnify or probe our environment. The instruments are not the black boxes of the constructivists because they obey the same physical laws as the thing being observed. To understand the world, we make systematic observations, and invent hypotheses and theories to explain our observations. When our observations and theories agree, we claim to have discovered something about the world, but discoveries are always tentative and subject to revision by more accurate observations or different theories that have better agreement with the observations. We can think of this tentativeness of knowledge as a norm which resulted from the numerous theories in history which turned out to be false.

Despite this rosy picture, the signals from nature are not always clear. They can be noisy, contaminated, ambiguous, and affected by our prejudices, preconceptions, biases, and expectations. When an experiment is successful, it is not always apparent that it is until it can be replicated or corroborated by other experiments or with theory. Scientists can disagree about the meaning and interpretation of these signals. The realist response is to call for more research, and scientists are always thinking about their next experiment.

Most scientists committed to a theory are aware of the possibility that new evidence might prove them wrong. But the amount of effort required to develop and test a theory requires a level of commitment that makes it difficult to keep an open mind to alternative theories (Mitroff, 1974). However, what scientists work toward is a plausible basis for further research, and this may involve abandoning one hypothesis in favor a more plausible one. Latour's levels of facticity are also relevant in a realist approach as a means of sorting the plausible from the implausible. Thus, an important question is what makes some theories more plausible than others?

The colloquialism "the facts speak for themselves" expresses the common sense notion that the plausibility of hypotheses depends on their degree of fit with the existing body of facts and theories, like the fitting together of pieces of a puzzle. For example, quantum mechanics fits with the ionization potentials of simple atoms. The double helix model of DNA fits with the concept of genetic replication. Human-caused global warming is consistent with an increase in the burning of fossil fuels and more carbon dioxide in the atmosphere.

Each of these examples of consistent theory and observation can be expanded into networks comprised of many elements that fit together in a larger puzzle. For example, the quantum mechanical theory of the atom is also consistent with the electromagnetic spectrum of the atom and the quantum nature of light. Anthropogenic global warming is consistent not only with rising carbon dioxide levels in the atmosphere, but also with the decrease in energy being re-radiated back into space, and the wavelength of the main component of re-radiated energy is consistent with the energy spectrum of carbon dioxide. A similar point was made by

Gingras and Schweber in their critique of a social constructivist account of quarks (1986, p. 379). In theory choice, scientists consider a network of multiple facts and predictions, not single facts in isolation.

The general approach to fitting facts and theories together is called consilience and derives from the work of the 19th century philosopher and historian William Whewell (1847) recently made popular by E. O. Wilson (1998). Whewell wrote: “The consilience of induction takes place when an induction, obtained from one class of facts, coincides with an induction obtained from another different class” (1847, p. 469). A related network approach called “explanatory coherence” has been proposed by Paul Thagard (1992; 2007). In this approach various types of coherence relations are treated as constraints and the network having the highest constraint satisfaction is considered the most likely to be true, or at least, the best one given the currently available facts and theories. Explanatory coherence provides an approximate guide to theory choice and a way of understanding the history of science.

In writing scientific papers, authors are also engaged in fitting together the pieces of a large puzzle that represents the problem space of their research area. Some of this fitting together (but not all) is evident in the references authors cite which associate facts, theories, or methods with prior papers. In fact, Thagard applies his explanatory coherence to the process by which scientists arrive at a consensus which, he notes, requires communication among researchers (2000, p. 223).

Referencing is a passive form of communication between the cited and citing authors so we can use this to illustrate consilience. Citation contexts, that is, the portions of text where the papers are referenced, can reveal how authors see the prior literature. For example, a citation might represent the linking of a cause and effect, where the effect is an observation and the cause is a deduction from theory (Hanson, 1972).

To illustrate how a scientific paper builds a network of coherent facts and theories through its references, we use a paper that was part of a co-citation cluster from 2007 on the water pollution by estrogens (Small & Klavans, 2011). Focusing on a single randomly selected citing paper (Thorpe, Benstead, Hutchinson, & Tyler, 2007) from this cluster, all the contexts were extracted in which references were made (see Table 1).

The 23 contexts are arranged sequentially by the section of the paper. Content words have been removed, leaving the non-technical or general words. This allows us to see more clearly the function of the reference in the authors’ presentation. The cited references within each context are denoted by integers in square brackets. Some contexts reference multiple items, so-called redundant references, and some items are cited repeatedly throughout the paper, the op. cit. references.

Tab. 1: Reference structure of a scientific paper: citation contexts stripped of content words.

Introduction	
1.	It is now well-established that … may impair … with potential detrimental consequences [5, 7, 8, 9, 13, 14]
2.	… implicated as causative [3] … were not previously subject to routine monitoring due to …
3.	In this regard … has been widely employed … that induce … response [23]
4.	… that is produced … in response to … [29, 17, 27]
5.	… but exposure to … has been shown to result in … [23, 29]
6.	There is some evidence that … production … as a consequence of … [6, 11, 21, 31, 4, 22, 10] but the implications of … are less clear.
7.	Even less is known on the consequences of … although an association between … has been reported [11] …
8.	… provide the majority … [30] and therefore … could potentially impact on …
9.	… but is of considerable … relevance and was therefore used in … to expand on the earlier work of [11] who investigated for association between …
Materials and Methods	
10.	… basic design … same … replicate treatment in experiment … [26 self]
11.	… samples were assayed … using [28]
Results and discussion	
12.	… is consistent with reports from earlier investigations using … [24 self, 19] and indicates that … significantly exceeds …
13.	This supports the results of an earlier investigation reporting … [20]
14.	… it has been hypothesized that … is thought to result in … [6, 21]
15.	Indeed, a number of investigators have reported … effects … which are hypothesised to result from … [11, 1, 12, 22, 10]
16.	The effect … [25 self] shows …
17.	This compares with previous observations where … were linked with … effects … but not in … [11, 1, 12, 22,10]
18.	It has previously been demonstrated that … are associated with … results in … leading to … [4].
19.	The observed … here is consistent with a previous investigation [2] …
20.	This may reflect alterations … due to … and supports the earlier work of [2] who showed … through … examination of …
Conclusions	
21.	The collective results from these investigations support an earlier investigation [11] in demonstrating that … signals … adverse … impact …
22.	The poor ability … however could lead to … that have adverse health effects during … as can occur … and should be considered further [16, 18]
23.	This further supports the work of [11, 15] in demonstrating that … could potentially be used to signal for adverse … health effects.

For example, the first context in the Introduction refers to "well established" facts that potentially lead to detrimental consequences. In other words, the established facts form a coherent picture and together with theory predict certain undesirable effects. In the ninth context earlier work was expanded on because an association was found. Associations suggest possible causes. The contexts in the introductory section emphasize causation using words such as "causative", "induce", "in response to", "result in", etc. Words like "may", "could", and "potentially" act as hypothetical connections that require further investigation.

Contexts in the Results and Discussion section focus mainly on documenting the consistency of the paper's results with empirical findings and hypotheses of earlier papers. Words such as "consistent", "supports", and "compares" build a web of relationships between the authors' current results and the previous literature. These relationships differ from the cause-effect relationships of the Introduction in that they are mainly about similarities and parallels between results by different investigators.

Instead of focusing on all contexts from a single paper, we can also look across all contexts at a particular cited reference. In the selected paper, reference #11 had the highest op. cit. rate, suggesting that it was of particular importance to the authors. There were a total of 16 contexts for reference #11 across the 868 contexts from all papers in the sample. We then selected the most characteristic of these contexts by computing the cosine vector similarity of each of the 16 contexts against a composite of all 16. The context with the highest cosine vector (0.61) was the seventh context in Table 1, and is thus the most representative of the 16:

> Even less is known on the consequences of disruptions in VTG dynamics in females and although an association between VTG induction and reduced egg production has been reported [11], the effects on egg production were only observed at concentrations that were toxic to males.
>
> (Thorpe et al., 2007, p. 177)

Thus, this most typical context reports an association, or consilience, between three observations: VTG induction, egg production, and toxicity to males. These examples show that references serve to link observations with theory and effects with varying degrees of certainty.

4 The Problem of Norms in Science

What is missing in the previous discussion of the coherence model of plausibility is what establishes the acceptable forms of coherence between the pieces of the puzzle. A few of these noted already are the degree of fit between theory and experiment, the accuracy of theoretical predictions, and the qualitative agreement of a model and the evidence. Thagard mentions explanation, deduction, and association (2000, p. 17). But where do these criteria come from?

One answer is that these are technical or epistemic norms analogous to the norms that govern the social conduct of scientists proposed by Robert Merton, such as, universalism, communality, disinterestedness, skepticism, and originality. Merton considered social norms necessary for the “extension of certified knowledge” (1973 p. 270). Among the technical methods that allowed the creation of certified knowledge were empirical confirmation and logical consistency. The social norms supported the goal of certified knowledge.

Merton, however, does not spell out the “technical methods” that should govern scientific practice, or where they came from. These are discussed in greater detail in a long article by Zuckerman (1977). She called them “cognitive norms and methodological canons” (1977, p. 87), and related them to the philosophical concept of demarcation, that is, the rules that define what it is to be scientific.

The problem with both technical and social norms is that they appear to come out of nowhere, although Merton’s early writings (1938) suggest a possible link to the Puritan values held by many members of the Royal Society in the 17th century. Zuckerman hints at the possible dynamic nature of norms when she discusses how the norm of disinterestedness, which encourages unrestrained pure research, comes into conflict with the social hazards of certain scientific findings (1977, p. 122). This suggests that under some circumstances restraints on pure research may be necessary. Thus, a dynamic and evolutionary theory of social and cognitive norms seems to be called for.

Mulkay rejects the idea that norms affect or control behavior. He sees moral precepts as embodied in Mertonian norms as flexible vocabularies that are invoked rhetorically to rationalize scientists’ interests (Mulkay, 1991, p. 69). Yet, just paying lip service to norms does not seem adequate to explain why so many scientists adhere to formal conventions and rules in their work, and how these conventions and rules arose and become sustained.

Technical norms could be collectively considered part of the “scientific method” (Gower, 1997) for a given historical period. Francis Bacon provided many examples of technical prescriptions for science including the method of induction, the gathering of systematic observations, and the conducting of experiments. Philosophers of science have proposed criteria on which to judge the adequacy of theories called criteria for theory choice. Kuhn provided a short list of what he considered key criteria or “values”: the accuracy of theoretical predictions, the consistency of the theory with other accepted knowledge, the ability of the theory to expand its scope to predict other phenomena, the need for a theory to be simple or parsimonious, and fruitful in generating new problems and solutions (1977, p. 322). Rather than seeing these as givens, however, it should be possible to trace their origins in history.

Technical norms clearly change over time and are subject to selection and extinction: what is acceptable or required in today's science is not what was acceptable or required in earlier historical periods. But norms probably change in a reactive rather than a proactive manner. As Hull puts it, "The nature of science is constantly under negotiation, and the currency of these negotiations is success" (1988, p. 297). The availability of a successful or highly visible social or technical practice might first become fashionable then later on required. That "accounts" should "save appearances" is one of the oldest technical norms perhaps having its origin in ancient Egyptian and Mesopotamian creation myths (Frankfort & Frankfort, 1949, p. 11).

Examples of technical norms that have gone extinct are the requirement that theories be consistent with the writings of the ancients, or with the teaching of the Church. A norm that emerged in the scientific revolution was that predictive theories should be mathematical in form. This norm was spurred by the success of Newton in predicting the motions of celestial bodies (and perhaps earlier according to Crombie [1959]). Likewise, Lavoisier's theory of combustion resulted in adoption of quantitative criteria in the explanation of chemical change (Kuhn, 1977, p. 336).

A technical norm of more recent origin is the notion of symmetry which was introduced in particle physics and relativity theory. Medicine has introduced its own norms such as double-blind clinical trials and evidence based medicine. Besides being induced by scientific discoveries or successes, it is likely that norms migrate from one branch of science to another. An example is the diffusion of statistical methods to various disciplines. Practical innovations can also be exemplars for new epistemic norms. For example, as new and more accurate scientific instruments are introduced, the standards of measurement increase and higher precision becomes required.

A potential difficulty in theory selection arises when a new discovery stimulates the adoption of a new norm which is then used to rationalize the discovery (Kuhn, 1977). It is not clear how often this situation arises, but it may account for the delays in acceptance of some theories, such as relativity or string theory where radical new ways to understand the world are proposed which are not easily testable. However, if the theory has multiple confirmatory paths, some of which rely on traditional criteria, this circularity is less problematic.

Mulkay argues that scientists often justify their behavior using a wide range of rules—some of which are contradictory—and their behavior can contradict their own stated rules (1980; 1991). He also notes, referencing Kuhn (1977), that when technical norms are applied to theory selection, different scientists can arrive at different choices. It is not difficult to envision situations where norms come into conflict, for example, when an author attempts to publish a paper that violates technical norms. Merton (1963) recognized that conflicting norms create ambivalence towards them, for example when originality conflicts with humility. The complex and contradictory nature of norms does not, however, invalidate their importance.

The social norms discussed by Merton under broad categories probably also evolved from exemplars of good practice or as reactions to new social realities. For example, the norm of universalism may be related to the rise of distinctive styles of national science in Europe (Ben-David, 1984), and the need to assert that scientific findings are valid across national boundaries. The invention of the scientific journal in the 1600s may have crystallized the norm of communality as well as numerous publishing conventions as the medium evolved.

Norms also carry different weights and are associated with varying degrees of sanctions, and the importance of a norm and the sanction that accompanies its violation would likely change over time. For example, the norm of openness is probably more important today than it was the in 17^{th} century when many scientists kept their discoveries secret for fear of not receiving proper credit.

The norm of honesty, which falls under Merton's category of disinterestedness, however, carries a more severe sanction. Fraud, if proved, can jeopardize the scientist's career. Without adherence to the norm of honesty the scientific community would probably cease to function. Scientists could no longer trust one another and would lose the support of the larger society (Zuckerman, 1977). The norm of honesty in reporting scientific results may derive, in part, from the impracticality of eye witnessing experiments (Shapin & Schaffer, 1985) and was a necessity if scientists were to work independently.

5 Norms and the Scientific Paper

Central to the integrity of science is the connection between what is done in the lab and the final written scientific paper. Knorr-Cetina has shown that the relationship between what happens in the lab and the final report for publication is complex (1981, p. 94). Results are selected, not reported as they actually happened, and the argument may be reframed for various audiences. However, through these transformations the author must take care not to misrepresent his or her results. In some labs this norm is enforced by requiring the maintenance of lab notebooks and diaries which can be reviewed in cases of suspected misconduct (Gaulton, 2004). What is missing from Knorr-Cetina's account is the powerful effect exerted on the authors by both social and technical norms. The social norm of honesty is the most important, but many other norms and conventions govern the form and content of the paper, its style, sections, and references. Failure to follow generally accepted technical norms and conventions may jeopardize publication.

We could speculate about what the scientific "paper" would look like if we stripped away the norms and editorial conventions. Loss of a standardized format or organization would make science appear more "literary" or perhaps autobiographical. The personalization of the paper would probably result in fewer collaborators and co-authors. The paper would likely not start with a review of the state of knowledge with references to prior literature. The most dramatic effect would be loss of trust. Authors would have no need to tell the truth, and would not be held accountable. Readers would no longer have confidence that the author actually observed what was observed or did what was said was done.

In 1988, Merton explicitly discussed referencing as a normative constraint in science as part of the "composite cognitive and moral framework" (p. 622) which had historically evolved. He pointed to its main function as a "moral obligation to acknowledge one's sources" and explained its origin as a response to the social problem of plagiary in the 17th century. In 1965, Kaplan noted that there were few if any normative guides for citation practices in the available style handbooks. Nowadays we find many such guides and prescriptive texts (e.g., see Kamat & Schatz, 2014).

So how do we know that a norm of citation is operating and has an effect on behavior? Referee reports and letters to the editor are filled with complaints that author X has failed to cite author Y (Retraction Watch, 2014; Hagstrom, 1974). One kind of evidence is psychological discomfort: an author's real or imagined embarrassment on failing to cite an obvious precursor (Wilson & O'Gorman, 2003). Perhaps worse than the guilt the author may feel is concern that the omission will be found out by colleagues. And there is always the nagging feeling that somewhere in the literature another author has made the same point and the feeling of relief when a literature search fails to find anything of relevance. Sanctions can be psychological as well as social.

As Merton suggests, the social norm of referencing perhaps began with scholars wanting to lay claim to their ideas and avoid priority disputes. At first, the only option was to keep their ideas secret, deposit sealed notes or anagrams, and refrain from publication as Newton and others did. With the advent of the scientific journal, scientists were able to disseminate and date their ideas. Thus, the journal acted as a registry of their contributions. The author could then point to this registry if a question of priority arose and this function gradually evolved into the formal bibliographic reference. Thus, we would expect that the early referencing would be skewed toward self-citations, and that self-citations would be more complete in terms of specifying the cited item than citations to others, and there is some evidence that this is the case (Small, 2010).

The reference format has evolved over time as shown by studies of journals such as the *Philosophical Transactions of the Royal Society of London* which began publication in 1665 (Allen, Qin & Lancaster, 1994). Early references were usually embedded in the text, and often consisted only of an author name (italicized or bolded), and, occasionally, a source. Later, references became more complete, giving pages, years, etc., and moved from embedded text to side notes, footnotes, and finally endnotes.

Normative expectations probably evolved along with these changes in printing and format. In addition to allowing the ownership of ideas and discouraging plagiarism, referencing became a tool to carve out a niche for your idea by demonstrating that it was different from those of other authors (Gilbert, 1977)—effectively an extension of knowledge claiming.

Another evolutionary thread developed around summarizing the current state of knowledge on a topic, what we would call a review of the literature. The tradition of reviewing prior opinions on a topic goes back to the writings of Aristotle (Small, 2010), and many examples of such proto-reviews can be found in the *Philosophical Transactions*. The review, while not a novel knowledge claim, can be a new synthesis and useful to others.

In referencing others either for differentiation or for review, we can speculate that the norm of generosity of referencing came into being. What was originally a defensive practice could also be used in a generous way to credit other authors for their ideas. From this point the practice evolved from being customary, to one that is expected and eventually required. Readers would then expect certain authors to be credited if a topic was reviewed or a related knowledge claim was made. Authors who failed to reference would be suspected of intellectual theft or, at best, ignorance, and referencing others became a norm of scholarly practice.

In contrast to this normative account, in the constructivist approach, references are only made for persuasive reasons motivated by self-interest. In this world authors would be less likely to cite prior work closely related to their own claim since it might jeopardize their own priority. Authors would be more likely to distort or misrepresent prior work to support their own point of view (Nicolaisen, 2007). Authors would be less likely to cite items that serve only to provide the reader with background information, and they would be more likely to self-cite. In addition, as White has argued (2004), constructivist authors would tend to cite leading figures in order to convince readers, but this was not empirically supported.

In constructivist citation contexts we would expect to frequently encounter modality terms that weaken or cast doubt on the cited work. However, studies of citation contexts have found the rate of negative citations to be relatively low, about 6 % over seven separate studies (Small, 1982). In addition, in a random sample of 265 citation contexts containing the word "not", it was found that in about 85 % of cases the citing authors were supporting a negative finding of an earlier author, and were not themselves directly negating a cited work, in effect a negation by indirection.

As we have seen, Latour's theory calls for a no-holds-barred approach to referencing. However, in a norm-governed publication world, misquoting or distorting a prior author's work would not be regarded with equanimity. These instances could be classified as "constructivist" (Small, 2004) and are relatively rare. Most references are normative in the sense of adhering to some literal message in the cited text. This is supported by the word similarity of citing and cited texts (Peters, Braam & van Raan, 1995).

This does not mean that a range of interpretations of the cited work is not possible. In fact, capsulizing, summarizing, and pigeonholing a prior text is part of the compacting of knowledge, the process of creating symbols for ideas (Small, 1978), and a step toward Merton's obliteration by incorporation (1968, p. 35). There is, in addition, a gray area between distortion and legitimate interpretation. This provides some room for reconciliation between normative and constructivist positions (Luukkonen, 1997) because differences of interpretation and debate are expected in cases where the signals from nature are ambiguous, or there is ambiguity in the cited text. Cozzens shows that interpretations of specific papers can differ within a field of science (1982). Cole (1992) also sees the lack of consensus at the research front as an area of potential agreement between realists and constructivists. Riviera (2013) uses normative theory to describe the phenomenon of high citation rate and constructivist theory to explain low rates. However, interpretations can also converge—as seen in the emergence of regularized language in citation contexts of highly cited papers indicating the formation of a consensus (Small, 1978). In such cases, the significance of the paper for a majority of citing authors is shared.

In the previous discussion we have shifted the focus from the citing side to the cited side and the formation of consensus. Here a reconciliation of normative and constructivist theories is less likely. For example, Mulkay (1980) argues that scientists do not apply technical norms in a consistent way, and the meaning of rules varies depending on the situation and who is applying them. In this view it is difficult to see how a consensus could emerge, and yet, citation studies have shown that consensus formation can be rapid and dramatic (Cole, 1982, p. 48).

A citing theory deals with individual decisions on what to cite and cited theory with aggregate citation phenomena and the perspective of a community. Since the sum of all the citing acts results in what we see on the cited side, it might appear that a theory of citing is all we need. The resulting distributions of citations are typical of cumulative advantage processes, or, to use current nomenclature, preferential attachment networks, where the number of future cites depends on the number already accumulated (Newman, 2010). To get such distributions there must be some kind of coordination of action among citing authors, an awareness of the references of other authors or a shared reaction to the cited work. Seeing that an author has referenced a particular paper may motivate other authors to read and cite the paper, but there also needs to be recognition of the paper's value or relevance (White, 2011). Of course, many social and intellectual factors could contribute to citation inequality. Following Thagard's (2007) theory, value may derive from better alignment of theory and observation or, following sociological theory, a higher degree of utility (Cole, 1982, p. 47; Hull, 1988, p. 301).

6 Generosity in Referencing

We tend to think of science as a competitive activity with scientists striving for priority, recognition, and funding (Hagstrom, 1974). But scientists also act generously and cooperatively by sharing work and collaborating. The Mertonian norms, of course, embody generosity in the norm of "communalism." In constructivism, by contrast, scientists are driven by self-interest. Mertonian referencing is generous in giving credit to others, but could such behavior also be motivated by self-interest? In biology, Richard Dawkins (2006) is known for his rejection of altruistic behavior, favoring selfish behavior at the level of the gene. However, others have argued that altruistic behavior—benefiting others at a personal cost—is reasonable both biologically and psychologically (Sober & Wilson, 1998). Evolutionary biologists and philosophers have long struggled with how cooperative behavior could have existed at all in the face of fierce evolutionary competition. Yet both cooperation and competition seem ubiquitous in human and animal societies. E. O. Wilson recently described the inherent conflict between wanting to behave competitively and cooperatively which has been hardwired in our genes by evolutionary forces (2014, p. 24).

The decision to cite or not to cite a finding similar to our own is a difficult one for authors who want to claim as much credit as possible. In some instances authors may not go out of their way to find others who have expressed similar ideas. When we reference others, we are giving up credit to others that could have, hypothetically at least, come to us (Small, 2004; Hull, 1988, p. 319). This is especially true when the cited work is intellectually close to our own. At first glance, this seems to be an act of generosity, a sacrificing of a portion of our originality to others. On the other hand, we are motivated to cite others whose work is similar to ours in order to demonstrate that our contribution is distinctive, and to avoid negative sanctions for failing to cite related work. Thus, whether referencing is a selfish or generous act is ambiguous. Sober and Wilson point out the hypothesis of generosity is difficult to prove because, regardless of the apparent selfless act, we can always think of some way the actor could have benefited.

Nicolaisen (2007) makes a related point inspired by a theory from evolutionary biology called the "handicap principle" or "costly signaling" (Zahavi, 1975). In nature, animals engage in behaviors such as ostentatious displays, bluffs, and mock threats which serve to enhance the fitness of the performer and protect the herd from predators. Hence, costly signaling is behavior that risks our own well-being for the apparent benefit of others and might be interpreted as generous or altruistic. Nicolaisen sees referencing as costly signaling because the author is taking a risk and going out on a limb which could easily be cut off if a diligent reader discovers that the reference is irrelevant or fallacious. Here he sees a connection to Latour's theory of citation which is based on self-interested manipulation of the prior literature. Hence, referencing is a handicap and a gamble in the interest of gaining advantage. Thus, although referencing may appear to benefit others, it is actually done out of self-interest, to advance our own interests. The handicap principle does not accord well with normative theory because the behavior is based on trickery and deception.

The handicap principle is one of a number of theories now current in evolutionary biology which may serve to stimulate further theorizing on citations and other issues in science studies. Two of the most relevant theories are "reciprocal altruism" and "strong reciprocity". "Reciprocal altruism" is the tendency to help those who are likely to return a favor (Arrow, 2007). But this form of altruism still has a selfish motivation. Reciprocal altruism would work for referencing only if the cited author is likely or capable of returning the citation. If the cited author is incapable of reciprocating, as is often the case, this mechanism fails.

In another approach called "strong reciprocity" (Fehr, Fischbacker, & Gachter, 2002) cooperators are rewarded and non-cooperators are punished but at a cost to those who punish (the strong reciprocators). Under certain conditions this model has been shown to lead to sustained cooperation in social groups. Unlike reciprocal altruism or costly signaling, strong reciprocity is consistent with the existence of norms which define what it means to cooperate, but it requires that someone is willing to sanction the norm violators.

One version of this model begins with a population of three types of agents: cooperators who obey the norms but do not punish, selfish agents who violate the norms, and strong reciprocators who obey the norms and punish violators (Bowles & Gintis, 2003). In referencing, we can imagine that the selfish agents do not cite others, the cooperators cite others, and the strong reciprocators cite others and "punish" the non-referencing violators. Of course interactions of these agents would run over years, not generations, as in evolutionary models.

Cozzens (1989) has studied the degree to which scientists involved in a priority dispute over the discovery of the opiate receptor behaved generously in their referencing. She looked at each of the main co-discoverers and how they credited the other co-discoverers. Over time there was a tendency for some co-discoverers to be more generous in their referencing, and less inclined to claim the credit exclusively for themselves. Also, the trend over time is toward a more standardized and less specific or qualified citation of the competitor's work as evidenced in citation contexts. The initial divergence of views at the time of discovery is followed by a convergence over the next few years as distinctions and qualifications are dropped, suggesting greater generosity over time. In addition, one co-discoverer played the role of enforcer by complaining to the others about the excessive priority claiming of one of the co-discoverers.

Another well known "multiple" is the discovery of oxygen involving Lavoisier, Priestley, and Scheele. Lavoisier had received information from the others that he was able to put to good use in making his discovery. However, initially he claimed credit for himself and failed to acknowledge the contributions of the others. Priestley, however, played the role of strong reciprocator and wrote a letter to Lavoisier complaining about his failure to credit others. Subsequently Lavoisier did acknowledge his fellow co-discoverers but not without pointing out how their discoveries differed from his (Small, 2010).

Einstein's special relativity paper, although not a multiple discovery, is also an example of delayed generosity. His celebrated 1905 paper contains no references to other papers. Notable for its absence was the work of Michelson and Morley on the constancy of the speed of light relative to the ether. However, two years later Einstein wrote a longer article on relativity which contained a number of references among them the Michelson-Morley paper (Small, 2010). Many years later, Einstein claimed that he had been unaware of the earlier work and would have cited it had he known about it (Holton, 1973, p. 282). It is not known if pressure was brought to bear on Einstein to acknowledge others.

A possible hypothesis is that over time there is an increase in generosity of referencing which is marked by a spreading of credit to multiple individuals, a lessening of the tendency toward the differentiation of the contributions, and an increasing standardization of language in the citation contexts. This can occur in the evolution of a single author's work, in the work of independent co-discoverers, or within an invisible college. There is some evidence of strong reciprocators acting to enforce normative compliance.

Other evolutionary models of cooperation use "multilevel" strategies, advocated by Sober and Wilson (1998), where selection occurs at the level of both the individual and the group. When the focus changes from individual to the group level success, it turns out that groups with many altruists are favored over groups with more selfish individuals (Arrow, 2007). Taking this perspective requires that we can think of science in functional terms at the group level. Because scientists comprise a relatively distinct social group, it is possible that behaviors could have evolved that enhanced group success. Historically we know that scientists often had to defend their views from attack by various outside authorities, whether political, economic, religious, or scientific. At the same time, scientists depended on these authorities for their support. Under these external threats it is not unexpected to find that cooperative behaviors and norms evolved that increased the fitness and success of the community. Such cooperative mechanisms could have included norms of behavior, the punishing of deviant behavior, and the mechanisms for recognition. These mutual support mechanisms would be magnified by the sub-structure of invisible colleges where individuals come face to face. Evolution at the sub-group level might also give rise to other specialized technical norms.

The next step might be to apply game theory to the process of writing papers and making references. For example, Chatterjee and Chowdhury (2012) have applied game theory to citation networks. Another possibility is to model the writing process as a game between an author and an imagined reader or critic. Each move in the writing process could be scored as cooperative or competitive in a balancing act to maximize the paper's strengths and minimize its weaknesses. For example, not citing a precursor or citing an irrelevant paper would be scored as selfish, while citing a review or rival would be seen as generous and enhance the score. Obviously the paper's fitness is not just a matter of what references are cited, but what connections are made to experiment and theory, that is, the paper's explanatory coherence, which is as we have seen partially revealed in its citation contexts.

7 Conclusions

Social construction leads to an anti-realist position on scientific knowledge and a community of scientists bent on self-interests. In this view, facts and theories have no basis in reality, and the only means of convincing others of the "truth" of a knowledge claim is rhetorical persuasion—even if that involves deception or fabrication. In constructivism it would be impossible to detect fraud or error in science or make a rational choice between theories. Deception, fabrication, and distortion would become institutionalized norms of behavior.

The realist view, on the other hand, affirms that "eternal and immutable regularities" exist in nature (Hull, 1988, p. 476). Because science relies on the arbiter of our senses, it does not require rhetorical persuasion or deception. The principle of consilience, favoring theories that have multiple empirical confirmations, offers an approximate guide to theory selection and how science evolves. Citation contexts in papers are shown to be a rich source of connections between theory and observations, and can be used for the construction of consilience networks.

Both technical and social norms are pervasive in science and are critical in regulating behavior. But their origin and evolution are little understood. Technical norms govern what counts as a consilience and the general procedures we call the "scientific method". Changes in technical norms are perhaps spurred on by major scientific successes or technical innovations that employ novel methods. If a new method becomes popular and incorporated into general practice, it will eventually be seen as a rule. Social norms may have evolved from general cultural values, but also from new social realities such as the rise of national styles of science and the need to insure the integrity of independent researchers.

The invention of the scientific journal gave rise to numerous norms and conventions, perhaps the most important of which was the communalism of scientific knowledge. Numerous other norms pertain to the acceptable style and structure of scientific papers. The norm of referencing may have originated as a means to claim priority, to show how your work differs from others, and to review what is known on a topic. Once specific concepts became associated with prior works, normative expectations were raised that these works would be cited when these concepts were used.

Competition and cooperation are pervasive in science as they are in all human endeavor, but cooperation is difficult to account for in an evolutionary view stressing individual survival. Strong reciprocity, where cooperators are rewarded and non-cooperators punished, is one viable mechanism for the emergence of cooperation. Here norms play a critical role in defining what it means to cooperate. An alternative explanation is multi-level selection, where individual selfishness is counteracted by society level norms. Here norms act as group level adaptations to maximize the fitness and success of the group. It seems obvious that science is a mix of selfish and altruistic individuals, but, perhaps, each individual is also a mix of these tendencies.

Referencing appears to fit the model of strong reciprocity where generous citation is rewarded and non-citers are sanctioned. Historical examples of codiscoveries offer a preliminary confirmation. Referencing may trend toward a sharing of credit and symbolic consensus in cases of multiple discovery or priority disputes.

The tools of game theory and computer simulation now being used by evolutionary biologists and economists to study cooperation and competition may offer a promising new avenue for research into citation practice and social norms in science. While referencing decisions are undoubtedly situationally complex, we can expect that both competitive and cooperative motives are at work.

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