Social behavior and scientific practice – missing pieces of the citation puzzle

Jeppe Nicolaisen

PhD thesis from Department of Information Studies
Royal School of Library and Information Science, Denmark

Social behavior and scientific practice – missing pieces of the citation puzzle

Jeppe Nicolaisen

PhD thesis from Department of Information Studies
Royal School of Library and Information Science, Denmark

CIP – Cataloguing in Publication

Nicolaisen, Jeppe
Social behavior and scientific practice – missing pieces of the citation puzzle / Jeppe Nicolaisen. – Copenhagen: Department of Information Studies, Royal School of Library and Information Science, 2004. x, 214 p.
Available: http://biblis.db.dk/uhtbin/hyperion.exe/db.jepnic04
ISBN 87-7415-285-8

ISBN 87-7415-285-8

Social adfærd og videnskabelig praksis – manglende brikker i citationspuslespillet

Jeppe Nicolaisen

Ph.d.-afhandling fra Institut for Informationsstudier
Danmarks Biblioteksskole

Avoid the crowd. Do your own thinking independently.
Be the chess player, not the chess piece.

- Ralph Charell

Acknowledgements

My clear and obvious debt, at least as I see it, is to those colleagues and friends with whom I have been interacting during my three years as a PhD-student. For they have, in one way or another, influenced my way of thinking and, consequently, my work.

First and foremost I want to thank my supervisor Research Professor Birger Hjørland whose assistance, direction, and encouragement I could not have done without. My debt to him is undoubtedly greater than occasional references may suggest.

Besides my supervisor, I owe great debts to a number of colleagues and friends who have commented on different aspects of my work and provided me with useful feedback. Especially Assistant Professor Jack Andersen, PhD-student Laura H.C. Skouvig, Dr. Alvin M. Schrader, and Assistant Professor Ellen Bonnevie. Thank you!

I also owe great debts to the organizers and lecturers of the many courses I have benefited from following during my years as a PhD-student. Especially the lecturers of the NorFA course *Today's research training - Tomorrow's scientific quality* (2003) made a lasting impression on my thinking. I would also like to thank the organizer and senior researchers of the *CoLIS 4 doctoral forum* (2002) for their destructive feedback. Although the obnoxious behavior of some of the senior researchers saddened me deeply, they made me reconsider my project, supervision, and ultimately my circle of academic acquaintances, which, I believe, have saved me from much grief.

I would also like to thank the members of the assessment committee (Associate Professor and Head of Department Søren Barlebo Rasmussen, Professor Olle Persson, and Associate Professor Piet Seiden) for their thorough reading and review of the dissertation and for pointing out a handful of stylistic defects in the argumentation that have now been eliminated from the text.

I am also indebted to the management, administration, and library of the Royal School of Library and Information Science for providing optimal conditions for my work. I especially want to thank Rector Leif Lørring, Former Head of Administration Tommy Jensen, Head of Department Mona Madsen, Secretary of Department Susanne Acevedo, Librarians Allan Dideriksen and Karen Margrethe Ørnstrup, and Chief Assistant Jan Jacobsen for their outstanding treatment.

Finally, I want to express my apologies to my wife and daughter, Tine and Erika, for many sporadic mental absences during the past three years, and for my never-ending lectures on different aspects of the dissertation. Now that "it" is over, I promise I will return to normal.

Abstract

This dissertation is concerned with understanding how citations reflect social behavior and scientific practice. The main objective is to answer two related questions:

1. What makes authors cite their influences?
2. What makes authors cite some resources and not others?

Citation theoreticians have debated these questions for more than thirty years. A detailed analysis of the debate reveals, however, that satisfactory answers are still missing.

A theoretical explanation for the first question is proposed. The proposal is inspired by research from the field of evolutionary biology, especially *the handicap principle* developed by the Israeli biologist Amotz Zahavi. The dissertation argues that theories of honesty and deception in animal communication can contribute to our understanding of human citation behavior. The dissertation suggests to view references as threat signals comparable to common threat signals found in Nature (i.e., approaching a rival, stretching, and vocalization).

The dissertation documents the existence of a blind spot in the bibliometric literature. Previous theoreticians have mainly argued that authors tend to cite sources from their own specialty while ignoring works from other specialties. The dissertation argues, however, that research traditions also play an important role for the allocation of references (and citations).

Empirical analyses of two bibliographies (a psychological bibliography and a communication theoretical bibliography) are presented. The aim is to test the hypothesis that specialties *and* research traditions seize a strong influnce on the structural dynamics of citation networks. Both tests confirm the hypothesis.

The implications for citation based information science are discussed.

Resumé

Afhandlingen forsøger at belyse hvorledes bibliografiske referencer genspejler social adfærd og videnskabelig praksis. Målet er at besvare to relaterede spørgsmål:

1. Hvad får forfattere til at citere deres kilder?
2. Hvad får forfattere til at citere nogle kilder frem for andre?

Begge spørgsmål har affødt en vis diskussion blandt citationsteoretikere. En detaljeret analyse af diskussionen afslører imidlertid at ingen hidtil har formået at besvare spørgsmålene tilfredsstillende.

Besvarelsen af det første spørgsmål tager udgangspunkt i nyere evolutionsbiologisk forskning, herunder det såkaldte *handicap princip* udviklet af den israelske biolog Amotz Zahavi. Afhandlingen foreslår at opfatte bibliografiske referencer som trusselssignaler på linie med naturens trusselssignaler (f.eks. at minimere afstanden til en rival, strækning og vokalisering).

Afhandlingen dokumenterer eksistensen af en blind plet i den bibliometriske faglitteratur. Tidligere citationsteoretikere har hovedsagelig fastholdt at videnskabelige forfattere har tendens til at citere kilder fra deres eget fagområde. Afhandlingen argumenterer imidlertid for at forskningstraditioner også spiller en væsentlig rolle for allokeringen af bibliografiske referencer. Dette aspekt har hidtil været negligeret i den bibliometriske faglitteratur.

Afhandlingen præsenterer empiriske analyser af to forskellige bibliografier: En psykologisk og en kommunikationsteoretisk Målet er at teste hypotesen at såvel fagområder som forskningstraditioner i væsentlig grad påvirker citationsnetværks strukturelle dynamik. Analyserne foretages som strukturelle bibliometriske analyser (co-citationsanalyse og hierarkisk klyngeanalyse). Begge analyser bekræfter hypotesen.

Implikationerne for citationsbaseret informationsvidenskab diskuteres.

Table of contents

List of figures

List of tables

There is nothing more difficult to take in hand, more perilous to conduct, or more uncertain in its success, than to take the lead in the introduction of a new order of things. Because the innovator has for enemies all those who have done well under the old conditions, and lukewarm defenders in those who may do well under the new (Niccoló Machiavelli)[1].

1 Introduction

This dissertation is concerned with understanding how citations reflect social behavior and scientific practice. The topic has attracted a great deal of attention from researchers in the fields of library and information science, sociology of science, as well as researchers in the interdisciplinary field of bibliometrics for more than 30 years. Although much have been said and written, and though a number of theories have been put forward, the puzzle is still not solved. The main aim of the dissertation is to provide a substantiated account of some vital aspects of social behavior and scientific practice that earlier theories have overlooked or ignored. Hopefully, these aspects will provide some of the missing pieces of the citation puzzle and help clarifying why citations are distributed the way they are, and, consequently, why citation networks are structured as they are.

From the outset it must be stressed that it has not been possible to explore all the issues concerned with the topic in the detail, which they deserve. This is not, nor is it intended to be, a finished piece of work. At many points, argument sketches pass for arguments and plausible intuitions are invoked where, ideally, explicit doctrines are called for. A great deal remains to be said on all the matters addressed. But the study of these matters is a cooperative venture of a community of minds. The purpose is merely to offer a fresh perspective on a few problems that have preoccupied reflective thinkers for quite some time.

Before turning to the specific objectives and organization of the dissertation (section 1.7), a number of issues need to be addressed. First of all: What are citations and citation networks? These questions are dealt with in sections 1.1 and 1.2. Secondly:

[1] http://www.the-prince-by-machiavelli.com/machiavelli-quotes.html. Visited August 24., 2004.

Why is it important to understand how citations reflect social behavior and scientific practice? This is discussed in sections 1.3 and 1.4. Thirdly: What are the most productive ways for studying and learning about these matters? This is discussed in sections 1.5 and 1.6.

1.1 References and citations

Scientific tradition requires that scientists, when documenting their own research, refer to earlier works, which relate to the subject matter of their reported work. These bibliographic references are supposed to identify those earlier researchers whose concepts, theories, methods, equipment, etc. inspired or were used by the author in the process of conducting and presenting his or her own research. On the nature of references in general, the following statements are typical:

> "A research paper requires a thoughtful balance between *your own* language and the words and sentences you *borrow* from other sources" (Marius & Wiener, 1991, p. 442).

Borrowing from other sources without proper recognition is generally regarded as plagiarism:

> "You commit **plagiarism** whenever you present words or ideas *taken from* another person as if they were your own […]. The prose we write ourselves is so individual that when we write something in a striking way or express a new idea, we have produced something that always *belongs to us*. To call someone else's writing your own is wrong and foolish" (Marius & Wiener, 1991, p. 465).

> "Plagiarism can result from not *giving credit* to the person who thought of an idea, calculated statistics, made a discovery. You cannot pass off as your own another person's work" (Carter & Skates, 1990, p. 482).

Thus, if one borrows from other sources, one has to credit the sources by citing them. However, not all ideas and discoveries need to be cited. *The Scott Foresman Handbook for Writers* explains that there is no need to cite:

> "facts, dates, events, information, and concepts that belong generally to an educated public. No individual owns the facts about history, physics, social behavior, geography, current events, popular culture, and so on [...]. What the experts know collectively constitutes the common knowledge within the field about the subject; what they assert individually – their opinions, studies, theories, research projects, and hypotheses – is the material you must document in a paper" (Hairston & Ruszkiewicz, 1988, p. 546-547).

Though the act of citing is sometimes said to be as old as scholarship itself (e.g., Price, 1963, p. 65), historians of science nevertheless disagree about the origins of this tradition. According to Grafton (1997), historians of science have placed the birth of the modern reference in the twelfth century, the seventeenth, the eighteenth, and the nineteenth – never without good reason. Mustelin (1988), however, maintains that authors prior to the sixteenth century often duplicated the work of their predecessors without proper recognition. From the latter part of the sixteenth century, authors of scientific works strived to give their texts a greater evidential weight partly by noting and referring to other sources. Among the proponents of this practice were philologists and text publishers. Historians and others followed later. Nowadays, explicit references are believed to be essential in order to communicate effectively and intelligently about scientific and technical subjects (Garfield, 1977a, p. 8), and the act of citing is thus second nature to anyone writing a scholarly or a scientific paper (Kaplan, 1965, p. 179).

So far I have used the terms *citations* and *references* (and their verb forms) interchangeably. But this is actually not their proper way of use. In 1970 Derek J. de Solla Price proposed and adopted the convention that "if Paper R contains a bibliographic footnote using and describing Paper C, then R contains a *reference* to C, and C has a *citation* from R" (Price, 1970, p. 7). In line with the *Dictionary of Bibliometrics* (Diodato, 1994, p. 32-33) Paper R should thus be termed the *citing document* and Paper C the *cited document*. Narin (1976, p. 334, 337) later reiterated Price's convention by stating that a citation is the acknowledgement one bibliographic unit receives from another whereas a reference is the acknowledgement one unit gives to another. Thus, *citations* and *references* denote two different things and should be used terminologically as such. For instance:

> "In a growing field, the characteristics (such as the average age and number) of the references in a paper will not necessarily be the same as those of the citations to a paper" (Gilbert & Woolgar, 1974, p. 283-284).

Although most authors are not as precise in their usage of these terms as Gilbert and Woolgar[2], Price (1970) was definitely right in stating that using the words *citation* and *reference* interchangeably is a deplorable waste of a good technical term.

References share an important feature. Each reference is an inscription (Latour & Woolgar, 1986, p. 45-53), describing a certain text by a standardized code. While different publication manuals give different codes and although many publishers and journals have 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) reviews a number of studies dealing with bibliographic errors and concludes, "To err bibliographically is human" (Garfield, 1990, p. 374). In a study of the incidence and variety of bibliographic errors in six medical journals, De Lacey, Record and Wade (1985) found, for instance, that almost a quarter of the references contained at least one mistake, and eight percent of these were judged serious enough to prevent retrieval of the article. Moed & Vriens (1989) examined discrepancies between 4.500 papers from five scientific journals and approximately 25.000 articles that cited these papers and found that almost ten 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 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 own references an erroneous reference to a 1964 article (one word was incorrectly substituted in the title). Examining the 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. Garfield (1990, p. 371) reports an informal discussion he and Henry Small once had over the idea of a *National Citation Facility* – an online database of verified references that would afford instantaneous access to authors and allow them to verify their own references. This, they supposed, would reduce bibliographic errors. Although Garfield and Small never realized their idea, a number of comparable tools now exist, which allow authors to import references from databases on the Internet and automatically reformat them according to predefined

[2] A glaring example is found in the paper by Garfield, Malin & Small (1979, p. 180): "Citation analysis is a bibliometric method that uses reference citations found in scientific papers as the primary analytical tool".

output styles[3]. If or preferably when these tools become common to authors, bibliographic errors will hopefully decrease as Garfield and Small predicted.

1.1.1 The demarcation principle

Price established the norm of scholarship as being equivalent to a paper with approximately ten to twenty-two references (1970). Price laid down the convention that papers with larger numbers of references should be seen as reflections of non-creative scholarship as they typically were review articles, and that papers with less than 10 references should be seen as unscholarly ex-cathedra pronouncements of innate knowledge (1970, p. 7-8). Such rigid numerical meters are of course highly time and field dependent. Bazerman (1988), for instance, has demonstrated how textual elements of spectroscopic articles in the *Physical Review* changed over time from the founding of the journal in 1893 to 1980. By implementing a mixture of statistics and close analytical reading, he was able to identify a variety of trends and implications. He found, for instance, remarkable shifts in the use of references during the investigated period. In the early years references were mainly concentrated in the introductory section of articles and rarely related to specific findings or works with an explicit relation to the citing work. Reference lists served instead as indexes of previous works in the general area. By 1910, the number of references had become remarkably reduced, but the very few that remained were all recent, had explicit publication dates, and were of direct relevance to the research being reported. From then on, the number of references trended upwards and spread from the introduction to all parts of the article, whilst maintaining specific relevance to the citing work.

Others have shown that the number of references varies substantially from one discipline to another. Hyland (2000, p. 24) lists the average number of references in articles in eight scientific disciplines (see table 1.1.) and concludes that there are clear disciplinary differences in extent to which authors rely on the work of others when presenting their own research.

In a study of referencing practices in academic book reviews, Nicolaisen (2002b) demonstrated significant differences in the use of references in six social science disciplines during a 30-year period (1972-2001). He found that book reviews with references to other sources than the reviewed book had generally been growing in numbers, but with different rates with respect to the total numbers of book reviews. This was found to result in quite different percentage growths and declines in book reviews citing other sources than the reviewed book. The general percentage growth were found

[3] For a recent review of some of these tools see Poehlmann (2002).

to be most marked in the field of psychology whereas book reviews bearing additional references to other sources than the reviewed book had actually declined in the field of history & philosophy of science & social sciences during the 30-year period.

Table 1.1. *Number of references by discipline (Hyland, 2000, p. 24)*[4].

Discipline	**Average per paper**	**Per 1.000 words**
Sociology	104,0	12,5
Marketing	94,9	10,1
Philosophy	85,2	10,8
Molecular biology[5]	82,7	15,5
Applied linguistics	75,3	10,8
Electronic engineering	42,8	8,4
Mechanical engineering	27,5	7,3
Magnetic physics	24,8	7,4

A number of researchers have modified Price's demarcation principle to some extent. Windsor & Windsor (1973) considered the distinction between scholarly and non-scholarly literature to rest on the presence or absence of references. The authors consequently measured the scholarliness of six years of information science literature by inferring the ratio of papers without references to papers containing references. Lockett & Khawam (1989), Metz (1989), Mittermeyer & Houser (1979), Schrader (1985), Schrader & Beswick (1989), and Stephenson (1993) all operate with the same distinction in their investigations of the scholarliness of different journals in the field of library & information science. This procedure seems reasonable[6], as most people will

[4] The table is the result of a computerized search of articles for canonical reference forms such as a date in brackets, a number in square brackets, and Latinate references to other references (for example, op. cit., ibid.). The actual number of references listed in the bibliographies of the articles is presumably somewhat lower.

[5] In a study of referencing practices in four natural science disciplines, Moed & Garfield (2003) also found that molecular biologists (and biochemists) tend to employ more references than their natural science colleagues.

[6] The demarcation principle of Windsor & Windsor (1974) has only met limited criticism (Terrant, 1974; Worthern & Shimko, 1974).

agree that the majority of research papers without references are very simple and unsophisticated pieces of work[7] (Peritz, 1981).

1.2 Citation networks

Egghe & Rousseau (1990, p. 228) explain "when a document d_i cites a document d_j, we can show this by an arrow going from the node representing d_i to the document representing d_j. In this way the documents from a collection D form a directed graph, which is called a '*citation graph*' or '*citation network*'". Figure 1.1. displays such a network, originally published in an article dealing with the problem how to locate articles in the anesthetics field of *perturbation of ion transport* (Cawkell, 1971). It illustrates the reference connections between nineteen articles published on the subject between 1954 and 1970. It would, however, be unusual if a network of the size of Figure 1.1. included the entire literature of a subject, but there is no reason why a citation network representing the literature of any subject could not be shown in a similar, larger diagram.

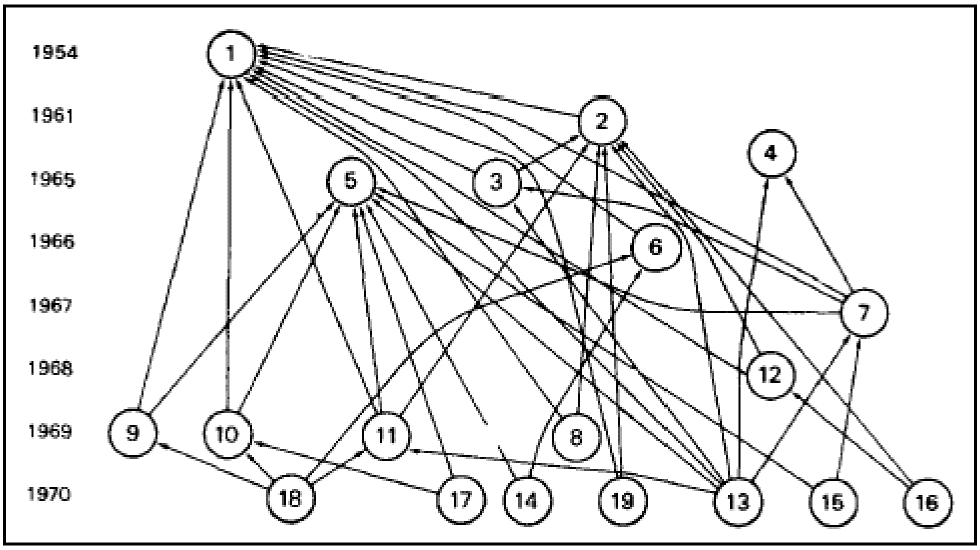

Figure 1.1. *Citation network (Cawkell, 1971, p. 814).*

[7] There are, of course, important exceptions. Worthern & Shimko (1974) reports a number of zero-reference cases, which hardly deserves to be labeled as unscholarly (e.g., Squibb's work on the purification of either, Ehrlich's report on Salvarsan ("606"), and Fleming's on penicillin).

A citation index eases the work of mapping a citation network. In fact, the network in Figure 1.1. was constructed using the *Science Citation Index* (SCI) (Cawkell, 1971, p. 814).

1.2.1 Citation indexes

Eugene Garfield got the idea for a citation index at some point in the early 1950's[8] and launched the first edition of the SCI in 1963[9]. Garfield's idea was simply to index a large part of the total world citation network. More precisely, Garfield wanted to register papers and references from a number of leading journals and then arrange the papers alphabetically in two ways: By cited author and by citing author. These indexes, he reasoned, would allow not only the detection of papers cited by a particular paper, but also the papers citing a particular paper. For instance, if one knew the bibliographic details of paper (6) in Figure 1.1., Garfield's citation index would allow one to find out that paper (6) was referring to paper (1), and that paper (6) was cited by papers (14) and (18).

The first edition of the SCI covered the journal literature of the natural sciences of the calendar year 1961. Covering 613 journals and containing about 1.4 million references, it required six volumes. From 1964 the SCI was launched on a current quarterly basis with an annual accumulation. In 1970 a five-year accumulation covering 1965 to 1969 was produced. Eventually, cumulated citation indexes for 1945 to 1954 and 1955 to 1964 were created. The *Social Sciences Citation Index* (SSCI) covering the social sciences was launched in 1973, and the *Arts and Humanities Citation Index* (A&HCI) was launched in 1978. Since 1980 the SCI, SSCI, and A&HCI have been accessible online and in CD-ROM format. In 1997 The *Institute for Scientific Information* (ISI) launched a Web-based and completely integrated continuation of the SCI, SSCI, and A&HCI called *Web of Science.*

Garfield's original idea with the SCI was to create a bibliographic system for the retrieval of science literature (Garfield, 1955). The rationale behind this idea is explained in Garfield (1979, p. 1):

---

[8] Garfield was neither the first to think of the idea of a citation index nor the first to apply it. The *Institute of Electrical Engineers*, for instance, used to have their own citation index (abandoned in 1922) (Garfield, 1977b). Another forerunner is the legal profession's *Shepard's Citations*, launched in 1873 by *Shepard's Citations, Inc.*

[9] For a comprehensive review of the origins of the Science Citation Index, see Wouters (1999).

> "Citations are the formal, explicit linkages between papers that have particular points in common. A citation index is built around these linkages. It lists publications that have been cited and identifies the sources of the citations. Anyone conducting a literature search can find from one to dozens of additional papers on a subject just by knowing one that has been cited. And every paper that is found provides a list of new citations with which to continue the search".

1.2.2 Citation analysis

Larsen (2002, p. 157) explains with great insight that citation indexes allow for a range of search strategies that could not have been carried out in practice before their creation. But Garfield's introduction of the citation indexes made further applications of citation analysis practicable as well. According to Nicolaisen (2003, p. 12), the SCI indirectly paved the way for three citation-based areas in information science (IS):

1. Information seeking based on citation search strategies.
2. Research evaluation based on citation counts.
3. Knowledge organization based on bibliographic coupling and co-citation analysis.

The object of study in these areas is in principle the total world network of scientific documents and their references. However, citation analysts usually focus on smaller parts of the network, which constitute particular research areas or knowledge domains. The raison d'être of citation-based IS is spelled out by Cawkell (1974, p. 123). According to him, several deductions can be made from a citation network without knowledge of its subject content. For instance, highly cited documents show that they have had a considerable *impact* upon the later works, *bibliographically coupled* documents posses a higher probability for being similar in subject content than other non-bibliographically coupled documents, and *co-cited* documents imply subject relatedness. Cawkell's claims, though frequently repeated by others[10], are far from universally accepted.

[10] For instance Egghe & Rousseau (1990, p. 229-230) in *Introduction to Informetrics*, a primer on citation analysis.

1.2.2.1 The nature of citation rates

Garfield (1979, p. 63) admits that the nature that citation rates measure is elusive. "It has been described variously as 'significance', 'impact', 'utility', and 'effectiveness', but no one has succeeded in defining it in more tangible terms". However, leaning on "a sizeable number of studies that show a strong, positive correlation between citation rates and peer judgments", Garfield (1979, p. 63) concludes that citation rates normally reflect credit on the scientific work involved.

Fourteen years earlier, Garfield proposed a list of fifteen reasons why authors cite (1965, p. 85)[11]. Five of these are instances where authors are correcting or criticizing earlier works and thus providing *negative citations* instead. Unfortunately, no large-scale investigations of when the critical attitude of scholars has lead to heavy criticism of particular works and indirectly to the allocation of numerous negative citations exist. But Garfield (1978a, 1978b) himself has investigated the types of citations received by a highly debated article by Arthur Jensen in the 1969 volume of *Harvard Educational Review*. According to Garfield (1978b), Jensen's article was heavily cited because it had been seriously criticized. In a later study, Garfield & Welljams-Dorof (1990) investigated the impact of fraudulent research on scientific literature by focusing on 20 publications from the Steven E. Breuning case[12]. The authors retrieved copies of 65 citing articles and identified in each text where Breuning's works was cited and how they was referred to. Findings of their content analysis indicated that less than 10 percent of the citations were of positive nature. Garfield's list of citer motivations and his works on the Arthur Jensen and Steven E. Breuning cases show collectively that citation rates in some cases reflect criticism rather than credit on the scientific work involved.

1.2.2.2 Bibliographic coupling

Documents are said to be *bibliographically coupled* if they share one or more bibliographic references[13]. The concept of bibliographic coupling was introduced by Kessler (1963) who demonstrated the existence of the phenomenon and argued for its usefulness as an indicator of subject relatedness. The major theoretical criticism of this notion appeared soon after in a one-page article by Martyn (1964, p. 236) who

---

[11] See table 2.1.

[12] Psychologist Stephen E. Breuning was the first researcher to be tried and convicted of fraud.

[13] Paper (9) and paper (17) in Figure 1.1. are, for instance, bibliographically coupled as they both cite paper (5).

sagaciously observed that there is no guarantee that two bibliographically coupled documents (A) and (B) cite the same piece of information in (C). He also observed that even if (A) and (B) cite the same piece of information in (C) we do not know the size of the conjunction and therefore, considering now the conjunction of two other documents (M) and (N), we may not equate (A)$\cap$(B) with (M)$\cap$(N). These observations led Martyn to conclude that a bibliographic coupling is merely an indication of the existence of the probability, value unknown, of relationship between two documents rather than a constant unit of similarity. His conclusion finds empirically support in Vladutz & Cook's (1984) results. In their validation study, a random selection of 10.000 articles from the SCI was coupled with papers from the entire SCI database and lists of the three most strongly bibliographically coupled items were compiled. Professional indexers were then asked to assess the relatedness of these papers for a random sample of 300 lists. The indexers were merely able to report some degree of subject relatedness in little more than 85% of the cases, which decreased to 81% when the frequency of occurrence in the entire database and the length of the reference lists were both taken into account. It consequently seems reasonable to assume that such quantitative meters provide more valid results in some areas as opposed to others. Virgo (1971, p. 289), for instance, anticipates that the critical threshold value for the coupling strengths possibly varies from field to field and even within fields, and Weinberg (1974) predicts that bibliographic coupling should work best for repetitive literature (e.g., review articles) because such literature often cite a lot of older works.

1.2.2.3 Co-citation analysis

Marshakova (1973) and Small (1973) proposed the same variation on bibliographic coupling when independently suggesting the relatedness of documents measured by their *co-citation* frequency. Two documents are said to be co-cited if they appear simultaneously in the reference list of a third document[14]. The co-citation frequency is defined as the frequency with which two documents are cited together. Price (1965, p. 515) maintains that networks of scientific papers that are linked, or "knitted" together by citations, reveal either the research front, which builds on very recent work, or taxonomic subjects, which are tied into "the eternal record of human knowledge". But Cole & Cole (1973, p. 220) draw attention to the fact that scientists ceremonially cite friends, colleges, mentors, or eminent people in the field, and that cited works may represent not only a significant, even necessary antecedent to the present work, but also merely a tangentially relevant piece of work, cited only to demonstrate the author's

[14] Paper (9) and paper (11) in Figure 1.1. are co-cited as they both are cited by paper (18).

knowledge of the literature. Different citer motives are treated equally by co-citation analysis, thus accentuating that Martyn's (1964) criticism on bibliographic coupling also applies to co-citation analysis.

1.3 Recurring calls for a theory of citing

During the 1970s, claims such as Cawkell's, that several deductions can be made from a citation network without knowledge of its subject content (1974, p. 123) appeared less frequently. Instead, sociologists, information scientists, and others began to recognize the need for a theory of citing that could answer the question why authors cite the way they do. Among the first of these were 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 other authors raised this problem independently, yet 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. Smith (1981, p. 99) concluded that not enough is known about the citation behavior of authors, and that such knowledge is essential in order to know whether it makes sense to use citation analysis in various application areas. Though Zuckerman (1987) some years later concluded that it seemed redundant to say that a theory of citation is badly needed, she nevertheless repeated the call for such a theory. In 1998 a whole issue of the journal *Scientometrics* was devoted to the discussion of, and recurring call for a citation theory. Leydesdorff (1998) initiated the discussion with a discussion paper entitled *Theories of Citation?* in which he argued that a variety of contexts for citation analysis has been proposed, but that a comprehensive theory of citation has not been formulated. The commentators gave different proposals for a theory of citing, but the 1998 discussion did not result in anything like a comprehensive theory.

1.4 Questioning the need for a theory of citing

A number of commentators have challenged the need for a theory of citing by stating either that 1. such a theory is impossible or 2. analysts does not need such a theory in order to perform citation analyses.

1.4.1 Wouters' reflexive citation theory

Paul Wouters considers the quest for a citation theory that seeks to explain the citation by relating it to the citing behavior of the scientist "a dead end" (1999, p. 211). According to him, we must abandon this pursuit and instead focus our attention on the symbolic characteristics of the citation and its indicator abilities. Citations are indicators, which create what Wouters calls a "formalized representation" of science, which initially neglects meaning (1999, p. 209). However, to interpret these formalized representations one needs to attribute meaning to the selfsame indicators. According to Wouters, "this attribution of meaning can be postponed" (1999, p. 209) and should be based not on the citing behavior of the citing scientists, but on how citations reflect the characteristics of science. Wouters entitles this "the reflexive citation theory" (1999, p. 213).

Wouters' theory rests on his interpretation of the *reference* and the *citation* being two different signs. Others have previously noted the technical difference between the two[15], but Wouters considers the difference to be not so much a technical as a crucial one:

> "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" (Wouters, 1998, p. 232).
>
> "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, p. 233).

[15] See section 1.1.

Wouters consequently sees the citation as a new sign - different from the reference it builds upon. Unlike the reference, the citation is dimensionless and meaningless (Wouters, 1999, p. 209). The meaning of a citation is something, which the citation analyst attributes. The (ISI) indexer's desk is the birthplace of the citation – not the scientist's, and studies of scientists' citation 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, 1999 p. 195).

Wouters' theory reflects the main idea of informational semantics. Informational semantics is a family of theories attempting 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 Dretske (1981). It is inspired by Claude Shannon's theory of information (1948), which provides a mathematical measure of the amount of information carried by a signal. Dretske (1981) supplemented Shannon's work with an account of what meaning a signal carries. His idea was that a signal carries the meaning that *p* if and only if it naturally means (that is, indicates) that *p*, as when smoke indicates fire. Consequently, the main idea of informational semantics is to ignore the actual history of a signal, and to focus on what the signal is a reliable indicator of. But informational semantics faces a major problem that has been brought up 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. The problem arises precisely because informational semantics holds that representation is a kind of correlation or causation. But as Godfrey-Smith (1989) has asked, how can a representation be caused by or 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 signals of horse. What horse is reliably correlated with is really the presence of horses, muddy zebras, odd cows etc. Thus, it should refer to horses, muddy zebras, odd cows and so on. To solve this problem, the informationalist claims that circumstances in which muddy zebras and odd cows cause horse are not appropriate for fixing its reference, i.e., a signal of horse represents what such signals are caused by in normal circumstances. However, as noted by Godfrey-Smith (1989), 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 any theory of representation that depend on reliable causation is consequently doomed. The reason is simply that certain recognition of *p* is impossible. Misrepresentation is common - *q, r, s…* is often confused with *p*. For instance, in nature it is common for an organism to represent a situation, as one in which there is a predator, and to be more often wrong than right. 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 pay-off:

> "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 is not seeking to provide safe mechanisms. Informational semantics seek to uncover signals, which are reliably correlated with specific situations and hence indicate these situations. In other words, informational semantics attempt to establish certain mechanisms, but only safe ones are logically possible.

Wouters' reflexive citation theory suffers from the same unsolvable problem as informational semantics does. His theory cannot handle false positives, i.e., citations, which do not indicate the situation they spontaneously appear to. Unfortunately, Wouters (1999) himself does not attempt to provide a certain mechanism of what citations are reliably correlated with, but the bibliometric literature is loaded with such attempts. Perhaps most notably is the quest for confirming that citations are indicators of quality. As we shall see in chapter 2., this is one of the basic assumptions of citation analysis and one, which can be traced back to the first citation analysis from 1927. The assumption rests on the presumption that citations and research quality go hand in hand and thereby shapes a linear relation. Studies of the predictive validity of citation analysis have tested this presumption that works with high citation frequencies are evaluated more positively that other works with lower frequencies. The aim of these studies has consequently been to demonstrate the existence of a linear relationship between research quality and citation counts. However, the majority of these studies have only been able to document low to moderate findings of linear correlation (e.g., Gottfredson, 1978; Nicolaisen, 2000; Schubert et al., 1984; Virgo, 1977; Wolfgang et al., 1978). Consequently, it seems justified to conclude that far from all citations are indicators of quality. But what then are they indicators of? Eugene Garfield tried to answer the question in his book *Citation Indexing: Its Theory and Application in Science, Technology, and Humanities* from 1979. According to Garfield (1979, p. 246), citations do not indicate elegance, importance, quality or significance. Instead, citations 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".

What Garfield established is consequently nothing but a safe mechanism: A citation indicates that a cited work has been referred to and used in a citing work. Nothing more, nothing less. He explicitly withstood from claiming any other correlation between citations and the world - evidently a clever move in light of the aforementioned error problem.

1.4.2 Anti-theory

A number of commentators have questioned whether a theory of citing is needed at all. One commentator has even suggested stopping the *theorizing* and returning to the logic 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).

Arunachalam's suggestion is plainly naïve. Thanks to Sir Karl Popper, philosophers have long ago realized the limitations of logical positivism and the principle of induction[16]. However, from a purely practical perspective, there is some truth in the argument put forward by various *anti-theorists,* that one does not need to know why citation analysis works in order to utilize it. Driving a car does not require an engineer's knowledge of how a combustion engine works, and drinking a beer does not require a brewer's education. Likewise, conducting citation analyses in practice does not require a theory explaining why authors cite as they do, as long as the results of the analyses are valid. Nowadays, practicing citation analysts no longer believe in Robert K. Merton's interpretation of citations and references:

> "Citations and references 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, 1979, p. viii).

Instead, modern citation analysts grant that many authors do not give credit where credit is due and consequently, that many authors fail to display the kind of behavior that the normative theory prescribes[17]. Instead, they assume that "the biases and deficiencies of individual citers are repaired to a tolerable degree by the combined activity of the many" (White, 2001, p. 102), and encourage us to "believe that there is a norm of citing – straightforward acknowledgement of related documents – and that the great majority of citations conform to it" (White, 1990, p. 91). Though it might be tempting to follow these anti-theorists, it appears to be an illegitimate scientific move. The old astronomers, for instance, did not satisfy themselves with a similar explanation of the sky: That *some mechanism* makes celestial bodies go around like a clockwork enabling man to keep a reliable calendar. They were scientists, and contrary to calendar makers,

[16] See chapter 2.

[17] See chapter 2.

scientists are concerned with the question *why?* Social scientists should also be motivated by the question *why?* For example, *why* the great majority of people behave the way they do, and *why* citing authors obey some rule on average. The assumption, that the biases and deficiencies of individual citers are repaired to a tolerable degree by the combined activity of the many, is simply not enough. We want to know *why* citing authors act honestly on average. We want to *understand* the regulating mechanisms behind.

1.5 Citation psychology

Among those who have provided inputs to the discussion of a theory of citing since the early calls for such a theory are Chubin & Moitra (1975), Cronin (1984), Case & Higgins (2000), and Harter (1992). These commentators share the widespread belief that citing is best understood as a psychological process[18], and that theories of citing should be constructed from studies of individual citers, conducted by interview techniques, thinking aloud methods, or the recording of behavioral patterns.

1.5.1 Psychological relevance

Harter (1992, p. 614) poses two related questions: What criteria do users employ when they select bibliographic items for subsequent reference in their published works? Why is an item found relevant to begin with? The debate concerning the concept of relevance has formed an important part of the ongoing discussions in the field of library and information science for more than fifty years[19]. However, a psychological understanding of relevance has dominated the debate from the very beginning (Hjørland, 2000a). Harter (1992) in his article entitled *Psychological Relevance and Information Science* continues this tradition. According to him, relevance is constructed in a user's mind and a clue to one user will not be one to another (Harter, 1992, p. 614). Therefore, in order to answer the two questions of relevance criteria posed above, Harter believes "we need to concentrate on the psychological processes themselves" (1992, p. 614).

---

[18] Chubin & Moitra (1975, p. 426) consider citing to be "a private process". Cronin (1984, p. 29) calls citing a "private practice". Case & Higgins (2000, p. 635) hold citing to be "an internal phenomenon". Harter (1992, p. 614) dubs the act of citing "a dynamic, complex, cognitive process".

[19] Recent reviews may be found in Borlund (2003) and Mizzaro (1998).

In his article, Harter (1992) explores a theory of relevance proposed six years earlier by Sperber & Wilson (1986). Sperber & Wilson's theory is a theory of the relevance of everyday speech utterances to hearers, but in the end of their book, the authors assert that the ideas presented are applicable to the idea of thought processes generally, and not simply human speech. Harter is convinced that this conjecture is correct, and consequently sets out to summarize the theory of relevance presented by Sperber & Wilson.

Sperber & Wilson (1986) begin by defining the idea of a fact being manifest to an individual and how this relates to an individual's cognitive environment. According to the authors, a fact is only manifest to a person at a given time if he can understand it at that time and accept its representation as true or probably true. Assumptions are held to be indistinguishable from facts. That is, mistaken assumptions are treated in the same way as genuine facts. The authors therefore maintain that the cognitive environment of an individual is the set of facts and assumptions that are manifest to him at a specific time. To be manifest is the same as being capable of being perceived or inferred. Harter (1986, p. 604) exemplifies this by referring to an individual who has not studied mathematics beyond elementary arithmetic. Such an individual would not be capable of imagining most of the ideas of integral calculus. In Sperber & Wilson's words, the ideas would not be *manifest* to such an individual.

Sperber & Wilson (1986) calls the act of making something manifest to another person *ostensive behavior* or simply *ostension*. They operate with two basic assumptions: Firstly, "ostension comes with a tacit guarantee of relevance" (p. 49). Secondly, "an individual's particular cognitive goal at a given moment is always an instance of a more general goal: maximizing the relevance of the information processed" (p. 49). Sperber & Wilson (1986, p. 118) explains what happens when two people engage in a conversation:

> "As a discourse proceeds, the hearer retrieves or constructs and then processes several assumptions. These form a gradually changing background against which new information is processed. Interpreting an utterance involves more than merely identifying the assumption explicitly expressed: it crucially involves working out the consequences of adding this assumption to a set of assumptions that have themselves already been processed".

Harter concludes that Sperber & Wilson see human beings as being in command of a number of manifest assumptions. "These cognitive constructs are products of each individual's cognitive ability, cultural and social group identity, educational background, and physical environment. As a discourse proceeds, some of these assumptions (correct or mistaken facts) are retrieved, or are inferred from two or more assumptions" (1992, p. 604). According to Sperber & Wilson (1986, p. 118-119) comprehension involves "the joint processing of a set of assumptions, and in that set some assumptions stand out as newly presented information being processed in the context of information that has itself been previously processed". Interpreting a comment made by another means, accordingly, to work out the consequences of adding the assumption expressed by the utterance to the *context* (the set of assumptions already present in the hearer's mind). Harter (1992) stresses that the context is a psychological concept, representing the hearer's assumptions about the world at any given time.

But how do the hearer choose among the potentially many possible contexts available to him? According to Sperber & Wilson (1986, p. 141-142), the search for relevance determines the hearer's choice of context. "The individual to whom another has directed an ostensive action acts according to the principle of relevance, and selects a context that will maximize the relevance of the information being processed". Harter (1992) thus concludes that relevance may be defined as a theoretical concept of psychology, as a relation between an assumption and a context: "*Briefly, a necessary condition for an assumption to be relevant in a context is for the assumption to have contextual effects in that context*" (p. 605).

After his summary of Sperber & Wilson's *Relevance*, Harter draws a number of implications for information retrieval and bibliometric theory, and outlines what appears to be 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, p. 612-612).

Harter's assertion that references, which have led to especially relevant sources, will be cited lacks empirical support[20]. But his hypothesis, that 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, finds empirical support in the longitudinal study by White & Wang (1997). The authors followed a group of agricultural economists' use of scientific literature over a longer period of time, and through the use of structured interviews, they were able to conclude that the literature's contribution to the research seemed to be a major factor in the decision to cite a document. However, Sperber & Wilson's account of relevance suffers from a number of serious problems[21]. And since Harter bases his psychological theory of relevance on Sperber & Wilson (1986), his theory suffers from the selfsame problems.

The main problem with the psychological theory of relevance is its disregard of sociocultural issues. Sperber & Wilson (1986) view human beings as information processors with an inbuilt capacity to infer relevance. This capacity is assumed to be the key to human communication and cognition, and it is around this assumption that the authors construct their model, which are claimed to offer a unified theory of cognition, to serve as the foundation for an approach to the study of the communication between human beings. However, as pointed out by Mey & Talbot (1988) and Talbot (1997) a drawback of the model is its lack of any social element. Harter (1992, p. 604) claims that Sperber & Wilson see 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 & Talbot (1988) and Talbot (1997) draw attention to the fact that considerations of cultural, social, and epistemological affiliation are notably absent from Sperber & Wilson's characterization of individuals' cognitive environments. Instead, differences between human beings are depicted as stemming solely from variations in physical environment and cognitive ability[22]. This makes it hard to understand how a

---

[20] A number of studies (e.g., MacRoberts & MacRoberts, 1986, 1989a, 1996) have noted that tertiary literature (e.g., review articles) frequently is cited instead of primary literature. However, what Harter seems to have in mind, when speaking about references that have led to relevant sources, is secondary literature (e.g. bibliographies). Secondary literature is normally not cited. (My distinction between secondary and tertiary sources stems from the UNISIST-model (UNISIST, 1971)).

[21] See e.g. the critical reviews of Gibbs (1987a, b), Hjørland (2000b), Mey & Talbot (1988), and Talbot (1997).

[22] In the second edition of their *Relevance*, Sperber & Wilson (1995, p. 279) admit they have developed their theory without taking sociocultural issues into account, and acknowledge that "the social character

hearer may infer a speaker's intentions. According to Sperber & Wilson (1986), a hearer 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) correctly observes, reading *Relevance* leaves one with the impression that everyone lives in the same kind of white, middle-class, 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" (Talbot, 1997, p. 447).

Harter (1992) must, perhaps unconsciously, have seen this. Why else would he add *cultural and social group identity* along with cognitive ability, educational background, and physical environment when defining what constitute an individual's manifest assumptions? In this light, the title of his article and his conclusion that relevance is a theoretical concept of psychology (Harter, 1992, p. 605) are difficult to comprehend. On the other hand, Harter just declares the significance of sociocultural issues without developing the point further. Instead, he dwells 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 asks (p. 614) what criteria users employ when they select bibliographic items for subsequent citation in their published works, and why an item is found relevant to begin with. But his narrow focus on cognitive issues restricts him from giving an adequate answer to the very same questions. His theory of citing does not provide an answer to the question why an item is found relevant to begin with.

1.5.2 Motives for citing

Empirical studies of authors' motives for citing emerged around the mid-1970s. In the first of these, Moravcsik & Murugesan (1975) constructed a classification scheme containing eight categories of references:

---

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 & Wilson (1997, p. 145) admitted that most relevance-theoretic work has largely ignored aspects of communication discussed in the sociological literature, but argued that they saw this more as "a reflection of a sound initial research strategy (which is likely to change as the field develops) than some silly anti-sociological bias".

1. Conceptual or Operational
2. Organic or Perfunctory
3. Evolutionary or Juxtapositional
4. Confirmative or Negational

The scheme was subsequently used for categorizing 706 references in 30 articles in the field of theoretical high-energy physics, published in the journal *Physical Review* during the period 1968-1972. The results of Moravcsik & Murugesan's study revealed, among other things, that 41 percent of the references were non-essential (Perfunctory), and that 14 percent of the references were negative (Negational). Shortly after Moravcsik & Murugesan's study, Chubin & Moitra (1975) published their results of a similar study. The authors had developed their own classification scheme and had categorized the references from 43 physics articles published in the period 1968-1969. The results of their content analysis revealed, among other things, that 20 percent of the references were non-essential, and that 5 percent of the references were negative. However, Chubin & Moitra (1975, p. 426) criticized the content analytical method for not being capable of describing the actual motives that authors have 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?)". Discussing how to conduct such an investigation, Chubin & Moitra (1975, p. 426) concluded that "direct questioning of authors about why they referenced who they did, and in what fashion, may be a beginning".

The first survey of authors' citation behavior was conducted ten years later. Brooks (1985) analyzed a number of theoretical models and isolated seven motives, which were shared by all models. He then asked 26 researchers from the University of Iowa to complete a questionnaire concerning their motives for their references in their recent articles. The results of Brooks' survey revealed, among other things, that persuasiveness was the most common purpose for citing, and that only two percent of the references were negational. In a similar study, Cano (1989) asked a group of engineers to complete a questionnaire concerning their motives for their references in 42 articles published in three different journals. Cano had designed the questionnaire using Moravcsik & Murugesan's classification scheme from 1975. The results of his survey revealed, among other things, that 26 percent of the references were deemed non-essential by the authors, and that only two percent were negational. Shadish et al. (1995) conducted the first major investigation of social scientists' motives for citing. Initially, the investigators prepared a list of 28 purposes, which afterwards was used to design a questionnaire. The questionnaire was mailed to a number of researchers who had published articles in psychological journals. 310 researchers completed the questionnaire. The results of the Shadish et al. survey revealed, among other things, 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. The reasons for citing a particular source and not citing another are very often partly unconscious or neglected by the individual. Consequently, 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 actually a variant of the *relevance dilemma*. According to Hjørland (2000a, 2002) and Hjørland & Sejer Christensen (2002) there are important precepts to science that are such an integral part of the researcher's life and culture that he or she is partially or wholly unaware of them. But the fact that the researcher is not conscious of their influence does not make them unimportant. In fact, the opposite is true. It is critical to recognize and understand what these precepts are, and how they affect the individual:

> "The relevance of observations (or, of course of documents presenting observations) depends on the theoretical assumptions, which guide the behavior of researchers. In other words: relevance is a function of theoretical assumptions" (Hjørland, 2000a, p. 210).

> "Theories exists more or less explicit in the literature and in the mind of other people, and they are more or less dominating in different professional discourses, in different places and they develop over time. They also exist as implicit assumptions in the existing forms of practices and institutions. Such theories, paradigms[23], and epistemologies form the basic *socio-cultural environment* in which the information seeking takes place, and they *imply* criteria for what information becomes relevant" (Hjørland & Sejer Christensen, 2002, p. 960).

[23] A paradigm is a way of thinking, perceiving, communicating and viewing the world. It is often called a worldview or a mindset. A paradigm is working at the subconscious level and people are rarely aware of their own paradigms. See chapter 2.

> "We mainly see the individual person as influenced by different theories, epistemologies, and paradigms, which are very often partly unconscious or neglected by the individual" (Hjørland, 2002, p. 261).

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 & Sejer Christensen (2002) breaks with the psychological understanding of relevance, which have dominated the field of library and information science for more than 50 years[24, 25, 26.]

1.6 Plea for a theoretical reorientation

Citation-based IS is largely influenced by methodological individualism, a view that conceives of knowledge as individual mental states rather than, or in opposition to, knowledge as a social process and the product of culture. Small's (1978) recognition of citations as concept symbols is one important exception though his later unraveling that "to understand the significance of references we need to examine the cognitive

---

[24] Hjørland (2002, p. 267) acknowledge Foskett (1972) and Swanson (1986) as sharing with him the same philosophical perspective on relevance. The suggestion of Foskett (1972, p. 78) that a useful explication of relevance may be found in Kuhn's concept of paradigm (1962), and in Ziman's definition of science as public knowledge (1968), do seem to anticipate some of Hjørland's thoughts. However, it is difficult to find similar suggestions in Swanson (1986), although he does write, "the question of whether a document is or is not about some topic can depend on the observer's point of view" (p. 397). O'Connor (1967, 1968) discussed the unfeasibility of speaking about relevance without defining in detail the particular circumstances. Possibly he deserves the credit for having been the first to address sociocultural issues of relevance.

[25] The relevance session at the 2003 ASIST Annual Meeting in Long Beach, CA revealed that the psychological understanding of relevance continues to dominate. One of the presenters, Tefko Saracevic, concluded his PowerPoint presentation by declaring (accompanied by a guitar fanfare!): "Relevance is still a mystery!". However, one cannot help thinking that if Saracevic and allied broadened their perspective, there would be no mystery at all. The presentations of Tefko Saracevic and the five other presenters are available here: http://web.utk.edu/~peilingw/ASIST/ and here: ftp://ftp.dimi.uniud.it/pub/mizzaro/ASIST03.zip. Both visited August 24., 2004.

[26] In fact, psychologizing is widespread in library and information science, not as an explicit position, but as an underlying tendency in most research. This has not always been so. The sub-field of knowledge organization, for instance, has developed its psychologizing tendencies gradually during the 20th century, as it shifted philosophically from a realist to an anti-realist position (Hjørland, 2004).

processes involved in generating written discourse" (Small, 1987, p. 339) and that "the view of 'citations as concept symbols' sees referencing as part of the cognitive process which produces written discourse" (Small, 1987, p. 340) gravely belittles sociocultural issues. Various other commentators have considered citing to be a private process (e.g., Chubin & Moitra, 1975, p. 426), an individual practice (e.g., Cronin, 1984, p. 29), or an internal phenomenon (e.g., Case & Higgins, 2000, p. 635). As a result, theories of citing are typically constructed from studies of individual citers conducted by interview techniques, thinking aloud methods, or the recording of behavioral patterns. But as pointed out by Van Raan (1998, p. 136), such inductive approaches are hardly appropriate for the construction of citation theories:

> "Most of this theorising is fixed at the role of the *citing author* – and his/her 'modal' references. This is not a very successful approach to explain the consequences 'at the other side', the *cited author*. It is if a physicist would strive for creating a framework of thermodynamics by making a 'theory' on the behaviour of individual molecules. Certainly there are crucial 'behaviour characteristics' of molecules: magnitude and direction of velocity, angular momentum. But *only* a statistical approach in terms of distribution-functions of these characteristic variables brings us to what we need: a thermodynamic theory. Pressure, volume and temperature are the main parameters of the thermodynamic ensemble of very many molecules. Likewise, citation analysis is at the 'thermodynamic side': it concerns an *ensemble of many citers*. Certainly, the individual characteristics of the citers are interesting, but the distribution-functions of these characteristics are the make-up of that part of the world which is relevant to bibliometric analysis".

Methodological collectivism represents an alternative to methodological individualism. The methodological collectivistic point of view conceives all human action as mediated by systems of social meanings. To enable understanding, explanation, or prediction of patterns of human activity, one needs first to penetrate the social world in which the individual is situated. Only if this is accomplished, is one able to analyze, interpret, and explain the activity of individuals. Hjørland (1997) makes a strong case in favor of methodological collectivism. Following a thorough examination of mentalist epistemologies and their shortcomings in relation to problems concerning human communication, he offers an alternative based on activity theory. The important feature of activity theory is that it considers cognition as an adaption to ecological and social environments. Agents are seen as members of discourse communities, disciplines, or collectives with varying degrees of typified needs, knowledge, and criteria of relevance.

IS should cease taking the individual's knowledge structures as its starting point, and instead focus on knowledge domains, disciplines, and other collective knowledge structures. An adequate theory of citing must thus desist from psychologizing the act of citing and instead recognize it as embedded within the sociocultural conventions of disciplines.

1.6.1 Scientific problem solving

Scientific disciplines are kinds of social institutions, whose fundamental function is to solve problems (Laudan, 1977). Any theory of citing must thus concern itself with studies of scientific problem-solving.

In his discussion of what defines a problem, Nickles (1981) proposes a model of scientific problem solving. He argues that a problem consists not exactly of its solution, but of all the conditions or constraints on the solution and the demand that the solution (an object satisfying the constraints) be found. According to Nickles (1981), a problem is a demand that a certain goal be achieved and constraints on the manner in which the goal is achieved; i.e. conditions of adequacy on the problem solution. This definition contradicts the earlier definition of erotetic logicians such as Belnap and Steel (1976) who defined a question as the set of acceptable answers and a request for an answer satisfying certain conditions of number, distinctness, and completeness. However, as pointed out by Nickles (1981), identifying a question with the set of its admissible answers and analogously a problem with its set of acceptable solutions logically generates the following dilemma: Either the possible solutions to a problem are known or they are not. If they are, one does not really have a problem at all, because one has all the solutions. If they are not known, again one does not have a problem, for how could one know what it is? This dilemma is in fact a variant from the Meno paradox found in Plato's Meno 80d-e: Either one knows what one is searching for or one does not. If one does know, one already has it; hence, inquiry is pointless. If one does not know, one would not recognize it even if one stumbled on it accidentally; hence, inquiry is impossible. Socrates proposed to resolve the paradox by appealing to religious authorities that taught immortality and reincarnation. According to Socrates, since the soul "has seen all things both here and in the other world" it "has learned everything that is" (81c). Hence what appears to be learning in this present life is actually recollection. However, as shown by Nickles (1981), the dilemma may be solved without resolving to spiritual philosophy. Inquiries are possible because a statement of a genuine question or problem is, aside from the demand, just a description of potential approaches to the answer – the object sought. If enough constraints are known to make the problem clear and well defined to investigators, that means they know what would count as a solution if they should happen to stumble on it. But the constraints not only tell them when they have found the solution. The constraints also designate the area of the problem space to look for it. This is because each constraint contributes to a characterization of the problem by helping to rule out some possible solutions as inadmissible. In a real sense, stating the problem is half the solution!

Laudan (1977), in his book *Progress and its problems: Towards a theory of scientific growth*, provides a similar account of scientific problem solving. In distinguishing empirical problems (roughly, problems concerning what the world is like) from conceptual problems (difficulties which arise in our effort to solve empirical problems), he argues that the aim of theorizing is to provide solutions to empirical problems and to avoid conceptual problems and anomalies. Theorizing, however, does not take place in a vacuum. Referring to various episodes in the history of the sciences, Laudan points out that theorizing occurs within the boundaries of *research traditions*. A research tradition, he claims, provides a set of guidelines for the development of specific theories. Part of those guidelines constitutes an ontology, which specifies the types of fundamental entities that exist in the domain or domains within which the research tradition is embedded. The function of specific theories is thus to explain all the empirical problems in the domain by reducing them to the ontology of the research tradition.

> "If the research tradition is behaviorism, for instance, it tells us that the only legitimate entities which behavioristic theories can postulate are directly and publicly observable physical and physiological signs. If the research tradition is that of Cartesian physics, it specifies that only matter and mind exists, and that theories which talk of other types of substances (or of "mixed" mind and matter) are unacceptable" (Laudan, 1977, p. 79).

In addition, Laudan argues, a research tradition will also specify certain modes of procedure that represent the legitimate methods of inquiry open to the researchers within that tradition. He therefore concludes that research traditions are simply sets of ontological and methodological do's and don'ts. To attempt what is forbidden by the metaphysics and methodology of a research tradition is to put oneself outside that tradition and to repudiate it. Needless to say, this is not necessarily bad. Some of the most important revolutions in scientific thought have indeed come from thinkers who had the ingenuity to break with the research traditions of their time and to establish new ones. But what we must preserve, if we are to understand either the logic or the history of science, is, according to Laudan, the notion of the integrity of a research tradition, for it is precisely that integrity which stimulates, defines and delimits what can count as solutions to scientific problems.

1.7 Objectives and organization of the dissertation

Although thirty years has passed since Mulkay (1974, p. 111) concluded that "in fact we know very little about who cites whom in science and why", a comprehensive theory of citing is still missing. A number of theories have been proposed over the years, but none of them seem to provide satisfactory answers. The main objective of this dissertation is thus to contribute with new knowledge that can help to reduce some of this ignorance. Specifically, it will attempt to answer two related questions that have puzzled citation theorists for quite some time:

1. What makes authors cite their influences?

2. What makes authors cite some resources and not others?

<u>Chapter 2.</u> analyzes the normative theory of citing and the social constructivist theory of citing in detail. The aim is to document that these two renowned theories of citing fail to provide adequate answers to the first question.

<u>Chapter 3.</u> seeks to provide a theoretical explanation for the first question. The proposal is inspired by research from the field of evolutionary biology - in particular by the work of the Israeli biologist Amotz Zahavi who has worked to explain honesty and deception in animal communication for more than thirty years.

<u>Chapter 4.</u> seeks to document the existence of a blind spot in the bibliometric literature, which possibly have prevented citation theorists from providing a comprehensive answer to the second question. Previous theorists have argued that authors tend to cite sources from their own specialty while ignoring works from other specialties. This is probably partly correct. The chapter argues, however, that research traditions play an important role in the allocation of references (and citations). It ends with a testable hypothesis, which is dealt with in the following chapter.

Chapter 5. presents a structural bibliometric analysis of two bibliographies: a psychological bibliography and a communication theoretical bibliography. The aim of the chapter is to test the hypothesis that specialties and research traditions seize a strong influnce on the structural dynamics of citation networks. A confirmation of this hypothesis is believed to bring us closer to an understanding of why authors cite some resources and not others.

Chapter 6. concludes the dissertation.

Your theory is crazy but it's not crazy enough to be true (Niels Bohr, to a young physicist)[27].

2 Theories of citing[28]

Why do authors cite? What is the rationale of this practice? Citation analysts have been concerned with these questions, at least since Norman Kaplan's article *The Norms of Citation Behaviour: Prolegomena to the Footnote* (1965), and have produced a variety of answers. As noted by Baldi (1998), trying to capture the various reasons why scientists cite became a "cottage industry" between 1965 and 1979. During this period, no less than ten different classification schemes were produced. These schemes had anywhere from 4 to 29 categories capturing various reasons for citing (e.g., "historical importance", "future research", "illustration", "disclaiming", "definition", "deliberate premeditation", "citations as reflection of author biases", etc.). During the same period, a number of researchers proposed specific theories of citing. Among these were Ravetz (1971) who introduced the idea of intellectual property and intellectual property rights based on the interpretation that the publication and citation process combine reward and recognition; Bavelas (1978) who proposed that citations are documentary evidence that the writer qualifies for membership in the target discourse community by demonstrating his or her familiarity with the field; Gilbert (1977) who proposed that authors use references as a tool for persuasion, a device that makes statements appear more authoritative; and Garfield (1965) and Small (1978) who discussed the symbolic function of citations and argued that authors use references as a shorthand for ideas which they deploy in the course of making arguments.

However, all these classification schemes and theories may be divided in two groups: In brief, theories of the first group view science and scholarship to consist of a unique set of institutional arrangements, behavioral norms, and methods, which enable scientists and scholars to study Nature as Nature really is. According to this view, the products of science are independent of personal and social forces and are, consequently, beyond the realm of psychology and sociology. Theories of the second group challenge

[27] http://wikiquote.org/wiki/Niels_Bohr. Visited August 24., 2004.

[28] A general reference for this chapter is Larry Laudan's *Science and Values* (1984).

the views of the first group. According to the second group, science is subjective and social, and the products of science and scholarship are thus depending on personal and social forces. The second group confronts, moreover, the idea that science and scholarship consist of a unique set of institutional arrangements, behavioral norms, and methods, which enable scientists and scholars to study Nature as Nature really is. According to the second group, scientific knowledge is socially negotiated, not given by Nature. The two different views on science and scholarship give rise to two different theoretical views on citing, normally known as the normative theory and the social constructivist theory of citing. In order to comprehend their differences and assess their strength and weaknesses, one needs to study their philosophical and sociological foundations.

2.1 The normative theory of citing

The normative theory of citing is based on the assumption that science is a normative institution governed by internal rewards and sanctions. Scientists are believed to 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 worth as well as the cognitive, methodological, or topical content of the cited articles (Baldi, 1998).

This sub-section seeks to demonstrate that the normative theory of citing is grounded in a view of science and scholarship comparable to the views held by the logical positivists and critical rationalists, or more precisely, to the view that we may attain knowledge about Nature by invoking appropriate rules of evidence. Specifically, logical positivists and critical rationalists argue that there are rules of scientific methodology, which are responsible for producing consensus in a rational community such as science. This is evident even in some early textbooks on the philosophy of science.

2.1.1 Philosophy of science

The first textbooks on the philosophy of science were published in the 1840's. William Whewell's *The Philosophy of the Inductive Sciences* from 1840 and John Stuart Mill's *A System of Logic* from 1843 were among the earliest of these.

Whewell (1840) analyzed scientific knowledge of supposed external nature (excluding the mind itself) holding that scientific knowledge is based upon sensations and ideas (the former being the objective element (caused by objects), the latter a subjective element (provided by the knowing subject)). Consciously entertained facts and theories correspond according to Whewell to sensations and ideas, but not completely, because all facts implicitly include ideas (and so, possibly, theory). He divided the methods of science into methods of observation, of obtaining clear ideas, and of induction. The methods of observation include quantitative observation and the perception of similarities. According to Whewell, clear ideas result from intellectual education, and from discussion, including discussions of definitions. But science proceeds by induction, including the use of quantitative techniques to smooth out the irregularities of observation and the formation and empirical testing of tentative hypotheses.

In *A System of Logic*, Mill (1843) analyzed the methods of science (including psychology and the social sciences) more fully than Whewell did. In his analysis of the experimental method, Mill included the methods of agreement, and added the method of residues, which directs the investigator to look for the causes of those effects that remain after all other effects have been assigned to known causes, and the method of concomitant variations, according to which those phenomena that vary regularly in quantitative degree with one another are assumed to be causally related. Mill, like Whewell, emphasized the role of new or pre-existing concepts and names in scientific observation, and the role that classification plays in induction. His faith in the inductive method led him to propose that psychology and the social sciences should adopt the same explanatory structures as the natural sciences.

These textbooks are clearly reflections of a positivistic understanding of science.

2.1.1.1 Logical positivism

In the philosophy of science *positivism* is shorthand for *logical positivism* or *logical empiricism*, terms that are not exactly identical, but will be treated so here for the sake of simplicity. The term *logical positivism* is usually associated with the philosophical position that emerged around the Vienna Circle, a group of Austrian and German philosophers and scientists. Among its members were Moritz Schlick (founder of the Vienna Circle), Rudolf Carnap, Hans Reichenbach, Herbert Feigl, Philipp Frank, Kurt Grelling, Hans Hahn, Carl Gustav Hempel, Victor Kraft, Otto Neurath, and Friedrich Waismann. The Vienna Circle played an important role in the philosophy of science from the 1930's to the 1950's.

The most significant concept associated with the positivist philosophy of science is the verifiability principle. The Vienna Circle supposed that it had derived its principle of verifiability from Ludwig Wittgenstein's *Tractatus Logico-Philosophicus* from

1922[29]. In the Tractatus Wittgenstein writes that "To understand a proposition means to know what is the case, if it is true" (Wittgenstein, 1922, § 4.024). This means, in effect, that to understand a proposition is to know what content it has, and to know what content it has is to know what would be the case if the proposition were true. Wittgenstein held that every proposition is a truth-function of atomic propositions – the principal contention of logical atomism (also known as the thesis of extensionality). Further, an atomic proposition, according to the Tractatus, is an arrangement of names or primitive signs and "the references of primitive signs can be made clear by elucidations. Elucidations are propositions containing the primitive signs. Thus they can only be understood, if one is acquainted with the references of these signs" (Wittgenstein, 1922, § 3.263). The Vienna Circle took this to mean that primitive signs must refer to objects of acquaintance, and these objects are, by definition, the objects that we directly experience. This seemed to lead inevitably to the supposition that Wittgenstein's elementary sentences expressing atomic propositions are observation sentences. And since the members of the Vienna Circle also accepted the thesis of extensionality, they concluded that the meaning of every genuine statement is completely expressible by means of observation sentences alone (Friedman, 1998).

The main theses of logical positivism were presented in many articles and books. They were given prominence, however, through Sir Alfred Ayer's *Language, Truth, and Logic*, first published in 1936. These doctrines, some of which were subsequently modified and refined by other logical positivists (and later rejected by Ayer himself[30]), may be briefly stated as follows (Ayer, 1936):

> (A) A proposition, or a statement, is factually meaningful only if it is verifiable. This is understood in the sense that the proposition can be judged probable from experience, not in the sense that its truth can be conclusively established by experience. (B) A proposition is verifiable only if it is either an experiential proposition or one from which some experiential proposition can be deduced in conjunction with other premises. (C) A proposition is formally meaningful only if it is true by virtue of the definitions of its terms - that is, tautologous. (D) The laws of logic and mathematics are all

[29] The major work of Wittgenstein's early period and the only book published during his lifetime. The German version was published in 1921 in *Annalen der Naturphilosophie.*

[30] Asked in 1978 what he now regarded as the main defects of logical positivism, Ayer replied: "I suppose the most important of the defects was that nearly all of it was false" (Magee, 1978, p. 107). However, he went on to agree that he still believed in "the same general approach".

> tautologous. (E) A proposition is literally meaningful only if it is either verifiable or tautologous. (F) Since metaphysical statements are neither verifiable nor tautologous, they are literally meaningless. (G) Since ethical, aesthetical, and theological statements also fail to meet the same conditions, they too are cognitively meaningless - although they may possess “emotive” meaning. (H) Since metaphysics, ethics, philosophy of religion, and aesthetics are all eliminated, the only tasks of philosophy are clarification and analysis. Thus, the propositions of philosophy are linguistic, not factual, and philosophy is an area of logic.

Positivists, as evident in the textbooks of Whewell and Mill, hold that methods are given a priory independent of the research question, and that induction is the only proper way of reasoning. The basic ideas of naïve inductivism are that scientific progress result from close, careful and accurate observation of facts, and that the process of observation can reveal certain regularities or patterns in the natural phenomena. According to this philosophy, the observer should move by a process of inductive generalization from the observed regularities to the formulation of laws and theories. To make an inductive generalization is to move from the evidence that some sequence or pattern has been observed in many cases to the conclusion that the same pattern will hold for all similar cases[31].

Logical positivism, though it has been declared dead a number of times (e.g., Passmore (1967, p. 56) and Popper (1974, p. 69)), continues to dominate many research areas (Hjørland, 2004).

2.1.1.2 Critical rationalism

In the mid-eighteenth century, David Hume (1711-1776) made a devastating critique of inductivism as the basis for scientific knowledge about the natural world. Specifically, he drew attention to the fact that there is no logically justifiable basis for generalizing from a set of singular statements to a universal statement. Such generalisation simply assumes that all future instances, or newly observed instances, will be the same as past ones, and we simply cannot know that. If we only have a finite number of observations out of an infinite number of possible observations, the probability that our postulate is true is virtually zero. Sir Karl Popper (1902-1994) recognized that Hume’s critique

[31] Induction is the procedure that Sherlock Holmes relied on (finding a clue and building an argument that accounted for the observed facts) although he (or Sir Arthur Conan Doyle) mistakenly called it “deduction”.

shows that inductive knowledge is impossible. Scientific theories entail universal generalizations, which logically cannot be inferred from inductive inferences. Popper, although a regular visitor at the meetings of the Vienna Circle, published in his book *Logik der Forschung* from 1934[32] a comprehensive critique of positivism. The book was meant to provide a theory of knowledge and, at the same time, to be a treatise on the falsificationist method of science (Popper, 1974, p. 67).

Popper held, contrary to the logical positivists, that a theory is trustworthy not because we have any positive or direct reasons in its favor, but because we have not managed to falsify it (yet). According to Popper, a scientific theory must be posed without any prior justification. It should gain its confirmation or corroboration not by experimental evidence demonstrating *why* it should hold, but by the fact that all attempts at showing *why* it does *not* hold have failed. Experimental researchers must thus try to refute or *falsify* scientific theories, rather than to justify or *verify* them. Theorists who design theories should do this in such a way that they can be refuted most easily. A theory must never be immunized against possible falsification, e.g., by the introduction of *ad hoc* hypotheses (Popper, 1995, p. 42).

The main argument in favor of Popper's falsificationist proposal is that the converse proposal, verificationism, is bound to fail. Verificationism, which underlies the inductivist methodology, claims that a scientific theory is supported by a collection of verifying instances. However, as noted above, the inductive reasoning from a finite set of observations to a general statement is not logically valid if the domain of reasoning is infinite or at least indefinite. Furthermore, any attempt at creating a special non-deductive form of reasoning (an inductive logic) can only be justified by means of induction itself, thus ending up in circular arguments. To quote Popper's famous example: "No matter how many instances of white swans we may have observed, this does not justify the conclusion that all swans are white" (Popper, 1995, p. 27).

Popper held that in reality we never argue from facts to theories, unless by way of refutation or falsification. Instead, we always start our investigations with problems and theories: "We always find ourselves in a certain problem situation; and we choose a problem which we hope we may be able to solve. The solution, always tentative, consists in a theory, a hypothesis, a conjecture. The various competing theories are compared and critically discussed, in order to detect their shortcomings; and the always changing, always inconclusive results of the critical discussion constitute what may be called "the science of the day"" (Popper, 1974, p. 68).

Popper held that falsification (unlike verification) of a general statement by experimental or observational means is logically unproblematic. The statement "All

[32] Published in English in 1959 as *The Logic of Scientific Discovery*.

swans are white" can be falsified by the observation of a single black swan. The logic involved here is just ordinary deductive logic, due to which the falsity of an instance derived from a general hypothesis falsifies the hypothesis itself. Deduction begins with theory; moves to hypotheses derived from the theory; tests these hypotheses by means of observation that may falsify the theory in question.

In the 1959 preface to the English translation of *Logik der Forschung* Popper (1995, p. 16) italicized the words "*rational discussion*" and "*critically*" in order to stress that he equated the rational attitude and the critical. He later coined the term *critical rationalism* to describe his philosophy of science (Popper, 1974, p. 92). This philosophy emphasizes firstly, that whenever we propose a solution to a problem, we must try as hard as we can to overthrow our solution, rather than defend it, and secondly, that criticism will be fruitful only if we state our problems as clearly as we can, allowing them to be critically discussed and possibly falsified.

2.1.1.3 The Leibnizian ideal

Logical positivists and critical rationalists subscribe to the *Leibnizian ideal*, an ideal normally held to be a result of Leibniz' (1646-1716) philosophy[33]. It rests on the notions that the world is governed by a variety of general principles, that there must be a sufficient reason for everything in the world, that there are no jumps in nature, and that there must be exactly the same power in the full cause as there is in the complete effect (Garber, 1998). As a lifelong project, Leibniz, like other great philosophers (e.g.,

[33] However, at least since Bacon (1561-1626) philosophers had assumed there to be an algorithm or set of algorithms which would permit any impartial observer to judge the degree to which a certain body of data rendered different explanations of those data true or false, probable or improbable. See, for instance, Bacon's three aphorisms regarding the basic themes of his *Novum Organum* (1620), a script intended as an account of a new logic, designed to substitute the Aristotelian syllogism, which Bacon saw as having held back and even corrupted the investigation of nature (cited from Milton, 1998):

> "Man, being the servant and interpreter of nature, can only do and understand so much… as he has observed in fact or in thought of the order of nature: beyond this he neither knows anything nor can do anything.
>
> Neither the naked hand nor the understanding left to itself can effect much. It is by instruments and helps that the work is done, which are as much wanted for the understanding as for the hand. And as the instruments of the hand either give motion or guide it, so the instruments of the mind supply either suggestions for the understanding or cautions.
>
> Human knowledge and human power meet in one, for where the cause is not known the effect cannot be produced. Nature to be commanded must be obeyed; and that which in contemplation is as the cause is in operation as the rule".

Descartes and Spinoza), sought to invent a universal language based not on geometry but on a calculus perfected down to the level of logic, which would then provide a common mathematical, philosophical, logical and scientific foundation to all thought. In Leibniz' ideal system all philosophical, scientific and mathematical disagreements could be resolved the same way: By a series of rigorous calculations.

Despite the fact that positivists and critical rationalists disagree on what might be called *the rule of the game* (verification or falsification), both camps agree that science is a rule-governed activity. Or more precisely, both camps subscribe to the Leibnizian ideal that we may attain knowledge about Nature by invoking appropriate rules of evidence. Specifically, these philosophers argue that there are rules of scientific methodology, which are responsible for producing consensus in a rational community such as science. If scientists disagree about the validity of two rival theories, they need only consult the appropriate rules of evidence to see, which theory is better supported. Should those rules fail to decide the issue immediately, all they are required to do are to collect new and more discriminating evidence, which will differentially confirm or disconfirm one of the theories under consideration. Thus, both camps hold science to be a consensual activity because scientists (insofar as they are rational) shape their beliefs according to the canons of shared scientific methodology or logic.

2.1.2 Mertonian sociology of science

Whereas the early philosophers of science located the consensual character of science in the scientists' adherence to the canons of a logic of scientific interference, early sociologists of science generally recognized that consensus in science is governed by a particular scientific ethos, i.e., by a set of rules that are supposed to establish trust in, and guarantee the reliability of, the knowledge created in the process. This ethos was given its most succinct and influential formulation by the American sociologist Robert King Merton (1910-2003) who defined it in terms of four basic norms. The four norms of science, often referred to by the acronym *CUDOS*, are, according to Merton ([1942] 1973), "Communism"[34], "Universalism"[35], "Disinterestedness"[36], and "Organized

[34] Communism (later termed "communality") expresses the norm that because all scientific inquiry relies on prior scientists' efforts to some degree, scientific advancements should be added to the pool of communal knowledge. Property rights should be kept to a minimum, and the members of a community must exchange the scientists' claims to intellectual property for recognition and esteem.

[35] Universalism is the principle that truth claims should be subjected to pre-established, impersonal justification criteria that exclude consideration of particularistic criteria such as a scientist's race, nationality, class, or religion. The norm of universalism embodies the notion that valid research findings

Skepticism”[37]. Merton later added the norms of “originality” and “humility” (Merton, [1957] 1973, p. 293-305), and other researchers have suggested supplementary norms that to some degree overlap with the CUDOS norms. Barber (1952) proposed “rationality”, “emotional neutrality”, and “individualism” as norms of science and Storer (1966) suggested that “objectivity” and “generality” are characteristic norms of scientific knowledge. Polanyi (1951) pointed to the importance of researchers’ individual autonomy and Hagstrom (1965) stressed the norm of “independence” as a necessity for the operation of science.

Merton ([1942] 1973, p. 269) held the CUDOS norms of science to be binding on the man of science: “The norms are expressed in the form of prescriptions, proscriptions, preferences and permissions. They are legitimatized in terms of institutional values. These imperatives, transmitted by precept and example and reinforced by sanctions are in varying degrees internalized by the scientist, thus fashioning his scientific conscience or, if one prefers the latter-day phrase, his superego”. With reference to these norms, Merton was able to account for the consensual character of science. Because men of science share the same norms or standards, they are able to form stable patterns of consensus.

Merton was later to find what he regarded as strong support for the hypothesis of shared scientific norms and standards in his and Zuckerman’s research on journal rejection rates[38]. In their study of evaluation patterns in science, Zuckerman & Merton ([1971] 1973) found significantly lower rejection rates for journals in the natural sciences than for journals in the social sciences and humanities. The physics journals of their sample rejected only 24 percent of the submitted articles, whereas their sociology and philosophy journals rejected more than 80 percent. These divergences were taken as evidence for higher levels of consensus in the natural sciences as opposed to the social sciences and humanities. Philosophers and sociologists were perceived as being unable

are valid regardless of the particular scientist or institution performing the research. In other words, that which is true is universally true for the entire scientific community.

[36] Disinterestedness conveys the idea that scientists should seek truth objectively, without considering their individual interests.

[37] Organized Scepticism captures the norm that before results should be deemed valid, the scientific community at large should examine their reliability. According to Merton ([1942] 1973: 277), scientists should engage in the “detached scrutiny of beliefs in terms of empirical and logical criteria” that is free from infections from outside institutions such as religion.

[38] Cole (1992, chapter 5) seriously disputes the results of Zuckerman & Merton ([1971] 1973).

to agree on what constituted significant or solid research, whereas the physicists were perceived as being able to agree because of their shared norms or standards. According to Zuckerman & Merton ([1971] 1973, p. 472) their results suggest that "these fields of learning [philosophy and sociology] are not greatly institutionalized in the reasonably precise sense that editors and referees on the one side and would-be contributors on the other almost always share norms of what constitutes adequate scholarship", and further that "the marked differences in rejection rates of journals in the various disciplines can be tentatively ascribed […] in part to differences in the extent of consensus with regard to standards of adequate science and scholarship" (Zuckerman & Merton, [1971] 1973, p. 474).

Although Merton never claimed the ethos of science to be operating 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. Michael Polanyi (1951) provided an explanation for the high degree of consensus in science, which echoes the views of Merton and thus aligns him with the Mertonian School. According to Polanyi, it is precisely the internalization of shared norms or standards, which explains what he dubbed the "spontaneous co-ordination of scientists" (Polanyi, 1951, p. 39). He therefore proposed that the coherence of science should be regarded as an expression of the common rootedness of scientists in the same spiritual reality. Because, as he reasoned: "Then only can we properly understand that at every step, each is pursuing a common underlying purpose and that each can sufficiently judge – in general accordance with other scientific opinion – whether his contribution is valid or not" (Polanyi, 1951, p. 39).

Evidently, as explained by Merton ([1942] 1973, p. 270) himself, the Mertonian School adopted a sociological version of the Leibnizian ideal:

> "The institutional goal of science is the extension of certified knowledge. The technical methods employed towards this end provide the relevant definition of knowledge: empirically confirmed and logically consistent statements of regularities (which are, in effect, predictions). The institutional imperatives (mores) derive from the goal and the methods. The entire structure of technical and moral norms implements the final objective. The technical norm of empirical evidence, adequate and reliable, is a prerequisite for sustained true prediction; the technical norm of logical consistency, a prerequisite for systematic and valid prediction. The mores of

> science possess a methodologic rationale but they are binding, not only because they are procedurally efficient, but because they are believed right and good. They are moral as well as technical prescriptions".

The Mertonian School thus shares its faith in science being a rule-governed activity with the philosophical schools of positivism and rationalism. Does this imply that Merton and his followers are/were positivists/rationalists? One could possibly argue that the Mertonian School subscribes to ideals resembling those of logical positivism. In fact, the Mertonian norm of universalism clearly reflects positivistic ideals. Compare, for instance, this quotation from Merton (1957, p. 607) with Ayers doctrines of logical positivism[39]:

> "The acceptance or rejection of claims entering the lists of science is not to depend on the personal or social attributes of their protagonist. […] The circumstance that scientifically verified formulations refer to objective sequences and correlations militates against all efforts to impose particularistic criteria of validity. […] The imperative of universalism is rooted deep in the impersonal character of science".

The early sociologists of science knew, of course, about many instances of scientists resisting scientific discoveries and instances of scientists disagreeing on the merits of particular findings. But sociologists such as Merton and in particular Bernard Barber 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. Barber (1961) described a number of factors influencing the working scientist. These include methodological conceptions, religious ideas, professional standing, professional specialization, and societies, schools, and seniority. Thus, if it seems odd from a normative perspective that scientists, supposedly sharing the same norms, had disagreed for centuries about the merits of specific theories (e.g., the astronomical theories of Ptolemy and Copernicus), then an early sociologist of science would just argue that the followers of one of the theories had not fully internalized the appropriate norms of science[40].

---

[39] See section 2.1.1.1.

[40] The Ptolemaists, for instance, were said to have been unable to break with the traditional patterns of thought about the earth's lack of motion because of their religious beliefs (Barber, 1961, p. 597).

It is not my intention to attribute a "naïve, idealized, Arcadian image of scientists" to Robert K. Merton as I agree with his biographer Piotr 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. Thus, for instance, 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 on the extent to which scientists adhere to the CUDOS norms, are not dressed with such reservations. On the contrary, the "younger" Merton ([1942] 1973, p. 276) speaks about "the virtual absence of fraud in the annals of science", and claims that "deviant practices" in general "are extremely infrequent" (Merton, [1957] 1973, p. 311). The "older" Merton sometimes seems to forget the reservations from 1963. In 1977 he discussed why he did not get the idea for the citation index before Eugene Garfield did: "all the substantive ingredients for invention of that tool were being observed back in 1942[41]" (Merton, 1977, p. 49):

> "The theoretical model had pictured science as a system of open publication involving a reciprocity of role obligations on the part of scientists, supported by the incentive of peer recognition. In this role system, each practicing scientist was required to publish the method and results of his investigations just as each scientist making use of that work was required to acknowledge the source in the publication of his own investigations. That acknowledgement (citation and reference) was in turn the principal form in which peer recognition routinely occurred".

Merton (1977) apparently believes his 1942 theory of scientific practice to be a sound foundation for a citation index, and thus for a theory of citing. As I will soon demonstrate, a number of other researchers and scholars 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 "older" Merton.

2.1.3. Basic hypotheses of the normative theory of citing

Various sources to the history of bibliometrics (e.g., Lawani, 1981, Hertzel, 1987) recognize the citation analysis conducted by Gross & Gross (1927) as the first of its

[41] Here, Merton refers without actual reference to his article *The Normative Structure of Science* ([1942] 1973).

kind. Faced with the question "what files of scientific periodicals are needed in a college library successfully to prepare the student for advanced work, taking into consideration also those materials necessary for the stimulation and intellectual development of the faculty", Gross and Gross "decided to seek an arbitrary standard of some kind by which to measure the desirability of purchasing a particular journal" (Gross & Gross, 1927, p. 386). In discussing potential procedures, they rejected the method of just sitting down and compiling a list of those journals, which were considered indispensable, because, as they wrote, "such a procedure might prove eminently successful in certain cases, but it seems reasonably certain that often the result would be seasoned too much by the needs, likes and dislikes of the compiler" (Gross & Gross, 1927, p. 386). Instead, they decided to tabulate the references in a single volume of *The Journal of the American Chemical Society* and subsequently to identify the candidate journals by their citation frequencies, i.e., by the number of times they were cited in the sample. According to Gross and Gross this tabulation could be considered statistically and employed to predict the future needs of scientific periodicals.

The Gross's faith in the existence of a free and independent standard that could be utilized for the prediction of future needs and the selection of library materials resembles a positivistic faith in the inductive method. Pure observation of references and their patterns were thought to make a method suitable for the selection of excellence. A method that would lead inevitably to the identification of the paramount of scholarly contributions: "Consideration of the method of investigation here employed will show that we are concerned not merely with the quantity of work published […], but that in reality we are concerned only with the good work, the work which have survived and which has proved of value to the investigators who followed" (Gross, 1927, p. 641).

Although the Gross's did not spell out their basic assumptions about scientists' citation practices and despite the fact that Merton formulated the CUDOS norms some fifteen years after their 1927 citation analysis, it can reasonably be assumed that the Gross's viewed the citing behavior of scientists in much the same way as the early sociologists of science. Firstly, investigators cite the materials that have proved of value to them. Secondly, scientists, when evaluating the work of others, are behaving universalistically, i.e., their decisions of what to cite are not influenced by functionally irrelevant characteristics such as the scientists' sex, race, religion, or rank. Thirdly, scientists are disinterested and do not seek to gain personal advantages by flattering others or citing themselves. And fourthly, scientists treat their own work with the same organized skepticism as the work of others. These assumptions must essentially have founded the Gross's belief, that tabulation of references reflects the quality and worth of cited items. Evidently, as illustrated by Linda Smith's list of basic assumptions motivating citation analysis in general, these assumptions have motivated many succeeding studies as well (Smith, 1981, p. 87-89):

> 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 Eugene Garfield's book *Citation Indexing: Its Theory and Application in Science, Technology, and Humanities (1979)*, Merton 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, 1979, p. viii). 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 and claimed 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).

In doing so, he aligned himself with his former students Jonathan R. Cole and Stephen Cole, who had previously legitimized the use of citation analysis in their work on social stratification in science by arguing that "the norms of science require scientists to cite the work that they have found useful in pursuing their own research, and for the most part they abide by these norms" (Cole & Cole, 1973, p. 221).

Table 2.1. *Citer motivations according to Garfield (1965, p. 85).*

1	Paying homage to pioneers
2	Giving credit for related work (homage to peers)
3	Identifying methodology, equipment, etc.
4	Providing background reading
5	Correcting one's own work
6	Correcting the work of others
7	Criticizing previous work
8	Substantiating claims
9	Alerting to forthcoming work
10	Providing leads to poorly disseminated, poorly indexed, or uncited work
11	Authenticating data and classes of fact-physical constants, etc.
12	Identifying original publications in which an idea or concept was discussed
13	Identifying original publication or other work describing an eponymic concept or term
14	Disclaiming work or ideas of others (negative claims)
15	Disputing priority claims of others (negative homage)

Norman Kaplan's article *The Norms of Citation Behaviour* from 1965 is generally believed to be the first explicit account of citing as normative bahavior (e.g. Small, 1998)[42]. Kaplan (1965) held that "footnoting practices" are passed on by word of mouth from professor to student and by an examination of the varying practices of different journals. Ravetz (1971, p. 256-257) equally held citing to be governed by an etiquette, which he described as "a purely informal, perhaps tacit and unselfconscious, craft knowledge". The major function of the etiquette or footnoting practice is, according to Kaplan, the reaffirmation of the underlying norms of scientific behavior[43].

[42] Larsen (2004, p. 53), however, claims that "research on citer motivations from the social constructivist position was initiated by Kaplan (1965)". He consequently seems to think that Kaplan's article reflects social constructivist tendencies. Although it is true that Kaplan (1965, p. 183) did note that little was known about the norms of citation behavior operating in actual practice, and that this was an important research topic, this hardly qualifies as initiating research from the social constructivist position. Others have noted correctly that Kaplan (1965) claims that scientists feel constrained by norms to give credit where credit is due (e.g., Cozzens, 1989, p. 439). This claim, as we shall see in section 2.2, clearly acquit Kaplan from social constructivism.

About the same time as Kaplan (1965, p. 181) issued his belief in citation practices being "in large part a social device for coping with problems of property rights and priority claims"[44] Eugene Garfield published a list consisting of fifteen reasons why authors cite (table 2.1). The list represents a mental picture of scientists being faithful disciples of the CUDOS norms. Although Garfield was familiar with the work of Robert King Merton[45] his list does not refer explicitly to him or the CUDOS norms. The fifteen reasons are just listed one after another without justification or discussion.

Melvin Weinstock, in his article on citation indexes in *Encyclopedia of Library and Information Science,* offers the same list of reasons why authors cite[46]. Weinstock's introduction to the fifteen reasons is worth quoting as it precisely discloses the most basic assumption of the normative theory of citing:

> "Scientific tradition requires that when a reputable scientist or technologist publishes an article, he should refer to earlier articles which relate to his theme. These references are supposed to identify those earlier researchers whose concepts, methods, apparatus, etc. inspired or were used by the author in developing his own article. Some specific reasons for using citations are as follows: [1-15]" (Weinstock, 1971, p. 19).

2.1.3.1 Early tests of the normative theory of citing

According to the normative theory, failure to give credit where credit is due is unusual. Cole & Cole (1972, p. 370), for example, state 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 (1977a, p. 7) concurs by declaring that "the vast majority of citations are

---

[43] As evidence for the actual existence of such norms, Kaplan (1965, p. 180) pointed to the statistical regularity of references in the periodical literature, which had earlier been reported by Derek de Solla Price (1964).

[44] Ravetz (1971, p. 257) also argued that citations function to divide the property in the published report, and to provide an "income" to the owner of the property used.

[45] Garfield had already met Merton in 1962 (Garfield, 1998b).

[46] Weinstock does not credit Eugene Garfield as the originator of the list.

accurate and the vast majority of papers do properly cite the earlier literature". However, in the next sentence, Garfield admits that this assertion had not been empirically substantiated: "Unfortunately, there has never been a definitive study of this assertion". Garfield was right. The basic assumption of the normative theory of citing was not tested before the 1980's. The pioneers of this work were not adherents of the normative theory, but a group of skeptics including among others the two biologists Michael H. MacRoberts and Barbara R. MacRoberts and the information scientist Terrence A. Brooks.

The MacRoberts couple authored and co-authored a number of articles during the 1980s and 1990s in which they argued that citation analysis is an invalid tool for research evaluation (MacRoberts, 1997, MacRoberts & MacRoberts, 1984, 1986, 1987a, 1987b, 1988, 1989a, 1989b, 1996). In these articles, MacRoberts & MacRoberts challenge the basic assumption of the normative theory of citing - that scientists cite their influences. In their 1986 paper entitled *Quantitative measures of communication in science: A study of the formal level* MacRoberts & MacRoberts report the results of their test of this assumption. The MacRoberts couple had read and analyzed fifteen randomly selected papers in the history of genetics, a subject with which they claimed to be familiar, and had found that from zero (paper had no references or footnotes) to 64 percent influence was captured in references and footnotes. After having reconstructed the bibliographies of the fifteen papers, they were able to estimate that the papers required some 719 references at a minimum to cover the influence evident in them, when in fact they contained only 216 - a coverage of thirty percent for the entire sample. In their 1996 paper MacRoberts & MacRoberts (1996, p. 436) claim that this percentage typifies all fields with which they are familiar (botany, zoology, ethology, sociology, and psychology) and conclude 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).

Terrence A. Brooks published two papers in the mid-1980s, which also challenge the basic assumption of the normative theory of citing (Brooks, 1985, 1986). In these papers, Brooks reports the results of a survey covering 26 authors at the University of Iowa. The authors had been asked to indicate their motivations for giving each reference in their recently published articles by rating seven motives for citing. One of the motives read "persuasiveness". Brooks claims that the results of his survey make probable that authors cite for many reasons, giving credit being the least important. Of the 900 references studied, Brooks actually found that about 70 percent were multiply motivated and concluded: “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” (Brooks, 1985, p. 228). However, as pointed out by White (2004), the results of Brooks’ survey need to be assessed with caution, as the respondents almost certainly misunderstood the motive reading “persuasiveness” to denote “citing to help build a case”, and not “citing to utilize previous literature in a calculated attempt to self-justify”.

2.1.3.2 The average mantra

The results of the MacRoberts couple and Terrence A. Brooks question the validity of citation analysis. When authors are revealed to be citing just a fraction of their true influences, it appears to be futile to measure influence, impact or quality by citation counts. Consequently, proponents of citation analysis had to justify that their tool was a legitimate tool for measuring influence and that it was capable of delivering trustworthy results. Their answer came soon after the first studies of MacRoberts & MacRoberts and Brooks’. In fact, in 1987 a whole issue of the journal *Scientometrics* was devoted to the discussion of a paper by MacRoberts & MacRoberts (1987b) in which the MacRoberts couple reiterated their criticism against citation analysis. Among the contributors were a number of commentators who responded to the criticism by stating that as long as citation analyses include many reference lists, results are valid. Small (1987, p. 339), for instance, 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. Narin (1987, p. 343-344) estimated that “citation is so heavily concentrated that, even if two-thirds of the citations were perfunctory or irrelevant or biased, there would still be very strong relationship between the earlier and the later elites, and this would still imply linkage between the citing and the cited authors and papers”. Nederhof and Van Raan (1987b) referred to two articles (Nederhof & Van Raan, 1987a and Moed et al., 1983) and held that these demonstrate “that by no means the assumption is necessary, that scientists cite in their papers all work used in their research but still, citations can be used to monitor scientific influence”:

> “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 serious mistaken picture of the main

> influences in a particular field would be obtained when only one particular paper is used for this purpose. If one samples additional papers, they may all be subject to similar important irregularities in their reference lists. Papers on a closely related topic may not even share one reference in common. Would this imply that, even if one took a larger sample of papers in a specific field of science, one would never be able to get any sensible idea at all of what papers are more important in one sense or another than other papers for that specific field in a certain period of time? This would be the case if researchers refer (give citations) in a completely arbitrary way.
>
> However, 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.
>
> A more serious matter – and directly related with the discussion on the [MacRoberts & MacRoberts] paper – would be if authors would cite in very biased ways, for instance by systematically referring to particular papers which did not contribute at all to their papers, or by systematically excluding papers which were important for their paper. But even these types of biases need not be problematic, provided that large numbers of scientists do not share the same biases. By statistical means, one would still be able to estimate within certain bounds whether two (or more) papers are cited significantly different or not. So far, research has failed to show that biases in citation studies are extensive, and do not cancel each other" (Nederhof and Van Raan, 1987b, p. 326).

Van Raan (1998) repeated much of the above quotation when he offered his views on theories of citation in a later special issue of Scientometrics devoted to the discussion of theories of citing. In fact, his argument, that citations are valid indicators of influence *on average*, is nowadays a widespread mantra among citation analysts. This is for example evident in Howard D. White's paper *Authors as Citers Over Time,* which notes that "citation analysts assume that the biases and deficiencies of individual citers are repaired to a tolerable degree by the combined activity of the many" (White, 2001, p. 102)[47].

[47] A.E. Cawkell is perhaps the father of the average mantra. As early as 1976 he wrote:

There is ample evidence to suggest the soundness of the average mantra. A number of studies have demonstrated that citation analysis and peer judgment usually correlate to some degree, which makes it hard to disprove the legitimacy of citation analysis for a number of purposes[48]. However, the assumption, "that the biases and deficiencies of individual citers are repaired to a tolerable degree by the combined activity of the many", is a far cry from the basic assumption of the normative theory of citing, which maintains that "authors generally abide by the norm of indicating their predecessors and sources" (Merton, 1995, p. 389). Thus, the average mantra is clearly at odds with the Mertonian School as the Mertonians have always claimed deviance from the ethos of science to be rare. Zuckerman (1977, p. 98), for instance, concludes in her article entitled *Deviant Behavior and Social Control in Science*:

> "Almost all those who have bothered to look conclude that serious deviant behavior in science is rare".
>
> "Taking the various forms of seriously deviant behavior together, that is forgery, data manipulation, data suppression, and plagiarism – willful acts of deceit – the known cases number perhaps several hundred in a cumulative population of research scientists which, over the generations, number something on the order of more than a million, each at work over a span of years".

In the same article Zuckerman (1977) refers a report by Hagstrom (1974) in which he reports that 25 percent of the 1.309 academics in his sample answered "yes, probably knew of my work" when asked "have you ever found that another scientist has published results you published earlier without referring to your work" (Hagstrom, 1974, p. 9). Zuckerman obviously finds it hard to accept these results and actively seeks to rub the shine of them by arguing that "the likelihood of honest but self-serving misperceptions in such cases does not go unnoticed" (Zuckerman, 1977, p. 102).

Though the results of MacRoberts & MacRoberts (1986) and Brooks (1985, 1986) may not invalidate citation analysis as a tool for research evaluation, information

> ”Citation anomalies [in sence of ”excessive self-citations, plagiarism of references, careless or omitted references etc." (Cawkell, 1976, p. 3)] have little effect - they are like random noise in the presence of strong repetitive signals" (Cawkell, 1976, p. 3).

[48] For reviews on the literature on correlation studies see Egghe and Rousseau (1990: 224-226), Garfield (1998a), Nicolaisen (2002a), White (1990).

seeking, and knowledge organization, they nevertheless challenge the basic assumption of the normative theory of citing. Likewise, the many correlation studies demonstrating that citation analysis and peer review correlate to some extent, do not confirm the assumption that authors cite the works that influence them. So far, nobody has been able to confirm the basic assumption of the normative theory of citing - that authors generally abide by the norm of indicating their predecessors and sources. As a consequence of this, citation analysts seem to have abandoned this theory, and appear to have replaced it with a more moderate version, which merely asserts that authors act in accordance with the normative theory *to a tolerable degree*. Although the modified theory may be correct, it is nevertheless a meager one, as it offers no explanation for the causes of authors' actions. Contrary to the normative theory of citing, which explains that normative behavior is reinforced by sanctions (e.g. Merton, [1942] 1973, p. 269), the modified theory does not elucidate what mechanism makes authors act in accordance with its prediction – why authors cite their influences on average or to a tolerable degree.

2.2 The social constructivist theory of citing

Although the roots of social constructivism can be traced as far back as Plato, who proposed a relationship between a citizen's knowledge and their place in society, Marx's claim that "it is not men's consciousness which determines their existence, but on the contrary their social existence which determines their consciousness" (Marx & Engels, 1970, p. 51) marks the starting point for contemporary social constructivism (Downes, 1998). However, there are at least two groups of intellectuals who commonly are referred to as *social constructivists*. Collin (1997) admits that both groups normally go by the name of social constructivists, but preserves the name for the first of these counting among others Émile Durkheim, Peter Berger, Thomas Luckmann, Don Zimmerman, Melvin Pollner, Alasdair MacIntyre and Peter Winch. Contrary to the members of the second group, *the science constructivists* (Collin, 1997, p. 13), these social constructivists concentrate on how social reality is produced by the cognitive efforts of ordinary social agents. The science constructivists, who are the center of attention in the present section, focus instead on scientific communities and on scientific research. Consequently, when using the term *social constructivism* in the following, I refer to the group of science constructivists counting among others Barry Barnes, David Bloor, Michael Callon, Harry Collins, Karin Knorr Cetina, Bruno Latour, and Steve Woolgar.

In brief, social constructivism is a view about the social nature of science. It rests on the methodological assumption that a sociological analysis of science and scientific knowledge can be empirically fruitful and epistemologically illuminating. This position is contrary to the position of the Mertonian School. Though the Mertonians have been concerned with understanding the social, political and institutional evolution of science, they have deliberately limited their efforts to study the *process* of science, rather than the actual *product*, and have assumed that the content of scientific knowledge is beyond their domain of study. This understanding of the role of the sociology of knowledge was reinforced by Hans Reichenbach (1938), member of the Vienna Circle, who distinguished *the context of discovery* from *the context of justification*. According to Merton (1957, p. 516): "A central point of agreement in all approaches to the sociology of knowledge is the thesis that thought has an existential basis in so far as it is not immanently determined and in so far as one or another of its aspects can be derived from extra-cognitive factors". Basically, this amounts to the claim that the sociology of knowledge may step in to explain beliefs if and only if those beliefs cannot be explained in terms of their rational merits.

The social constructivists have generated detailed empirical studies of scientific practices (for instance, of what is going on in laboratories on a day-to-day basis). These studies, they claim, demonstrate that scientific knowledge does not depend exclusively on the objective external, but rather also (or even mainly or exclusively) on social arrangements resulting from negotiations between scientists taking place in the course of scientific practices. It is in this sense that scientific knowledge and scientific facts are supposed to be socially constructed.

Social constructivism is not a unique specified doctrine, however, but rather a bunch of related studies representing different versions of the general approach (Collin, 2003).

The theory of citing examined in this section is usually held to reflect a social constructivist theory of citing (e.g., Baldi, 1997, p. 17, 1998, MacRoberts & MacRoberts, 1996, p. 439, Small, 1998, p. 143, White, 2004). The theory is presented in some detail by MacRoberts & MacRoberts (1996, p. 439-441):

> "During the 1970's, stimulated by the views of Thomas Kuhn, Ludwig Wittgenstein, and others, a number of scientists made the unusual step of going straight to the center of science to study its knowledge claims [Barnes, 1982, Collins, 1983]. This new breed – all of whom where trained as scientists and knew science first hand – found that traditional claims about science were false [Collins, 1983, Shapin, 1995]. The "storybook" scientist – the objective, disinterested, humble, universal, and sceptical

seeker of truth – simply does not exist [Mitroff, 1974, Mahoney, 1976, Mulkay, 1991]. Science was found to be subjective, contingent, social, and historical [Knorr-Cetina and Mulkay, 1983]. It is not independent of personal and social forces, nor are its disputes settled entirely by evidence and rational discussion [Collins, 1986]. Science is not disembodied interaction between non-social automata and a single independent world but is closely enmeshed with prevailing cultural history and beliefs [Barnes and Shapin, 1979]. These researchers discovered that scientific history is constantly being rewritten, sanitized, and fictionalized to make it appear autonomous and rational [Collins & Pinch, 1994, Brannigan, 1981]. Scientific knowledge is socially negotiated, not given by nature [Collins, 1986]. They found that fact construction is a collective process: "great men" do not build science; it is built by teams of hundreds and thousands, even though scientific historians, the media, and scientists themselves emphasize the heroic feats of individuals [Latour, 1987, Woolgar, 1988]. Scientists' beliefs explain natural reality, natural reality does not explain scientists' beliefs [Collins & Pinch, 1994]. Scientific theories are underdetermined by evidence and observation is theory laden [Knorr-Cetina & Mulkay, 1983]. Closure of scientific disputes is not the simple result of one set of results matching nature and thus being "right" and the other side's data not matching nature and thus being "wrong" but instead is a complicated business involving consensual judgments and interpretations emerging from argument and negotiation [Collins, 1986]. Nature has ceased to have a capital "N"; in fact, even the idea of a single, orderly independent world so long a part of western scientific tradition is recognized to be a social construction. "Nature" emerges only when consensus is reached among scientists [Latour, 1987]. One of the main interests of the constructivists has been scientific communication, of which papers and citations are a part [Mulkay, 1991, Latour, 1987, Knorr-Cetina, 1981, Gilbert & Mulkay, 1984]. They found that informal interaction constituted the major form of communication in science [Edge, 1979], and that when scientists write scientific papers, they reconstitute what they have done and rewrite scientific history [Brannigan, 1984]. Research papers are written as if the researcher had worked inductively. The author's personal involvement is suppressed; controversy is toned down and often eliminated altogether, findings are construed as logically situated in a rational scientific history, and the scientific method is portrayed as central [Gilbert & Mulkay, 1984].

Results and conclusions are represented as leading inevitably from methods, logic, and history, and as such, take on an appearance of objectivity: the account could be anyone's account. What happened at the lab bench is cleaned up, and nature is depicted as telling her story without intermediary.

As Mulkay [1991] has pointed out, the formal language of the scientific paper is a language of certainty, mastery, and domination. It is a language that denies its social origins and its human limitations. It is a language that actively misleads non-scientists and, ultimately, misleads the scientists themselves about their own role in the creation of facts and theories. In their examination of scientific communication, the constructivists found that paper writing is basically a rewriting of recent history to give the discovered object its ontological priority and to deny the cultural nature of the enterprise [Woolgar, 1988]. To this creation, citations are added: the names of recognized and respected individuals are prominently displayed at traditional places to persuade an audience [Gilbert, 1977]. The cumulative effect of citing more and more people who similarly agree with the author is to concretize the universality of the knowledge claim [Woolgar, 1988]. An author's main objective is not to cite their influences but to present as authoritative an argument as possible [Brooks, 1986, Liu, 1993, Gilbert, 1977].

Latour [1987] likens paper construction to Machiavellian or Byzantine politics. Authors are literally lining-up the literature (he could have said stacking the deck): "A given paper may be cited by others for completely different reasons in a manner far from its own interests. It may be cited without being read … or to support a claim which is exactly the opposite of what the author intended; or for technical details so minute that they escaped their author's attention; or because of intentions attributed to the authors but not explicitly stated in the text …" [Latour, 1987].

As Latour [1987] further indicates, citations are not put in papers to indicate to others who has influenced the production of the work but to display the "black boxed" (established) knowledge. If one does not agree with a referenced statement, one must, in essence, dispute it with the cited definitive authority.

Papers are meant to sell a product, which is wrapped up with "the passionate pursuit of research grants and professional success" [Carpenter, 1994]. The scientific paper as Knorr-Cetina, Latour, Woolgar, and many others [Woolgar, 1988, Latour & Woolgar, 1986] have demonstrated does

> not reflect what happens at the lab bench nor, as others have shown, is it a blueprint, or is it even supposed to be a blueprint, of the author's intellectual history [MacRoberts & MacRoberts, 1986, 1987a, 1988, Edge, 1979, Collins, 1974, Baird and Oppenheim, 1994, Brooks, 1985, 1986, Liu, 1993, Sancho, 1992]. It is, rather, only the last in a series of often dozens of laborious and painful and continuously changing drafts in which authors and co-authors construct, reconstruct, and negotiate knowledge and in which outsiders, notably referees, colleagues, and editors, add their two-bits, often including references (usually their own) the author has never seen.
>
> With these discoveries about the nature of science, the scientific paper itself becomes a part or phase of ongoing science, not its end point. Paper writing is but one act in the creative process to be followed by other published reworkings of the basic theme. Indeed, as Gilbert & Mulkay [1984] found, not only do scientists' accounts vary between the lab, over beers at a bar, in letters, lab notes, conferences, and within a single recorded interview, but between successive published versions of the same data!
>
> The formal paper presents a story, a nice rational story, but not the story, and the citations present an array, but not the only array possible".

The following section 2.2.1 is a brief summary of Thomas S. Kuhn's *The Structure of Scientific Revolutions* (Kuhn 1962, 1970), deemed by many as the most influential book on science in the second half of the twentieth century (Fuller, 2000, p. 1). As noted by MacRoberts & MacRoberts (1996), a group of sociologists of science came to see Kuhn's work as a warrant for adopting a sociology of scientific knowledge very different from the Mertonian School's. The philosophical roots of social constructivism are outlined further in section 2.2.2. Section 2.2.3 presents the core assumption of the social constructivist theory of citing, normally known as *the persuasion hypothesis.* A number of empirical studies, which claim to have tested the hypothesis, are reviewed, and an original test is designed and carried out.

2.2.1 T. S. Kuhn's philosophy of science

In his book, *The Structure of Scientific Revolutions (*1962, 1970), Kuhn argues that scientific research and thought are defined by "paradigms", or conceptual world-views, that consist of formal theories, classic experiments, and trusted methods. According to Kuhn, scientists typically agree to work within an existing paradigm. In their day-to-day work they seek to extend the scope of the paradigm by refining derived theories, explaining puzzling data, and establishing more precise measures of standards and phenomena. Eventually, however, their efforts may generate unsolvable theoretical problems or experimental anomalies that expose their paradigm's inadequacies or contradict it altogether. This accumulation of problems and anomalies set off a crisis that can only be resolved by a scientific revolution that replaces the old paradigm with a new one. The overthrow of Ptolemaic cosmology by Copernican heliocentrism, and the displacement of Newtonian mechanics by quantum physics and general relativity, is, according to Kuhn, examples of such revolutionary paradigm shifts.

Kuhn (1962, 1970) questions the traditional conception of scientific progress as a gradual, cumulative acquisition of knowledge based on rationally chosen experimental frameworks. Instead, he argues that the paradigm determines the kinds of experiments scientists perform, the types of questions they ask, and the problems they consider important. A shift in the paradigm alters the fundamental concepts underlying research and inspires new standards of evidence, new research techniques, and new pathways of theory and experiment that are radically incommensurable with the old ones.

The adequacy of Kuhn's account has been challenged in a number of ways. I will return to some of the objections later (section 4.1.1). But first I shall explore how a group of sociologists of science came to see Kuhn's work as a warrant for adopting a sociology of scientific knowledge very different from the Mertonian School's sociology of science.

2.2.2 The philosophical roots of social constructivism

Apparently much against Kuhn's own wishes[49], his philosophy of science contributed to inspire a group of sociologists of science to formulate a constructivist sociology of scientific knowledge.

With Kuhn, the social constructivists felt justified in believing that science could and should be treated on the same level as other aspects of cultural and social life. In his book *T.S. Kuhn and Social Science* the Edinburgh School social constructivist Barry Barnes argues that Kuhn revealed that science should be amenable to sociological study in fundamentally the same way as any other form of knowledge or culture:

> "Science is not a set of universal standards, sustaining true descriptions and valid inferences in different specific cultural contexts; authority and control in science do not operate simply to guarantee an unimpeded interaction

[49] Kuhn (2000, p. 110), dissociating himself from the sociological appropriation of his work, proclaimed that the claims of the social constructivist movement are "an example of deconstruction gone mad".

> between 'reason' and experience. Scientific standards themselves are part of a specific form of culture; authority and control are essential to maintain a sense of the reasonableness of that specific form" (Barnes, 1982, p. 10).

Indeed, Kuhn does point out that there are many episodes in the history of science, which seem to involve decisions about theories where non-cognitive factors were prominent. For instance, "the man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem-solving […]. *A decision of that kind can only be made on faith*" (Kuhn, 1962, p. 157 [my italicizing]).

Whereas the traditional Mertonian sociology of science struggled to explain the common disagreement among scientists, the constructivists could with Kuhn account for disagreement. Kuhn's studies of science history had revealed that science is not a rule-governed activity, and that disagreements frequently do occur. Harry Collins, a Bath School social constructivist, studied controversies in theoretical physics in the late 1970s/early 1980s. In each of the cases Collins examined, he found that ingenious scientists could fabricate a way to circumvent arguments and evidence against their pet theories. In effect, Collins' claim is that the experimental evidence is always so ambiguous that virtually any theory can be maintained in the face of any evidence. In his own words: "The natural world in no way constrains what is believed to be" (Collins, 1981a, p. 54); and: "The natural world has a small or non-existent role in the construction of scientific knowledge" (Collins, 1981b, p. 3).

That scientific theories are unconstrained by Nature is a dogma frequently put forward by radical social constructivists[50]. The legacy of Kuhn is evident. Kuhn (1962) actually maintains that no logical argument can prove the superiority of one paradigm over another, and that refusal to accept a theory or a paradigm is not a violation of scientific standards.

Where Merton held that "the acceptance or rejection of claims entering the lists of science is not to depend on the personal or social attributes of their protagonist" (Merton, 1957, p. 607), the social constructivists hold that knowledge, without personal and social attributes is impossible[51]. Whereas the philosophical basis of the Mertonian

---

[50] Another example is found in Barnes, Bloor & Henry (1996, p. 48) who points out that it is not "the existence of nature" which accounts for certain behaviors and that "attention to nature" will not be able to judge the qualities of our theories.

[51] Collin (2003) makes a distinction between *ontological constructivism* and *epistemological constructivism*. The latter merely claims that our knowledge of the physical world is always expressed in concepts and categories determined by other factors than the world itself, normally by social

School may be seen as close to the canons of logical positivism, the radical social constructivists are to some extent embracing a modern hermeneutic position.

Martin Heidegger (1899-1976) provided the conceptual framework for the modern philosophical hermeneutics. Its major theses are expressed in the book *Truth and Method* (1960), written by Hans-Georg Gadamer (1900-2002), one of Heidegger's students[52].

The first thesis is that understanding is not an activity human beings engage in primarily or only when they approach linguistic phenomena. It is rather the basic mode of human existence in all its manifestations. To be a human being is to constantly structuring ones world in terms of meaning.

The second major thesis is normally known as *the hermeneutic circle*. A reader setting out to interpret a text must assume that there is something to be understood there. Furthermore, the interpreter must have preconceptions about what it is to communicate something through a text, what types of texts there are, and the like. The idea of the hermeneutic circle is closely linked to the thesis that understanding is a basic condition of human existence. Understanding a text is to integrate it into the horizons of intelligibility by which we structure our world.

The third thesis of philosophical hermeneutics gave Gadamer's book its somewhat misleading name *Truth and Method*. The title is misleading because its thesis is precisely that there is no such thing as a method for arriving at and validating an interpretation of texts. The criterion of successful interpretation must ultimately remain the interpreter's experience of truly having approached the text. The interpretation cannot be measured relative to some external, absolute standard. Only the fact that the text has become transparent within the horizon of intelligibility of the interpreter can serve as the hallmark of appropriation.

The question immediately arises of how interpretive practice can be rescued from falling into subjectivity. If the criterion for successful understanding is the experience of having understood the text, every interpreter can only rely on his own intuitions, which would make objectivity and rational discussion of interpretations impossible. The answer proposed by philosophical hermeneutics is as follows: Interpreters are not individuals detached from socially constituted forms of life. Every individual grows into a world structured by the meanings embodied in the form of life of his society. His horizons of intelligibility are thus not private but shared by the members of his culture.

---

circumstances surrounding the knowledge process. The former, which is the radical version of social constructivism, claims that the physical world is socially constructed.

[52] For this and what follows on Gadamer's *Truth and Method* I rely on Lübcke (1982) and Wright (1998).

The result is that every individual interpreter must be seen as part of an interpretive community. His criteria for successful understanding are grounded in the meaning structures underlying the culture, which has formed him as a human being. His initiation into the discipline occurs less by learning of explicit rules than by participation in the interpretive practice of the community. Correspondingly, interpretive practice allows for objectivity despite the impossibility of providing algorithm like rules for the correctness of interpretations. The intersubjectivity is guaranteed by the fact that individual interpreters are part of the same tradition or form of life. Their criteria for judging interpretations are embodied in their skills, enabling them to participate in their common practice.

According to Lübcke (1982, p. 177) Gadamer holds that philosophical hermeneutics does not end up in relativistic subjectivism. However, like radical social constructivism, philosophical hermeneutics seems to end up in social relativism. The view that different things are not just thought true in different societies – rather they are true in different societies (Musgrave, 1998). Radical social constructivism rests on the view that alternative theory choices are not only available, but also equally good, because theories are believed to be adopted by convention. Barnes, Bloor & Henry (1996) asserts, for instance, that as "conventions could always be otherwise, and how precisely they have been applied could always have been otherwise" knowledge might have been negotiated differently in different social settings. Harry Collins actually holds that the study of social variables and how they explain the actual content of scientific ideas is exactly what distinguishes social constructivism from the Mertonian School. In his reply to Gieryn (1982), who had argued that Merton actually anticipated much of the work of the social constructivists, Collins (1982, p. 302) argues that Merton never claimed that social variables could explain the actual content of scientific ideas:

> "Even Merton's programmic statements as quoted by Gieryn do not begin to be mistakable for an anticipation of the modern programme unless Merton intended to say not just that the conditions for the existence of scientific practice are social, and not just that the direction and focus of attention of science is in part socially determined, but also that the very findings of science are in part socially determined. The relativist programme tries to do analyses that imply that in one set of social circumstances "correct scientific method" applied to a problem would precipitate result p whereas in another set of social circumstances "correct scientific method" applied to the same problem would precipitate result q, perhaps q implies not-p"

Steve Woolgar, another well-known social constructivist, is even more precise. In his book, *Science: The Very Idea*, he presents a figure similar to the one below:

Scientific knowledge → The natural world

Woolgar (1988, p. 60) explains the figure by stating:

> "It is not that the (objective) right-hand side pre-exists our (human) efforts to come across it. Rather the right-hand side is the end result or accomplishment of work done by participants. Baldly speaking, discoveries create, rather than merely produce accounts of, the right-hand side".

2.2.3 The persuasion hypothesis

As the radical social constructivists reject the pre-existence of the natural world and claim that the natural world is socially constructed, it logically follows that what is true and false can never be judged or determined by referring to "natural phenomena". Although scientific discussions may appear to reach closure by one part referring to "Nature", scientific discussions are actually closed by something else. The social constructivists believe to have demonstrated that scientific closure is the outcome of a negotiation process in which one part convinces the other by mere persuasion. In fact, Latour & Woolgar (1986, p. 69) maintain that science is "the art of persuasion". In science, the successful 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 pinches when reporting research (*stacking*[53], *staging and framing*[54], and *captation*[55], to name but a few). One of the alleged pinches is the social constructivists' claim that when authors cite, they are marshalling earlier documents in such a way as to persuade readers of the goodness of their claims. MacRoberts & MacRoberts (1987b, p. 294) go as far as stating that

[53] "Bringing pictures, figures, numbers and names into the text and then folding them" (Latour, 1987, p. 50).

[54] Explaining how and by whom the report should be read (Latour, 1987, p. 52).

[55] Laying out the text so that wherever the reader is there is only *one* way to go (Latour, 1987, p. 57).

"persuasion, not a desire to 'give credit where credit is due', is the major motivation to cite". Clearly, this position stands in opposition to the normative theory of citing, which, as demonstrated in section 2.1, precisely maintains that citing is distribution of credit where credit is due. The contrasting argument, much influenced by Gilbert's (1977) article *Referencing as Persuasion*, is sometimes called *the persuasion hypothesis*:

> "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" (Gilbert 1977, p. 116).

Latour (1987, p. 33–34) speculates in the same way that in order to put up a persuasive facade, authors basically fake their scholarship:

> "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) does not present these actions as inconsequential. On the contrary, he maintains that if readers were to find out what is actually going on, such as citations made just for display, the result would be "disastrous" for authors. As pointed out by White (2004), the issue is *not* the ordinary claim that scientists and scholars write to persuade and use citations as a rhetorical resource. Edge (1979, p. 114) maintains, for instance that "the object of a formal research paper is to *persuade* and *convince* the appropriate audience that the results presented should be *accepted as valid knowledge*: the form and style of argumentation is determined by that institutionalized goal, and the paper is assembled, and its list of references compiled, with that end in view". However, truisms like Edge's are not what are meant by the persuasion hypothesis. The persuasion hypothesis, rather, is the idea that persuasion in science and scholarship relies on misleading manipulation indistinguishable from commercial advertising. MacRoberts & MacRoberts (1996, p. 441) claim, for instance, that "papers are meant to sell a product", while Law & Williams (1982, p. 543) likens scientists' choice of references to "packaging a product for market".

White (2004) presents a careful analysis of the persuasion hypothesis and reveals that it actually comes in two parts. The first part 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" with the attachment "citers often misrepresent the works they allude to, twisting their meaning for their own ends". The second part has to do with the choice of the cited works themselves, regardless of what is being said about them. White (2004) 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".

Compact versions of both parts of the hypothesis are evident in the long MacRoberts & MacRoberts quotation quoted in section 2.2. The first part of the hypothesis is evident in the following passage:

> "Latour likens paper construction to Machiavellian or Byzantine politics. Authors are literally lining-up the literature (he could have said stacking the deck): "A given paper may be cited by others for completely different reasons in a manner far from its own interests. It may be cited without being read … or to support a claim which is exactly the opposite of what the author intended; or for technical details so minute that they escaped their author's attention; or because of intentions attributed to the authors but not explicitly stated in the text …[Latour, 1987]".

The second part of the persuasion hypothesis is evident in this passage:

> "In their examination of scientific communication, the constructivists found that paper writing is basically a rewriting of recent history to give the discovered object its ontological priority and to deny the cultural nature of the enterprise [Woolgar, 1988]. To this creation, citations are added: the names of recognized and respected individuals are prominently displayed at traditional places to persuade an audience [Gilbert, 1977]. The cumulative effect of citing more and more people who similarly agree with the author is to concretize the universality of the knowledge claim [Woolgar, 1988]. An author's main objective is not to cite their influences but to present as authoritative an argument as possible [Brooks, 1986, Liu, 1993, Gilbert, 1977]".

A number of researchers have sought to test the persuasion hypothesis empirically. The following sub-sections review their results and demonstrate that only the second part of the hypothesis has been adequately dealt with. Sub-section 2.2.3.1.1.1 plots one possible course for dealing empirically with the first part of the hypothesis.

2.2.3.1 Empirical tests of the persuasion hypothesis

Most researchers have tested either the first or the second part of the persuasion hypothesis. One important exception, however, is Stéphane 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 astrophysics research area, and discovered that "authors are most likely to cite articles that are relevant to their work in terms of subject, recency of knowledge, theoretical orientation, and seem to have little concern with the characteristics of authors who wrote them" (Baldi, 1998, p. 843). However, both Small (1998) and Collins (2000) have questioned the adequacy of Baldi's method.

2.2.3.1.1 Empirical tests of the first part of the persuasion hypothesis

According to the first part of the persuasion hypothesis (persuasion by distortion (White, 2004)) citers often misrepresent the works 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 citation theory assumes that references signal direct semantic relationships between the citing and the cited works. Garfield (1979, p. 3) maintains, for instance, after having discussed the semantic problems of subject indexes that "the citation is a precise, unambiguous representation of a subject". The assumption, however, has only been tested a few times, and the four available studies disagree over its validity.

Trivison (1987) examined the occurrence of terms in the titles and abstracts of citing and cited journal articles from three information science journals. She found that term co-occurrence was much greater for cited/citing article pairs than for randomly selected pairs of articles. However, her results also revealed that 23 percent of the cited/citing document pairs were not term related[56].

---

[56] Pairs of cited/citing articles with five or more significant word stems in common were deemed "term-related".

Harter, Nisonger & Weng (1993) investigated the semantic relationship between citing and cited documents for a sample of document pairs in three information science journals. They conducted two analyses: A so-called *macroanalysis*, based on a comparison of the Library of Congress class numbers assigned citing and cited documents, and a so-called *microanalysis*, based on a comparison of descriptors assigned citing and cited documents by three indexing and abstracting journals (*ERIC*, *LISA* and *Library Literature*). Both the macro- and the microanalysis suggested that the subject similarity among pairs of cited and citing documents were very low.

Peters, Braam & Van Raan (1995) measured word-profile similarities between highly cited papers and citing papers in the field of chemical engineering by different similarity measures based on keywords and classification terms. The results of their study indicated that publications with a citation relationship were significantly more content-related than other publications. According to the authors, their findings were clearly contrary to "opinions in circles of sociologists of science that authors refer to publications in a rather arbitrary way mainly for adornment of their claims" (Peters, Braam & Van Raan (1995, p. 9).

Song & Galardi (2001) retrieved 21 highly cited articles from the Web of Science database with the search term "Information Retrieval". Using word-profiles by automatically extracting terms from various bibliographic elements of the 21 highly cited articles and 843 articles citing them, they measured the similarity of cited/citing articles having a citation relationship and articles having no citation relationship. Their results indicated a statistically significant semantic relationship between the highly cited articles and their citing articles in comparison to the highly cited articles and their non-citing articles.

The four studies obviously present contradictory results. It is thus not possible to conclude whether cited and citing documents are semantically related as proposed by the normative citation theory. Moreover, the results do not confirm or contradict the persuasion hypothesis. Peters, Braam & Van Raan (1995) claim that their results *do* contradict the persuasion hypothesis, but the authors seem to forget that *content relatedness* is not necessarily the same as *semantic relatedness*. Hjørland (1993, p. 70) provides a good example that demonstrates the fundamental difference between the two:

> "Books about astronomy and astrology may be said to "be alike" because both, for instance, contain partially the same concepts (names of celestial bodies). Based on these shared concepts, one could imagine a computer program extracting a class or cluster containing astronomical as well as astrological documents. For a user, believing in astrology, the subjects may

> be said to be related, but if one adopts the view of modern science, astrology is more related to occultism than to astronomy" [my translation].

Future tests of the persuasion hypothesis need to acknowledge the fundamental difference between content relatedness and semantic relatedness. Instead of measuring the semantic relationship, future tests must develop ways to study the content relatedness of cited and citing documents[57]. One possible course of action is outlined below.

2.2.3.1.1.1 Measuring the content relatedness of cited/citing documents

In order to test the social constructivists claims that "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" (Gilbert 1977, p. 116), or that "many of the articles alluded to might have no bearing whatsoever on the claim and might be there just for display" (Latour, 1987, p. 34), one needs to find a way to measure the content relatedness of cited and citing documents. This, however, is not an easy task. Classification researchers are actually disagreeing a whole lot on how to do it. Moreover, they are even disagreeing on how to determine the subjects of documents. Unfortunately, neither Gilbert (1977) nor Latour (1987) suggest how we might assess their claims. But insofar their claims are not just tautologies they should be assessable.

The following paragraphs present a concise attempt of measuring the content relatedness of cited and citing documents. It is rooted on the assumption of realist and materialist subject theory that documents have objective properties, and that documents having the same properties are content related (Hjørland, 1992). The properties of a document are all the true statements that can be said about the document. Hjørland (1992, p. 182) explains what true statements are by the following example: "If a document states that 'a person's intelligence is correlated with the size of his brain', this is a subjective (and false) judgement. But it is an objective fact that this document contains this (false) judgement".

Scientific disciplines may be characterized by their fields of study. History, for instance, is the discipline that studies human societies as they have evolved over time. It consequently follows that scientific documents may be characterized accordingly by

[57] Some of the previous studies suffer from a possible problem that should be avoided in future studies. The persuasion hypothesis is a hypothesis about the relationship between cited and citing documents. Thus, the question concerns the relationship between cited and citing documents, not merely *highly* cited documents and the documents citing them. Future studies should consequently be designed in order to answer the actual question.

their fields of study. A history document may, for instance, be characterized by the human society or the historical period it studies. Frederick L. Nussbaum's *The Triumph of Science and Reason*, 1660-1685 (1953) studies, for example, the scientific community as it was in the period 1660-1685. In Hjørland's words, that is an objective fact. It is also an objective fact that Robert K. Merton's *Science, Technology, and Society in Seventeenth Century England* (1938) studies the scientific community as it was in the same period. The two books are having (some of) the same objective properties, and they are thus content related to some extent. Consequently, if Nussbaum's book is citing Merton's it is not a case of persuasion by distortion. That would only be the case if the two shared no objective properties (Gilbert, 1977, Latour, 1987). Consequently, if one could collect a large enough sample of history documents and determine their objective properties, one could compare pairs of cited and citing documents to assess the validity of Gilbert and Latour's claims.

The databases *Historical Abstracts* and *America: History and Life* index the world's periodical literature in history. Each document is provided a historical period descriptor, which enables the searcher to identify documents dealing with particular periods. The two databases consequently enable the searcher to identify documents, which, to some extent, possess the same objective properties. Accessing a citation database that indexes a segment of the same documents as the two other databases would enable the searcher, although with some effort, to determine the frequency by which cited and citing history documents are content related.

Fortunately, I happen to have access to the required databases. I therefore decided to draw a sample of cited/citing history documents to measure their content relatedness. For no particular reason, I chose articles published between 1976-2000 in *Journal of Economic History*[58] as my sample. I was able to identify 811 cited/citing pairs in the sample using SSCI. In 131 of the 811 pairs the citing first author was found to be the same as the cited first author. The 131 pairs are thus author self-citations. The historical periods of all 811 cited and citing documents was retrieved from *Historical Abstracts* or *America: History and Life.* The data was analyzed using SPSS (*Statistical Package for the Social Sciences*).

[58] According to the journals website, "*The Journal of Economic History* is devoted to the interdisciplinary study of history and economics, and is of interest not only to economic historians but to social and demographic historians, as well as economists in general. Topics examined include money and banking, trade, manufacturing, technology, transportation, industrial organisation, labour, agriculture, servitude, demography, education, economic growth, and the role of government and regulation" (http://titles.cambridge.org/journals/journal_catalogue.asp?historylinks=ALPHA&mnemonic=JEH). Visited August 24., 2004.

Results: 649 of the 811 cited/citing pairs were found to have overlapping historical periods. 80,02% of the pairs were in other words found to be content related to some extent. 162 of the 811 pairs (19,98%) did not have overlapping historical periods, and their content relatedness could consequently not be determined definitively. Narrowing the analysis to the 131 author self-citation pairs provided approximately the same results: 103 of the pairs (78,63%) were found to have overlapping historical periods.

That approximately 20% of the pairs did not have overlapping historical periods does not imply that these cited and citing documents are not content related at all. The analysis focused on just one objective property (historical periods). Broadening the focus to include other properties (e.g., human societies) might reduce the percentage of unrelated pairs further.

Obviously, the examination of one single case cannot establish any general claims about how science works. However, the case in hand does not lend much plausibility to the persuasion hypothesis, or more exactly to the first part of it. Most of the cited articles alluded to appear to have at least some bearing on the claims of the citing articles and are consequently not cited just for display. Further assessments of the first part of the persuasion hypothesis are certainly needed. The case in hand plots one possible course.

2.2.3.1.2 Empirical tests of the second part of the persuasion hypothesis

Contrary to the first part of the persuasion hypothesis, the second part has been thoroughly falsified. According to the second part of the persuasion hypothesis (persuasion by name-dropping (White, 2004)), authors disproportionately cite works by established authorities, so as to gain credibility by association. In the words of MacRoberts & MacRoberts (1996, p. 440-441): “An author’s main objective is not to cite their influences but to present as authoritative an argument as possible”. At least three studies have questioned the validity of this claim.

Zuckerman (1987, p. 334) posed the question: If persuasion really were the major motivation to cite, would citation distributions look as they do? She answered herself by stating “plainly not”, referring to data provided by Garfield. Garfield (1985, p. 406) presented a table illustrating the number of citations retrieved by items cited one or more times in the 1975-1979 cumulated SCI (see table 2.2). The table reveals, among other things, that only 6,3 percent of the 10.6 million citations went to documents cited 10 or more times in the five-year period. Zuckerman (1987) points to the low percentage as evidence against the plausibility of the persuasion hypothesis. According to the persuasion hypothesis, the percentage should be much higher. Zuckerman (1987, p. 334) refers to Gilbert (1977, p. 113), one of the “inventors” of the persuasion hypothesis, who stated that it is the papers seen as "important and correct" which "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* being equal to those, which have been cited at least ten times in five years (or twice annually), the persuasion hypothesis needs to be radically adjusted. 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.

Table 2.2. *Citations received by items cited one or more times in the 1975-1979 cumulated SCI. The table includes an unspecified number of duplicates cited in variant forms. A= total citations, B= cumulative number of items, C = cumulative percent of items (Garfield, 1985, p. 406).*

A	B	C
≥ 1	10.641.000	100,00%
≥ 2	3.874.000	36,00%
≥ 5	1.531.000	14,00%
≥ 10	670.000	6,30%
≥ 17	313.000	3,00%
≥ 25	155.000	1,50%
≥ 51	44.000	0,40%
≥ 101	10.500	0,10%

White (2004) realized that if an author cites a "world figure" (e.g., Noam Chomsky or Thomas S. Kuhn), the author might be accused of dropping names 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 like 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. He carried out such a test and reported the results in the article *Reward, Persuasion, and the Sokal Hoax: A Study in Citation Identities* (White, 2004).

Initially, White (2004) had to determine how to measure the reputation of cited authors. He solved the task by counting the number of citations the cited authors had received. Table 2.3 demonstrates how.

He subsequently drew a judgment sample that consisted of 28 citing authors from different disciplines (ten from information science, eight from science studies, six from various natural sciences and four from cultural studies in the humanities). Finally, he tabulated the references provided by the 28 citing authors in their accessible publications according to the logarithmic scale in table 2.3. The method enabled White (2004) to determine the frequencies by which reputable and non-reputable authors appeared in the bibliographies under study. His findings do not support the persuasion hypotheses. Most of the 28 authors cited at all levels over the entire scale of reputation, and they did not exclusively favor high-end names with authoritative reputations. If anything, his findings suggest that the authors tended to favor low-end names slightly.

Table 2.3.
Logarithmic scale of cited authors' reputations (White, 2004)

Reputation	**Raw citation count**
Obscure	1 – 10
Recognized in specialty	11 – 100
Well known in discipline	101 – 1000
Well known beyond discipline	1001 – 10000
World famous	10001 – and higher

Moed & Garfield (2003) added another dimension to the analysis of citation distributions. In their study they sought 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, "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" (Moed & Garfield, 2003, p. 192).

The authors analyzed the references cited in all source items denoted as 'normal articles' included in the 2001 edition of the SCI on CD-ROM. The source papers were arranged by research field, defined in terms of aggregates of journal categories. The authors focused on four such fields: Molecular biology & biochemistry, physics & astronomy, applied physics & chemistry, and engineering. The cited references were classified in two groups: those published in journals processed for the ISI citation indexes, and those published in non-ISI sources, including monographs, multi-authored books and proceedings volumes. In each research field the distribution of citations among cited items was compiled in each group separately, and the ninetieth percentile of that distribution was determined. Thus, the ten percent most frequently cited items published in ISI journals, and the ten percent most frequently cited documents in non-ISI sources were identified. These two sets were then combined. The combined set was assumed to represent the documents perceived in the year 2001 as 'authoritative' in a research field. Source articles were arranged in classes on the basis of the number of references they contained. The percentage of references to 'authoritative' documents was finally calculated per class.

The findings of their analysis clearly suggest that authors in all four fields cite proportionally fewer 'authoritative' documents as their bibliographies become shorter. In other words, when the authors display selective referencing behavior, references to 'authoritative' documents are found to drop radically. Moed & Garfield (2003, p. 195) therefore concluded, "In this sense, persuasion is not the major motivation to cite".

2.3 Summing up

In this chapter it has been argued that two opposing theories of citing have emerged since the first article on the subject (Kaplan, 1965). These theories are normally known as *the normative theory of citing* and *the social constructivist theory of citing*. The two theories are founded on opposing views of science, scholarship and knowledge. Both the normative and the social constructivist theory of citing appear incapable of accounting for the citation behavior of most authors. The average mantra and its admittance that citing scientists only behave in accordance with the alleged norms of science to a tolerable degree, seems to be better capable of accounting for the actual behavior of citing authors. However, the average mantra does not explain what makes citing authors behave the way they apparently do.

Never expose yourself unnecessarily to danger; a miracle may not save you... and if it does, it will be deducted from your share of luck or merit (The Talmud)[59].

3 The risk of citing[60]

In this chapter I want to sketch out one plausible theoretical explanation for why authors cite their influences and sources to a tolerable degree. As pointed out in the previous chapter, the average mantra merely suggests that authors cite their influences and sources to a tolerable degree or just enough to make citation analyses feasible. However, the average mantra fails to provide an explanation that can adequately account for such behavior. Citing ones influences and sources to a tolerable degree does not apply well with neither the normative theory of citing that holds that authors generally abide by the norm of citing their influences and sources, nor with the social constructivist theory of citing that holds that authors use references as tools for manipulative persuasion and that they generally desist from citing their influences and sources.

The theoretical explanation, which I am about to outline, is inspired by research from the field of evolutionary biology or more precisely by the ideas of the Israeli biologist Amotz Zahavi. Psychologists and sociologists have recently begun to take an interest in evolutionary theories of behavior. In the spring semester of 2004 the Department of Sociology at the University of Copenhagen, Denmark offered their Masters students an elective course entitled *Evolutionary Theories of Human Behavior*[61].

---

[59] http://www.quotationspage.com/quotes/The_Talmud/. Visited August 24., 2004.

[60] This chapter is based on a speech I gave at the *ASIS&T Annual Meeting* in Long Beach, CA: October 22, 2003.

[61] http://www.sis.ku.dk/LP/VisKursus.asp?Knr=46827&Sprog=DK&InFrame=0. Visited August 24., 2004.

3.1 Honesty and deception in animal communication

Questions about honesty and deception in animal communication emerged with a provoking article by Richard Dawkins and John R. Krebs in 1978 in which the authors argued that signals are vehicles for manipulation (Dawkins & Krebs, 1978).

Before, displays and signals were thought to reflect accurately the emotional or motivational state of an animal, or its future behavior, and information was thought to be shared among individuals. The Latin roots of the verb communicate are synonymous with sharing and participating[62], and most attempts to define communication in animals through the 1960s emphasized the benefits that both the senders and receivers of signals derived by interactions.

The view that communication involves a sharing of accurate information was first challenged by Dawkins and Krebs, who argued that selection would favor signalers that could influence the behavior of others in ways beneficial to themselves, but not necessarily to the attendants of the signals. Their perspective was that communication is a way for animals to manipulate others, and not as a means to achieve mutual benefits by shared information. On the contrary, Dawkins and Krebs stressed that most of the time communication should be viewed as evolutionarily selfish. Deceptive or inaccurate signals should therefore be common.

Like the social constructivists' persuasion hypothesis, the problem with defining animal communication as manipulation is to clarify why cheating does not destroy the communication system. The problem was solved in 1975 by the Israeli biologist Amotz Zahavi who argued for a rather different perspective than Dawkins and Krebs.

3.1.1 The handicap principle

Zahavi (1975, 1977a, 1977b, 1980, 1987, 1993, 2003) and Zahavi & Zahavi (1997) contend that receivers have been selected to ignore signals that are not honest and therefore are the ultimate determiners of how evolution has shaped communication. According to Zahavi, 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 is thus known as *the handicap principle*.

The handicap principle suggests that if an individual is of high quality and its quality is not known, it may benefit from investing a part of its advantage in advertising that

[62] *Oxford Latin Dictionary*, 1969.

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 (Zahavi, 2003). To illustrate, suppose that females select mates from a population of males that vary in fitness, but are unable to assess the quality of the males directly. However, males signal their quality by displays, some of which are energetically costly to produce, or perhaps even detrimental to survival. The more extreme the expression of the expensive signals, the more costly they are, but the expenditure is relatively greater for less fit or poorly conditioned males. The fitter males can better afford the energetic outlay or risk in the production of the signals. Females with a genetically based preference for males that are able to produce the costly signal will secure fitter mates and, over time, be selected to ignore other signals. According to the handicap principle, costliness is thus essential to the evolution of honesty.

Shortly after having formulated his theory in 1975, Zahavi found himself debating the logic of the handicap principle with mathematicians and theoreticians (Arnold, 1983; Davis and 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 to be intuitive and the skeptics insisted on having mathematical models to show its feasible operation in evolution. In 1990 the Oxford biologist Alan Grafen successfully formulated the required model (Grafen, 1990a, 1990b) and made the handicap principle acceptable to mathematical minded evolutionary biologist[63].

Grafen (1990a, 1990b) demonstrated moreover that signals need only be honest on average to be evolutionary stable. This idea had been put forward three years earlier by Zahavi (1987, p. 319) who had recognized 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

[63] As noted by Møller (1994), the controversy over the handicap principle may also have been caused by personal conflicts. One of the major contributors to modern evolutionary biology, John Maynard Smith, who had been skeptical about the handicap principle during the 1970s and most of the 1980s, addressed the handicap principle in a lecture given at the Third International Conference on Behavioural Ecology in Uppsala, Sweden in August 1990. During his lecture, Maynard Smith formally apologized to the inventor of the handicap principle, Amotz Zahavi, for not having understood the simple mechanism earlier. After the lecture, Zahavi stood up, acknowledged the apology, and replied that he believed that Maynard Smith still did not understand the handicap principle!

> 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".

According to Számadó (2000) there are several well-documented examples of such a mixture of honest and deceptive signals in nature, for example: Batesian mimicry in butterflies (Whiley, 1983), reproductive strategies of bluegill sunfish, *Lepomis macrochirus* (Dominey, 1980, Gross & Charnov, 1980) and damselflies, *Ischnura ramburi* (Robertson, 1985), and aggressive communication in stomatopods (Caldwell & Dingle, 1975, Adams & Caldwell, 1990). 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. For instance: 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., Bird, Smith & Bird, 2001, Hawkes & Bird, 2002, Smith, Bird & Bird, 2003), the human body and its decorations (Zahavi & Zahavi, 1997), and the evolution of art (Zahavi & Zahavi, 1997).

3.1.2 Handicaps as explanations for reliable threats

The handicap principle has also provided a constructive explanation for threatening behavior. Zahavi & Zahavi (1997) notes that rivals rarely attack each other without initially signaling their intentions. Most of the time, they do not attack at all. Instead, the conflict is typically solved by the exchange of threats. If we were to turn of the TV, cancel our newspapers, and rely on our own observations (like a zoologist studying an animal species) we would reach the same conclusion about humans. Humans, like other animals, resolve most of their conflicts by communication, which often includes the exchange of threats. According to Zahavi & Zahavi (1997, p. 16) this is true both at the personal and the international level: "Wars are terrible, and partly for that reason, most conflicts are resolved without them".

Zahavi & Zahavi (1997) point out that all living creatures that communicate in some way or another make use of threats. Resolving a conflict just 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 part back down? What convinces one of the rivals to give up food, a potential mating opportunity, or a territory without a fight? Maynard Smith & 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 return of winning is not worth the risk of fighting?

Zahavi (1977b) answered the questions proposing that threats are reliable indicators of each rival's chances in a fight. Threat displays communicate reliable information about the opponent's ability and willingness to fight. Assessing such information against own abilities and willingness provides a good idea of one's chances in a fight. If chances are slim or fictional, then one better give up the fight and back off. Yet how can threat displays work this way? Why can the one, who is most likely to win a fight, threaten more effectively than the other part? Zahavi (1977b) proposed that in order to function this way, the threat itself must increase the risk that the threatening part will be attacked, or will be at a disadvantage if attacked. An individual who is genuinely willing to fight and confident of own abilities will accept such a risk, whereas another, who lacks strengths or motivation will find the stakes too high and thus be unable to threaten to the same extent. In Zahavi's 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" (Zahavi, 1977b, p. 256).

Perhaps a brief review of some common threat signals will clarify Zahavi's idea even further.

3.1.3 Common threat signals

Threat signals are often involved in approaching a rival, in stretching, and in vocalization.

3.1.3.1 Approaching a rival

When approaching a rival, the approaching part decreases the distance between the rivals thus increasing the potential for physical conflict. Hence, walking towards a rival is a reliable signal because a cheater, with no intentions to fight, would be reluctant to do so. Zahavi & Zahavi (1997, p. 17) give this example from the days before razors:

> "A man's thrown-out chin presented [a] risk: it brought the threatener's beard nearer to his rival and made it easier for the latter to grab it. By putting his chin out, a threatener shows his confidence that his rival will not dare or will not be able to grab him by the beard or punch him on the chin – and that he, the threatener, is still confident of winning the fight if the other does dare".

3.1.3.2 Stretching

Many animals threaten by stretching their bodies. When stretching, the threateners do not show their weapons (e.g., teeth or claws) to their opponents, but rather expose their whole bodies to attack. Threatening dogs, for instance, stand side by side on tiptoe, with their bodies stretched and their hair raised (Ewer, 1968).

According to Zahavi & Zahavi (1997, p. 18), the point of stretching is not to show size but to convey confidence:

> "One who is not sure the object of the fight is worth the risk of injury will hesitate before exposing its body to danger […]. The appearance of another rival on the scene, the approach of a predator, the sound of one's offspring calling for help – all can instantly change one's willingness to take risks. Thus stretching one's body in the presence of a rival is a reliable, moment-by-moment indicator of one's current willingness to engage in the prospective conflict".

Zahavi & Zahavi (1997) argues that a stretching individual both reveals the precise size of its muscles while at the same time exposes its body to attack. By contrast, another individual likely of losing a battle cannot afford to expend much on showing off.

Stretching displays may last for a while before coming to a resolution. Clutton-Brock, Guinness & Albon (1982), for instance, reported red deer walking in parallel along their territorial line with bodies stretched for as long as thirty minutes before one of them decided to attack or withdraw.

3.1.3.3 Vocalization

According to Zahavi & Zahavi (1997, p. 19) the pitch of the voice reliably discloses the tension of the signaler's body:

> "A tense body makes a more high-pitched sound than a relaxed one. A frightened individual is tensed to take flight or to fight back. Only one who is relaxed, not poised to take instant action can sound a low-pitched, threatening note. Such an individual discloses reliably that it does not fear its rival; it is not coiled like a tightly wound spring and thus has exposed itself to a first strike".

The cost involved in making a low-pitched sound in front of a rival is thus the very element that makes the message reliable.

It is well known that posture affects the ability to produce a persuasive vocal threat. Actors, for instance, can sound convincingly threatening even though they have no intention at all of actually fighting. Zahavi & Zahavi (1997) refer a conversation they have had with an acting teacher who told them that actors do not worry about how to sound menacing because once they assume the body posture of a confident aggressor the correct tonal quality comes by itself. However, this does not mean that actors are able to bluff in real life:

> "In real life, facing an actual enemy who might attack at any moment, even the best actor would find it just about impossible to bluff and threaten with a relaxed, low pitched voice. Any tension in his body – any readiness to fight or flee instantly – would reflect itself in his vocalization" (Zahavi & Zahavi, 1997, p. 20).

Vocal threats are sometimes used from a distance, when attack is not yet immanent and tension versus relaxation is irrelevant, and humans sometimes shout in the presence of nearby listeners. Zahavi (1979) argue that shouting often is aimed not at the purported listener but rather at others farther away who are not part of the conflict. According to Zahavi & Zahavi (1997, p. 75), the reason is simple:

> "When someone threatens another in private and then does not carry out the threat, he or she loses standing in the eyes of that person only. When others are made witnesses, failure to carry out the threat will cause the threatener to lose standing not only in the eyes of the one threatened but also in the

> eyes of the witnesses. By shouting, the threatener raises the stakes and makes the threat more reliable; only confident individuals can afford to shout their threats before the crowd".

3.2 Honesty and deception in citing

The handicap principle has not been discussed much in the LIS literature. The only example I have been able to find is Koch & Davison (2003) who address the handicap principle in a research article published in *MIS Quarterly*:

> It is not unreasonable to assume that publications in a select group of journals are seen by the IS research community as fitness indicators, especially since publishing in them is rather difficult to achieve, which fits well with Zahavi and Zahavi's handicap principle. Also, we cannot forget that we humans are also animals, and thus are likely to have the propensity to behave in general accordance with the handicap principle when we assess other people's abilities (Koch & Davison, 2003, p. 523).

I first became acquainted with the handicap principle when reading Tor Nørretranders' popular science book *Det Generøse Menneske [The Generous Man]* (2002)[64]. In this book, Nørretranders develops what he considers an overlooked strand of Darwinian thought. In addition to the efficiency and survival instincts essential to successful evolution or natural selection, Nørretranders argues that animals and humans alike must show their worth by doing something difficult in order to impress in terms of sexual selection. On the cover of the book is a picture of a peacock feather. The awkward plumage of a male peacock is precisely the sort of generous signal that Nørretranders has in mind to convey that sexual selection almost seems to go against the cold adeptness usually associated with the survival of the fittest.

When reading Nørretranders' book with its chapter on the handicap principle, it struck me right away that here was a theory that could possibly account for much of the social behavior (including citation behavior) that have puzzled sociologists and others for quite some time. My confidence was only strengthened when I later read some of the works of Amotz Zahavi. Especially his personal views on theories of social behavior,

[64] I am grateful to my mother for giving me this wonderful book.

expressed in an anniversary essay in 2003, convinced me to attempt to provide a theoretical framework for citation behavior based on the logic of the handicap principle:

> "I believe that in future years ethologists, sociologists and others trying to find the ultimate reasons for the workings of social systems and for the patterns and reliability of signals will benefit from taking into account the importance of the quest for and the effect of social prestige as a mechanism that explains much of what happens in social systems. I also predict that the handicap principle (or 'costly signaling', as some who do not wish to refer to the handicap principle prefer to call it) will be found to be an inherent component in all signals" (Zahavi, 2003, p. 862).

3.2.1 References as threat signals

I propose that references are threat signals. Although it may seem a bit crazy at first, it is actually not an entirely new idea. In fact, Bruno Latour seems to propose the very same in is 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" (Latour, 1987, p. 33 [My italicizing]).

Although Latour and I seem to share the idea that references are threat signals, we do not agree entirely on the scenery of these signals or how authors may utilize such signals. Before outlining my own position on these matters I shall therefore briefly review Latour's position as expressed in *Science in Action*.

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 (1987) examines two strategies that might do the trick. He calls the first one *stacking*, and the other one *modalizing*. Latour maintains that the presence or absence of references in a scientific text signifies whether the text is serious and strong or not[65]. In order to appear serious and strong, the author should thus make sure to cite a number of other documents; what Latour (1987, p. 33) calls "stacking masses of references". The reason why this might do the trick of turning fiction into fact is that a potential reader would have to read, or be acquainted with the cited documents in order to be able to determine the strength and

[65] See section 1.1.1 for a related discussion.

accuracy of the citing text. That would be a tough job when enough documents are cited. However, as noted by Latour (1987, p. 33) himself, stacking masses of references is not enough 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 authors' argument:

> "If the reader is courageous enough, the result may be disastrous for the author" (Latour, 1987, p. 33).

Therefore, another strategy is needed to pull of the trick. The author has to *modalize* the status of the cited documents. According to Latour (1987), modalizing a reference means to modify or qualify the reference to make it more in keeping with the argument of the citing text. Latour (1987) claims, for instance, that the quotation in figure 3.1, located in a biochemistry paper by Schally et al. (1971, p. 6650), contains precisely such a modification.

D.F. Veber et al. have pointed out the similarity between the structure of our decapeptide and the amino-terminal sequence of the β-chain of porcine hemoglobin [reference]. The significance of this observation remains to be established.

Figure 3.1. *A modalized reference.*

According to Latour (1987, p. 35), the cited document (D.F. Veber et al.) contains a result, which disputes the validity of Schally et al's work, and Schally et al. therefore add the last sentence in order to modify the status of the cited document to their own interest:

> "What Schally does […] is done by all articles to their references. Instead of passively linking their fate to other papers, the article actively modifies the status of these papers. Depending on their interests, they turn them more into facts or more into fictions, thus replacing crowds of uncertain allies by well-arrayed sets of obedient supporters".

Apparently, Latour believes that authors are free to do whatever they need to the former literature to render it as helpful as possible for their own arguments. This belief is founded on an understanding of the scenery of citing and scholarship portrayed in figure 3.2, scanned from Latour's *Science in Action*. The figure shows a citing author, her article, and its (modalized) references. The authors of the cited documents are also shown, as are an unidentified man and an "isolated reader". What Latour means by "isolated reader" is not clear. However, for the sake of argument I shall 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 believe that authors would be free to do whatever they needed to the former literature to render it as helpful as possible for their own arguments. But this condition is highly unlikely and Latour's belief is thus a little too naïve. Most readers are not isolated in this sense. On the contrary, potential readers are generally well-read specialists with broad knowledge of the literature and field of the text. This becomes evident if one reflects on the common life cycle of a typical scientific or scholarly text: The journal article. Figure 3.3 on the next page illustrates the typical life cycle of journal articles. It reveals, among other things, two potential groups of readers: Readers prior to journal publication and readers after journal publication. Readers prior to journal publication count, among others, journal editors and referees. These potential readers participate in the typical pre-publication peer review process of academic journals. As it is commonly understood, "peer review is the process whereby authorities in a given field determine the validity and assess the relative significance of a particular contribution of a scholar or scientist within that field" (Osburn, 1989, p. 279). Weller (2001) argues that some form of prepublication review has been part of the journal production process since the first scientific journals appeared more than 300 years ago. Her description of the modern peer review process is presented in figure 3.4.

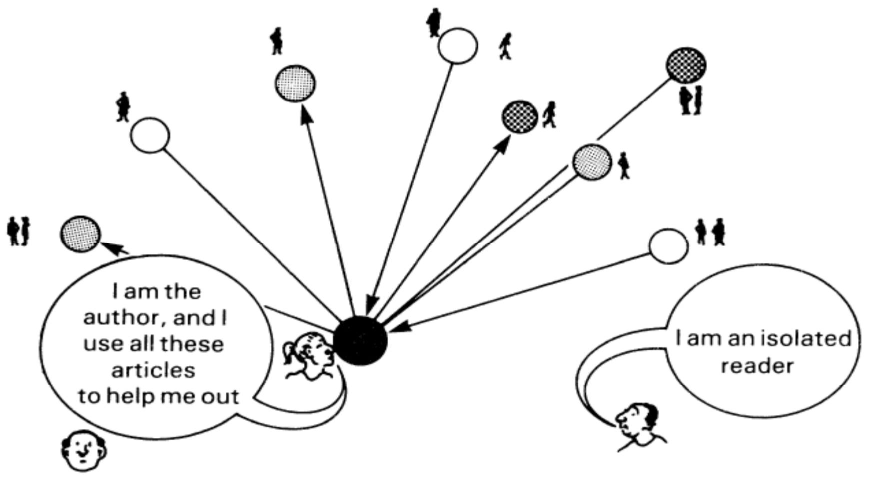

Figure 3.2. *The scenery of citing and scholarship according to Latour (1987, p. 38).*

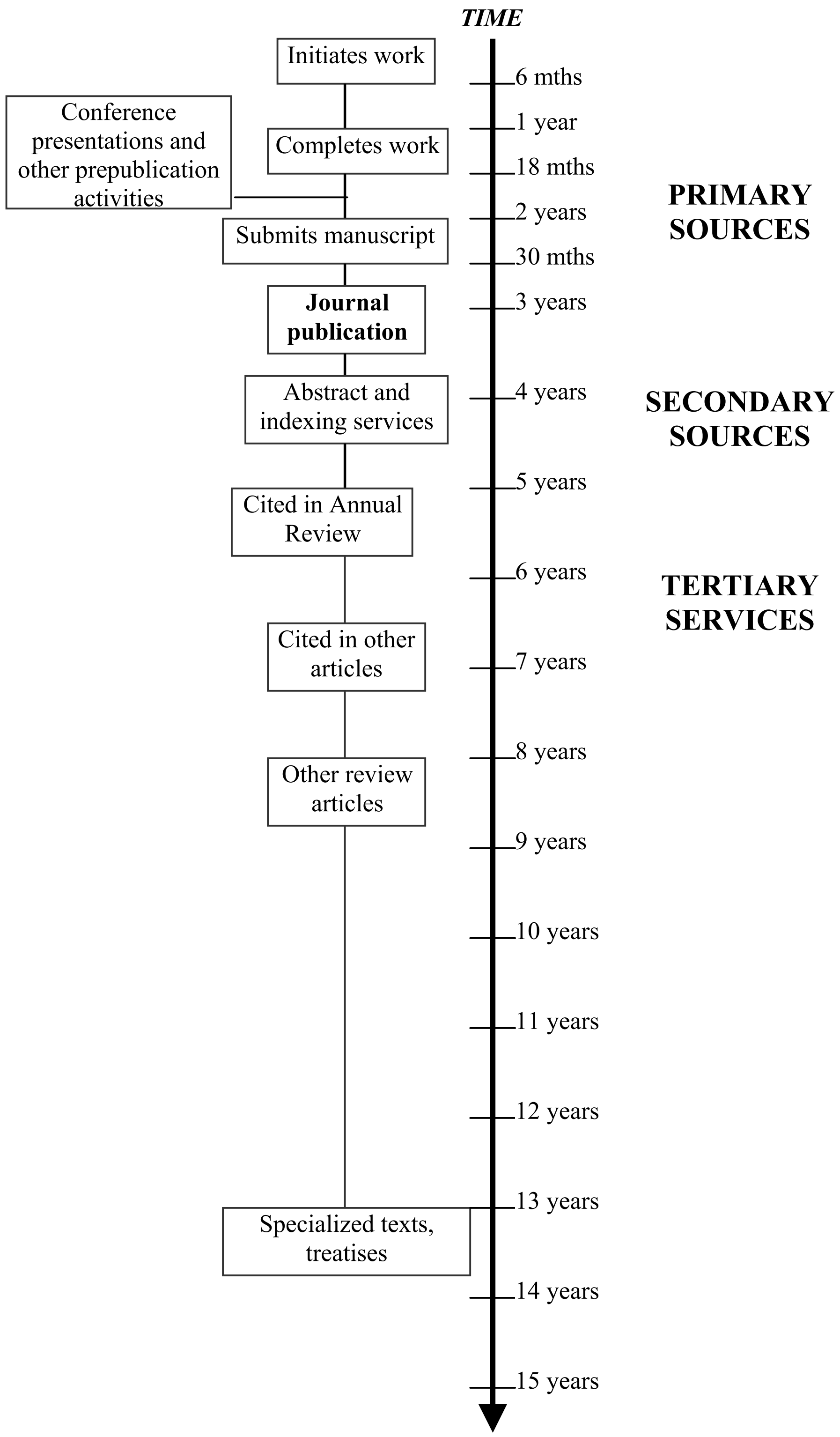

Figure 3.3. *The typical life cycle of a journal article*
(Fjordback Søndergaard, Andersen & Hjørland, 2003, p. 289[Modifed from Garvey & Griffith, 1972]).

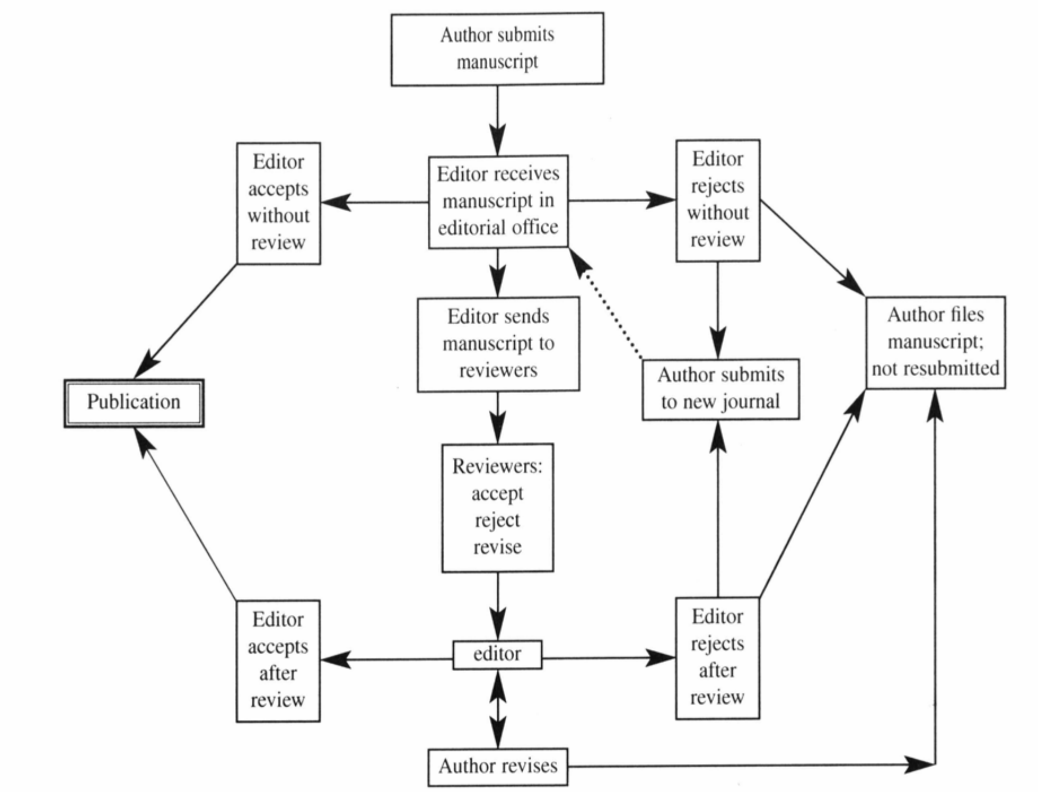

Figure 3.4. *Path of a manuscript through the editorial peer review process (Weller, 2001, p. 2).*

Weller (2001, p. 1-2) describes the usual path of a manuscript through the editorial peer review process:

> "An aspiring author sends a manuscript to a journal's editorial office. The journal editor, or for large journals one of the associate editors, logs in the manuscript, selects two or three reviewers to evaluate the manuscript, and sends each a copy. Reviewers are asked to assess the manuscript and make a recommendation to accept, accept with revisions, or reject the manuscript. The editors then decide if they will accept the recommendation of the reviewers. If the reviewers disagree, the editor may subject the manuscript to another round of reviews, or the editor may adjudicate, deciding without further review if the manuscript should be accepted or rejected.

Within these standard parameters, many variations exist. Sometimes editors accept or reject a particular manuscript with no input from reviewers".

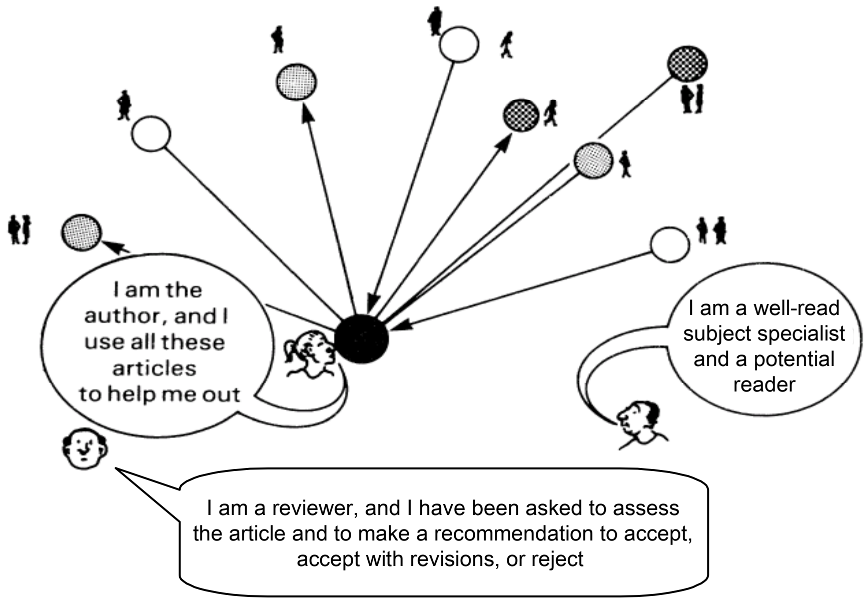

Figure 3.5. *A more exact illustration of the scenery of citing and scholarship.*

Among the potential post-publication readers are the members of the field of the journal article, and perhaps most important, the authors that are cited in the article. Like the editors and referees, many of these potential post-publication readers posses expert knowledge about the field and its literature. Hence, a more exact illustration of the scenery of citing and scholarship would be closer to the one presented in figure 3.5 than to the one portrayed by Latour. Such change of scenery does make a difference. A citing author that does whatever he likes to the former literature and thus commits willful acts of deceit risks getting exposed as the cheater he is by his potential readers. An honest author, who cites his sources and inspiration properly, need not fear the same exposure. The two authors may appeal to the same set of sources as backup for their arguments but not at the same potential cost. The potential cost is much higher for the cheating author than it is for the honest one. Thus, the essential requirement for the handicap principle to work is in place.

I propose to see references in much the same way as the threat signals briefly reviewed in section 3.1.3.

Latour is surely right when stating that attacking a text full of references necessitates the weakening of the cited documents. However, like the stretched body of an animal, the cited documents of a citing text are a sign of confidence. A stack of references is a handicap that only the honest author can afford. Like the beard of a thrown-out chin, it presents an open target for the bold opponent or rival.

Modalized references expose themselves like the vocalization of a bluffing threatener. A skilled rival will detect the false sound right away and then know where to attack. The potential cost of making such a sound will often make the author reconsider his deceitful behavior.

When the references are made in public, the stakes are raised even further. Like a shouting human, references may have witnesses. Yet, only a confident author can afford to "shout his threats before the crowd". Unconfident authors would usually not dare to risk the potential loss of reputation.

However, in line with Zahavi, who do not claim that cheating is never encountered in nature, I do *not* propose that all references are honest. There are enough cases of fraud and deceit in science and scholarship to falsify such a proposal without ado. What I propose is that the handicap principle secures that citing authors credit their inspirations and sources in an honest way, to a tolerable degree - enough to save the scientific communication system from collapsing.

The intellectual function of an established conceptual scheme is to determine the patterns of theory, the meaningful questions, the legitimate interpretations, etc. (Toulmin, 1970, p. 40).

The primary structuring unit in science is the scientific specialty (Small, 1976, p. 67).

4 The primary structuring units in science

Theories of science may be formulated as descriptive generalizations about how science actually *is* conducted[66] or as prescriptive recommendations about how science *ought to be* conducted. The theories of citing reviewed in chapter 2 are founded on descriptive generalizations of how science and scholarship is conducted[67]. Attempting to construct a theory of citing from an understanding of how science and scholarship actually work seems to be a very good idea. After all, citing is an academic practice as old as scholarship itself (Price, 1963), and many studies have demonstrated that the act of citing is closely tied to the actual operation of science.

In the words of Henry G. Small (1998, p. 143):

> "A theory of citation makes sense only as part of a larger theory of how science and scholarship work".

Later in the same article, Small (1998) makes clear that such a theory of citing should be anchored in a larger sociocultural theory rather than a psychological.

---

[66] No descriptions are independent of conceptual frameworks. Consequently, a descriptive generalization about how science is conducted is always anchored in such a framework. See also the quotation from Toulmin (1970, p. 40) above.

[67] The normative theory of citing has, of course, a prescriptive element as well. Recall Merton's definition of the scientific ethos: "The norms are expressed in the form of prescriptions, proscriptions, preferences and permissions" (Merton, [1942] 1973, p. 269). However, as Merton held the norms to be "binding on scientists", he consequently formulated a descriptive generalization about how science and scholarship actually is/was conducted.

Appeals to larger theories in order to comprehend various physical or social phenomena are common in all disciplines. In physics, for instance, string theory is the only known theory that unifies general relativity and quantum mechanics in a consistent way. In recent years there has been a dramatic progress in the understanding of the so-called non-perturbative aspects of string theory, and it is now believed that string theory is part of a larger theory called M-theory (Duff, 2003). In economy, different researchers understand the market concept in different ways. To certain economists, market relations exemplify the competitive struggle between individuals. To others, market mechanisms work to secure social harmony between diverse private interests. In either case, a theory of markets always implies a larger theory of social integration and regulation (Birner & Ege, 1999).

Small's plea for a theory of citing founded on a larger sociocultural theory of how science and scholarship work is in opposition to the dominating viewpoint in LIS (i.e., the cognitive viewpoint), and even contrary to what he himself has previously asked for. The central point of the cognitive viewpoint, which has dominated the thinking in LIS at least since the early 1980s, was formulated by De Mey (1977, p. xvi-xvii):

> "The central point of the cognitive view is that any *processing of information*, whether perceptual or symbolic, is *mediated* by a system of categories or concepts which, for the information-processing device, are a *model* of his world".

According to Ingwersen (1992, p. 18) the cognitive viewpoint is an approach intended to "investigate the variety of individual world models and knowledge structures that underly the surface structures of the variables of interaction". Thus, a cognitive theory of citing would stress that to understand why authors cite as they do, one needs to focus on the individual author and his or her world model or cognitive set-up rather than the social world of the individual[68]. Small (1987, p. 339), for instance, has argued that "to understand the significance of references we need to examine the cognitive processes involved in generating written discourse", and that "the view of 'citations as concept symbols' sees referencing as part of the cognitive process which produces written

[68] Proponents of the cognitive viewpoint sometimes argue for the importance of the social dimension and claim to have taken this into account as well in their theories. Borlund (2000, p. 16), for instance, claims that Ingwersen (1982, p. 168) adds to De Mey's definition of the cognitive viewpoint by stating that "the world model consists of cognitive structures that are determined by the individual and its social/collective experiences, education, training etc". Stating the importance of the social dimension is apparently much easier than actually investigating it. Ingwersen's scientific production contains very little or no research on the actual connection between the individual level and the social.

discourse" (Small, 1987, p. 340). However, Small has apparently abandoned the cognitive viewpoint. In his 1998 article published in the special issue of the journal *Scientometrics,* devoted to the discussion of theories of citing, he outlines a new strategy:

> "My strategy then is to see what light citations can shed on the relativism of knowledge. What I will outline might be called a network epistemology, and is oriented to a collective rather than an individual knower, what some have called a social epistemology" (Small, 1998, p. 144).

Unfortunately, Small does not explain why he has now abandoned psychological theories concerning the cognitive processes involved in generating written discourse for a larger sociocultural theory of how science and scholarship work. Critique of the cognitive viewpoint in LIS emerged around 1990 with Frohmann's article *Rules of Indexing: A Critique of Mentalism in Information Retrieval Theory* published in *Journal of Documentation* (1990). Other critical books and papers have followed since (e.g., Frohmann, 1992a, 1992b, Hjørland, 1992, 1997[69]). Perhaps this critique convinced Small to abandon the cognitive viewpoint and to acknowledge that a theory of citing makes sense only if founded on a sociocultural theory of how science and scholarship work.

The core of the critique centers on the cognitive viewpoint's exclusion of the sociocultural environment(s) in which the individual participates, or in Frohmann's words: "The erasure of the social" (Frohmann, 1992b, p. 376)[70]. The critics have argued that human cognition evolve in social milieus where humans communicate and corporate. The individual knowledge structures can thus be understood only from analyzing collectives of social agents[71]. Proponents of the cognitive viewpoint have to date not attempted to counter the core of the critique. In fact, they rarely mention it. Borlund (2000) and Ingwersen (1999) represent two good examples of how the proponents of the cognitive viewpoint have answered. Borlund (2000, p. 18) simply declares that the criticism is basically "of a philosophical or attitudinal nature". Ingwersen (1999, p. 32) states in his ARIST chapter entitled *Cognitive Information*

[69] Hjørland has also formulated his critique of the cognitive viewpoint in a Danish article published in the journal *Biblioteksarbejde* (1991).

[70] The critique is summarized by Jacob & Shaw in their ARIST chapter *Sociocognitive Perspectives on Representation* (1998).

[71] See also section 1.5.1.

Retrieval that "such metatheoretical criticism is unavoidable in a research field which, in reality, is heavily technology-dependent but progressing from a natural science-like IT-focused framework into the domains of social science with all its variety of philosophical attributes and schools". A number of critics have pointed out that proponents of the cognitive viewpoint tend to make use of opaque language[72]. I will restrict myself to conclude that the proponents of the cognitive viewpoint thus far have not delivered a sufficient answer to the core of the critique. Whether it is because they are unable to counter the critique in a meaningful way or just do not bother, I will avoid speculating on here. However, it seems reasonable to suggest that until the core of the critique have been dealt with, it will at least be suspect to attempt to formulate a theory of citing on a cognitive theory of how science and scholarship work. Instead, theorists should take the same starting point as the theorists who formulated the citation theories reviewed in chapter 2. and thus attempt to understand how sociocultural factors influence the individual researcher and affect his or her citation behavior.

The present chapter discusses the possible influence of two specific sociocultural factors. It takes the average mantra and the handicap principle as its points of departure, and assumes that authors credit their inspirations and sources in an honest way, to a tolerable degree – enough to save the scientific communication system from collapsing. The aim is consequently not to examine why or to what extent authors credit their sources, but rather to isolate and discuss the primary structuring units, which influence the way science and scholarship work, scholarly communication, and consequently the structural dynamics of citation networks. The two primary structuring units are termed *research traditions* and *specialties.*

4.1 Research traditions

The most influential development in philosophy of science following the demise of logical positivism has been the introduction of an historical perspective into philosophical thinking about science. This has resulted from a greater concern among philosophers of science to describe the actual character of scientific investigation and the ways in which researchers choose which theories to accept and pursue. Within the framework of the positivists, it was generally accepted that the primary factor that

[72] Soergel (2003, p. 112), for instance, ends his review of Ingwersen's ARIST chapter by concluding: "Many of the ideas have been around for much longer than this chapter documents, and many are common sense and do not require the sometimes a bit convoluted and dense expression they receive in the cognitive approach literature, including this chapter".

should govern decisions about the acceptability of a theory was the degree to which it corresponded to the evidence. However, as argued by the post-positivist philosophers, this is not the primary factor governing researchers' decisions, and the positivists have thus developed an account of science that in fact do not describe how science really works[73].

Having criticized the positivists for failing to describe real science, a number of post-positivist philosophers have proposed to develop the analysis of how science works not from general logical considerations but instead on the basis of examinations of the actual processes of science, particularly as revealed through its history. Thomas S. Kuhn's book *The Structure of Scientific Revolutions* (1962, 1970) has without a doubt been the major inspiration for the development of the historical perspective in the philosophy of science, as well as for some lines of thought in the sociology of science[74]. The adequacy of Kuhn's account has, however, been seriously challenged, and others have formulated their own historically grounded accounts of how science works. Before exploring these other accounts I will briefly present the central parts of the criticism that have been raised against Kuhn.

4.1.1 Critique of Kuhn's account of scientific practice

Thomas S. Kuhn's book *The Structure of Scientific Revolutions* (1962, 1970) was briefly summarized in section 2.2.1. In short, Kuhn's book challenged the assumption of many previous philosophers of science that science offers a steadily accumulation of knowledge. In contrast, Kuhn claimed that scientific disciplines go through distinct stages and that the character of research in the discipline varies between stages. Kuhn differentiated five stages: Immature science, normal mature science, crisis science, revolutionary science, and resolution, which are followed by a normal science.

Normal science requires the establishment of what Kuhn called a *paradigm*. In his original book, Kuhn was not entirely precise in his characterization of paradigms. In fact, Kuhn's use of the word *paradigm* has been shown to be systematically ambiguous[75]. However, a paradigm does posses certain identifiable characteristics. It provides a framework for characterizing phenomena that a particular discipline takes as

[73] See also section 2.2.

[74] See section 2.2.2.

[75] Masterman (1970) found 21 different ways in which Kuhn (1962) used the word *paradigm*. Not all of them were deemed to be compatible with each other.

its subject matter (according to Kuhn, in any mature scientific discipline, every scientist will accept the same paradigm most of the time). This might involve a basic model or a general theory. However, a paradigm is not simply a model or a theory, but also includes instructions as to how such a theory or model is to be developed and applied. Thus, once a researcher accepts a paradigm, he or she can proceed with the process of paradigm articulation or normal science.

According to Kuhn, a paradigm remains unchallenged until enough anomalies accumulate. Then researchers begin to speculate whether the paradigm is really appropriate, and crisis emerges. When a crisis emerges, researchers begin to look for alternative paradigms, and if an alternative paradigm proves to be more empirically successful than the former, then a scientific revolution occurs. The crisis has then been resolved, and another period of normal science begins.

A number of critics have praised Kuhn's book as a very important contribution to the philosophy of science, but have pointed out that Kuhn's description of science suffers from some acute conceptual and empirical difficulties (e.g. Shapere, 1964). Feyerabend (1970), Lakatos (1970, 1978) and Laudan (1977) have furthermore stressed the historical incorrectness of Kuhn's notion that normal science is characterized by a period of the sole existence of one dominating paradigm. Instead they maintain that every major period in the history of science is characterized by the co-existence of competing paradigms. Other critics have asked how the researcher determines the crisis point. According to Kuhn, a few anomalies do not produce a crisis, but many do. Exactly when, then, is the accumulation of anomalies big enough to make researchers begin to look for alternative paradigms?

Laudan (1977, p. 74-76) have identified five other "serious flaws" in Kuhn's description of science:

1. Kuhn's failure to see the role of conceptual problems in scientific debate and in paradigm evaluation.

2. Kuhn never really resolves the crucial question of the relationship between a paradigm and its constituent theories. It is not clear whether a paradigm precedes its theories or arises after their formulation.

3. Kuhn's paradigms have a rigidity of structure, which precludes them from evolving through the course of time in response to the weaknesses and anomalies, which they generate. Moreover, because Kuhn makes the core assumption of the paradigm immune from criticism, there can be no

corrective relationship between the paradigm and the data. Accordingly, it is very difficult to square the inflexibility of Kuhnian paradigms with the historical fact that many maxi-theories [or conceptual frameworks] have evolved through time.

4. Kuhn's paradigms are always implicit, never fully articulated. As a result it is difficult to understand how he can account for the many theoretical controversies which have occurred in the development of science, since scientists can presumably only debate about assumptions which have been made reasonably explicit. Kuhn is running squarely in the face of the historical fact that the core assumptions of paradigms often are explicit from their inception.

5. Because paradigms are so implicit and can only be identified by pointing to their exemplars, it follows that whenever two scientists utilize the same exemplars, they are, for Kuhn, *ipso facto* committed to the same paradigm. Such an approach ignores the persistent fact that different scientists often utilize the same laws or exemplars, yet subscribe to radically divergent views about the most basic questions of scientific ontology and methodology. To this extent, analyzing science in terms of paradigms is unlikely to reveal that "strong network of commitments – conceptual, theoretical, instrumental, and methodological" which Kuhn (1962, p. 42) hoped to localize with his theory of paradigms.

Two other philosophers of science have addressed themselves to the question of the nature of conceptual frameworks. Imre Lakatos has developed a theory about *research programmes* and Larry Laudan has developed a theory about *research traditions*.

4.1.2 Lakatos' research programmes

Imre Lakatos (1970, 1978) has produced an alternative to Kuhn's account of the scientific enterprise. Lakatos accepts Kuhn's argument that theories co-exist within an ocean of anomalies, but like Feyerabend (1970), he takes issue with Kuhn's claim that we can differentiate distinct stages of normal and revolutionary science. Instead he contends that science is rarely dominated by just one paradigm, but rather that competition between paradigms generally co-occurs with processes of development within a paradigm. Lakatos also takes issue with Kuhn's conception of normal science as filling in and further applying a single paradigm. He contends instead that research often consists in developing a succession of theories, in which new theories replace older ones while preserving important features of the older theories. To allow for this idea of a succession of theories, Lakatos replaces Kuhn's *paradigm* with the term *research programme*. According to Lakatos, the common thread linking different theories into a common research programme is a *hard core* of basic assumptions shared by all investigators. These cannot be abandoned or modified without repudiation of the research programme (Lakatos, 1970, p. 133-134). A protective belt of auxiliary assumptions surrounds the core. Contrary to the basic assumptions of the hard core, researchers are free to change, modify, or sophisticate the auxiliary assumptions of the protective belt to accommodate evidence that either has accumulated or is developed in the course of research (Lakatos, 1970, p. 135).

For Lakatos, the ultimate measure of a research programme is whether it is progressive. Progress consists, according to him, in developing new theories in the protective belt surrounding the core.

Laudan (1977) argues that Lakatos' account in many ways is an improvement on Kuhn's. His argument about the co-existence of several alternative research programmes at the same time, within the same domain, is in accordance with the historical records of science. Moreover, Lakatos grants that researchers can objectively compare the relative progress of competing research programmes. Kuhn (1962, 1970) maintains on the contrary that it is, in principle, impossible to establish that any paradigm ever rationally triumph over another. According to Kuhn, such comparison would imply a neutral observation language, and such is in principle impossible. In some of his later writings, Kuhn seems to have changed his mind a bit, and talks almost exclusively about the incommensurability of theories, terms, vocabularies, or languages, and thus not about the incommensurability of paradigms (Hoyningen-Huene, 1993, p. 212). Perhaps he realized that his own book *The Structure of Scientific Revolutions* (1962, 1970) implies the existence of a neutral observation language. For as pointed out by Laudan (1996, p. 9), what is it to write the history of science as Kuhn did in his *Structure of Scientific Revolutions* (1962, 1970), if it is not to suppose that paradigms different from ours can nonetheless be made intelligible to us?

Although Laudan (1977) acknowledges the improvements of Lakatos' account, he nevertheless rejects it, as he maintains that Lakatos' research programmes share many of the flaws of Kuhn's paradigms. He demonstrates a number of these flaws of which these three appear especially central:

1. As with Kuhn, Lakatos' conception of progress is exclusively empirical; the only progressive modifications in a theory are those, which increase the scope of its empirical claims.

2. Lakatos' claim that the accumulation of anomalies has no bearing on the appraisal of a research programme is massively refuted by the history of science.

3. Lakatos' research programmes, like Kuhn's paradigms, are rigid in their hard-core structure and admit no fundamental changes.

4.1.3 Laudan's research traditions

Laudan (1977) proposes an account of scientific practice that captures the strengths of Kuhn and Lakatos' accounts and overcomes their weaknesses[76]. In his book *Progress and Its Problems: Toward a Theory of Scientific Growth* (1977), Laudan offers a more complete account of the kinds of problems researchers encounter and also a finer grained analysis of how researchers evaluate the seriousness of problems and the importance of problem solutions. According to Laudan, science is essentially a problem-solving activity. Thus, a view of science as a problem-solving system "holds out more hope of capturing what is most characteristic about science than any alternative framework" (Laudan, 1977, p. 12). In developing his account of science as a problem-solving system, Laudan invokes the idea of a large-scale unit in science that he calls a *research tradition*. Like Lakatos' research programmes, Laudan's research traditions are associated with a series of specific theories, but they lack a common core that is immune to revision (Laudan, 1977, p. 97). What hold a research tradition together are common ontological assumptions about the nature of the world and methodological principles about how to revise theories and develop new ones. For a research tradition is:

> "A set of general assumptions about the entities and processes in a domain of study, and about the appropriate methods to be used for investigating the problems constructing the theories in that domain" (Laudan, 1977, p. 81).

According to Laudan, such ontological and methodological assumptions individuate the research tradition and distinguish it from others. He explains this individuation by

[76] Laudan (1977, p. 78) concedes a great debt to the pioneering work of both Kuhn and Lakatos.

arguing that a research tradition provides a set of guidelines for specific theories. Part of those guidelines constitutes an ontology, which specifies the types of basic entities existing in the domain or domains within which the research tradition is embedded. The function of specific theories associated with the research tradition is to explain all the empirical problems in the domain by "reducing them to the ontology of the research tradition" (Laudan, 1977, p. 79):

> "If the research tradition is behaviorism, for instance, it tells us that the only legitimate entities which behavioristic theories can postulate are directly and publicly observable physical and physiological signs. If the research tradition is that of Cartesian physics, it specifies that only matter and minds exist, and that theories which talk of other types of substances (or of "mixed" mind and matter) are unacceptable".

Moreover, Laudan (1977, p. 79) argues, a research tradition also delineates how these entities can interact with each other:

> "Cartesian particles can only interact by contact, not by action-at-a-distance. Entities, within a Marxist research tradition, can only interact by virtue of the economic forces influencing them".

A research tradition furthermore specifies the legitimate procedures or methods available to the researchers within that research tradition. Laudan (1977, p. 79-80) provides two specific examples:

> "The methodological posture of the scientist in a strict Newtonian research tradition is inevitably inductivist, allowing for the espousal of only those theories which have been "inductively inferred" from the data. The methods of procedure outlined for a behavioristic psychologist are what is usually called "operationalist"".

If a researcher breaks with the ontology or methodology of his or her research tradition, the researcher violates the strictures of the research tradition and divorces him or herself from it. However, as noted by (Laudan, 1977, p. 80), that need not be a bad thing. A number of important revolutions in science have come from researchers who broke with their research traditions and established new ones. However, breaking with ones research tradition is not something researchers usually do. Thus, Laudan (1977, p. 80) argues, if philosophers and historians want to understand the logic and the history of science they need to recognize the integrity of research traditions. For it is precisely that integrity, which stimulates, defines and delimits what can count as a solution to scientific problems.

According to Laudan (1977, p. 82), research traditions do not offer solutions to specific problems; they merely specify what the world is made of, and how it should be studied. Philosophers sometimes refer to such *specifiers* as *absolute presuppositions* (e.g. Collingwood, 1940). Absolute presuppositions cannot be verified as true or false. Their use is their logical efficacy, and does thus not depend on being true - they need only be supposed. Researchers therefore rarely question the absolute presuppositions of their research traditions. They simply take them for granted, and work from there. Collingwood provides a good illustration of this in his book *Metaphysics* (1940):

> "If you were talking to a pathologist about a certain disease and asked him 'What is the cause of the event E which you say sometimes happens in this disease?' he will reply 'The cause of E is C'; and if he were in a communicative mood he might go on to say 'That was established by So-and-so, in a piece of research that is now regarded as classical.' You might go on to ask: 'I suppose before So-and-so found out what the cause of E was, he was quite sure it had a cause?' The answer would be 'Quite sure, of course.' If you now say 'Why?' he will probably answer 'Because everything that happens has a cause.' If you are importunate enough to ask 'But how do you know that everything that happens has a cause?' he will probably blow right up in your face, because you have put your finger on one of his absolute presuppositions, and people are apt to be ticklish in their absolute presuppositions. But if he keeps his temper and gives you a civil and candid answer, it will be to the following effect. 'That is a thing we take for granted in my job. We don't question it. We don't try to verify it. It isn't a thing anybody has discovered, like microbes or the circulation of the blood. It is a thing we just take for granted'" (Collingwood, 1940, p. 31).

4.1.3.1 How research traditions shape research

Laudan distinguishes two kinds of problems that usually confront research traditions: Empirical inadequacies of the current theories and conceptual problems with the theories comprising the traditions. According to Laudan (1977, p. 66), the aim of science is to maximize the scope of solved empirical problems, while minimizing the scope of anomalous and conceptual problems.

Laudan's treatment of empirical problems is generally consistent with that of Kuhn and Lakatos'. Researchers face empirical problems when expectations based on the theories within their research traditions fail. Laudan (1977, p. 15) terms empirical problems *first-order problems,* and explains that these are problems concerning objects in some domain and, in general, are anything about the world that strikes the researcher as odd and in need of explanation. Empirical problems come in three major types: unsolved problems (those not adequately solved by any theory), solved problems (those that rival theories have solved, perhaps in different ways), and anomalous problems (those a particular theory has not solved, but at least one rival has) (Laudan, 1977, p. 17).

According to Laudan (1977, p. 45) conceptual problems are also part of the scientific practice, and it is by introducing them that he claims to be making a new contribution. Laudan (1977, p. 49) differentiates between two basic types of conceptual problems. First, *internal* conceptual problems arise for a theory, *T*, when *T* exhibits certain internal inconsistencies, or when its basic categories of analysis are vague and unclear. Second, *external* conceptual problems arise for a scientific theory, *T*, when *T* conflicts with another theory, *T´,* which proponents of *T* hold to be rationally well founded.

Laudan (1977) demonstrates throughout his book, by historical examples how the ontology and methodology of research traditions have influenced the range and the weighting of numerous empirical problems. Empirical problems, he proposes, are evaluated in terms of how important a challenge they seem to offer to the research tradition and whether other competing research traditions have been able to solve them. Laudan shows, moreover, that research traditions often generate acute conceptual problems.

Laudan (1977, p. 87) provides, among other examples, an example he calls *classical*. The example illustrates how the methodology of a research tradition influences the range of empirical problems:

> "Scientists in [the nineteenth-century phenomenological chemistry] tradition argued that the only legitimate problems to be solved by the chemist were those which concerned the *observable* reactions of chemical reagents. Thus, to ask how this acid and this base react to form this salt is to pose an authentic problem. But to ask how atoms combine to form diatomic molecules cannot conceivably count as an empirical problem because the methodology of the research tradition denies the possibility of empirical knowledge of entities the size of atoms and molecules. For other research traditions in the nineteenth-century chemistry, questions about the

> combining properties of certain entities not directly observable constituted authentic problems for empirical research".

Another example provided by Laudan (1977, p. 87) illustrates how the ontology of a research tradition excludes certain empirical problems from its domain:

> "The rise of the Cartesian mechanistic research tradition in the seventeenth century radically transformed the accepted problem domain for optical theories. It did so by arguing, or rather by simply postulating, that problems of perception and vision – problems which had classically been regarded as legitimate empirical problems for any optical theory – should be relegated to psychology and to physiology, fields outside the domain of optics".

These two examples illustrates how research traditions restrict what can count as empirical problems and what cannot. In Laudan's (1977, p. 87) words, research traditions separate genuine empirical problems from "pseudo-problems". One of the central roles of research traditions is thus to guide researchers in the "right" directions by indicating the important empirical problems, which must be solved, and the "pseudo-problems", which can legitimately be ignored.

Research traditions play another, but equally important problem determining role. According to Laudan, research traditions sometimes generate conceptual problems for their constituent theories. In fact, most of the conceptual problems, which any theory may face, result from tensions between that theory and the research tradition of which it is part. That is because the detailed articulation of a theory often leads to the adoption of assumptions, which violate the absolute presuppositions of the research tradition. Laudan (1977, p. 88) provides two historical examples, which illustrates such dissonance between research traditions and theories:

> "When […] Huygens came to develop a general theory of motion, he found that the only empirically satisfactory theories were those which assumed vacua in nature. Unfortunately, Huygens was working squarely within the Cartesian research tradition, at tradition which identified space and matter and thus forbade empty spaces. As Leibniz and other pointed out to Huygens, his theories were running counter to the research tradition which they claimed to instantiate. This was an acute conceptual problem of the first magnitude, as Huygens himself sometimes acknowledged"

> "When Thomas Young – working within the Newtonian optical research tradition – found himself offering explanations for optical interferences which presupposed a wave-theoretic interpretation of light, he was chastised for not recognizing the extent to which his wave theory violated certain canons of the research tradition of which he seemingly paid allegiance".

Laudan's examples justify his argument that research traditions play a decisive role as problem determiners. But Laudan also argues that research traditions play an equally important role as *solution constrainers*. According to Laudan, research traditions establish general ontologies and methodologies for tackling the problems of their domains. As such, they constrain the types of theories, which may be developed within their domains. For instance, if the ontology of a research tradition denies the existence of forces acting-at–a-distance (as, for instance, the Cartesian research tradition did), then the research tradition excludes theories, which relies on non-contact action. Proponents of a research tradition that denies the existence of forces acting-at–a-distance will consequently not attempt to solve their empirical or conceptual problems by adopting theories, which are incompatible with the ontology of their research tradition. They will, on the contrary, seek solutions that are compatible with the ontology of their research tradition. Laudan (1977) lists a number of examples, which illustrate how the ontologies of research traditions constrain the range of acceptable theories. One of these concerns Einstein's theory of the equivalence of matter and energy. Laudan (1977, p. 89) argues that Einstein's theory excludes any specific theory that postulates the absolute conservation of mass from consideration.

Often the methodology of a research tradition excludes certain types of theories. Any research tradition that has a strong inductivist or observationalist methodology will, for instance, regard specific theories, which postulates the existence of entities that cannot be observed, as inadmissible (Laudan, 1977, p. 89):

> "Much of the opposition to subtle fluid theories in the eighteenth century and to atomic theories in the nineteenth century was due to the fact that the dominant methodology of the period denied the epistemic and scientific well-foundedness of theories which dealt with "unobservable entities"".

What is very important to understand about research traditions is therefore that they significantly prohibit their proponents from adopting specific theories, which are incompatible with the ontologies or methodologies of their research traditions.

However, as noted by Laudan, research traditions do not function solely as negative solution constrainers. Research traditions have a number of positive functions as well. Of great importance is the historical fact that research traditions often have provided central clues for theory construction. Laudan (1977) provides a number of historical examples, which support this claim by illustrating how research traditions have functioned heuristically to suggest initial theories for their domains. One of these concerns Descartes' attempt to develop a theory of light and colors. Although Descartes' theory did not follow logically from the Cartesian research tradition, to which he adhered, the research tradition nevertheless directed the construction of his theory. The ontology of the Cartesian research tradition amounted to the assertion that the only properties, which bodies can have are those of size, shape, position, and motion. Although the research tradition did not specify precisely what sizes, shapes, positions, and motions particular bodies could exhibit, it made it clear that any specific theory would have to deal exclusively with these four parameters. Descartes therefore knew in advance that his optical theory would have to be constructed using these, and only these four parameters. Laudan (1977, p. 91) maintains that this explains why Descartes sat out to explain colors in terms of the shape and rotational velocity of certain particles, and refraction in terms of differential velocities of such particles. Laudan (1977, p. 91) moreover argues that as the Cartesian research tradition made it clear that particles of light possess exactly the same characteristics as any other material bodies, Descartes recognized that he could make use of general mechanical theorems when constructing his theoretical analysis of light.

Research traditions also function positively by rationalizing or justifying specific theories. Laudan explains that specific theories often make assumptions about nature, which are generally not justified by the theory itself or by the data, which confirm the theory. Such absolute presuppositions usually concern basic causal entities and processes, whose existence and operation the specific theories take "as given" (Laudan, 1977, p. 92). Laudan maintains that a research tradition sanctions certain assumptions in advance, and thus frees the researchers working within the tradition from having to justify these assumptions repeatedly. Although critics outside the research tradition may criticize the researchers for constructing theories based on such assumptions, the researchers know, according to Laudan (1977, p. 93) that their primary audience – "fellow researchers within the same tradition" – will not find these assumptions problematic. Laudan (1977) again provides a number of historical examples, which illustrates his point. One of them concerns Carnot's theory of the steam engine. When Carnot developed his theory of the steam engine he presupposed that no heat was lost in performing the work of driving a piston. According to Laudan (1977, p. 92), Carnot did not offer any rationale for that assumption, and, quite rightly, felt no need to. The reason is that Carnot worked within a research tradition (the caloricist) that accepted the basic ontological assumption that heat is always conserved. Although this assumption later turned out the be unacceptable, and although Carnot never was able to establish his theory of the steam engine (not even in principle), the example demonstrates how Carnot was able to presuppose certain things about nature when developing his theory.

In summary, Laudan (1977) has demonstrated that research traditions play a number of important roles in scientific practice. He has shown that research traditions justify many of the assertions that their theories make; that research traditions often serve to stamp certain theories as inadmissible because they are incompatible with their ontology or methodology; that research traditions influence the recognition and the weighting of both empirical and conceptual problems; and that research traditions often provide heuristic guidelines for the generation or modification of specific theories.

Laudan (1977) was not the first to point out the importance of a research tradition's ontology and methodology for scientific practice. Besides Kuhn and Lakatos, a number of other philosophers of science have suggested roughly the same. Andersen (1999, p. 89) notes, for instance, that Törnebohm (1974) when defining the components of a *paradigm* distinguishes between 1. Ideals and beliefs about science, such as epistemic goals, methods and criteria in the production and evaluation of scientific results inside the discipline; 2. Worldview hypotheses, including basic social ontological assumptions about the part of the world studied inside the discipline; and 3. Ideals concerning the extra-scientific significance of knowledge produced inside the discipline. The first and second of these seem to correspond somewhat to the methodological and ontological components of Laudan's research traditions. It would thus be wrong to credit Laudan (1977) solely for this idea. However, Laudan surely deserves much credit for having documented the functioning of these components in scientific practice.

4.1.3.2 How research traditions affect citation behavior

Laudan (1977) does not articulate how research traditions affect authors' citation behavior and ultimately how research traditions influence the structural dynamics of citation networks. However, his rich account on how research traditions shape research indirectly inform about citation behavior.

Hjørland (2003, p. 91) argues that:

> "A scientific article is the solving of a specific research problem. The problem is formulated in the article, and this problem has determined what kind of information was needed by the author in order to contribute to this

> problem. Based on the information need, information has been sought and selected, and the documents actually used are finally cited in the article".

Rephrased as a model (figure 4.1.), one sees that Hjørland (2003) actually offers a deterministic explanation for the structural dynamics of citation networks when arguing that research problems control the actual references in scholarly writings.

Research problem

▼

Information need

▼

Selection of information

▼

Use of information

▼

References in text

Figure 4.1. *The citation process (modified from Hjørland (2003)).*

His argument, that "the documents actually used are finally cited in the article" (Hjørland, 2003, p. 91), is definitely too optimistic. Chapter 2. discusses this at some length, and chapter 3. concludes that authors merely cite their sources to a tolerable degree. However, the model can easily be changed to a stochastic model by rewriting Hjørland's (2003, p. 91) argument to read:

> A scientific article is the solving of a specific research problem. The problem is formulated in the article, and this problem has determined what kind of information was needed by the author in order to contribute to this

> problem. Based on the information need, information has been sought and selected, and *the documents actually used are cited to a tolerable degree in the article.*

This revision still implies a strong connection between research problems and references. Authors cite documents because the documents have satisfied certain information needs and ultimately facilitated the solving of specific research problems. From this follows that knowledge on problem determination and problem solution is central in order to understand citation behavior and structural dynamics of citation networks. Laudan's (1977) account on these matters therefore appears apt to the point.

The preceding sub-section provided a number of examples from Laudan's (1977) book, which illustrate how the ontology and methodology of research traditions have influenced the range and weighting of research problems. Laudan argues that research traditions constantly influence the range and weighting of such problems. If he is right, research traditions indirectly influence authors' citation behavior and thus the structural dynamics of citation networks. A few examples may prove the point. Recall, for instance, Laudan's (1977, p. 87) *classical* example:

> "Scientists in [the nineteenth-century phenomenological chemistry] tradition argued that the only legitimate problems to be solved by the chemist were those which concerned the *observable* reactions of chemical reagents. Thus, to ask how this acid and this base react to form this salt is to pose an authentic problem. But to ask how atoms combine to form diatomic molecules cannot conceivably count as an empirical problem because the methodology of the research tradition denies the possibility of empirical knowledge of entities the size of atoms and molecules. For other research traditions in the nineteenth-century chemistry, questions about the combining properties of certain entities not directly observable constituted authentic problems for empirical research".

Scientists belonging to the nineteenth-century phenomenological chemistry tradition could not cite the same sources as their contemporaries who subscribed to other research traditions, which accepted the existence of unobservable reactions in chemical reagents. Questions about the combining properties of certain entities, not directly observable, constituted authentic problems for empirical research only for those who accepted the existence of unobservable reactions in chemical reagents. Thus, a scientist belonging to the nineteenth-century phenomenological chemistry tradition would not attempt to solve such a problem. He or she would therefore not need to cite the same information as the scientist who tried to solve such a problem and made use of information about unobservable reactions in chemical reagents in order to do it.

According to Laudan (1977), research traditions function as solution constrainers. As such, they constrain the types of theories, which may be accepted in their domains. The preceding sub-section offered a number of examples, which illustrated this point. One of these concerned the ontology of a research tradition. It was argued, that if the ontology of a research tradition denies the existence of forces acting-at–a-distance (as, for instance, the Cartesian research tradition did), then the research tradition excludes theories, which relies on non-contact action. Proponents of a research tradition that denies the existence of forces acting-at–a-distance will consequently not attempt to solve their empirical and conceptual problems by adopting theories, which are incompatible with the ontology of their research tradition. They will, on the contrary, seek solutions that are *compatible* with the ontology of their research tradition. Such behavior will obviously affect what information are sought, used, and cited.

Laudan (1977, p. 91) came close to address how research traditions affect citation behavior when discussing Descartes' attempt to develop a theory of light and colors:

> "Since his [Descartes'] research tradition made it clear that particles of light are exactly like other material bodies, he recognized that he could apply general mechanical theorems (such as the laws of impact and the principle of conservation of motion) to a theoretical analysis of light".

What Descartes actually realized was that he could use some information that others could not (those that disbelieved that particles of light were exactly like other material bodies). Thus, Descartes could cite something that the disbelievers could not.

Laudan's (1977, p. 92) example about Carnot, who did not feel a need to offer any rationale for his assumption that no heat was lost in driving the piston of a steam engine, also implies something about citation behavior. It actually demonstrates the *obliteration phenomenon.* The obliteration phenomenon describes the process that a scientist's work becomes so integrated into a field's body of knowledge that people frequently neglect to cite it explicitly[77]. Garfield (1975, p. 6) argues that:

> "Each scientific discipline has its own language: its own peculiar shorthand, its jargon, its slang, and its protocol regarding citations. Every paper on

[77] According to Garfield (1975) this process was first described by Merton (1968).

> atomic physics, for example, doesn't need to cite Einstein's 1905 paper; nor does every mathematics paper need to cite Archimedes".

Garfield (1975) maintains that the obliteration phenomenon operates on the disciplinary level. Laudan (1977, p. 93) maintains that a researcher's primary audience consists of "fellow researchers within the same tradition". Obviously both cannot be correct. Perhaps Garfield's (1975) quotation should be revised to read: Each *research tradition* has its own language: its own peculiar shorthand, its jargon, its slang, and its protocol regarding citations.

4.2 Specialties

The literature on scientific practice sometimes claims that scientific *specialties* influence scientists' citation behavior[78]. Whether the concept of specialties is essentially different from Laudan's concept of *research traditions* depends, of course, on how one defines the concept of *specialties*. Unfortunately, the literature on specialties sometimes provides rather blurred definitions of the concept. The following sub-section presents a definition of specialties as it is commonly found in the science studies literature.

4.2.1 The common definition of specialties

The common definition of the concept of *specialties* is found, for instance, in A.J. Meadows' book *Communicating Research* (1998). In this book Meadows (1998) discuss, among other things, the rapid growth of scientific research and how the research community has developed a mechanism for coping with the excessive information output. This mechanism is, according to Meadows (1998, p. 20), *specialization*. To understand exactly what he means by specialization, one has to examine his argument somewhat further.

Meadows (1998, p. 19) asks the reader to listen to Faraday's complaint from 1826[79]:

[78] Hagstrom (1970, p. 91-92) argues that "it is reasonable to believe that scientists will communicate most often and intensively with others in their specialties, exchanging preprints with them, citing their work, and exchanging reprints". Ziman (2000, p. 190) notes that "scientific specialties often seem to be shut off from one another by walls of mutual ignorance". Gieryn (1978) and Whitley (1974) notes that specialties usually have their own journals and scientific societies.

[79] Meadows (1998) found the passage in Crowther (1940, p. 113).

> "It is certainly impossible for any person who wishes to devote a portion of his time to chemical experiment, to read all the books and papers that are published in connection with his pursuit; their number is immense, and the labour of winnowing out the few experimental and theoretical truths which in many of them are embarrassed by a very large proportion of uninteresting matter, of imagination, and error, is such, that most persons who try the experiments are quickly induced to make a selection in their reading, and thus inadvertently, at times, pass by what is really good".

Today there is much more chemical information to cope with than in the days of Faraday's. Therefore, one could consequently be lead to believe that the problem, which Faraday described, is much worse today. However, according to Meadows (1998), it is not! The reason is that modern chemists no longer try to command what Meadows (1998, p. 20) terms "the same broad sweep of their subject" as chemists did on Faraday's time. Modern chemists concentrate instead on much more restricted *topics* (Meadows, 1998, p. 20). Researchers have become much more *specialized* (Meadows, 1998, p. 20). As research has expanded, researchers have confined their attention to selected *parts* of it (Meadows, 1998, p. 20). Members of a discipline are therefore typically interested in only *part of the field* (Meadows, 1998, p. 21).

This definition of specialties resembles the idea of a social division of labor in society. In all known societies the production of goods and services are divided as different work tasks, in such a way, that none of the members of a society conduct all tasks. On the contrary, the types of work tasks, which an individual may conduct, are often regulated by rules, and individuals are often obliged to conduct certain tasks. Adam Smith (1723-1790) was the first to formulate how the social division of labor leads to increased productivity. In his book, *On the Wealth of Nations* published in 1776, he even maintains that division of labor is the most important cause of economic growth. A famous example from the book illustrates his point. The example concerns a pin factory. According to Smith ([1776] 1976), a pin factory that adopts a division of labor may produce tens of thousands of pins a day whereas a pin factory in which each worker attempt to produce pins, from start to finish, by performing all the tasks associated with pin production will produce very few pins. What Meadows (1998) seems to have in mind, when describing the strategy adopted by modern chemists, is thus the strategy of a successful pin factory. Like the workers of a successful pin factory, modern chemists have divided their work tasks between them and are consequently working on different, but related tasks. Today, there are several specialties in chemistry, including organic chemistry, inorganic chemistry, chemical engineering, and many more. The same holds true for all other scientific fields. Sociologists, for instance, usually work within one of the specialties described in *Handbook of Sociology* (Smelser, 1988). These include, among others, the sociology of education, the sociology of religion, the sociology of science, medical sociology, mass media sociology, sociology of age, and sociology of gender and sex.

Meadows (1998, p. 44) mentions that disciplines and specialties also can be produced by *fusion*. The combination of part of biology with part of chemistry to produce biochemistry is just one example.

Consequently, what characterize a specialty are, according to Meadows (1998), the phenomenon or phenomena, which members of the specialty study. Organic and inorganic chemistry, for instance, are different specialties because the researchers in these specialties study different phenomena. Organic chemists study materials that are carbon based, such as oil or coal, while inorganic chemists work with materials that contain no carbon or carbon-based synthetics. Sociologists of science study scientific societies while sociologists of religion study religious societies. Though most of the members of these two groups have been trained in the discipline of sociology, they belong to different sociological specialties because they study different sociological phenomena[80].

4.2.2 Identification and mapping of specialties

As noted above, Meadows' definition of specialties corresponds to the definition usually employed in science studies[81]. A basic idea underlying many science studies is that members of a specialty communicate much more with each other than with members of other specialties. The explanation is thought to be simple: Members of a

[80] Chubin (1976, p. 448) explains the distinction between disciplines and specialties by arguing that disciplines form the teaching domain of science, while specialties comprise the research domain.

[81] Crane & Small (1992, p. 198), for instance, explain the concept of *specialties* by arguing that "clusters of related research areas constitute specialties whose members are linked by a common interest in a particular type of phenomenon or method (such as crime, the family, population, etc.). Disciplines, in turn, are composed of clusters of specialties". Small & Griffith (1974, p. 17) maintain that "science is a mosaic of specialties, and not a unified whole", and note that specialties are the "building blocks" of science. Gieryn (1978), Whitley (1974), and Zuckerman (1978) claim that a problem area is made up of a number of related though discrete problems, that a cluster of related problem areas comprise a specialty, and that a scientific discipline covers a set of related specialties.

certain specialty share a common interest in a certain phenomenon and therefore has something to communicate about. During the 1970s, this simple idea convinced a number of information scientists that it would be possible to map the specialties of any scientific discipline by studying the patterns of communication between its members. What was needed was simply some method for clustering such communication patterns. Marshakova (1973) and Small's (1973) co-citation technique[82] was found to provide the required method. The strength of co-citation is defined as the number of times two documents have been cited together. Co-citation analysis thus provides a quantitative technique for grouping or clustering cited documents or cited authors. By measuring the strength of co-citation in a large enough sample of units (e.g. documents or authors) it is possible to detect clusters of units, which are highly co-cited. The information scientists, who became interested in this technique during the 1970s, hypothesized that such clusters would represent scientific specialties.

Small & Griffith (1974) were the first to test the hypothesis. The source of data for their study was the magnetic tape version of the SCI for the first quarter of 1972. A total of 1.832 distinct cited documents were selected from the file on the basis of having been cited at least ten times during that quarter. The authors found that 7.241 (7,7%) of the total source items in the quarterly file cited two or more of the selected 1.832 documents. A total of 36.316 co-citations were generated by the 7.241 source papers, involving 20.414 distinct pairs. These 20.414 co-cited documents were found to vary in co-citation frequency from one to eighty-one. By clustering co-cited documents together which were co-cited beyond a certain threshold (the authors used three different levels (3, 6 and 10)), the authors were able to form a number of individual clusters. A linguistic analysis of word usage in the titles of the citing papers revealed that the clusters were linguistically consistent. This was taken as evidence that the clusters, in fact, corresponded to scientific specialties. Since Small & Griffith's (1974) pioneering study, many others have used documents as the unit of analysis and co-citations of pairs of documents as the variable that enables the clustering of cited documents. Some of these studies have made use of a statistical technique known as *multidimensional scaling*. This technique enables the construction of two-dimensional maps, which illustrate the clusters of co-cited documents. Such maps are commonly held to reflect the relationships between documents at a given level: That of science as a whole, or of particular disciplines, specialties, or sub-specialties.

White & Griffith (1981) were the first to construct a map based not on individual documents, but instead on sets of documents associated with different authors. Such sets are sometimes referred to as *oeuvres*. An oeuvre is a body of writings by a person and

[82] See section 1.2.2.3.

thus not the person him/herself. For their study White & Griffith (1981) used the oeuvres of 39 “well-known” information researchers. Using approximately the same technique as Small & Griffith (1974) they created a map of information science that showed five distinct clusters. The authors found that these clusters corresponded to five specialties. Since their pioneering author co-citation analysis (ACA) many others have conducted similar studies. A famous example is White & McCain (1998) who conducted an ACA of information science, 1972-1995. The authors claimed that their study was the fullest example of ACA to date, with the largest data exhibits. Their study concerned no less than 120 authors. These authors were selected because they were found to be the 120 most highly cited in the field[83] during the entire period. A factor analysis of the 120 authors for the entire 24-year span was found to reveal the specialty structure of the discipline.

4.3 How the primary structuring units in science affect the structural dynamics of citation networks

Section 4.1 and 4.2 presented two accounts of how science and scholarship work. The first of these (Larry Laudan’s) holds that science and scientific disciplines embrace numerous *research traditions*, which, in various ways, influence research and communication. The other account (Meadows’) depicts scientific disciplines as mosaics of *specialties*, and holds that these influence research and communication. Thus, both accounts claim that their proposal (research traditions or specialties) influences the way scientists and scholars conduct their work and how scholars interact with each other. The two accounts explain why scholars find certain sources attractive and others unattractive, and, ultimately, why authors cite the sources they do. However, the two accounts seem to contradict each other somehow. The first account claims that scholars tend to communicate with colleagues working within the same research tradition, but not necessarily within the same specialty. The second account claims, on the contrary, that scholars tend to communicate with colleagues working within the same specialty, but not necessarily within the same research tradition. Obviously, both cannot be correct. Something is wrong. At least one of them fails to depict accurately how science and scholarship really work.

[83] White & McCain (1998) used 12 journals to define the field of information science.

4.3.1 Reexamining the role of specialties

As noted in section 4.2, a basic idea underlying many science studies is that members of a specialty communicate much more with each other than with members of other specialties. The explanation is thought to be simple: Members of specialties share a common interest in a certain phenomenon and therefore has something to communicate about. If this is true, one would expect to find that members of a certain specialty referred to each other's work much more frequently than to the work of scholars in other specialties. A co-citation analysis should thus reveal such patterns of strong specialty dependence. Section 4.2. reviewed a number of co-citation analyses, which all demonstrated such strong specialty dependence. These results indicate that authors' citation behavior is highly influenced by specialty membership. Thus, why don't we skip the rest and jump right to the conclusion and conclude that authors cite the sources they do due to the fact that the cited author and the citing author are members of the same scientific specialty and thus share a common interest in a certain phenomenon? The reason is that the co-citation analyses, which support this conclusion, are biased in ways that make their results biased as well. It is not that the results are wrong, they merely fail to tell the whole story.

4.3.1.1 The common bias of co-citation maps

A co-citation analysis usually consists of six separate steps. McCain (1990, p. 434) illustrates the separate steps in an ACA. These are similar to the steps in a co-citation analysis of other units (e.g. documents). Figure 4.2 illustrates the six steps. The common bias of co-citation analyses is introduced with the first step. We shall therefore narrow the present discussion to this step[84].

To conduct a co-citation analysis one has to have something to analyze. This usually means that one has to draw a sample of some sort, normally a sample of documents or a sample of authors. Drawing such a sample can be a complicated task. However, it is often necessary as it is often impossible, impractical, or extremely expensive to collect data from all the potential units of analysis covered by the research problem. Fortunately, analysts should be able to draw precise inferences on all the units (a set) based on a relatively small number of units (a subset) when the subset accurately represent the relevant attributes of the whole set. To achieve such precision the analyst needs to deal effectively with three issues (Frankfort-Nachmias & Nachmias, 1996, chapter 8):

---

[84] The remaining steps will be dealt with in chapter 5.

1. Definition of the population
2. Selection of a representative sample
3. Determination of the sample size

A population is usually defined as the "aggregate of all cases that conform to some designated set of specifications" (Chein, 1981, p. 419). For example, by the specifications "authors" and "having published in *Journal of Documentation*" one can define a population consisting of all authors having published in *Journal of Documentation*". As the specific nature of the population depends on the research problem, the population often has to be defined in terms of a) content, b) extent, and c) time. If the research problem only concerns authors having published articles in *Journal of Documentation* during the last 25 years, the population consequently has to be defined in terms of a) authors, b) having published articles in *Journal of Documentation*, c) in the period 1979-2003.

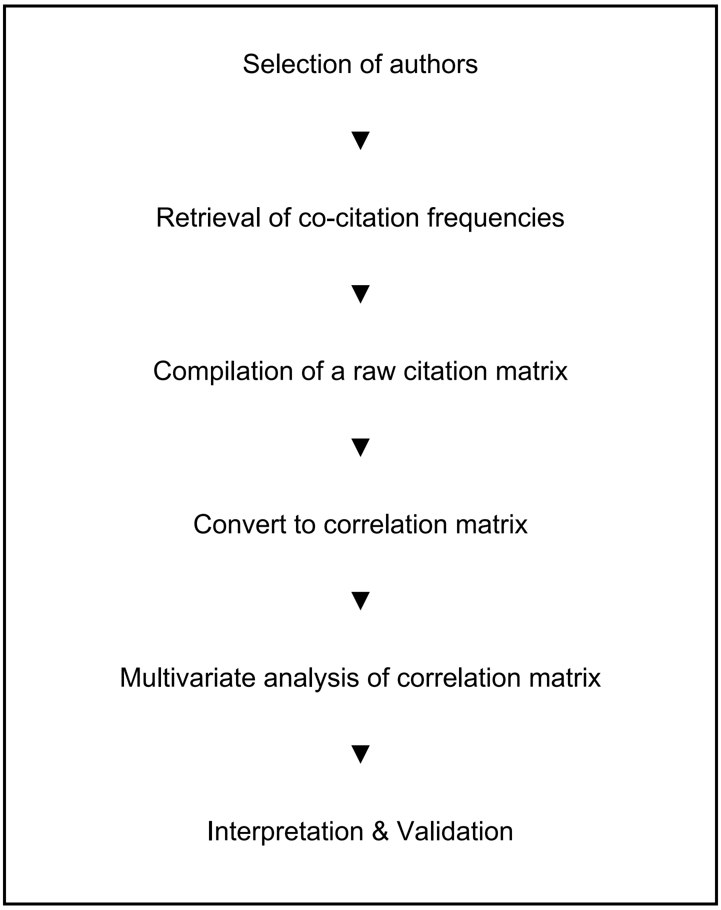

Figure 4.2. Steps in author co-citation analysis.
(Modified from McCain (1990, p. 434).

A single member of a sampling population (e.g., an author having published in *Journal of Documentation*) is called *a sampling unit*. The essential requirement of any sample is that it should represent the population from which it was drawn. A sample is considered to be representative if analyses made using sampling units produce results similar to results achieved using the entire population of units. A well-designed sample ensures that if a study were to be repeated on a number of different samples drawn from a given population, the findings from each sample would not differ (much).

A sample may be *a probability sample* or *a non-probability sample*. The distinguishing characteristic of probability sampling is that for each sampling unit of the population, one can specify the probability that the unit will be included in the sample. In non-probability sampling one cannot specify the probability of each unit's inclusion in the sample, and there is no assurance that all units has the same chance of being included. Chein (1981, p. 421) argues that if a set of units has no chance of being included in the sample then the definition of the population must be restricted.

To estimate the proper size of a sample, the analyst needs to determine what level of accuracy is expected of his or her estimates. The literature on sampling provides different techniques for determination of sample size[85].

Thus, if an analyst sets out to construct a co-citation map that illustrates clusters of units in some field or discipline of study, he or she must see to it that the sample used for analysis is constructed in a way that makes it possible to conclude from the sample to the population. Such a conclusion is only possible if the sample has been drawn adequately and thus reflects the population. This implies that the sample must be designed in a way that gives all units an equal chance of being selected. Thus in order to visualize a discipline by author co-citation analysis, the sample of authors must be a random sample of authors who have published in the field during some specific period. One would thus expect White & McCain's (1998) famous author co-citation analysis of information science (1972-1995) to be based on a random sample of authors having published information scientific books, papers, reviews, etc. during the specified period. Likewise, one would expect Small & Griffith's (1974) attempt to map the scientific literature by co-citation analysis to be based on a comparable random sample. However, this is not the case. Like most other co-citation analyses these are based on so-called *convenience samples*. Recall that White & McCain (1998) limited their sample to the 120 most cited authors in twelve selected international journals while Small & Griffith (1974) based their investigation on documents having been cited at least ten times

[85] Kvanli, Guynes & Pavur (1995, p. 232) provide the rule of thumb that if the sample is a random sample (i.e., all units have exactly the same chance of being selected) then a sample of >30 units will be sufficient.

during a specific quarter of 1972 by documents indexed in the SCI. The main problem with convenience samples is according to Frankfort-Nachmias & Nachmias (1996, p. 184) that researchers have no way of estimating the representativeness of their samples, and therefore are unable to estimate the population's parameters. In the cases of White & McCain (1998) and Small & Griffith (1974) it means that we have no way of knowing whether their results actually visualize information science and scientific literature as claimed or whether they just visualize the artificial samples. However, there are good reasons to believe that co-citation maps based on highly cited units do not provide adequate illustrations of whole disciplines or fields. The main reason is that highly cited authors and documents do not represent the "average" author and document. A simple example borrowed from Hjørland (1981, p. 184) explains why:

> "Examining the history of psychology we find it has been marked by shifting theoretical orientations. For example, during the last ten years there has been a rapidly increasing interest for classical authors such as Sigmund Freud, C.G. Jung and Karl Marx. When such shifts occur it is obvious that they influence the distributions of citations. Now, in contrast to earlier periods dominated by, for instance, behavioristic theories, it is these persons and their successors who in particular are being cited" [my translation].

Hjørland's (1981) claim about shifting theoretical orientations having marked the history of psychology is, for instance, supported by Robins, Gosling & Craik's (1999) empirical analysis of trends in psychology. Examining the distribution of references in four leading psychological journals between 1977 and 1996, the authors found that a shift had occurred around 1979. More precisely they found that the behavioral school, which previously had been the most cited school, lost approximately half its share of annual citations from 1977 to 1980. During the same period, the cognitive school almost doubled its share of citations. Since 1980 the cognitive school was found to have increased its share further. Thus, in 1996 the cognitive school had been cited almost 500 times in the four leading journals whereas the behavioral school had been cited only about 100 times. The authors found that two other schools, the psychoanalytic and the neuroscientific, had been cited approximately by the same annual frequency throughout the investigated period, but considerably less than the two other schools. Thus, if one draws a sample of highly cited authors from articles published in 1996 in these four journals, it will not reflect adequately the population of cited authors, as it will probably include only the highly cited authors working within the cognitive school.

Hjørland's other claim, that the ten-year period preceding 1981 had seen a rapidly increasing interest for classical authors such as Sigmund Freud and C.G. Jung, does not find support in Robins, Gosling & Craik's (1999) study. As mentioned above, the authors found that the psychoanalytical school had been cited by almost the same frequency from 1977 to 1996 and much less than the behavioral and cognitive schools. Actually, its share of citations was found to drop slightly from 1977 to 1981. Robins, Gosling & Craik's (1999) study concerns the distribution of references in four international journals. Whether Hjørland (1981) was referring to the international state of affairs or to the Danish/Nordic state of affairs is not entirely clear[86]. He ends his paper by concluding that there are important national differences, and that some "research paradigms" prefer the journal article as communication medium while others prefer the monograph. This is another important observation. If this holds true for other disciplines as well[87], it more or less disqualifies SCI, SSCI, and A&HCI as sample populations for co-citation analyses, which seek to map larger areas (as, for instance, White & McCain (1998) and Small & Griffith (1974) did). The reason is that the citation indexes almost exclusively index references from international journals. Thus, if one draws a sample of highly cited authors from these indexes it is likely to contain a number of those authors, who are being frequently cited in the international journal literature, but not necessarily in national journals or monographs. A number of empirical studies have demonstrated this to be the case. The bibliometric consequences of scholars' national orientation are, for example, well illustrated by Webster's (1998) analysis of a Polish sociological citation index (PSCI[88]) and the SSCI. By counting and comparing the number of citations to Polish sociologists in the two indexes between 1981 and 1995 she was able to conclude:

- Lists of the top 20 most cited Polish sociologists in each index had 12 names in common. The most cited sociologist in the PSCI (253 citations) was ranked as number 41 in the SSCI (19 citations). The most cited sociologist in the SSCI (254 citations) was ranked as number 20 in the PSCI (41 citations).

[86] Hjørland's (1981) article was published in the journal *Nordisk Psykologi* [Nordic Psychology].

[87] Hicks (1999, p. 193), for instance, claims that "social science research is characterized by more competing paradigms and a national orientation".

[88] The PSCI was constructed from the four leading Polish sociological journals.

- Lists of the top 20 most cited documents by Polish sociologists in each index contained none in common.
- Warsaw sociologists dominate Polish sociology. 60% of the citations in PSCI went to Warsaw sociologists. 80% of the citations to Polish sociologists in SSCI went to Warsaw sociologists.

Webster's (1998) findings strongly suggest that bibliometric indicators based on SSCI paint one picture of Polish sociology, and the PSCI another. A number of related studies demonstrate that indicators constructed from journal references alone will differ from indicators that include book references as well. In one of these, Cronin, Snyder & Atkins (1997) constructed a database comprising 30.000 references from 90 books randomly chosen among those reviewed in top sociological journals and published between 1985 and 1993. The authors compared lists of the 26 authors most cited in the books and in the top 24 sociology journals[89], and found:

- Nine authors featured on both lists.
- The five authors ranked 22 to 26 on the book list did not appear among the top 532 authors most cited in the top journals.

Cronin, Snyder, and Atkins' (1997) findings suggest that there are two distinct populations of highly cited authors in sociology: One consisting of authors cited in the journal literature, another of authors cited in the monographic literature. Given the citation indexes' limited coverage of monographic citing material, the latter population may regularly go unrecognized.

The majority of co-citation analyses are based on ISI data. ISI data are normally used for two related purposes: 1) for selecting a sample for further analysis, and 2) for detecting how many times the sample units are co-cited. Reliance on ISI's citation indexes is clearly problematic. Such a sample may at best be regarded a fractionated sample, and any results based on such a sample has limited generalizability.

A number of researchers have sought to bypass the sampling problem and have made use of "authoritative" lists of authors instead. White & Griffith (1981), for example, constructed a map of information science using a list of 39 authors taken in considerable part from *Key Papers in Information Science*, edited by Griffith (1980). The table of contents of that volume supplied 22 names, and the authors added 17 more, which they judged to be "major contributors to the field" (White & Griffith (1981, p. 164). The

[89] ISI's impact factor was used to identify the 24 top journals.

authors note that the resulting list is biased towards established figures with multiple contributions over the years. This is probably true. Another, and perhaps more important bias has to do with Laudan's (1977) theory of research traditions. If Laudan (1977) is correct, then researchers from different research traditions will evaluate the merit of different papers differently. Thus, if one asked researchers from competing research traditions to point out the key papers of their discipline, one would probably end up with quite different lists of papers. Consequently, co-citation maps based on so-called "authoritative" lists probably fail to provide a full picture of disciplines and specialties with different competing research traditions.

4.3.2 The relationship between specialties and research traditions

Specialties are different from research traditions. A research tradition is "a set of ontological and methodological do's and don'ts" (Laudan, 1977; p. 80) whereas a specialty is usually a specific part, fraction or division of a larger discipline. Small (1977, p. 139) maintains that "the 'specialty' is the principal mode of social and cognitive organization in modern science whereas Laudan (1977) argues that if philosophers and historians want to understand the logic and the history of science, they need to recognize the integrity of research traditions. Although the concepts of research traditions and specialties are different, they appear, however, to be somewhat interwoven in each other.

Laudan (1977) provides a number of historical examples, which demonstrate the co-existence of research traditions within the boundaries of specialties. These examples support Feyerabend (1970) and Lakatos' (1970, 1978) critique of Kuhn (1962, 1970) who held that science is characterized by periods of normal science dominated by one sole paradigm. The co-existence of "rival" research traditions is actually a crucial component in Laudan's (1977) account on how to evaluate research traditions. Unlike Kuhn (1962, 1970), Laudan (1977) maintains that it is quite possible for scientists to make sensible choices between alternative research traditions. According to him, a choice of research tradition is always a comparative matter:

> "All evaluations of research traditions and theories must be made within a comparative context. What matters is not, in some absolute sense, how effective or progressive a tradition or theory is, but, rather, how its effectiveness or progressiveness compares with its competitors" (Laudan, 1977, p. 120).

Laudan (1977) presents a detailed account of how to compare and choose between research traditions. Leaving the specific details aside, Laudan (1977, p. 109) basically urges the researcher to “choose the theory (or research tradition) with the highest problem-solving adequacy”, and he adds that “the choice of one tradition over its rivals is a progressive (and thus a rational) choice precisely to the extent that the chosen tradition is a better problem solver than its rivals”. Laudan (1977) apparently accepts that research traditions frequently share a sum of ontological assumptions, which make comparison and evaluation possible. Discussing, for instance, whether it was rational for astronomers and physicists of the seventeenth century to accept the Galilean research tradition instead of its primary competitor, Aristotelianism, Laudan (1977, p. 112) argues that as both were studying some of *the same phenomena*, the astronomers and physicists were able to make a rational choice between the two:

> “What Galilean astronomy and physics did have going for it was its impressive ability to explain successfully some well-known phenomena which constituted empirical anomalies for the cosmological tradition of Aristotle and Ptolemy. Galileo could explain, for example, why heavier bodies fell no faster to the ground than lighter ones. He could explain the irregularities on the surface of the moon, the moons of Jupiter, the phases of Venus, and the spots on the sun. […] Galileo was taken so seriously by later scientists of the seventeenth century, not because his system as a whole could explain more than its medieval and renaissance predecessors (for it palpably could *not*), but rather because it showed promise by being able, in a short span of time, to offer solutions to problems which constituted anomalies for <u>the other research traditions in the field</u>” [my underlining].

What Laudan (1977) seems to be arguing is that members of a given specialty[90] agree to some extent on the phenomena, which constitute their specialty[91]. Thus, although a given specialty may embrace a number of competing research traditions that differ ontologically and methodologically, the very same research traditions share a sum of ontological assumptions, which constitute the specialty. This is a radical argument,

[90] Laudan (1977) uses the term *field*.

[91] Recall also that Crane & Small (1992, p. 198) explains the concept of specialties by arguing that “clusters of related research areas constitute specialties whose members are linked by a common interest in a particular type of phenomenon …”.

which breaks with the received view of specialties and paradigms. Law (1973, p. 276-277), for instance, claims that:

> "It is clear from Kuhn's work that within a normally operating specialty, scientists will interpret each other's work in terms of a paradigm, and judge it by criteria implicit in that paradigm; it follows that acceptable research actions in a specialty are limited […].. In any specialty there will be a class of problems that is *permissible* (because those problems are held to be within the compass of current theory and methods), and a much larger class that is impermissible".

	Research tradition X	**Research tradition Y**	**Research tradition Z**
Specialty A	•		
Specialty B	•	•	
Specialty C	•	•	•

Figure 4.3. *Some possible interconnections between scientific specialties and research traditions. (The dots mark the interconnections).*

Laudan's (1977) argument about the co-existence of competing research traditions within the boundaries of specialties stirs the picture completely. According to him, there may not be just one class of permissible problems in a specialty, but several classes. Problems, which relate to the sum of shared ontological assumptions, will be shared by all research traditions in the specialty. Other problems will be limited to a single research tradition[92]. Moreover, the acceptable research actions in a specialty may be directed by several research traditions. Thus, what counts as an interesting problem and as a legitimate solution varies from one research tradition to another and within the same specialty. A specialty may thus include or contain a number of theories, which are regulated by different research traditions. Moreover, a research tradition may regulate theories in different specialties. Figure 4.3. illustrates some possible interconnections

[92] If two or more research traditions share a sum of ontological and/or methodological assumptions they may, of course, face some of the same problems.

between four simulated specialties (*A, B, C*) and three simulated research traditions (*X, Y, Z*). A quick look at the figure reveals that research tradition *X* regulates theories in all three specialties (*A, B* and *C*), that research tradition *Y* regulates theories in two specialties (*B* and *C*), and that research tradition *Z* regulates theories in one specialty (*C*). The theories of specialty *A* are thus regulated by one research tradition (*X*), theories of specialty B is regulated by two different research traditions (*X* and *Y*), and the theories of specialty *C* by all three research traditions (*X, Y* and *Z*).

4.3.2.1 How specialties and research traditions affect communication

As any specialty may embrace a number of competing research traditions, it is difficult to accept Small's (1976, p. 67) claim that "the primary structuring unit in science is the scientific specialty". Likewise, it is hard to believe that specialties are really shut off from one another by walls of mutual ignorance (Ziman, 2000) with the result that scholars communicate mostly with fellow specialty members (e.g., Hagstrom, 1970). Instead, sections 4.1 and 4.2 suggest that research traditions *and* specialties are the primary structuring units of science, and, consequently, that scholars communicate *within* and *between* specialties and research traditions. This, of course, is a testable hypothesis. It will be dealt with in the following chapter 5. However, before we skip to the test, the hypothesis needs to be clarified further. The core of the hypothesis amounts to the following two statements.

1. If researchers from different specialties communicate with each other they will probably belong to the same or related research traditions.

2. If researchers from conflicting research traditions communicate with each other they will probably belong to the same specialty.

The reasoning behind the two statements are as follows: Communication necessitates something to communicate about. Researchers from different specialties and research traditions are engaged with the study of different phenomena and posses conflicting ontological and methodological assumptions. Moreover, they would probably fail to understand each other if "forced" to discuss their research. They have next to nothing to communicate about. Thus, they will not attempt to communicate with each other. To have *something* to communicate about implies a mutual interest in one or several phenomena. However, such mutual interest need not be sufficient for communication to occur. Researchers from different research traditions may possess very different methodological assumptions. Such assumptions often dictate how to study and talk about the mutual research phenomena. Researchers from conflicting research traditions will often find it hard to accept the way researchers from other traditions study and talk about the research phenomena, and will consequently avoid communication. From this follows that communication has the best conditions in an environment of shared ontological and methodological assumptions. The hypothesis breaks with the received view of specialties and paradigms. It is consequently a minority view, and we need therefore examine the most prevalent objections to it.

Some may possibly question whether conflicting ontological and methodological assumptions really produce less communication. On the contrary, they may argue, such disagreement will surely stimulate communication, as members of the conflicting parties will seek to solve their differences by debate. In doing so, they will have to cite each others work (negatively), and the debating parties will consequently materialize as a unified cluster if one created a co-citation map of the field. Though this may seem like a proper and logical explanation at first look, it is probably mistaken. Researchers tend to ignore conflicting ideas rather than debating them. As observed by Delamont (1989, p. 335):

> "The most devastating way of demonstrating that another scholar is not part of the in-crowd is to leave them out of debate all together – to render them invisible".

Consequently, the argument that disagreement will stimulate communication is more often wrong than right. As noted by Meadows (1974, p. 45):

> "If incorrect results stand in the way of the further development of a subject, or if they contradict work in which someone else has a vested interest, then it may become necessary to launch a frontal attack […]. Otherwise, it generally takes less time and energy to bypass erroneous material, and simply allow it to fade into obscurity".

Thus, disagreement has probably very little influence on the distribution of references and citations. As noted by Cole & Cole (1974: 33):

> "Papers which are trivial and receive critical citations will not accumulate large numbers of citations".

This is, of course, a generalization. Garfield (1989, p. 10), however, claims it is one he has found to apply in most cases. Yet there are important exceptions. The famous article on cold fusion from 1989, for instance[93]. Meadows (1998) found that this article had been cited several hundred times, mostly by researchers who disputed its results. Reiterating the essentials of his 1974 argument, Meadows (1998, p. 90) concluded:

> "Such citation depends on the importance of the topic: questionable articles dealing with less important topics are likely to be ignored rather than cited".

Delamont (1989) conducted a qualitative study of citation patterns between schools of researchers studying social mobility in Britain. In her study she was able to demonstrate systematic neglect by each school of the work of the others. Her article is loaded with examples showing the failure of leading scholars to address the work of others. Delamont (1989) concludes that this, in fact, is the most striking feature on the literature on social mobility. One of her examples concerns a social mobility researcher named Goldthorpe:

> "Goldthorpe is a case in point. His theories about the class structure, the categories of occupational classification sociologists should use, and other related matters have been controversial for twenty years. Normally Goldthorpe does not deign to debate his position with his critics. Thus, Hopper (1981, p. 2) claims that the whole of the Oxford Mobility Project was a waste of public funds because the research team, particularly Goldthorpe, had failed to read, or perhaps to understand, Hopper's theories and research. To an outsider, this appears to be a fundamental criticism, yet Goldthorpe has not published a reply. Hopper's monograph does not even get cited in Goldthorpe (1987) […]. Hopper himself fails to articulate his own critique of Goldthorpe in his monograph, and then does not cite most of the other work on the topic compounding his own isolation. Goldthorpe may find Hopper's approach deeply flawed, but he has not explained his objections for the rest of us" (Delamont, 1989, p. 334).

Nicolaisen (2002a) tested a hypothesis put forward by Bornstein (1991, p. 139):

[93] See also section 1.2.2.1.

> "The relationship between research quality and citation frequency probably takes the form of a J-shaped curve, with exceedingly bad research cited more frequently than mediocre research".

The test was conducted using a sample of sociological monographs reviewed in the book-reviewing journal *Contemporary Sociology* between 1985-1994 as the test collection. The quality of each monograph was determined from searches in the database *Sociological Abstracts* (SA). Book reviews indexed in SA are provided with evaluative ratings. These ratings are given by the editors of SA who read the book reviews and assess the reviewers' general opinions of the books under review on a five point rating scale:

1. Very favourable
2. Favorable
3. Neutral
4. Unfavorable
5. Very unfavorable

The citation count of each monograph was determined from searches in SSCI. The sample consisted of 420 monographs. When correlating the evaluative ratings of these 420 monographs with their respective citation counts, Nicolaisen (2002a) found a statistically significant ($p<0.0185$) J-shaped distribution of citedness. Monographs that had been considered "very unfavorable" by reviewers were found to be cited more than the "unfavorable" ones, roughly as much as the "neutral" ones, a little less than the "favorable" ones, and much less than the "very favorable" ones. Nicolaisen (2002a, p. 392) argues that the J-shaped distribution may be caused by a skewed allocation of negative citations:

> "[The] J-shaped relationship between research quality and citation counts indicates that erroneous research is criticised and referred to negatively by some scholars in their publications and as a consequence is being more frequently cited than mediocre or ordinary research, which is mostly left unnoticed".

Thus, this empirical study supports Meadows' (1974, p. 45) claim that "if incorrect results stand in the way of the further development of a subject, or if they contradict work in which someone else has a vested interest, then it may become necessary to launch a frontal attack […]. Otherwise, it generally takes less time and energy to bypass erroneous material, and simply allow it to fade into obscurity". However, as noted by Feitelson & Yovel (2004), the J-shaped distribution of citedness demonstrates one of many problems associated with the interpretation of citations as a metric for quality.

In summary, nothing suggests that disagreement over ontological and/or methodological assumptions stimulates much debate and communication. On the contrary, communication is most likely to occur when researchers agree. Thus, one would expect references and citations to occur between researchers on the same "wavelength" as stated in the hypothesis above.

Learning is finding out what you already know (Richard Bach)[94].

5 Case studies

The previous chapter discussed the possible influence of two specific sociocultural factors on citation behavior. It concluded that specialties *and* research traditions probably are the primary structuring units in science, and suggested that these two seize a strong influence on the structural dynamics of citation networks. The present chapter seeks to test this suggestion empirically. The test is designed as a structural bibliometric analysis of two bibliographies: a psychological bibliography and a communication theoretical bibliography. The specific objective of the chapter is to conduct co-citation analyses of the two bibliographies in order to investigate their sub-structure. Co-citation analysis is based on the idea that it is possible to uncover the sub-structure of bibliographies by examining patterns of citations. When two or more documents, authors, or works are located in the same reference list they are said to be co-cited. Small (1973, p. 265) argues that:

> "Cocitation is a relationship which is established by the citing authors. In measuring cocitation strength, we measure the degree of relationship or association between papers as perceived by the population of citing authors".

Co-citation analyses of the two bibliographies in question are thus assumed to reveal the internal relationships within the two.

The psychological bibliography consists of 16 journals from four different research traditions. If research traditions really seize strong influence on the structural dynamics of citation networks, a co-citation analysis of the 16 psychological journals should reflect this: Journals representing the same research tradition should be found to be more related than journals representing different research traditions. The communication theoretical bibliography consists of documents citing 35 communication

---

[94] http://en.thinkexist.com/quotation/Learning_is_finding_out_what_you_already_know/223547.html. Visited August 24., 2004.

theorists. These 35 theorists have conducted work within one of three specialties from the perspective of one of seven research traditions. If specialties *and* research traditions really are the primary structuring units in science, and, as a result, seize strong influence on the structural dynamics of citation networks, a co-citation analysis of the 35 theorists should reflect this: Theorists working within the same specialty and/or research tradition should be found to be more related than theorists working in different specialties and research traditions.

5.1 The test bibliographies

5.1.1 The psychological test bibliography

The psychological test bibliography consists of articles from 16 psychological journals that cite articles in at least two of the same 16 journals. Robins, Gosling & Craik (1999) selected these 16 journals for their study of research trends in psychology. According to the authors, these journals represent (four by four) four different research traditions in psychology.

Hjørland (2002) conducted two related analyses of the same 16 journals. In the first, he retrieved papers on schizophrenia published in each of the 16 journals. In the second, he retrieved papers published in each of the 16 journals during 1999. Results of the two searches were ranked according to most cited work. Visual examination of the resulting outputs led Hjørland (2002, p. 265) to conclude that the general citation patterns from 1999 clearly indicated that scholars from different research traditions tend to use information that share the same basic view as their own. Although the search on schizophrenia was found to be less clear, Hjørland (2002, p. 265) concluded that it revealed the same basic tendency. Instead of Hjørland’s (2002) visual examination technique, I will make use of multivariate statistical techniques. Thus, to some extent, the co-citation analysis of the psychological test bibliography can be seen as an attempt to validate Hjørland’s (2002) results.

Scientific psychology is concerned with the explanation and prediction of behavior, thinking, emotions, relationships, potentials and pathologies (Wikipedia, 2004). It differs from sociology, anthropology, economics, and political science, in part, by studying the behavior of individuals (alone or in groups) rather than the behavior of the groups or aggregates (Wikipedia, 2004). According to Robins, Gosling & Craik (1999), psychologists typically work within one of the following four research traditions:

- Psychoanalysis
- Behaviorism
- Cognitive psychology
- Neuroscience

These four are different research traditions because they hold different ontological assumptions about psychological phenomena as well as different ideas about how to study them. For instance, the four research traditions understand human behavior and its causes in very different ways. In the 1890's Sigmund Freud invented and utilized a therapeutic method uncovering repressed wishes known as psychoanalysis. Psychoanalysts believe that behavior is caused by the unconscious and repressed. John B. Watson and later behaviorists believe that behavior is caused by specific environmental stimuli, and argue that the observation of behavior (stimulus and response) is the best or most convenient way of investigating psychological and mental processes. Cognitive psychologists argue instead that behavior should be seen as a function of an organism's mental processes. They consequently study cognition. Neoroscientists study the biology of the brain as they see behavior as a function of an organism's biology. Table 5.1. presents the four research traditions view of human behavior schematically.

Table 5.1. *Four research traditions in psychology.*

Psychoanalysis *Behavior seen as a function of an organism's unconscious mental processes*	Cognitive psychology *Behavior seen as a function of an organism's mental information processing*
Behaviorism *Behavior seen as a function of an organism's environment*	Neuroscience *Behavior seen as a function of an organism's biology*

The following sub-sections present a short description of the four research traditions[95]. The primary aim is to uncover the specific ontology and methodology underlying each tradition in order to expose their differences, possible similarities, and relevance criteria.

5.1.1.1 Psychoanalysis

The founder of the psychoanalytic research tradition was the Austrian doctor and neurologist Sigmund Freud (1856-1939). Freud viewed his research tradition as part of psychology, but held it to be different from other psychological research traditions as it dealt with the *unconscious*. This concept, the unconscious, is the most important concept in the psychoanalytic research tradition. Psychoanalysts believe that the unconscious is the most important determinant and motivator for human life and behavior. According to Freud, there are three levels in the human psyche:

- The conscious
- The preconscious
- The unconscious

Although human beings do not have direct access to the unconscious, psychoanalysts nevertheless hold that the unconscious determines our feelings, thoughts, and actions. Many episodes and experiences, unfulfilled desires and anxieties are believed to be located in the unconscious. They are often unconscious because the individual has repressed them because they were too painful or shameful. However, according to the psychoanalysts, the repressed material remains active and influences our conscious life. Sometimes the unconscious reveals itself in dreams or in slip of the tongues (Freudian slips). The unconscious may also reveal itself in free associations - the most significant technique in the evolution of psychoanalysis. In free associations the analysed person lies on a couch and is encouraged to talk openly and spontaneously, giving complete expression to every idea, no matter how embarrassing or painful it may seem. Psychoanalysts believe that there is nothing random about the material uncovered during such free associations.

The preconscious is believed to represent the psychic content which, presently, is not conscious, but which easily can be made so. Psychoanalysts usually make use of the iceberg metaphor to illustrate the proportion between the preconscious/conscious and the unconscious. Hence, psychoanalysts hold approximately 90 percent of human mental processes to be unconscious and only 10 percent to be preconscious/conscious.

---

[95] I am grateful to my supervisor, Research professor Birger Hjørland, for sharing with me his unpublished notes on three of the four research traditions in question.

Freud and the psychoanalysts believe that repressed material can cause psychiatric diseases. Such diseases may be healed if the burdensome and repressed elements are made conscious to the diseased. The purpose of Freud's psychoanalysis is thus to bring into conscious awareness repressed memories or thoughts, which are assumed to be the source of (abnormal) behavior. Psychoanalysts make use of a number of techniques in order to access the unconscious material. Free association is, as mentioned above, one important technique. Another is dream analysis. Freud believed that the essence of a dream is wish fulfillment. Moreover, he believed that dreams have both a manifest and a latent content. The manifest content of a dream is the actual story told in recalling the events occurring in the dream. The true significance of the dream, however, lies in the latent content; i.e., in the hidden or symbolic meaning of the dream. To interpret the hidden meaning, the therapist must proceed from manifest to latent content, interpreting the symbolic meaning from the events that the dreamer relates in the dream story. Dream analysis clearly reflects the strong interpretative element of psychoanalytic methodology. Though Freud himself viewed psychoanalysis as a natural science, the strong interpretative element of psychoanalysis is more in line with the perspectives of the humanities.

Psychoanalysis is not a homogeneous research tradition. Within it, there exist a number of different schools[96]. Although some of these schools are disagreeing to some extent, the general ontological assumption, that unconscious processes are essential causal forces, binds the different schools together and makes them psychoanalytic (Flanagan, 1984).

Critics of the psychoanalytic research tradition have raised a number of critiques. A key point of criticism concerns the difficulty of deriving empirically testable propositions of many of the psychoanalysts' hypotheses. Schultz & Schultz (1992, p. 452) ask, for instance:

> "How, for instance, would we test for the notion of a death wish? Psychoanalysts may use the idea to explain behavior such as suicide, after the fact, but how can it be studied in the laboratory?"

This, as we shall see in the next sub-section, is the typical behaviorist response to psychoanalytic theories.

[96] Andkjær Olsen & Køppe (1996) contains a good introduction in Danish to the different schools.

Table 5.2. *Leading psychoanalytic journals*
(Robins, Gosling & Craik (1999, p. 121).

Journal	Published since
International Journal of Psychoanalysis	1920
Psychoanalytic Quarterly	1932
Journal of the American Psychoanalytic Association	1953
Contemporary Psychoanalysis	1964

Robins, Gosling & Craik (1999, p. 121) identify four journals as leading journals from the psychoanalytic research tradition (table 5.2). All four journals are general psychoanalytic journals.

5.1.1.2 Behaviorism

Psychological behaviorism is a doctrine, or set of doctrines, about human and animal behavior. John B. Watson's article from 1913 entitled *Psychology as the Behaviorist views it* ignited the behaviorist revolution in psychology. Watson's article stresses the following characteristics and assumptions of the behavioral research tradition:

- Psychology is a pure objective, experimental science.
- Psychology belongs to the natural sciences.
- The theoretical goals of psychology are prediction and control of behavior.
- In principle, the behaviorist does not acknowledge a distinction between human beings and animals.
- Psychology can be conducted in terms of stimulus and response.
- Stimulus can be predicted from behavior, and behavior from stimulus.

Watson and his followers consequently hold that psychology should concern itself with the behavior of organisms – not with mental states or with constructing internal information processing accounts of behavior. Reference to mental events (e.g., beliefs and desires) is held to add nothing to what psychology can and should understand about the causes of behavior. Accordingly, mental events are private entities that do not form proper objects for empirical study. The behaviorists claim instead that human and animal behavior must be explained in terms of external physical stimuli, responses, learning histories, and reinforcements. To illustrate, consider a food-deprived rat at a choice point in a maze. If a particular movement, such as going left instead of right, is followed by the presentation of food, then the likelihood of the rat's turning left the next time it stands hungry at a similar choice point, is increased. Such presentations are reinforcements, such choice points are stimuli, such turning lefts are responses, and such trials or associations are learning histories. Thus, as declared by Tolman (1938, p. 34):

> "Everything important in psychology […] can be investigated in essence through the continued experimental and theoretical analysis of the determiners of rat behavior at a choice point in a maze".

From this follows that the basic task of psychological behaviorism is to specify types of association, understand how environmental events control behavior, discover the causal regularities that govern the formation of associations, and finally to predict how behavior will change with changes in the environment.

Like the psychoanalytic research tradition, the behaviorist research tradition is not a homogeneous one. Within it, there exist a number of different schools. The most radical of these are associated with the influential and controversial behaviorist B.F. Skinner (1904-1990)[97]. However, what unite these schools under the label *Behaviorism* are the shared ontological and methodological assumptions that the appropriate subject matter for psychology is observable behavior, and that the appropriate methods for psychology are those of the natural sciences (i.e., laboratory experiments).

Behaviorism has been heavily criticized. The most devastating critique has come from psychologists and others arguing that far from all behavior can be traced to physical reinforcement. These critics claim that behaviorists cannot adequately demonstrate that all mental events are indeed a product of the environment[98]. On the contrary, Gestalt psychology has demonstrated through its research that there is something internal, which manipulates stimuli input (e.g., memory, expectation, motivation, and attention). Moreover, experiments have shown that people can learn by observation and not just experience and reinforcement, which suggest that mental

---

[97] According to Skinner (1974, p. 213):

> "A person is first of all an organism, a member of a species and a subspecies, possessing a genetic endowment of anatomical and physiological characteristics, which are the product of the contingencies of survival to which the species has been exposed in the process of evolution. The organism becomes a person as it acquires a repertoire of behavior under the contingencies of reinforcement to which it is exposed in its lifetime. The behavior it exhibits at any moment is under the control of a current setting. It is able to acquire such a repertoire because of processes of conditioning, to which it is susceptible because of its genetic endowment".

[98] Chomsky (1959), for instance, refutes the behaviorist idea that language is a product of interaction with the environment.

processes are present before responses. Thus, according to Graham (2002), "the deepest and most complex reason for behaviorism's demise is its commitment to the thesis that behavior can be explained without reference to mental activity". Proponents of the three other research traditions find this thesis too restrictive, and rejects behaviorism largely because of it.

Robins, Gosling & Craik (1999, p. 121) identify four journals as leading journals from the behavioral research tradition (table 5.3). All four journals are general behavioral journals.

Table 5.3. *Leading behavioral journals*
(Robins, Gosling & Craik (1999, p. 121).

Journal	Published since
Journal of the Experimental Analysis of Behavior	1958
Behaviour Research and Therapy	1963
Journal of Applied Behavior Analysis	1968
Behaviour Therapy	1970

5.1.1.3 Cognitive psychology

Cognitive psychology is normally held to have been founded in 1956 with Jerome Bruner's book *A study of Thinking* and Noam Chomsky's *Logical Structure and Linguistic Theory*. Greenwood (1999), however, argues that it is more correct to view contemporary cognitive psychology as a return to the form of *structuralist* psychology practiced, for example, by Wilhelm Wundt (1832-1920) and Edward Titchener (1867-1927).

Contrary to the behavioral research tradition, the cognitive psychology research tradition centers its focus on mental processes. It studies how people perceive, learn, remember, and think. Its focus on *conscious* mental processes differentiates it from psychoanalysis.

The basic assumption of cognitive psychology is that behavior and thinking can best be understood in terms of representational structures in the mind and computational procedures that operate on those structures. Thagard (2002) argues that although there is much disagreement about the nature of the representations and computations that constitute thinking [and behavior], the basic hypothesis is general enough to encompass the different cognitive schools.

Most cognitive psychologists assume that the mind has mental representations analogous to computer data structures, and computational procedures similar to computational algorithms (Thagard, 2002). The computer (read: the mind) is considered to have three basic levels of description: the semantic level, the syntactic level, and mechanical level. The goals or objectives of the computer are described at the semantic level (e.g., solving a chess puzzle, computing a square root, reading a map, etc.). The computer's code or symbolic system is described at the syntactic level. The mechanical level concerns the nature of the computer's architecture (i.e., its physical constraints and capacities). Theories of cognitive information processing operate on the first two levels. Cognitive psychologists are engaged with studying the dynamics of the semantic level as governed by the logical rules and control mechanisms at the syntactic level.

Cognitive psychologists make use of a broad range of methods, including experiments, psychobiological techniques, self-reports, case studies, naturalistic observation, and computer simulations and artificial intelligence (Sternberg, 1996).

Critics of cognitive psychology (e.g., Dreyfus (1992) and Searle (1992)) claim that this approach is fundamentally mistaken. The basic assumption that human minds work by representation and computation is simply believed to be wrong. The critics have challenged cognitive psychology in a number of ways, e.g.:

- It neglects the important role of emotions in human thinking.
- It disregards the significant role of the environment (both physical and social) in human thinking.
- Human thoughts are essentially social in ways that cognitive science ignores.
- The mind is a dynamical system, not a computational system.

Thagard (1996) argues that these (and other) challenges can best be met by expanding and supplementing the computational-representational approach, not by abandoning it.

Robins, Gosling & Craik (1999, p. 121) identify four journals as leading journals from the cognitive psychology research tradition (table 5.4). All four journals are general cognitive psychology journals.

Table 5.4. *Leading cognitive psychology journals*
(Robins, Gosling & Craik (1999, p. 121).

Journal	Published since
Cognitive Psychology	1970
Cognition	1972
Memory and Cognition	1973
Journal of Experimental Psychology: Learning, … Memory, and cognition	1975

5.1.1.4 Neuroscience

The neuroscientific research tradition studies the structure, function, development, genetics, biochemistry, physiology, pharmacology and pathology of the nervous system. The study of behavior, memory, and learning is also part of neuroscience. The biological study of the human brain is an interdisciplinary field, which involves many levels of study, from the molecular level through the cellular level (individual neurons), the level of relatively small assemblies of neurons, the level of larger neurobiological subsystems, up to large systems, and at the highest level the nervous system as a whole. At this highest level, neuroscientific approaches combine with cognitive science to create cognitive neuroscience. Some researchers believe that cognitive neuroscience provides a bottom-up approach to understanding the mind. Others, like Fodor (1981) and Pylyshyn (1980), claim that certain generalizations, vital for the explanation of the human mind, are missed by neuroscience. That is because the categories of cognitive psychology theory will radically cross-classify the categories of neuroscientific theory. Thus, neuroscientific explanations will not explain the same things as cognitive psychology explain. Pylyshyn (1980) provides the following example to illustrate the irreducibility and autonomy of cognitive psychology:

> "Suppose someone comes to believe that there is a fire in her building and so dials the fire department on her telephone. Now coming to believe there is a fire in the building is coming to have a certain kind of representation, and whoever believes there is a fire in her building has the same representation. And if in consequence she dials the fire department, we understand the whys and wherefores of her behavior in the same way. However, underlying the commonality of representation may be a dismaying diversity of physiological states. Someone can come to believe the building is on fire in a variety of ways: she might smell smoke, see smoke, see flames, feel a hot wall, hear someone call "Fire", hear a smoke detector, and so on and – indefinitely – on. Each of these distinct ways of coming to have the representation "There is a fire in the building" will have a distinct physiological causal story, at least for the obvious reason that different sensory receptors are involved. In this sense, the categories of psychology cross-classify or are orthogonal to the categories of neuroscience.
>
> Because the neurophysiological realizations are distinct, the neurophysiological generalizations will not capture what is similar in each of these instances – to wit, that the person believed there was a fire in the

> building. The neurophysiological explanations of why the person dialed the number 911 will be *different* in each case, whereas the psychological explanation will do justice to the abstract – semantically coherent – similarity prevailing in each case and will explain the behavior by reference to the commonly held belief. In sum, certain generalizations, vital for the explanation of behavior, will be missed by neuroscience but will be captured by psychological theory" (cited from Churchland, 1986, p. 379).

The neuroscientists' reply to Pylyshyn (1980) goes like this:

> "If there really is a commonality of psychological state in the heads of those who come to believe there is a fire in the building, then there is every reason to expect that at *some appropriate level* of neurophysiological organization, this commonality corresponds to a common neurobiological configuration. After all, the heads all house human brains, and human brains have a common evolutionary history. They are not the product of entirely different designs in Silicon Valley and at Mitsubishi. It should be possible, therefore, to explain neurobiologically what is going on, unless the psychological level is indeterministic with respect to the relevant neurophysiological level. For that there is no evidence, nor do the antireductionists wish to embrace such an unlikely view" (Churchland, 1986, p. 381).

Consequently, while some cognitive psychologists claim that theories of the human mind and cognitive information processing must be based on studies of the dynamics of the mind's semantic level as governed by the logical rules and control mechanisms at the mind's syntactic level (see section 5.1.1.3), neuroscientists are engaged with the study of the mechanical level. They believe psychology can be reduced to neuroscience, and their goal is thus to explain and predict human behavior, thinking, emotions, relationships, potentials and pathologies from physiological studies of the human brain.

Table 5.5. *Leading neuroscientific journals*
(Robins, Gosling & Craik (1999, p. 121).

Journal	Year first published
Journal of Neurophysiology	1938
Annual Review of Neuroscience	1978
Trends in Neurosciences	1978
Journal of Neuroscience	1981

Robins, Gosling & Craik (1999, p. 121) identify four journals as leading neuroscientific journals (table 5.5). All four journals are general neuroscience journals.

5.1.1.5 Relevance criteria

The brief summary of the four psychological research traditions reveals some of their differences, similarities, and relevance criteria. It is clear that these four traditions subscribe to radically different ontological and methodological assumptions, which lead to mutual rejection. Behaviorists, for instance, reject psychoanalytic theories largely because they believe it is impossible to derive empirically testable propositions of many of the psychoanalysts' hypotheses; Neuroscientists, on the other hand, reject behavioristic theories largely because they believe that direct study of the brain's biology is the only way to understand behavior; Cognitive psychologists reject neuroscientific theories largely because they believe that there are empirical generalizations about mental states that can not be formulated in the vocabulary of neurological or physical theories, and so on.

Table 5.6. *Simplified relevance criteria in four psychological research traditions* (Hjørland, 2002, p. 263)

Psychoanalysis	Behaviorism	Cognitive psychology	Neuroscience
Relevant: Information about dreams, symbols, mental associations, personal meanings associated with stimuli, etc. Data collected in therapeutic sessions by trained therapists who can interpret the data (thus giving lower priority to intersubjective controlled information).	Relevant: Information about responses to specific kinds of stimuli. Kind or organism is of minor importance. (High priority to intersubjective controlled data). Non-relevant: Introspective data, data referring to mental concepts, experiences, or meanings of stimuli. (Information about brain processes).	Relevant: Information about mental information mechanisms and processing. Analogies between psychological and computer processes. Measures of channel capacities, etc.	Relevant: Information correlating brain processes or structures with forms of behavior or experience.

The different ontological and methodological assumptions of the four psychological research traditions bring about very different relevance assumptions or criteria. As pointed out by Hjørland (2002), these criteria are very different even when they work on the same problem. This leads to the hypothesis that researchers working within the four research traditions will use and cite information primarily consistent with their own ontological and methodological assumptions. Consequently, one expects to find that the four journals of each research tradition will cite each other much more than they will cite the 12 other journals of the psychological test bibliography. The simplified relevance criteria of the four research traditions are listed in table 5.6. The table is slightly modified from Hjørland (2002, p. 263).

5.1.2 The communication theoretical test bibliography

The communication theoretical test bibliography consists of papers co-citing at least two of the 35 communication theorists.

The concept of *communication* covers a multitude of meanings, and has been studied by many different sciences (e.g., philosophy, history, geography, psychology, sociology, ethnology, economics, political science, and biology (Mattelhart & Mattelhart, 1998)). Craig (1989) suggests, however, that communication theory is a coherent field when we understand communication as a practical discipline. This discipline covers a number of specialties and research traditions.

5.1.2.1 Specialties in communication theory

Griffin (2003) identifies a number of specialties in communication theory. Among these are:

- Interpersonal communication
- Group and public communication
- Mass communication

Scholars working within these three specialties are engaged with the study of many different phenomena relating to communication. Thus, it is not possible to give a detailed account of their work. However, broadly speaking, scholars working in the interpersonal communication specialty study interpersonal messages, relationship development and relationship maintenance. Scholars working in the group and public communication specialty study organizational communication and group decision-making. Scholars working in the mass communication specialty study how media affects culture (and vice versa).

5.1.2.2 Research traditions in communication theory

Craig (1999) identifies seven established research traditions of communication theory:

- Socio-psychological
- Cybernetic
- Rhetorical
- Semiotic
- Socio-cultural
- Critical
- Phenomenological

These seven research traditions hold different ontological assumptions about the nature of communication as well as different methodological convictions about how to study communication. Here follows a short introduction to each of the seven research traditions[99].

The socio-psychological research tradition sees communication as interpersonal influence. This tradition takes a natural science like experimental approach to the study of the effects of communication. Carl Hovland (1912-1961) was one of the founders of this perspective. His Yale team studied the relationships between communication stimuli, audience predisposition, and opinion change.

The cybernetic research tradition sees communication as information processing. It was Norbert Wiener (1894-1964) who coined the term *cybernetics*:

> "We have decided to call the entire field of control and communication theory, whether in the machine or in the animal, by the name of Cybernetics" (Wiener, 1948, p. 19).

Proponents of the cybernetics research tradition believe that communication problems are caused by breakdowns in the flow of information resulting, for instance, from noise or information overload. They offer a variety of methods for solving such problems including, among others, specialized information-processing technologies.

The rhetorical research tradition sees communication as persuasion. Proponents of this tradition emphasize and study how power and beauty of language move people emotionally and bring them to action. They challenge the beliefs that words are less important than actions, and that true knowledge is more than just a matter of opinion. Proponents of the rhetorical research tradition accept as true that people can become better communicators if they learn and practice the right methods of communication.

[99] In writing the short introductions I have benefited from consulting the works of Craig (1999), Griffin (2003), and Mattelhart & Mattelhart (1998).

They moreover believe that these methods can be invented or discovered through research, and that the methods can be systematically taught. According to Craig (1999), the rhetorical research tradition can be seen as a branch of the semiotic research tradition that studies the structure of language and argument that mediate between communicators and audiences.

The semiotic research tradition sees communication as the process of sharing meaning through signs. Proponents of this tradition are primarily concerned with problems of presentation, representation and transmission of meaning, and with bridging gaps between subjectivities by the use of shared systems of signs. They challenge, among other things, the beliefs that ideas exist in people's minds, that words have correct meanings, and that meaning can be made explicit. According to Craig (1999), semiotics may be thought of as a particular theory of rhetoric that studies the resources available for conveying meaning in rhetorical messages.

The socio-cultural research tradition sees communication as the creation and enactment of social reality. Communication is believed to produce and reproduce culture. Thus, the proponents argue, it is through language that reality is produced, maintained, repaired, and transformed. Communication problems are seen as cultural gaps that disable interaction by deleting the stock of shared patterns on which interaction depends. Craig (1999) points out that hybrids of sociocultural and other research traditions of communication theory are quite common. Thus, relatively "pure" exemplars of sociocultural communication theory may be hard to come by.

The critical research tradition sees communication as a reflective challenge of unjust discourse. It is inspired by the philosophers of the German Frankfurt School. The Frankfurt School performs an unorthodox variety of Marxist theory: Philosophers of this school reject Marx's economic determinism, but embrace the Marxist tradition of critiquing society. Critical theorists warn against blind reliance on the scientific method and uncritical acceptance of empirical findings.

The phenomenological research tradition sees communication as the experience of self and others through dialogue. Phenomenology refers to the intentional analysis of everyday life from the standpoint of the person who is living it. Thus, proponents of the phenomenological research tradition place great emphasis on people's perceptions and interpretations of their own subjective experiences. Proponents of the phenomenological research tradition argue that if we set aside the dualisms of mind and body, and subject and object, we will see that direct, unmediated contact with others are a very real and absolutely necessary human experience. Thus, phenomenology challenges the semiotic

Table 5.7. *Topoi for argumentation across seven research traditions in communication theory* (Craig, 1999, p. 134).

	Rhetorical	Semiotic	Phenomenological	Cybernetic	Socio-psychological	Socio-cultural	Critical
Against rhetoric	The art of rhetoric can be learned only by practice; theory merely distracts	We do not use signs; rather they use us	Strategic communication is inherently inauthentic & often counterproductive	Intervention in complex systems involves technical problems rhetoric fails to grasp	Rhetoric lacks good empirical evidence that its persuasive techniques actually work as intended	Rhetorical theory is culture bound & overemphasizes individual agency vs. social structure	Rhetoric reflects traditionalist, instrumentalist, & individualist ideologies
Against semiotics	All use of signs is rhetorical	Language is fiction; meaning & intersubjectivity are indeterminate	Language-parole & signifier-signified are false distinctions. Languaging constitutes world	"Meaning" consists of functional relationships within dynamic information systems	Semiotics fails to explain factors that influence the production & interpretation of messages	Sign systems aren't autonomous; they exist only in the shared practices of actual communities	Meaning is not fixed by a code; it is a site of social conflict
Against phenomenology	Authenticity is a dangerous myth; good communication must be artful, hence strategic	Self & other are Semiotically determined subject positions & exist only in/as signs	Others experience is not experienced directly but only as constituted in ego's consciousness	Phenomenological "experience" must occur in the brain as information processing	Phenomenological introspection falsely assumes self-awareness of cognitive processes	Intersubjectivity is produced by social processes that phenomenology fails to explain	Individual consciousness is socially constituted, thus ideologically distorted
Against cybernetics	Practical reason cannot (or should not) be reduced to formal calculation	Functionalist explanations ignore subtleties of sign systems	Functionalism fails to explain meaning as embodied, conscious experience	The observer must be included in the system, rendering it indeterminate	Cybernetics is too rationalistic; e.g., it underestimates the role of emotion	Cybernetic models fail to explain how meaning emerges in social interaction	Cybernetics reflects the dominance of instrumental reason
Against socio-psychology	Effects are situational and cannot be precisely predicted	Socio-psychological "effects" are internal properties of sign systems	The subject-object dichotomy of socio-psychology must be transcended	Communication involves circular causation, not linear causation	Socio-psychological theories have limited predictive power, even in laboratory	Socio-psychological "laws" are culture bound & biased by individualism	Socio-psychology reflects ideologies of individualism, instrumentalism
Against socio-cultural theory	Sociocultural rules, etc., are contexts & resources for rhetorical discourse	Socio-cultural rules, etc., are all systems of signs	The social life-world has a phenomenological foundation	The functional organization of any social system can be modelled formally	Socio-cultural theory is vague, untestable, ignores psychological processes that underlie all social order	Socio-cultural order is particular & locally negotiated but theory must be abstract & general	Sociocultural theory privileges consensus over conflict & change
Against critical theory	Practical reason is based in particular situations, not universal principles	There is nothing outside the text	Critique is immanent in every authentic encounter with tradition	Self-organizing systems models account for social conflict & change	Critical theory confuses facts & values, imposes a dogmatic ideology	Critical theory imposes an interpretative frame, fails to appreciate local meanings	Critical theory is elitist & without real influence on social change

notion that only signs can mediate intersubjective understanding. Yet, it shares with semiotics the assumption that what is fundamentally problematic in communication has to do with intersubjective understanding.

Table 5.7. replicates an identical table found in Craig's *Communication Theory as a Field* (1999, p. 134). The table indicates the distinctive critical objections that each research tradition typically raises against the others[100]. However, despite their differences, some of the seven research traditions are closer together in their basic assumptions. Griffin (2003, p. 33) charts the seven research traditions as equal area parcels of land that collectively make up the larger field of study (figure 5.1.).

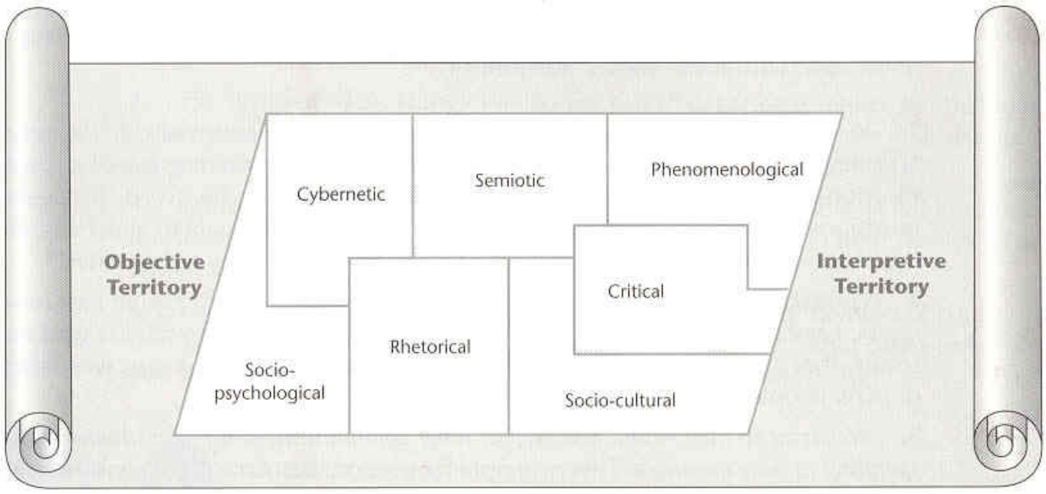

Figure 5.1. *Research traditions in the field of communication theory* (Griffin, 2003, p. 33)

The location of each tradition on the map is based on a distinction between "objective" and "interpretive" approaches to communication:

> "Objective theorists hold to a singular, independent, and autonomous social reality. The evidence speaks for itself. Conversely, interpretive scholars assume that reality is conferred status. Interpretation is a human accomplishment that creates data. Texts never interpret themselves.
>
> Objective scientists assume that there is a timeless and far-flung quality to social reality. They see major theoretical principles as ahistorical and not dependent on local conditions. According to interpretive scholars, this isn't so. Knowledge is always viewed from a particular standpoint. A word,

[100] The reflexive self-criticisms of each research tradition from its own standpoint are indicated in the diagonal cells from upper left to lower right.

> gesture, or act may have constancy within a given community, but it's dangerous to assume that interpretations can cross lines of time and space.
>
> As for language, objective theorists tend to treat words as referential and representational. Words have specific meaning. The interpretive scholar, however, is more suspicious of the sign" (Griffin, 2003, p. 509).

Griffin (2003) argues that scholars working in adjacent research traditions usually find it easier to appreciate each other's work. On the map they share a common border. Hence, for example, scholars working within the socio-cultural tradition find it easier to appreciate the work of their colleagues in the rhetorical, semiotic, and critical research tradition.

5.1.2.3 Communication theorists

Griffin (2003) presents a wide range of communication theories that reflect the diversity within the discipline. Each theory is associated with its founder(s). For example: George Herbert Mead's *symbolic interactionism*, Marshall Scott Poole's *adaptive structuration theory*, and Maxwell McCombs & Donald Shaw's *agenda-setting theory*. Griffin (2003) relates each theory to one of the three specialties and to one (or two) of the seven research traditions. It is thus possible to categorize the theorists according to the three specialties and seven research traditions. Table 5.8. illustrates the result of such categorization. When a theory has major roots in two traditions, Griffin (2003) notes the two, but points out which of the two he finds most influential. The brackets following some of the theorists listed in the table marks the minor influencing research tradition. The classification of the 35 communication theorists according to objective/interpretive worldview may be inferred from the table when compared to figure 5.1. However, the position of Paul Watzlawick, Karl Weick, and George Gerbner is somewhat inaccurate. Although the communication theories of both Paul Watzlawick and Karl Weick may be classified as belonging to the cybernetics research tradition, Griffin (2003, p. 510) argues that their theories reflect a more interpretive worldview. George Gerbner's *cultivation theory* is classified first as belonging to the socio-cultural research tradition. Griffin (2003, p. 510) argues, nevertheless, that it clearly reflect an objective worldview.

In chapter 4. it was argued that specialties *and* research traditions are the primary structuring units in science, and, as a result, seize strong influence on the structural dynamics of citation networks. To the extent that this is an accurate or roughly exact description of how science and scholarship work, a co-citation analysis of the 35 theorists should reflect this: Theorists working within the same specialty and/or

Table 5.8. *Categorization of communication theoreticians according to specialty and research tradition.*

	Socio-psychological [Sp]	Cybernetics [Cy]	Rhetorical [R]	Semiotic [S]	Socio-cultural [SC]	Critical [C]	Phenomenological [P]
Interpersonal communication	Judee Burgoon David Buller Jesse Delia [R] Irwin Altman Charles Berger Dalmas Taylor Muzafer Sherif Richard Petty John Cacioppo Leon Festinger	Paul Watzlawick			W. Barnet Pearce [P] Vernon Cronen [P] George Herbert Mead		Leslie Baxter Barbara Montgomery
Group and public communication	Randy Hirokawa [Cy] Dennis Gouran [Cy]	Karl Weick	Ernest Bormann [Sp] Walter Fisher Aristotle Kenneth Burke [S]		Marshall Scott Poole [Cy] Michael Pacanowsky Clifford Geertz	Stanley Deetz [P]	
Mass communication	Maxwell McCombs Donald Shaw Byron Reeves Clifford Nass			Roland Barthes	Marshall McLuhan George Gerbner [Sp]	Stuart Hall	

research tradition should be found to be more related than theoreticians working in different specialties and research traditions.

5.2 Test procedures

The test procedures are shown schematically in figure 5.2.

Selection of test bibliographies
↓
Retrieval of co-citation frequencies
↓
Compilation of raw co-citation matrices
↓
Transformation to correlation matrices
↓
Multivariate statistical analysis of correlation matrices
- Hierarchical cluster analysis
↓
Discussion and interpretation of findings

Figure 5.2. *Schematically representation of test procedures*.

The test procedures replicate partly the procedures used by White & Griffith (1981, 1982), McCain (1990), and Rowlands (1998).

The selection of the test bibliographies has already been discussed to some extent. However, the restrictions of the two bibliographies need to be addressed somewhat further.

In order to limit computer expenses, the psychological test bibliography has been restricted to include papers[101]:

a) published in the 16 journals between 1998-2002,
b) referring to papers[101] published in at least two of the same 16 journals

The communication theoretical test bibliography has been restricted to include papers[101]:

[101] Including all types of items (i.e., articles, book reviews, letters, etc.).

a) indexed in SSCI or A&HCI between 1974-2003,
b) referring to the oeuvres of at least two of the 35 communication theoreticians

5.2.1 Retrieval of co-citation frequencies

Retrieval of co-citation frequencies is accomplished by searching the three citation databases on DIALOG. However, the retrieval of co-citation frequencies for the two studies differs slightly. The specific retrieval procedures are therefore described in separate sub-sections.

5.2.1.1 Psychology

Using SCI and SSCI, it is relatively easy to retrieve the bibliographical records for the papers published in the 16 psychological journals under study. Limiting the search to papers published between 1998-2002 is also without complications. In SSCI on DIALOG, the request to retrieve papers published in *Journal of Applied Behavior Analysis* between 1998-2002 is:

SELECT PY=1998:2002 AND JN=JOURNAL OF APPLIED BEHAVIOR ANALYSIS

Expanding the request further to include papers from all 16 journals is easily accomplished by the OR command:

SELECT PY=1998:2002 AND (JN="1" OR JN="2" OR JN="3" … OR JN="16")

Limiting the retrieved set [S1] of papers to those referring to papers published in at least two of the 16 journals is a little more complicated. It is complicated because cited journals are indexed under various names in the citation databases. To give an example: *Journal of Neuroscience* is indexed as cited journal under these names:

J NERUOSCI
J NEUROCI
J NEUROSCI
J NEUROSCID
J NEUROSCIO
J NEUROSI
J NEUROSIC
J NEUSCI
J NEUROSCI 1

J NEUROSCI 2

To make sure that all variants are included in the search for papers referring to papers published in the 16 journals, one must first identify all variants. Scanning the relevant indexes of SCI and SSCI for variant names are consequently necessary. The different cited journal names of the 16 journals are listed in appendix 1. Once all variants are identified, it is relatively easy to retrieve sets of papers that refer to the journals in question. In the case of *Journal of Neuroscience*:

SELECT CR=J NERUOSCI OR CR=J NEUROCI OR CR=J NEUROSCI OR CR= J NEUROSCID OR CR=J NEUROSCIO OR CR=J NEUROSI OR CR=J NEUROSIC OR CR=J NEUSCI OR CR=J NEUROSCI 1 OR CR=J NEUROSCI 2

The retrieval of papers that co-cite papers from two of the 16 journals is a relatively easy task when such sets of cited journals are generated. In the following, let S2 be the set of papers citing papers from *Journal of Neuroscience* and S3 the set of papers citing papers from the journal *Cognition.* On DIALOG, the command to retrieve all papers co-citing papers from these two journals is:

SELECT S2 AND S3

Combining the resulting set [S4] with S1 above will retrieve the papers from the 16 journals that were published between 1998-2002 and have cited papers from both *Journal of Neuroscience* and *Cognition*:

SELECT S1 AND S4

The result of this final search [S5] is transferred to the raw citation matrix (see section 5.2.2).

5.2.1.2 Communication theory

It is relatively easy to retrieve all papers indexed in SSCI or A&HCI between 1974-2003 that refer to the oeuvres of at least two of the 35 communication theoreticians. The retrieval of a set of papers indexed in the two databases during the specified period is accomplished by the command:

SELECT UD=1974:2003

The resulting set [S6] will contain the bibliographic records of all items indexed in the two databases during the specified period. Limiting S6 to papers citing at least two of the 35 oeuvres is also relatively uncomplicated. To give an example: Papers co-citing the oeuvres of Judee Burgoon and Vernon Cronen is retrieved by the following command[102]:

SELECT CA=BURGOON J? AND CA=CRONEN V?

Combining the resulting set [S7] with S6, generates a set of papers, indexed in SSCI or A&HCI between 1974-2003, referring to the oeuvres of both Judee Burgoon and Vernon Cronen:

SELECT S6 AND S7

However, due to the fact that the resulting set [S8] is generated from two databases, the result needs to be cleaned for duplicates. This is accomplished by the following command:

RD S8

The result of this final search [S9] is transferred to the raw citation matrix (see section 5.2.2).

5.2.2 Compilation of raw co-citation matrices

After having retrieved all co-citation frequencies, these are transferred to raw co-citation matrices. Table 5.9. shows the upper left fraction of the psychological co-citation matrix. The table is arranged as a symmetrical matrix with identical ordered journal names on the rows and columns. The table shows the raw co-citation counts of some of the 16 journals. To give but a few examples: *International Journal of Psychoanalysis* and *Psychoanalytic Quarterly* is found to have been co-cited 612 times; *Psychoanalytic Quarterly* and *Contemporary Psychoanalysis* is found to have been co-cited 236 times;

[102] Using the truncation symbol (?) allows for the retrieval of citing papers whether they cite the theoreticians by first-name initial only or by those for additional names. White & McCain (1998) note that some erroneously individual high counts may result from this procedure, but argue that these are largely corrected when pairs are AND'ed.

Journal of the American Psychoanalytic Association and *Behaviour Research and Therapy* is found to have been co-cited 50 times[103].

The complete matrix including co-citation frequencies of all 16 journals are listed in appendix 2. Appendix 3. lists the raw co-citation frequencies of the 35 communication theoreticians.

Table 5.9. *A fraction of journal co-citation frequencies.*

	International Journal of Psychoanalysis	Psychoanalytic Quarterly	Journal of the American Psychoanalytic Association	Contemporary Psychoanalysis	Journal of the Experimental Analysis of Behavior	Behaviour Research and Therapy	Journal of Applied Behavior Analysis
International Journal of Psychoanalysis	•						
Psychoanalytic Quarterly	612	•					
Journal of the American Psychoanalytic Association	935	536	•				
Contemporary Psychoanalysis	338	236	270	•			
Journal of the Experimental Analysis of Behavior	0	0	0	0	•		
Behaviour Research and Therapy	69	21	50	7	94	•	
Journal of Applied Behavior Analysis	4	0	6	1	283	360	•

5.2.3 Generation of Pearson's product-moment correlation coefficient profiles

The next step is to convert the raw co-citation matrix to a matrix of proximity values, which indicate the relative similarity/dissimilarity of journal-pairs/theoretician-pairs.

[103] McCain (1991, p. 292) points out that the possible impact of varying journal size is eliminated when the raw co-citation frequencies are converted to Pearson's product-moment correlation coefficient profiles.

Most co-citation studies have used Pearson's product-moment correlation coefficient (Pearson's r) as the measure of similarity/dissimilarity[104]. Pearson's r functions as a measure, not just of how often pairs of journals and theoreticians are co-cited, but of how similar their co-citation profiles are.

Table 5.10. *Partial co-citation counts for two journal pairs.*

	International Journal of Psychoanalysis	Psychoanalytic Quarterly	International Journal of Psychoanalysis	Behaviour Research and Therapy
International Journal of Psychoanalysis	*missing*	612	*missing*	69
Psychoanalytic Quarterly	612	*missing*	612	21
Journal of the American Psychoanalytic … Association	935	536	935	50
Contemporary Psychoanalysis	338	236	338	7
Journal of the Experimental Analysis of … Behavior	0	0	0	94
Behaviour Research and Therapy	69	21	69	*missing*
Journal of Applied Behavior Analysis	4	0	4	360
Behaviour Therapy	27	7	27	2025
Cognitive Psychology	18	3	18	113
Cognition	35	6	35	80
Memory and Cognition	9	4	9	165
Journal of Experimental Psychology: … Learning, Memory, and cognition	20	11	20	192
Journal of Neurophysiology	10	5	10	83
Annual Review of Neuroscience	14	7	14	99
Trends in Neurosciences	22	11	22	106
Journal of Neuroscience	23	13	23	242
		r = 0,95		r = -0,24

To accomplish the generation of Pearson's r correlation coefficient profiles one need to decide what to put in the empty diagonal axes of the matrices. These cells represent the intersection of a particular journal or theoretician with it/him/herself (e.g. *International Journal of Psychoanalysis* - *International Journal of Psychoanalysis*). McCain (1990, p. 435) suggests to treat the diagonal cell values as missing data and to calculate the co-cited correlations accordingly. This technique is chosen for the generation of both similarity matrices.

104 Ahlgren, Jarneving & Rousseau (2003) have recently criticized the use of Pearson's r for this purpose. Their critique stimulated a number of co-citation analysts to write replies claiming that Pearson's r is a perfectly valid measure (White, 2003, 2004a) or that Ahlgren, Jarneving & Rousseau's (2003) objection is overly mathematical and, therefore, too axiomatic and deductive (Bensman, 2004).

Example: Table 5.10. shows the co-citation counts for two journal pairs: *International Journal of Psychoanalysis - Psychoanalytic Quarterly* and *International Journal of Psychoanalysis - Behaviour Research and Therapy.* The first pair of journals shows a high positive correlation (r = 0,95) not primarily because they are highly co-cited with each other, but because they tend to be co-cited frequently or infrequently with the same journals. For the second pair of journals the reverse is true (r = -0,24).

The correlation coefficients of all pairs of the 16 psychological journals are shown as a correlation matrix in appendix 4. Appendix 5. shows the correlation matrix containing the correlation coefficients of all pairs of the 35 communication theoreticians.

5.2.4 Hierarchical agglomerative cluster analysis

Hierarchical agglomerate cluster analysis is used as a tool for examining the sub-structure of the correlation matrices. This tool is frequently used in co-citation studies.

The most crucial step in any cluster analysis is to determine the number of clusters present in the results of a clustering study. Unfortunately, this fundamental step is among the as yet unsolved problems of cluster analysis. Everitt, Landau & Leese (2001) review a number of formal procedures, but conclude that there is very little consensus about which rule to apply. In practice, heuristic procedures are by far the most commonly used methods. In most studies, the hierarchical tree is simply cut by the subjective inspection of the different levels of the tree. Analysts typically choose a single cluster level for detailed analysis and then refer down to sub-clusters or up to macro-clusters where this is useful (McCain, 1990). However, as a practical rule of thumb, analysts are normally not recommended to form clusters containing less than ten percent of all entities[105].

Another vital choice has to do with the choice of clustering method. The hierarchical agglomerative cluster analyses of the two correlation matrices are performed using Ward's (1963) method. Ward's method is chosen because previous studies have found that it performs well on co-cited data (McCain, 1990)[106].

[105] Personal correspondence with Associate Professor Niels Ole Pors.

[106] Following McCain (1990) and Rowlands (1998) Ward's method is employed using a simple Euclidian measure of distance.

5.3 Results

Results of the hierarchical agglomerative cluster analyses are shown as so-called *dendograms*. A dendogram allows the analyst to trace backward or forward to any individual case or cluster at any level. The 0 to 25 scales along the top of the charts visualize the distance between cases or groups that are clustered at a particular step (0 representing no distance and 25 marking the greatest distance).

5.3.1 Psychology

Figure 5.3. shows the resulting dendogram. Table 5.11. translates the journal numbers.

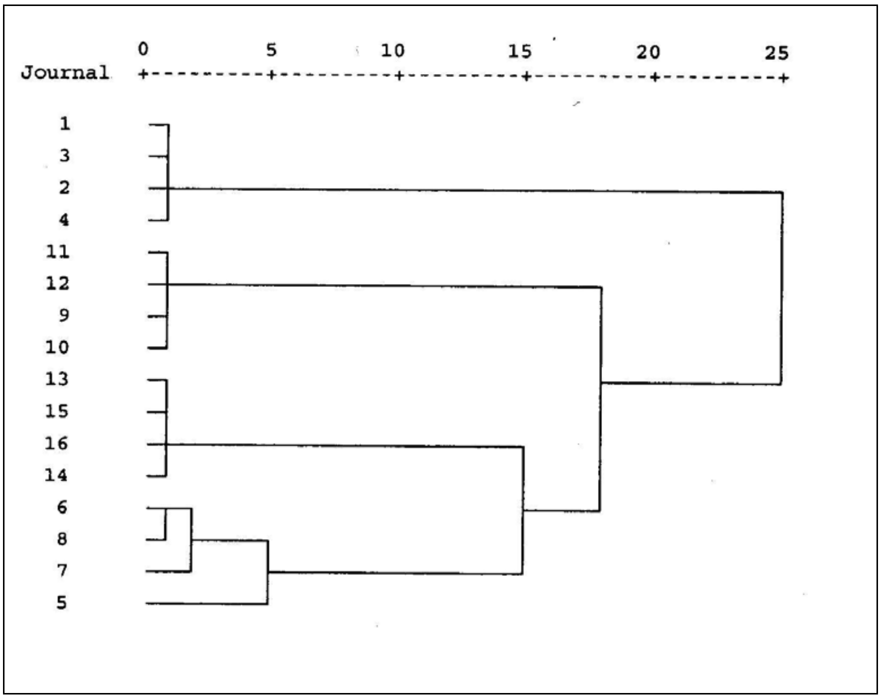

Figure 5.3. *Psychology dendogram*

Table 5.11. *Key to journals.*

1	*International Journal of Psychoanalysis*
2	*Psychoanalytic Quarterly*
3	*Journal of the American Psychoanalytic Association*
4	*Contemporary Psychoanalysis*
5	*Journal of the Experimental Analysis of Behavior*
6	*Behaviour Research and Therapy*
7	*Journal of Applied Behavior Analysis*
8	*Behaviour Therapy*
9	*Cognitive Psychology*
10	*Cognition*
11	*Memory and Cognition*
12	*Journal of Experimental Psychology: Learning, Memory, and cognition*
13	*Journal of Neurophysiology*
14	*Annual Review of Neuroscience*
15	*Trends in Neurosciences*
16	*Journal of Neuroscience*

5.3.2 Communication theory

Figure 5.4. shows the resulting dendogram. Table 5.12. translates the theoretician numbers.

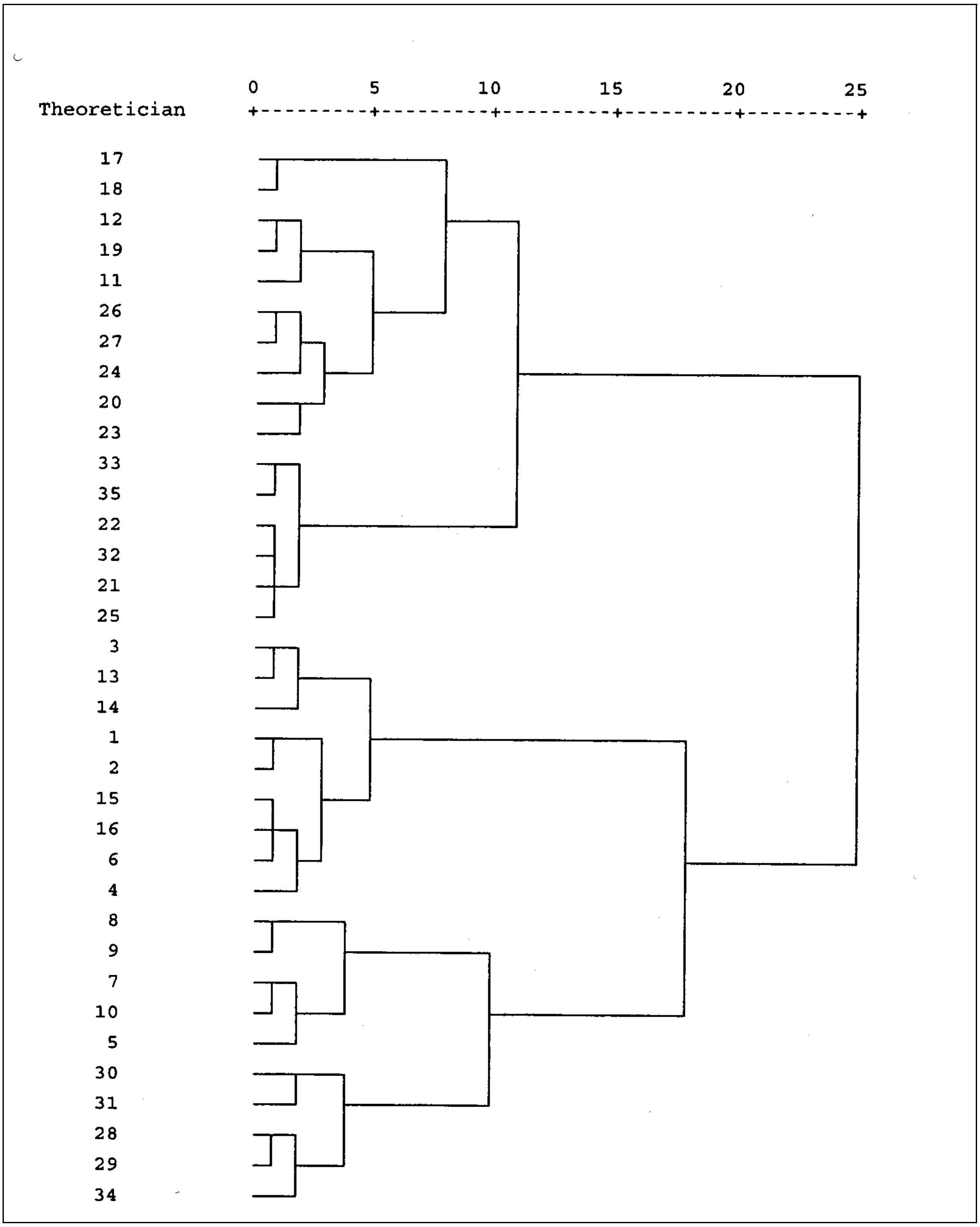

Figure 5.4. *Communication theory dendogram*

Table 5.12. *Key to theoreticians*.

1	Judee Burgoon	19	Karl Weick
2	David Buller	20	Ernest Bormann
3	Jesse Delia	21	Aristotle
4	Irwin Altman	22	Kenneth Burke
5	Dalmas Taylor	23	Walter Fisher
6	Charles Berger	24	Marshall Scott Poole
7	Muzafer Sherif	25	Clifford Geertz
8	Richard Petty	26	Michael Pacanowsky
9	John Cacioppo	27	Stanley Deetz
10	Leon Festiginger	28	Maxwell McCombs
11	Paul Watzlawick	29	Donald Shaw
12	George Herbert Mead	30	Byron Reeves
13	W. Barnet Pearce	31	Clifford Nass
14	Vernon Cronen	32	Roland Barthes
15	Leslie Baxter	33	Marshall McLuhan
16	Barbara Montgomery	34	George Gerbner
17	Randy Hirokawa	35	Stuart Hall
18	Dennis Gouran		

5.4 Discussion and interpretation

The results of the structural bibliometric analyses of the two bibliographies need careful interpretation.

5.4.1 Psychology

The 16 journals represent four different psychological research traditions. Thus, when interpreting the results of the psychology dendogram (figure 5.3.) it seems reasonable to start the discussion from the four-cluster level.

If the hierarchical tree is cut at the four-cluster level we find that the 16 journals cluster exactly as predicted. The four psychoanalytic journals cluster together; the four behavioral journals cluster together; the four cognitive journals cluster together; and the four neuroscientific journals cluster together. This result clearly suggests that scholars do tend to cite journals representing the same research tradition. Hence, the result demonstrates that research traditions, at least in the field of psychology, strongly influence the structural dynamics of citation networks.

Moving up through the hierarchical tree, we find that the behavioral and neuroscientific journals cluster together around the 15 mark on the distance scale. They are joined by the cognitive journals around the 18 mark. The psychoanalytic journals join in around the 25 mark. The distance between the behavioral journals and the neuroscientific is consequently found to be the shortest. Apparently, these two groups of journals are more alike than any other two-group combination. Section 5.1.1.4 discussed the idea of a possible connection between neuroscience and cognitive psychology. Some researchers believe that cognitive neuroscience provides a bottom-up approach to understanding the mind. Yet, the results do not lend much credence to that idea. Neuroscience seems to be closer to behavioral psychology. *Behavioral neuroscience* actually exists[107]. Proponents of this research tradition emphasizes the importance of understanding the brain and nervous system if psychologists are to understand behavior, thought and emotion.

That the psychoanalytic group of journals is found to be somewhat isolated from the three other groups comes as no surprise. The behavioral, cognitive, and neuroscience research traditions are keen on natural science-like methods with documented predictive power and mathematical precision. Psychoanalytic theories do not meet these methodological standards. In fact, psychoanalysis, by its very nature, is not even looking for such qualities (Schafer, 1976, p. 205). Moreover, the basic ontological assumption of psychoanalysis makes psychoanalysts pay special attention to unconscious mental processes. Proponents of the three other research traditions do not share the same belief in the unconscious being the most important determinant and motivator for human life and behavior.

Moving down the hierarchical tree to the five-cluster solution, we find that the behavioral journals split into two clusters. One contains three journals, the other only one. The resulting cluster formation suggests that *Journal of the Experimental Analysis of Behavior* is slightly different from the three other behavioral journals[108]. As noted in section 5.1.1.2, there exist a number of schools in the behaviorist research tradition. The split possibly reflects the existence of such schools within the behaviorist research tradition[109].

The structural bibliometric analysis of the psychological test bibliography validates Hjørland's (2002) results. The results clearly indicate that scholars from different

---

[107] See, for instance, the homepage of *The International Behavioral Neuroscience Society:* http://www.ibnshomepage.org/

[108] The resulting cluster formation breaks with the practical rule of thumb noted in section 5.2.4. The result must thus be valued carefully.

[109] Charles B. Ferster, who had earned his Ph.D. in 1950 at Columbia University and subsequently spent five years with B. F. Skinner at Harvard, founded *Journal of the Experimental Analysis of Behavior* in 1958.

psychological research traditions tend to use and cite information that share the same basic view.

5.4.2 Communication theory

Examination of the communication theory dendogram shows that one is going to violate the practical rule of thumb, if one generates solutions including more than five clusters. Thus, for practical reasons the discussion and interpretation of the results are initiated from the five-cluster level.

Cutting the hierarchical tree at the five-cluster level (see table 5.13.) we find that the 16 theoreticians that represent the interpersonal communication specialty split into three clusters (yellow, green, and blue); that the 11 theoreticians that represent the group and public communication specialty split into two clusters (blue and pink); and that the eight theoreticians that represent the mass communication specialty split into two clusters (pink and gray).

The green cluster contains theoreticians working in the same specialty and research tradition.

The gray and yellow clusters contain theoreticians working in the same specialties.

The pink cluster contains theoreticians working in two different specialties and four different research traditions. These theoreticians probably cluster together because their theories rest on somewhat comparable ontological and methodological assumptions. The theoreticians of the pink cluster represent four related research traditions (the rhetorical, semiotic, socio-cultural, and critical). Griffin's (2003) map of research traditions in the field of communication theory (figure 5.1.) reveals that scholars working within the socio-cultural tradition find it easier to appreciate the work of their colleagues in the rhetorical, semiotic, and critical research tradition. On the map they share common borders. The semiotic research tradition shares common borders with the rhetorical, the socio-cultural, and the critical research traditions. The rhetorical and the critical research traditions share common borders with the semiotic and the socio-cultural research traditions.

The blue cluster contains theoreticians working in two different specialties and five different research traditions. These theoreticians probably cluster together mainly because they are engaged with studies of the same or related phenomena. However, a closer inspection of the hierarchy of this cluster reveals three interesting sub-clusters:

1. Randy Hirokawa and Dennis Gouran

2. Ernest Bormann, Walter Fisher, Marshall Scott Poole, Michael Pacanowsky, and Stanley Deetz
3. Paul Watzlawick, Karl Weick, and George Herbert Mead

The first two sub-clusters fit the idea that theoreticians from the same or related research traditions will tend to cluster together. The third sub-cluster is a little more difficult to account for. Paul Watzlawick and Karl Weick represent two different specialties. However, both are working in the cybernetics research tradition. Yet, they cluster with George Herbert Mead from the socio-cultural tradition. Part of the explanation is probably that although the communication theories of both Paul Watzlawick and Karl Weick may be classified as belonging to the cybernetics research tradition, their theories reflect interpretive worldviews (Griffin, 2003, p. 510). This may account for their close connections with George Herbert Mead, as his *symbolic interactionism theory* also reflect an interpretive worldview (Griffin, 2003, p. 510).

If the hierarchical tree is cut at the four-cluster level, we find that the gray and the green clusters collapse into one cluster around the 10 mark on the distance scale. This collapse is quite logical since both clusters represent theoreticians from the socio-psychological research tradition[110].

If the hierarchical tree is cut at the three-cluster level, we find that the red and blue clusters collapse into one cluster around the 12 mark on the distance scale. It is interesting to see that the pink cluster do not collapse with the gray/green cluster. After all, half of the pink theoreticians represent the same specialty as the gray theoreticians. Yet, a number of the blue theoreticians represent the same or related research traditions as these three pink theoreticians. This probably strengthens the bonds between them, and explains the resulting cluster formation.

Finally, if the hierarchical tree is cut at the two-cluster level, we find that the gray/green cluster and the yellow cluster collapse into one cluster around the 17 mark on the distance scale. 90 percent of the gray/green theoreticians represent the socio-psychological research tradition. Bearing in mind that almost 67 percent of the green theoreticians represent the socio-psychological research tradition, this collapse appears quite logical.

In conclusion, the results indicate two things: Theoreticians from different specialties cluster together only if they represent the same or related research tradition.

---

[110] George Gerbner actually belongs to the socio-cultural research tradition, but Griffin (2003, p. A-17) notes that Gerbner's *cultivation theory* to some extent reflects the socio-psychological tradition as well. The strong objective worldview of Gerbner's theory (Griffin, 2003, p. 510) is in line with the worldview of the socio-psychological research tradition.

Table 5.13. *Five clusters of communication theoreticians.*

	Socio-psychological [Sp]	Cybernetics [Cy]	Rhetorical [R]	Semiotic [S]	Socio-cultural [SC]	Critical [C]	Phenomenological [P]
Interpersonal communication	Judee Burgoon David Buller Jesse Delia [R] Irwin Altman Charles Berger Dalmas Taylor Muzafer Sherif Richard Petty John Cacioppo Leon Festinger	Paul Watzlawick			W. Barnet Pearce [P] Vernon Cronen [P] George Herbert Mead		Leslie Baxter Barbara Montgomery
Group and public communication	Randy Hirokawa [Cy] Dennis Gouran [Cy]	Karl Weick	Ernest Bormann [Sp] Walter Fisher Aristotle Kenneth Burke [S]		Marshall Scott Poole [Cy] Michael Pacanowsky Clifford Geertz	Stanley Deetz [P]	
Mass communication	Maxwell McCombs Donald Shaw Byron Reeves Clifford Nass			Roland Barthes	Marshall McLuhan George Gerbner [Sp]	Stuart Hall	

Theorists representing conflicting research traditions cluster together only if they belong to the same specialty. These findings consequently confirm the hypothesis put forward in section 4.3.2.1.

Bibliometrics can study the actual use of different kinds of sources, but we need epistemological theories in order to interpret these patterns (Birger Hjørland, 1997, p. 126).

6 Summary, implications, recommendations & conclusion

This chapter discuss how citation based information science is affected by the findings reported earlier. Specifically, what are the implications for information seeking based on citation search strategies, research evaluation based on citation counts, and knowledge organization based on bibliographic coupling and co-citation analysis? Yet, before initiating these discussions we need to sum up the findings and consider their generalizability.

6.1 Summary

The dissertation sat out to answer two related questions, which have puzzled citation theorists for quite some time. Firstly, what makes authors cite their influences; and secondly, what makes authors cite some resources and not others?

Chapter 2. documented that neither the normative theory nor the social constructivist theory of citing are able to provide acceptable answers to the first question.

Actually, the social constructivist theory of citing does not attempt to answer the first question. On the contrary, social constructivists struggle to answer the opposite question of why authors *fail* to cite their influences. Social constructivists portray scientists as using citations as persuasion tools. They claim that scientists select citations largely based on the location of a cited publication's author within the stratification structure of science rather than on the worth or content of the publication itself. According to them, science is subjective and social, and the products of science and scholarship are thus depending on personal and social forces. The social constructivists confronts the idea that science and scholarship consist of a unique set of institutional arrangements, behavioral norms, and methods, which enable scientists and scholars to study Nature as

Nature really is. According to them, scientific knowledge is socially negotiated, not given by Nature. According to the social constructivists, it logically follows that what is true and false can never be judged or determined by referring to “natural phenomena”. Although scientific discussions may appear to reach closure by one part referring to “Nature”, scientific discussions are actually closed by something else. The social constructivists believe that scientific closure is the outcome of a negotiation process in which one part convinces the other by mere persuasion understood as misleading manipulation indistinguishable from commercial advertising. The persuasion hypothesis has two parts. The first part asserts that authors often misrepresent the works they allude to, twisting their meaning for their own ends (White, 2004). The second part asserts that authors disproportionately cite works by established authorities, so as to gain credibility by association (White, 2004). The second part of the hypothesis has been falsified by empirical counterevidence on at least three occasions (Moed & Garfield, 2003; White, 2004; Zuckerman, 1987). The first part of the hypothesis is more difficult to assess empirically. Yet, the bibliometric study of *Journal of Economic History* presented in section 2.2.3.1.1.1 does not lend much credence to the hypothesis that authors often misrepresent the works they allude to, twisting their meaning for their own ends. Thus, the social constructivists’ citation theory has been shown to be empirically unsupported.

The normative theory of citing is based on the assumption that science is a normative institution governed by internal rewards and sanctions. Scientists are believed to 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 worth as well as the cognitive, methodological, or topical content of the cited articles (Baldi, 1998). Proponents of this theory view science and scholarship to consist of a unique set of institutional arrangements, behavioral norms, and methods, which enable scientists and scholars to study Nature as Nature really is. According to this view, the products of science such as references and citations are independent of personal and social forces and are, consequently, beyond the realm of psychology and sociology. Citing scientists are believed to behave according to a shared set of norms that guarantees, among other things, that citation of a document implies use of that document by the citing author, that citation of a document reflects its merit, that citations are made to the best possible works, and that cited documents are related in content to the citing document (Smith, 1981). The normative theory of science shares its view of science being a normative or rule-governed activity with the Mertonian School of sociology and the philosophical schools of positivism and critical rationalism.

Early sociologists of science held that consensus in science are governed by a particular scientific ethos, i.e., by a set of rules that were supposed to establish trust in, and guarantee the reliability of, the knowledge created in the process. This ethos was given its most succinct and influential formulation by the American sociologist Robert King Merton (1910-2003). Merton held the norms of science to be binding on the man of science and was thus able to account for the consensual character of science. Because men of science share the same norms or standards, they are able to form stable patterns of consensus.

Despite the fact that positivists and critical rationalists disagree on what might be called *the rule of the game* (verification or falsification), both camps agree that science is a rule-governed activity. Or more precisely, both camps subscribe to the *Leibnizian ideal* that we may attain knowledge about Nature by invoking appropriate rules of evidence. Specifically, these philosophers argue that there are rules of scientific methodology, which are responsible for producing consensus in a rational community such as science. If scientists disagree about the validity of two rival theories, they need only consult the appropriate rules of evidence to see, which theory is better supported. Should those rules fail to decide the issue immediately, all they are required to do are to collect new and more discriminating evidence, which will differentially confirm or disconfirm one of the theories under consideration. Thus, both camps hold science to be a consensual activity because scientists (insofar as they are rational) shape their beliefs according to the canons of shared scientific methodology or logic.

Examining the works of recent citation analysts made it clear that no one seems to believe in the normative theory of citing anymore. Instead, a somewhat softer version pervades the writings of recent citation analysts. This version, which I have termed *the average mantra*, admits that citing authors far from always behave in accordance with the alleged norms of science. Proponents of the average mantra admit, for instance, that authors do not cite all works used during their research, that citation of a document not necessarily reflects its merit, that citations far from always are made to the best possible works, and that cited documents frequently are unrelated in content to the citing document. However, the proponents of the average mantra maintain that these biases and deficiencies are repaired to a tolerable degree when a sufficient large number of cited documents are being analyzed. There is ample evidence to suggest the soundness of the average mantra. A number of studies have demonstrated that citation analysis and peer judgment usually correlate to some degree, which makes it hard to disprove the legitimacy of citation analysis for a number of purposes. However, the assumption that "the biases and deficiencies of individual citers are repaired to a tolerable degree by the combined activity of the many" (White, 2001, p. 102), is not the same as the basic assumption of the normative theory of citing, which maintains that "authors generally abide by the norm of indicating their predecessors and sources" (Merton, 1995, p. 389).

Although the average mantra may be correct, chapter 2. argues that it is an insufficient theory of citing, as the average mantra offers no explanation for the causes of authors' actions.

Chapter 3. outlines a theoretical explanation for the average mantra. Inspired by research from the field of evolutionary biology, especially *the handicap principle* developed by the Israeli biologist Amotz Zahavi, it demonstrates that theories of honesty and deception in animal communication can contribute to our understanding of human citation behavior. The handicap principle suggests that if an individual is of high quality and its quality is not known, it 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. The chapter argues that references may be seen as threat signals similar to the common threat signals found in Nature (i.e., approaching a rival, stretching, and vocalization). Attacking a text full of references necessitates the weakening of the cited documents. Like the stretched body of an animal, the cited documents of a citing text are a sign of confidence. A stack of references is a handicap that only the honest author can afford. Like the beard of a thrown-out chin, it presents an open target for the bold opponent or rival. Modalized references expose themselves like the vocalization of a bluffing threatener. A skilled rival will detect the false sound right away and then know where to attack. The potential cost of making such a sound will often make the author reconsider his deceitful behavior. When references are made in public, the stakes are raised even further. The references may have witnesses. Yet, only a confident author can afford to "shout his threats before the crowd". Unconfident authors would usually not dare to risk the potential loss of reputation. However, in line with Zahavi, who do not claim that cheating is never encountered in nature, chapter 3. do not propose that all references are honest. It merely suggests that the handicap principle secures that citing authors credit their inspirations and sources in an honest way, to a tolerable degree - enough to save the scientific communication system from collapsing.

The handicap principle is a logical model expressed verbally. However, as noted by Zahavi (2003), such models are often rejected as being *intuitive*. Grafen (1990a, 1990b) formulated a mathematical model for the handicap principle, but even he concluded:

> "The handicap principle is a strategic principle, properly elucidated by game theory, but actually simple enough that no formal elucidation is really required" (Grafen, 1990b, p. 541).

Since Grafen's articles were published in 1990, the handicap principle has been generally accepted as a mechanism than can explain the evolution of the reliability of signals. Zahavi (2003) argues, however, that the conclusive evidence to support his suggestion, that the handicap principle is of use in the evolution of *all* communication systems, comes from chemical signals within the multicellular body, which he have shown to be loaded with handicaps as well (Zahavi, 1993; Zahavi & Zahavi, 1997).

Regarding the generalizability of the handicap principle in relation to human citation behavior: The level of honesty and deceit probably varies from one community to another. Authors in young and immature fields may thus be able to get away with a higher level of deceit than authors in established fields. An article on scientific fraud from the Economist (2002) suggests almost the same. The article in question reports the confirmation of the fraud committed by Dr J.H. Schön from the well-respected Bell Laboratories. Dr Schön was exposed to have fabricated the results of his 90 papers on organic semiconductors. The article concludes that there are at least three lessons to be learnt from this deceitful case. First of all, scientists are humans too, and some of them will do stupid and mendacious things. Secondly, nature is not to be cheated. The more significant a result is, the more it will be tested by that reality. And thirdly, if cheating, make sure to do it in a field, which is so insignificant that no one will bother to check. Whether such fields really exist remains to be seen. However, what the article does suggest is that the level of honesty and deceit varies from community to community. Still, the handicap principle makes sure the balance will never tip all the way to *the dark side*.

Chapter 4. provides an answer to the second research question. It reviews two accounts of how science and scholarship work. Both accounts explain why authors find certain sources attractive and others unattractive, and, ultimately, why authors cite some sources instead of others. The first account argues that science and scientific disciplines embrace numerous *research traditions*, which, in various ways, influence research and communication. The other account depicts scientific disciplines as mosaics of *specialties*, and holds that these influence research and communication. The chapter demonstrates that these two accounts contradict each other somehow. The first account claims that scholars tend to communicate with colleagues working within the same research tradition, but not necessarily within the same specialty. The second account claims, on the contrary, that scholars tend to communicate with colleagues working within the same specialty, but not necessarily within the same research tradition. However, the chapter argues that it should be possible to combine the two accounts. It suggests that specialties *and* research traditions are the primary structuring units of science, and that scholars communicate *within* and *between* specialties and research traditions. Specifically, it hypothesizes that if researchers from different specialties communicate with each other they will probably belong to the same or related research traditions, and if researchers from conflicting research traditions communicate with each other they will probably belong to the same specialty. If correct, this may contribute to explain why authors cite some resources and not others.

Chapter 5. sat out to test the hypotheses. The test was designed as a structural bibliometric analysis of two bibliographies: A psychological bibliography and a communication theoretical bibliography. Both tests confirmed the hypotheses. Thus, the conclusion, that authors tend to cite resources from their own specialty and/or research tradition, suggests itself. Yet, the generalizability of the conclusion may not be definite. Recall the philosophical discussions regarding the co-existence of competing conceptual frameworks. Kuhn (1962,1970) argues that in any mature discipline, every scientist will accept the same paradigm most of the time. Feyerabend (1970), Lakatos (1970, 1978), and Laudan (1977) challenge this view. They argue on the contrary that every major period in the history of science has been characterized by the co-existence of competing paradigms/research programmes/research traditions. If Kuhn is correct, the conclusion still holds. Yet, the part about authors tending to cite resources from their own research tradition becomes self-evident, as no other research tradition exists. If, on the other hand, Feyerabend, Lakatos, and Laudan are correct, the last part of the conclusion appears to be an important novelty in citation theory.

Kuhn (1970, viii) suggests that paradigms only materialize in the natural sciences:

> "I was struck by the number and extent of the overt disagreement between social scientists about the nature of legitimate scientific problems and methods. Both history and acquaintance made me doubt that practitioners of the natural sciences possess firmer or more permanent answers to such questions than their colleagues in social science. Yet, somehow, the practice of astronomy, physics, chemistry, or biology normally fails to evoke the controversies over fundamentals that today often seem endemic among, say, psychologists or sociologists. Attempting to discover the source of that difference led me to recognize the role in scientific research of what I have called "paradigms". These I take to be universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners".

Laudan (1977) clearly disagrees with Kuhn. In his book, he presents several examples of research traditions in the natural sciences as well as the social sciences (e.g., behaviorism in psychology and Marxism in economy (Laudan, 1977, p. 79)). Feyerabend (1970, p. 206) provides the following example to illustrate the co-existence of paradigms (in the natural sciences):

> "In the second third of the [19th] century there existed at least three different and mutually incompatible paradigms. They were: (1) *the mechanical point of view* which found expression in astronomy, in the kinetic theory, in the various mechanical models for electrodynamics as well as in the biological sciences, especially in medicine […]; (2) the point of view connected with the invention of an independent and phenomenological *theory of heat* which finally turned out to be inconsistent with mechanics; (3) the point of view implicit in Faraday's and Maxwell's *electrodynamics* which was developed, and freed from its mechanical concomitants, by Hertz.
>
> Now these different paradigms were far from being 'quasi-independent'. Quite the contrary, it was their *active interaction* which brought about the downfall of classical physics".

Thus, it seems safe to conclude, like others have done before (e.g., Dogan, 2001) that conceptual frameworks (paradigms, research programmes, research traditions) may co-exist and materialize in the natural sciences as well as the social sciences. Consequently, the right ingredients are in place. Authors from all disciplines may tend to cite resources from their own specialty and/or research tradition. The conclusion derived from the two case studies reported in chapter 5. may apply generally. Whether it does, must, of course, await further testing. Such tests are not easy to conduct. One needs to identify specialties and research traditions within a specific domain and find a way to measure their influence in bibliometric terms. Yet, it should be possible to test the conclusion or parts of it in other disciplines as well. Here are a few suggestions:

<u>Philosophy:</u> Philosophers are concerned with many questions. One of these concerns the question: *what is real?* To the extent that different philosophers give different answers, one could possibly argue that they belong to different research traditions. Niiniluoto (1999) analyzes how 20 philosophers have answered the question. He finds that it is possible to group these 20 philosophers in eight groups: *Critical realists, scepticists, methodological anti-realists, entity realists, instrumentalists, internal realists, pragmatists,* and *relativists.* For the conclusion to hold, a co-citation analysis involving the oeuvres of the same 20 philosophers should come to the same or comparable result.

Statistics: In statistics one could test the influence of research traditions on citation behavior by investigating the distribution of references in the specialty of statistical inference. Members of this specialty are concerned with the problem of inferring from a body of sample data to some feature of the underlying distribution from which the sample is drawn. According to Woodward (1998), there are two conflicting research traditions in this specialty:

> "The classical tradition derives from ideas of Ronald Fisher, Jerzy Neyman and Egon Pearson and embodies the standard treatments of hypothesis testing, confidence intervals and estimation found in many statistics textbooks. Classicists adopt a relative-frequency conception of probability and, except in special circumstances, eschew the assignment of probabilities to hypotheses, seeking instead a rationale for statistical inference in facts about the error characteristics of testing procedures. By contrast, the Bayesian tradition, so-called because of the central role it assigns to Bayes' theorem, adopts a subjective or degree-of-belief conception of probability and represents the upshot of a statistical inference as a claim about how probable a statistical hypothesis is in the light of the evidence".

If one could find a way to isolate the writings of the proponents of the classical tradition and the writings of the proponents of the Bayesian tradition, one could investigate the distribution of citations among and between the two camps.

Musicology: Abrahamsen (2004) exemplifies how two different research traditions in musicology influence the way music is defined, described, classified and indexed, and how they are part of an historical context. One could possibly extent Abrahamsen's analysis further to include the distribution of citations among and between proponents of the two.

Art scholarship: Ørom (2004) isolates and discusses three research traditions in art scholarship (the iconographic, the stylistic and the materialist). Again, if one could find a way to isolate the writings of the proponents of the three, one could conduct various co-citation analyses to test the generalizability of the conclusion, that authors tend to cite resources from their own specialty and/or research tradition.

6.2 Implications

6.2.1 Information seeking based on citation search strategies

Hjørland (1997, 1998) and Hjørland & Kyllesbech Nielsen (2001) discuss how to evaluate the relative strengths and weaknesses of term searching vis-à-vis citation or chain searching. I will use their discussions as basis for my own discussion of the dissertation's implications for citation based information seeking.

Hjørland (1997) argues that the goal of information seeking is to identify potential knowledge, data, information, or raw material – whatever one prefers to call it – that will contribute to the theoretical or empirical development of a field or to the solution of practical problems. He states that many librarians appear to hold the view that bibliographic searching is a more methodological process than citation searching. Yet, he claims to demonstrate that neither of these search strategies can be said to be more methodical than the other. Hjørland & Kyllesbech Nielsen (2001, p. 274) argue that the efficiency of citation searching is determined by how well documents identify and cite relevant information in their reference lists:

> "The method presupposes that the scientific literature in the field is neither unrelated to other research in the field nor simply redundant. In other words, it assumes that researchers are extremely conscientious in their literature searching and their referencing to relevant sources and that the references are selected with a view to informing the reader of important literature. It also presupposes that the scientist does not cite on purely formal or presentational grounds, for example. Most importantly, it presupposes that authors are not biased in selecting information but give even consideration to papers that argue both for and against their own view".

Thus, information seeking based on citation search strategies should perform well if:

1. Authors cite their influences and sources.
2. Authors are not biased in selecting information but give even consideration to papers that argue both for and against their own view.

Both of these presuppositions were dealt with earlier in this dissertation.

Chapter 2. presented the so-called *average mantra*. According to the average mantra, authors cite their influences and sources to a tolerable degree:

> "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 serious mistaken picture of the main influences in a particular field would be obtained when only one particular paper is used for this purpose. If one samples additional papers, they may all be subject to similar important irregularities in their reference lists. Papers on a closely related topic may not even share one reference in common. Would this imply that, even if one took a larger sample of papers in a specific field of science, one would never be able to get any sensible idea at all of what papers are more important in one sense or another than other papers for that specific field in a certain period of time? This would be the case if researchers refer (give citations) in a completely arbitrary way.
>
> However, 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.
>
> A more serious matter – and directly related with the discussion on the [MacRoberts & MacRoberts] paper – would be if authors would cite in very biased ways, for instance by systematically referring to particular papers which did not contribute at all to their papers, or by systematically excluding papers which were important for their paper. But even these types of biases need not be problematic, provided that large numbers of scientists do not share the same biases. By statistical means, one would still be able to estimate within certain bounds whether two (or more) papers are cited significantly different or not. So far, research has failed to show that biases in citation studies are extensive, and do not cancel each other" (Nederhof & Van Raan, 1987b, p. 326).

Chapter 3. gave a theoretical explanation for the average mantra. It was argued that the handicap principle secures that authors cite their inspirations and sources in an honest way, to a tolerable degree - enough to save the scientific communication system from collapsing. Thus, to the extent that the average mantra has general validity, the first presumption is honored.

The second presumption is wrong. Chapter 4. argued that communication has the best conditions in an environment of shared ontological and methodological assumptions, and that authors tend to ignore conflicting views rather than debating them. Chapter 5. backed up the argument with empirical data that illustrate how authors in two fields tend to cite the views that correspond to their own while ignoring those that do not.

The implications for information seeking based on citation search strategies are perceptible: The result of a citation search will mainly contain references, which the citing authors have found relevant (influenced by their worth as well as the cognitive, methodological, or topical content) for the solving of specific problems. These references reflect the citing authors' ontological and methodological views. Thus, they do not represent a balanced consideration of alternative views. In other words, citation search strategies are biased as citing authors usually do not give even consideration to works that argue both for and against their own views. A citation search should thus be preferred if one wants to search for additional information on a topic from the same view as a seed document. If one wants information on a topic from a number of different views, one should prefer a bibliographic search. To give an example: A search for the word "bulimia" in the basic index of PsycINFO on Dialog generates a set of 2420 records[111]. The set contains a number of papers from the 16 journals employed in the first test reported in chapter 5. It consequently contains at least four ontological and methodological views on the same phenomena or problem. This illustrates simultaneously the main problem associated with bibliographic searching: One is usually not able to limit one's search to papers representing a specific view on a subject. This is a problem because most databases contain papers from a variety of journals representing a diversity of research traditions.

6.2.2 Research evaluation based on citation counts

Cole & Cole (1973) discuss whether citation counts are a useful measure of the quality of scientific work. They find that the answer depends on how the concept of quality is defined. According to Cole & Cole (1973, p. 23-24), scientific quality may be defined in two different ways:

> "A traditional historian of science might apply a set of absolute criteria in assessing the quality of a scientific paper. Those papers which embody scientific truth and enable us to better understand empirical phenomena are high-quality papers. The fact that a particular set of works may be momentarily in fad or temporarily ignored tells us nothing about the quality

[111] July 27, 2004.

> of the work, if we use the absolute definition. Using this definition, the quality of work could only be measured in historical retrospect.
>
> Another way to conceptualize quality is to use a social as opposed to an absolute definition. The social definition is built upon the philosophical view that there is no absolute truth, and that truth is socially determined. Since what is believed to be true today may not be true tomorrow, few if any scientific discoveries will ever meet the absolute criteria. In the long run all discoveries will be seen as being in some fundamental aspect incorrect".

The Coles therefore conclude:

> "We define high-quality work as that which is currently thought useful by one's colleagues. […]. Current citations are not a measure of the absolute quality of work, they are an adequate measure of the quality of work socially defined" (Cole & Cole, 1973, p. 24).

Yet, the Coles mention a number of problems associated with assessing the quality of scientific works by citation analysis. One of these concerns the size of scientific fields. The Coles question if scientists from different fields who have produced work of roughly equal quality, will receive the same amount of citations. They conclude that the size of the fields probably influence the citation counts. Thus, Cole & Cole (1973, p. 28) recommend that "whenever a study contains scientists from more than one field the citation data should be statistically standardized separately for each field and standardized scores used in the analysis"[112].

Like most other citation analysts, the Coles do not discuss whether research traditions (or paradigms) influence the results of citation analyses. However, their argument regarding the possible problem of field size can easily be extended to research traditions. It logically follows that two research traditions of unequal size will produce disparate numbers of citations. Proponents of the bigger research tradition will tend to get more citations than proponents of the smaller research tradition[113]. Thus, we can easily rewrite the Coles' recommendation to say: Whenever a study contains scientists

[112] The Coles provide an example of such a score: "How could we say an outstanding physicist is a "better" scientist than, for example, an outstanding biochemist? The only way to make such a comparison would be to evaluate the relative position of the scientists in their respective fields. Thus the work produced by physicists or biochemists in the top 1 percent of their respective fields, as measured by citations, would be seen as roughly equal" (Cole & Cole, 1973, p. 28).

[113] See also Hjørland (1981, p. 184). Quoted in section 4.3.1.1.

from more than one research tradition the citation data should be statistically standardized separately for each tradition and standardized scores used in the analysis. To see why this is absolutely crucial we need only go back to Chapter 5. In section 5.1.1 I referred Hjørland's (2002) analysis of the most cited sources in 16 journals representing four different research traditions in psychology. Hjørland's findings clearly indicate that each tradition favor its own sources. The result of my empirical test (presented in section 5.3.1) supports Hjørland's findings. The four psychological research traditions vary tremendously in size. Robins, Gosling & Craik (1999) found remarkable differences in their empirical study of the development of the four psychological research traditions between 1950-1995. They found, for instance, that around 1971 the cognitive research tradition replaced behaviorism as the dominating research tradition in psychology. In 1995 the behavioral research tradition was found to be about the same size as the psychoanalytic and the neuroscientific traditions. The size of these two was found to have been quite small throughout the investigated period. Due to the large differences in size of the four psychological research traditions, one needs to make use of standardized scores if one is to undertake a citation analysis of psychology with the aim of finding the (socially defined) quality papers of that field. Otherwise one will falsely conclude that cognitive psychology is of high (socially defined) quality while the three other research traditions are inferior.

A related problem concerns the selection of literature for analysis. Most citation analysts use the SCI, SSCI, or A&HCI for their citation analyses. They are consequently basing their analyses on the literature indexed in these indexes. Garfield (1970) explains that journals covered by the citation indexes are chosen by advisory boards of experts. Apparently, their choice is highly selective. According to Carpenter & Narin (1981, p. 431), "the SCI is not trying to cover all the world's science […], but rather the significant, recognized, influential, mainstream science". MacRoberts & MacRoberts (1989a, p. 346) pose the question: "Since what is to be included is not a random sample but a selected group, does the selection process reflect the interests and scientific philosophy of the selectors, and in what ways?" They do not seek to answer the question themselves. To my knowledge, no one has yet conducted an empirical test in order to answer the question. Wiener (1974, p. 592, 1977, p. 177) suggests, however, that there are strong philosophical biases operating in the citation indexes:

> "The editors of the *Citation Index* made some curious choices about which journals to index and which to exclude. Among the 3,200 indexed journals selective coverage is given to such unlikely titles as *Mosquito News, Soap Cosmetics, Digestion*, and the *Tasmanian Journal of Agriculture*, but there

> is no coverage at all of journals like the *Review of Radical Political Economy, Radical America, Socialist Revolution, Telos, Insurgent Sociologist, Working Papers for a New Society*, or *Monthly Review*, to name a few. If a footnote to your work appears in *Commentary* or the *Public Interest*, you get counted; if it appears in *New Politics*, or *Social Policy*, or *Dissent*, you don't. Could it be that there is some logic behind these choices of the editors? Those who want their work to be indexed ought to ponder this question with particular care".

If Wiener is right, the citation indexes appear in many cases to be unsuitable tools for research evaluation.

6.2.3 Knowledge organization based on bibliographic coupling and co-citation analysis

Findings of the present dissertation suggest that research traditions *and* specialties are the primary structuring units of science, and that scholars communicate *within* and *between* specialties and research traditions. These findings have great implications for knowledge organization based on bibliographic coupling and co-citation analysis.

Citation analysts often conclude that bibliometric maps based on co-citation analysis or bibliographic coupling reflect the specialty structure of given fields. Section 4.3.1.1 demonstrates that for the most part citation analysts are led to this conclusion because of sampling biases. Most bibliometric maps are based on biased samples. As a result, they usually illustrate the bibliometric distribution of authors, journals, etc. belonging to just one or related research traditions. When such sampling biases are minimized, different pictures emerge. The two structural bibliometric analyses reported in chapter 5 demonstrate this. They illustrate that both specialties and research traditions seize a strong influence on the structural dynamics of citation networks. This insight is very important as it facilitates both the improvement of existing bibliometric mapping techniques as well as the improved interpretation and evaluation of previous studies.

To illustrate the latter, let us examine a recent Webmetrics article by Thelwall & Wilkinson (2004) entitled *Finding Similar Academic Web Sites with Links, Bibliometric Couplings and Colinks.* In this article the authors assess the extent to which links, co-links and bibliographically coupled links can be used to identify similar domains[114]. For their experiment, the authors chose a random sample of 500 domain pairs from the UK

[114] The authors use unique domain names as a proxy for academic Web sites.

academic Web. Their classification of similar and identical domains is described on page 519:

> "A pair of domains were classed as similar if they belonged to the same UK Research Assessment Exercise (RAE, http://www.rae.ac.uk) subject area and identical if they appeared to concern precisely the same topic, or were the main domains of organisations with the same or heavily overlapping RAE category subject coverage" (Thelwall & Wilkinson, 2004).

The authors found among other things that just 42 percent of the linked domain pairs were similar or identical; that only 22 percent of the bibliographically coupled domain pairs were similar or identical; and that a mere 10 percent of the co-linked domain pairs were similar or identical. The authors express overt surprise over these modest percentages and promise a follow-up paper that "will investigate this phenomenon in an attempt to provide an explanation" (Thelwall & Wilkinson, 2004, p. 524). The authors are evidently mystified. Thelwall & Wilkinson's (2004, p. 524) remark that the results may be caused by a special UK custom of link creation [sic!] and/or by technical problems. These factors may, of course, influence the result. However, the major cause of the low percentages reported by Thelwall & Wilkinson, 2004) is probably associated with the authors' naïve classification of similar and identical domains. According to Thelwall & Wilkinson (2004) a pair of domains is similar if and only if they belong to the same RAE subject area and identical if and only if they concern precisely the same topic. According to the RAE homepage[115], there are 69 RAE subject areas. Among these are the following:

- Anatomy
- Physiology

- Pharmacology
- Pharmacy

- Business and Management Studies
- Accounting and Finance

[115] http://www.hero.ac.uk/rae/overview/

- Pure Mathematics
- Applied Mathematics

In Thelwall & Wilkinson's study, domains classified as Anatomy and domains classified as Physiology are treated as dissimilar. The same goes for Pharmacology/Pharmacy, Business and Management Studies/Accounting and Finance, and Pure Mathematics/Applied Mathematics. Thus, whenever a link, a co-link or a bibliographic coupling appears between two domains from these pairs, it is treated as "a miss". Evidently, this is very problematic. Just because one domain is classified as e.g. Anatomy and another as Physiology it does not follow that the two domains are completely unlike. Practitioners from both fields may, on the contrary, subscribe to the same or related ontological and/or methodological views, and the two domains may thus be more similar than two other domains classified as belonging to the same RAE subject category (e.g. Psychology or Communication, Cultural and Media Studies). Consequently, it is almost certainly the naïve classification of domains rather than the actual links that is causing the "mysterious" results reported by Thelwall & Wilkinson (2004).

6.3 Recommendations

Ever since the first ISI citation index was launched in 1963, users and non-users have been arguing over the potentials of citation indexes. Specifically, the debaters have pondered whether citation analysis is an appropriate technique for information seeking, research evaluation, and knowledge organization. Much have been said and written about these issues. Editorials, letters and other commentaries have been published in journals from almost all fields. Entire journal issues have been devoted to the discussion. A search on any Web search engine results in thousands of hits when fed with words and phrases like "citation analysis", "co-citation", "bibliographic coupling", etc. The debating parties split in two groups, "disbelievers" and "believers", who offer very different recommendations regarding the use of citation analysis. According to the disbelievers, citation analysis is an invalid and/or worthless tool for information seeking, research evaluation and knowledge organization. In fact, the most radical of them dismiss citation analysis as simple "numerology" (e.g., Boone, 2004; Jennings, 1999; Walter et al., 2003). Thus, the disbelievers basically recommend abandoning citation analysis.

It is hard to find any truly radicals among the group of believers. The believers have often stressed that citation analysis is not a shortcut to be used as a substitute for thinking (e.g., Egghe & Rousseau, 1990, p. 226; Garfield, 1983, p. 371; Garfield, 1985, p. 408) and that citation analyses always should be augmented by subjective analyses (e.g., Garfield, 1997, p. 963). Thus, to use the words of Arunachalam (1998, p. 142), the believers see citation analysis as "an imperfect tool but which one could still use with some caveats to arrive at reasonable conclusions of different levels of validity and acceptability". The believers consequently recommend using citation analysis and "thinking" simultaneously. So far, a lot have been said and written about the citation analytical part of the tandem, but only very little or nothing about the "thinking" part. In other words, the believers have been quite reluctant to go into details explaining what should guide the analyst's thinking when conducting his or her citation analyses. Take, for instance, Egghe & Rousseau's *Introduction to Informetrics: Quantitative Methods in Library, Documentation and Information Science* (1990). This book provides a splendid account for the technical side of citation analysis, but leaves the reader practically to him or herself regarding the reading and interpretation of citation data. This is actually quite shocking as the authors begin their book stating that:

> "We expect this book to be of help to the informetrics teacher in organising his or her course and to be interesting and useful both as a course book and as a background reading for students in library and information science" (Egghe & Rousseau, 1990, p. v).

Egghe & Rousseau's introduction to informetrics (including citation analysis) is not sufficient background reading for LIS students and others wishing to be able to master the art of citation analysis. Something is missing. The book does not address how science and scholarship work. Instead, the reader is left with a number of tools for counting and calculating. Thus, the reader is basically not imparted knowledge that enables him or her to infer conclusions from data.

The present dissertation has demonstrated that in order to be able to conduct reliable analyses, citation analysts need knowledge about social behavior and scientific practice. Specifically, it has provided a theoretical explanation for authors' citation behavior, which help to explain why authors cite their inspirations and sources to a tolerable degree. Moreover, it has provided empirical support for the hypothesis that specialties and research traditions are the primary structuring units of science by demonstrating that references and citations typically occur between researchers on the same ontological and methodological level. Without this knowledge it would have been impossible to understand and explain the results of the empirical analyses reported in chapter 5. Bibliometrics teachers must therefore recognize the importance of such knowledge. A course in bibliometrics should not only dwell on technical issues, but also introduce the students to subjects like science history, philosophy and sociology of science, and epistemology. Topics like scientific problem solving, scholarly communication, genre analysis, and disciplinary cultures should form part of the bibliometric curriculum. Citation analysts must raise their eyes and look beyond their own narrow field, as there is every reason to believe that research from related disciplines such as anthropology, composition studies, cultural studies, philosophy, and sociology can strengthen their work. The bibliometric society needs to acknowledge the importance of these issues. Only then may citation analysis reveal itself to be a legitimate tool for information seeking, research evaluation and knowledge organization.

6.4 Conclusion

Fischerisms is the title of an amusing book that contains various quotations collected and published by students attending the courses of Dr. Martin H. Fischer, a professor of physiology at the University of Cincinnati. The book was first published in 1930. The following "fischerism" is found in the third and enlarged edition (Marr, 1944, p. 3):

> "A conclusion is the place where you got tired thinking".

Whether the "you" refers to the author or the reader is an open question. Perhaps Fischer meant both? Yet, we need to stay awake a little bit longer as most guides to academic writing (and reading) urge us to pay considerable attention to the conclusion.

Moxley (1992, p. 69) in his *Publish, don't Perish: The Scholars Guide to Academic Writing and Publishing* advice the author, who wants to draft a memorable conclusion, to consider the following questions:

1. Did you pose a question in the introduction that can now be answered? Is there a way of extending a metaphor that was presented in the introduction?

2. What are the broad implications of your work? What recommendations can you make based on the material you have presented?

3. Would it be appropriate for you to speculate on what will happen next?

4. What do you want readers to do once they have reviewed your document? Should they agree with you about the validity of an argument or theory? Should they change their teaching practices? Should they pour their creative energies into examining an innovative research question?

I have decided to follow Moxley's advice. I will therefore attempt to answer his four questions in turn:

1. The introduction posed two related research questions. These were answered in section 6.1.

2. The implications and recommendations of the dissertation were discussed in section 6.2 and 6.3.

3. What will happen next? That is difficult to say. Perhaps someone will read the dissertation and find that it explains how citations reflect social behavior and scientific practice. Hopefully someone will be motivated to conduct research on the same topics. For, as stated already on page one of the introduction, a great deal remains to be said on all the matters addressed. But the study of these matters is a cooperative venture of a community of minds.

   Further research is needed on whether or not the level of honesty and deceit varies from research community to research community. Such research would contribute to disclose the generalizability of the handicap principle, and thus to a better understanding of human citation behavior. Further research is also needed on how specialties and research traditions affect communication, references and citation networks. Specifically, the hypotheses that - (1) if researchers from different specialties communicate with each other they will probably belong to the same or related research traditions, and (2) if researchers from conflicting research traditions communicate with each other they will probably belong to the same specialty - need further testing. A few suggestions for such tests are outlined in the end of section 6.1.

4. What would I like readers to do once they have read the dissertation? Preferably, readers will recognize the need for new perspectives in citation theory. This recognition will make them receptive for new ideas. They will realize that I am on the right track, and ride with me towards new horizons in citation theory. Together we will revolutionize bibliometrics. We will change the traditional bibliometric curriculum, and introduce our students to epistemology, science history, philosophy and sociology of science. The younger, educated generation will take over, and their improved citation analyses will finally gain respect from the global scientific community. Is that too much to ask?

References

Abrahamsen, K.T. (2003). Indexing of musical genres: An epistemological perspective. *Knowledge Organization*, 30(3/4): 144-169.

Adams, E.S. & Caldwell, R.L. (1990). Deceptive communication in asymmetric fights of the stomatopod crustacean Gonodactylus bredini. *Animal Behaviour*, 39: 706-716.

Ahlgren, P., Jarneving, B. & Rousseau, R. (2003). Requirements for a cocitation similarity measure, with special reference to Pearson's correlation coefficient. *Journal of the American Society for Information Science and Technology*, 54(6): 550-560.

Andersen, H. (1999). Political attitudes and cognitive convictions among Danish social science researchers. *Scientometrics*, 46(1): 87-108.

Andkjær Olsen, O. & Køppe S. (1996). *Psykoanalysen efter Freud 1-2*. Copenhagen, DK: Nordisk Forlag.

Arnold, S.J. (1983). Sexual selection: The interface of theory and empiricism. In: Bateson, P. (ed.), *Mate Choice.* Cambridge, UK: Cambridge University Press: 67-108.

Arunachalam, S. (1998). Citation analysis: Do we need a theory?. *Scientometrics*, 43: 141-142.

Ayer, A.J. (1936). *Language, Truth and Logic.* London, UK: Gollancz.

Baird, L.M. & Oppenheim, C. (1994). Do citations matter?. *Journal of Information Science*, 20: 2-15.

Baldi, S. (1997). *A Network Approach to the Analysis of Citation Flows: A Comparative Study of two Research Areas in the Natural and the Social Sciences.* Columbus, OH: Ohio State University. Ph.D. Thesis.

Baldi, S. (1998). Normative versus social constructivist processes in the allocation of citations: A network-analytic model. *American Sociological Review*, 63: 829-846.

Barber, B. (1952). *Science and the Social Order.* New York, NY: Collier Books.

Barber, B. (1961). Resistance by scientists to scientific discovery. *Science*, 134: 596-602.

Barnes, B. (1982). *T.S. Kuhn and Social Science.* New York, NY: Columbia University Press.

Barnes, B., Bloor, D. & Henry, J. (1996). *Scientific Knowledge: A Sociological Analysis.* Chicago, IL: University of Chicago Press.

Barnes, B. & Shapin, S. (1994). *Natural Order: Historical Studies of Scientific Culture*. London, UK: Sage.

Bavelas, J.B. (1978). The social psychology of citations. *Canadian Psychological Review*, 19: 158-163.

Bazerman, C. (1988). *Shaping Written Knowledge: The Genre and Activity of the Experimental Article in Science*. Madison, WI: University of Wisconsin Press.

Belnap, N. & Steel, T. (1976). *The Logic of Questions and Answers*. New Haven, CT: Yale University Press.

Bensman, S.J. (2004). Pearson's r and author cocitation analysis: A commentary on the controversy. *Journal of the American Society for Information Science and Technology*, 55(10): 935.

Bird, R.B., Smith, E.A. & Bird, D.W. (2001). The hunting handicap: Costly signaling in human male foraging strategies. *Behavioral Ecology and Sociobiology,* 50: 9–19.

Birner, J. & Ege, R. (1999). Two views on social stability: An unsettled question. *American Journal of Economics and Sociology*, 58(4): 749-780.

Boone, T. (2004). Journal impact factor: A critical review. *Professionalization of Exercise Physiology Online*, 7(1). [Available at http://www.css.edu/users/tboone2/asep/journalIMPACTfactor.html]. Visited August 7., 2004.

Borlund, P. (2000). *Evaluation of Interactive Information Retrieval Systems*. Åbo, FI: Åbo Akademi University Press. Ph.D. Thesis.

Borlund, P. (2003). The concept of relevance in IR. *Journal of the American Society for Information Science and Technology,* 54(10): 913-925.

Bornstein, R.F. (1991). The predictive validity of peer review: A neglected issue. *Behavioral and Brain Sciences*, 14(1): 138-139.

Brannigan, A. (1981). *The Social Basis of Scientific Discoveries*. Cambridge, UK: Cambridge University Press.

Broadus R.N. (1983). An investigation of the validity of bibliographic citations. *Journal of the American Society for Information Science,* 34: 132-135.

Brooks, T.A. (1985). Private acts and public objects: An investigation of citer motivations. *Journal of the American Society for Information Science,* 36(4): 223-229.

Brooks, T.A. (1986). Evidence of complex citer motivations. *Journal of the American Society for Information Science*, 37(1): 34-36.

Caldwell, R.L. & Dingle, H. (1975). The ecology and evolution of agonostic behavior in stomatopods. *Naturwissenschaften*, 62: 214-220.

Cano, V. (1989). Citation behavior: Classification, utility, and location. *Journal of the American Society for Information Science*, 40(4): 284-290.

Carpenter, K.J. (1994). Hard truth to swallow. *Nature*, 372: 329.

Carpenter, M.P. & Narin, F. The adequacy of the *Science* Citation *Index* (SCI) as an indicator of international scientific activity. *Journal of the American Society for Information Science*, 32(6): 430-439.

Carter, B. & Skates, C. (1990). The *Rinehart Handbook for Writers*. Fort Worth, TX: Holt, Rinehart and Winston.

Case, D.O. & Higgins, G.M. (2000). How can we investigate citation behavior?: A study of reasons for citing literature in communication. *Journal of the American Society for Information Science*, 51(4): 635-645.

Cawkell, A.E. (1971). Science Citation Index: Effectiveness in locating articles in the anaesthetics field: 'Perturbation of ion transport'. *British Journal of Anaesthesia*, 43: 814.

Cawkell, A.E. (1974). Search strategy, construction and use of citation networks, with a socio-scientific example: "Amorphous semi-conductors and S.R. Ovshinsky". *Journal of the American Society for Information Science*, 25: 123-130.

Cawkell, A.E. (1976). Understanding science by analysing its literature. *The Information Scientist*, 10(1): 3-10.

Chein, I. (1981). Appendix: An introduction to sampling. In: Kidder, L.H. et al. (eds.), *Research Methods in Social Relations*. New York, NY: Holt, Rinehart, & Winston: 418-441.

Chomsky, N. (1959). Review of Verbal Behavior [book review]. *Language*, 35: 26-58.

Chubin, D.E. (1976). The conceptualization of scientific specialties. *Sociological Quarterly*, 17: 448-476.

Chubin, D.E. & Moitra, S.D. (1975). Content analysis of references adjunct or alternative to citation counting?. *Social Studies of Science*, 5: 426-441.

Churchland, P.S. (1986). *Neurophilosophy: Toward a Unified Science of the Mind/Brain*. Cambridge, MA: MIT Press.

Clutton-Brock, T.H., Guinness, F.E. & Albon, S.D. (1982). *Red Deer: The Behaviour and Ecology of Two Sexes*. Chicago, IL: University of Chicago Press.

Cole, J.R. & Cole, S. (1972). The Ortega hypothesis. *Science*, 178(4059): 368-375.

Cole, J.R. & Cole, S. (1973). *Social Stratification in Science.* Chicago, IL: University of Chicago Press.

Cole, J.R. & Cole, S. (1973). Citation analysis. *Science*, 183 (4120): 28-33.

Collin, F. (1997). *Social Reality*. London, UK: Routledge.

Collin, F. (2003). *Konstruktivisme*. Frederiksberg, DK: Roskilde Universitetsforlag.

Collingwood, R.G. (1940). *Metaphysics*. Oxford, UK: The Clarendon Press.

Collins, H.M. (1974). The TEA set: Tacit knowledge and scientific networks. *Science Studies*, 4: 165-186.

Collins, H.M. (1981a). Son of seven sexes: The social destruction of a physical phenomenon. *Social Studies of Science*, 11: 33-62.

Collins, H.M. (1981b). Stages in the empirical programme of relativism. *Social Studies of Science*, 11: 3-10.

Collins, H.M. (1982). Knowledge, norms, and rules in the sociology of science. *Social Studies of Science*, 12: 299-309.

Collins, H.M. (1983). The sociology of scientific knowledge: Studies of contemporary science. *Annual Review of Sociology*, 9: 265-285.

Collins, H.M. (1986). *Changing Order: Replication and Induction in Scientific Practice*. London, UK: Sage.

Collins, H.M. (2000). Surviving closure: Post-rejection adaptation and plurality in science. *American Sociological Review*, 65(6): 824-845.

Collins, H.M. & Pinch, T. (1994). *The Golem: What Everyone Should Know About Science*. London, UK: Cambridge University Press.

Cozzens, S.E. (1981). Taking the measure of science: A review of citation theories. *International Society for the Sociology of Knowledge Newsletter*, 7(1-2): 16-20.

Cozzens, S.E. (1989). What do citations count?. The rhetoric-first model. *Scientometrics*, 15(5-6): 437-447.

Craig, R.T. (1989). Communication as a practical discipline. In: Dervin, B. et al. (eds.), *Rethinking Communication: Vol. 1. Paradigm Issues*. Newbury Park, CA: SAGE Publications: 97-122.

Craig, R.T. (1998). Communication theory as a field. *Communication Theory*, 9: 119-161.

Crane, D. & Small, H. (1992). American sociology since the seventies: The emerging identity crisis in the discipline. In: Halliday, T.C. & Janowitz, M. (eds.), *Sociology and its Publics: The forms and fates of disciplinary organization*. Chicagi, IL: University of Chicago Press: 197-234.

Cronin, B. (1981). The need for a theory of citation. *Journal of Documentation,* 37: 16-24.

Cronin, B. (1984). *The Citation Process: The Role and Significance of Citations in Scientific Communication*. London, UK: Taylor Graham.

Cronin, B., Snyder, H. & Atkins, H. (1997). Comparative citation rankings of authors in monographic and journal literature: A study of sociology. *Journal of Documentation*, 53(3): 263-273.

Crowther, J.G. (1940). *British Scientists of the Nineteenth Century*. Harmondsworth, UK: Penguin Books.

Davis, G.F.W. & O'Donald, P. (1976). Sexual selection for a handicap: A critical analysis of Zahavi's model. *Journal of Theoretical Biology*, 57: 345-354.

Dawkins, R. & Krebs, J.R. (1978). Animal signals: Information or manipulation?. In: Krebs, J.R. & Davies, N.B. (eds.), *Behavioural Ecology*. Oxford, UK: Blackwell Scientific Publishers.

Delamont, S. (1989). Citation and social mobility research: Self defeating behaviour?. *Sociological Review*, 37: 332-337.

De Mey, M. (1977). The cognitive viewpoint: Its development and its scope. In: De Mey, M. et al. (eds.), *International Workshop on the Cognitive Viewpoint*. Ghent, NL: University of Ghent: xvi-xxxii.

De Lacey, G., Record, C. & Wade, J. (1985). How accurate are quotations and references in medical journals?. *British Medical Journal,* 291: 884-886.

Devitt, M. (1991). Naturalistic representation: A review article on David Papineau's Reality and Representation. *British Journal of the Philosophy of Science*, 42: 425-443.

Devitt, M. (1998). Reference. In: Edward, C. (ed.), *Routledge Encyclopedia of Philosophy* [compact disc]. London, UK: Routledge.

Diodato, V. (1984). Impact and scholarliness in arts and humanities book reviews: A citation analysis. *Proceedings of the 47th ASIS Annual Meeting*: 217–220.

Dogan, M. (2001). Specialization and recombination of specialties. In Smelser, N.J. & Baltes, P.B. (eds.), *Encyclopedia of Social and Behavioral Sciences*. London, UK: Pergamon-Elsevier Science: 14851-14855.

Dominey, W.J. (1980). Female mimicry in male bluegill sunfish: A genetic polymorphism?. *Nature*, 284: 546-548.

Downes, S.M. (1998). Constructivism. In: Edward, C. (ed.), *Routledge Encyclopedia of Philosophy* [compact disc]. London, UK: Routledge.

Dretske, F. (1981). *Knowledge and the Flow of Information*. Cambridge, MA: MIT Press.

Dreyfus, H.L. (1992). *What Computers Still Can't Do*. 3rd edition. Cambridge, MA: MIT Press.

Duff, M.J. (2003). *The Theory Formerly Known as Strings*. [Available at http://feynman.physics.lsa.umich.edu/~mduff/talks/1998%20-%20The%20Theory%20Formerly%20Known%20as%20Strings.pdf]. Visited May 19., 2004.

Economist (2002). Outside the Bell curve. *The Economist*, September 26. [Available at http://www.economist.com/displaystory.cfm?story_id=1352850]. Visited March 26., 2004.

Edge, D. (1979). Quantitative measures of communication in science: A critical Review. *History of Science*, 17: 102-134.

Egghe, L. & Rousseau, R. (1990). *Introduction to Informetrics: Quantitative Methods in Library, Documentation and Information Science*. Amsterdam, NL: Elsevier.

Everitt, B.S., Landau, S. & Leese, M. (2001). *Cluster Analysis*. 4th edition. London, UK: Arnold.

Ewer, R.F. (1968). *Ethology of Mammals*. London, UK: Logos Press.

Faigley, L. (1986). Competing theories of process. *College English*, 48: 527-542.

Feitelson, D.G. & Yovel, U. (2004). Predictive ranking of computer scientists using CiteSeer data. *Journal of Documentation*, 60(1): 44-61.

Feyerabend, P.K. (1970). Consolations for the specialist. In Lakatos, I. & Musgrave, A. (eds.), *Criticism and the Growth of Knowledge*. Cambridge, UK: Cambridge University Press: 197-230.

Fjordback Søndergaard, T., Andersen, J. & Hjørland, B. (2003). Documents and the communication of scientific and scholarly information: Revising and updating the UNISIST-model. *Journal of Documentation*, 59(3): 278-320.

Flanagan, O. (1984). *The Science of the Mind*. Cambridge, MA: A Bradford Book.

Fodor, J.A. (1981). *Representations*. Cambridge, MA: MIT Press.

Foskett, D.J. (1972). A note on the concept of „relevance“. *Information Storage and Retrieval*, 8: 77-78.

Frankfort-Nachmias, C. & Nachmias, D. (1996). *Research Methods in the Social Sciences*. London, UK: Arnold.

Friedman, M. (1998). Logical positivism. In: Edward, C. (ed.), *Routledge Encyclopedia of Philosophy* [compact disc]. London, UK: Routledge.

Frohmann, B. (1990). Rules of indexing: A critique of mentalism in information retrieval theory. *Journal of Documentation*, 46(2): 81-101.

Frohmann, B. (1992a). Knowledge and power in library and information science: Toward a discourse analysis of the cognitive viewpoint. In: Vakkari, P. & Cronin, B. (eds.), *Conceptions of Library and Information Science: Historical Empirical and Theoretical Perspectives: Proceedings of the International Conference Held for the Celebration of the 20th Anniversary of the Department of Information Studies, University of Tampere; 1991*. London, UK: Taylor Graham Publishing: 135-147.

Frohmann, B. (1992b). The power of images: A discourse analysis of the cognitive viewpoint. *Journal of Documentation*, 48(4): 365-386.

Fuller, S. (2000). *Thomas Kuhn: A Philosophical Story of Our Times*. Chicago, IL: University of Chicago Press.

Gadamer, H.G. (1960). *Truth and Method*. New York, NY: Seabury Press.

Garber, D. (1998). Leibniz, Gottfried Wilhelm (1646-1716). In: Edward, C. (ed.), *Routledge Encyclopedia of Philosophy* [compact disc]. London, UK: Routledge.

Garfield E. (1955). Citation indexes for science: A new dimension in documentation through association of ideas. *Science*, 122(3159): 108-111.

Garfield, E. (1965). Can citation indexing be automated?. In: Stevens, M.E., Giuliano, V.E. & Helprin, L.B. (eds.), *Statistical Association Methods for Mechanized Documentation, Symposium Proceedings*. Washington, DC: National Bureau of Standards: 189-192.

Garfield, E. (1970). Citation indexing for studying science. *Nature*, 227(5259): 669-671.

Garfield, E. (1975). The 'obliteration phenomenon' in science: And the advantage of being obliterated!. *Current Contents*, 51/52: 5-7.

Garfield, E. (1977a). To cite or not to cite: A note of annoyance. *Current Contents*, 35(August 29): 5–8.

Garfield, E. (1977b). Using the SCI® to avoid unwitting duplication of research. *Essays of an Information Scientist*, 1: 219-221.

Garfield, E. (1978a). The 100 articles most cited by social scientists, 1969-1977. *Current Contents*, 32(August 7): 5-14.

Garfield, E. (1978b). High impact science and the case of Arthur Jensen. *Current Contents,* 41(October 9): 652-662.

Garfield, E. (1979). *Citation Indexing: Its theory and Applications in Science, Technology and Humanities*. New York, NY: Wiley.

Garfield, E. (1983). How to use citation analysis for faculty evaluations, and when is it relevant?: Part 2. *Essays of an Information Scientist*, 6: 363-372.

Garfield, E. (1985). Uses and misuses of citation frequency. *Essays of an Information Scientist*, 8: 403-409.

Garfield, E. (1989). Of hot papers and "critical" acclaim. *The Scientist*, 3(4): 10.

Garfield, E. (1990). Journal editors awaken to the impact of citation errors: How we control them at ISI. *Essays of an Information Scientist*, 13: 367-375.

Garfield, E. (1997). Validation of citation analysis. *Journal of the American Society for Information Science*, 48(10): 962.

Garfield, E. (1998a). From citation indexes to informetrics: Is the tail now wagging the dog?. *Libri*, 48(2): 67-80.

Garfield, E. (1998b). On the shoulders of giants. Paper presented at the *Conference on the History and Heritage of Science Information Systems*. Pittsburgh, PA: October 24.

Garfield, E., Malin, M.V. & Small, H. (1978). Citation data as science indicators. In: Elkana, Y. et al. (ed.), *Toward a Metric of Science: The Advent of Science Indicators.* New York, NY: Wiley: 179-207.

Garfield, E. & Welljams-Dorof, A. (1990). The consequences of a fraudulent scientist on his innocent coinvestigators. *Journal of the American Medical Association*, 264(24): 3145-3146.

Garvey, W.D. & Griffith, B.C. (1972). Communication and information processing within scientific disciplines: Empirical findings for psychology. *Information Storage and Retrieval*, 8: 123-136.

Gibbs, R.W. (1987a). Mutual knowledge and the psychology of conversational inference. *Journal of Pragmatics,* 11: 561-88.

Gibbs, R.W. (1987b). The relevance of Relevance for psychological theory. *Behavioral and Brain Sciences*, 10(4): 718-719.

Gieryn, T.F. (1978). Problem retention and problem change in science. *Sociological Inquiry*, 48: 96-115.

Gieryn, T.F. (1982). Relativist/Constructivist programmes in the sociology of science: Redundance and retreat. *Social Studies of Science*, 12: 279-297.

Gilbert, G.N. (1977). Referencing as persuasion. *Social Studies of Science*, 7: 113-122.

Gilbert, G.N. & Mulkay, M. (1984). *Opening Pandora's Box: A Sociological Analysis of Scientist's Discourse*. London, UK: Cambridge University Press.

Gilbert, G.N. & Woolgar, S. (1974). The quantitative study of science. *Science Studies,* 4: 279 - 294.

Godfrey-Smith, P. (1989). Misinformation. *Canadian Journal of Philosophy*, 19(4): 533-550.

Godfrey-Smith, P. (1992). Indication and adaptation. *Synthese*, 92: 283-312.

Goldthorpe, J. (1987). *Social Mobility and Class Structure in Modern Britain*. Oxford, UK: Clarendon.

Gottfredson, S.D. (1978). Evaluating psychological research reports: Dimensions, reliability, and correlates of quality judgements. *American Psychologist*, 33: 920-939.

Grafen, A. (1990a). Sexual selection unhandicapped by the Fisher process. *Journal of Theoretical Biology*, 144: 473-516.

Grafen, A. (1990b). Biological signals as handicaps. *Journal of Theoretical Biology*, 144: 517-546.

Grafton, A. (1997). *The Footnote: A Curious History*. Cambridge, MA: Harvard University Press.

Graham, G. (2002). Behaviorism. *The Stanford Encyclopedia of Philosophy.* [Available at http://plato.stanford.edu/entries/behaviorism/]. Visited June 28, 2004.

Greenwood, J.D. (1999). Understanding the "cognitive revolution" in psychology. *Journal of the Behavioral Sciences*, 35(1): 1-22.

Griffin, E. (2003). *A First Look at Communication Theory*. 5th edition. Boston, MA: McGraw-Hill.

Griffith, B.C. (1980). *Key Papers in Information Science*. White Plains, NY: Knowledge Industry Publications.

Gross, M.R. & Charnov, E.L. (1980). Alternative male life histories in bluegill sunfish. *Proceedings of the National Academy of Sciences U.S.A*, 77: 6937-6940.

Gross, P.L.K. (1927). Fundamental science and war. *Science,* 66: 640-645.

Gross, P.L.K. & Gross, E.M. (1927). College libraries and chemical education. *Science*, 66: 385-389.

Hagstrom, W.O. (1965). *The Scientific Community*. New York, NY: Basic Books Inc.

Hagstrom, W.O. (1970). Factors related to the use of different modes of publishing research in four scientific fields. In: Nelsen, C.E. & Pollock, D.K. (eds.), *Communication Among Scientists and Engineers*. Lexington, MA: Lexington Books: 85-124.

Hagstrom, W.O. (1974). Competition in science. *American Sociological Review*, 39(1): 1-18.

Hairston, M. & Ruszkiewicz, J.J. (1988). *The Scott, Foresman Handbook for Writers.* Glenview, IL: Scott, Foresman.

Harter, S.P. (1992). Psychological relevance and information science. *Journal of the American Society for Information Science*, 43(9): 602-615.

Harter, S.P., Nisonger, T.E. & Weng, A. (1993). Semantic relationships between cited and citing articles in library and information science journals. *Journal of the American Society for Information Science*, 44(9): 543-552.

Hawkes, K. & Bird, R.B. (2002). Showing off, handicap signals, and the evolution of men's work. *Evolutionary Anthropology*, 11: 58-67.

Hertzel, D. (1987). Bibliometrics, history of the development of ideas in. In Kent, A. (ed), *Encyclopedia of Library and Information Science*. New York, NY: Marcel Dekker: 144-219.

Hicks, D. (1999). The difficulty of achieving full coverage of international social science literature and the bibliometric consequences. *Scientometrics*, 44(2): 193-215.

Hjørland, B. (1981). Bibliometriske analyser i psykologien. *Nordisk Psykologi*, 33(3): 176-190.

Hjørland, B. (1991). Det kognitive paradigme i biblioteks- og informationsvidenskaben. *Biblioteksarbejde*, 33: 5-37.

Hjørland, B. (1992). The concept of “subject” in information science. *Journal of Documentation*, 48(2): 172-200.

Hjørland, B. (1993). *Emnerepræsentation og Informationssøgning: Bidrag til en Teori på Kundskabsteoretisk Grundlag*. Göteborg, SE: Centrum för Biblioteks- och informationsvetenskap.

Hjørland, B. (1997). *Information Seeking and Subject Representation: An Activity-Theoretical Approach to Information Science*. Westport, CT: Greenwood Press.

Hjørland, B. (1998). Information retrieval, text composition, and semantics. *Knowledge Organization*, 25(1/2): 16-31.

Hjørland, B. (2000a). Relevance research: The missing perspective(s): “Non-relevance” and “epistemological relevance”. *Journal of the American Society for Information Science*, 51(2): 209-211.

Hjørland, B. (2000b), *A Problematic Understanding of Relevance*. [Available at http://www.amazon.com/exec/obidos/tg/detail/-/0631198784/103-4664838-4415840?v=glance]. Visited February 2, 2004.

Hjørland, B. (2002). Epistemology and the socio-cognitive perspective in information science. *Journal of the American Society of Information Science and Technology*, 53(4): 257-270.

Hjørland, B. (2003). Fundamentals of knowledge organization. *Knowledge Organization*, 30(2): 87-111.

Hjørland, B. (2004). Arguments for Philosophical Realism in Library and information Science. *Library Trends*, 52(3): 488-506.

Hjørland, B. & Kyllesbech Nielsen, L. (2001). Subject access points in electronic retrieval. *Annual Review of Information Science and technology*, 35: 249-298.

Hjørland, B. & Sejer Christensen, F. (2002). Work tasks and socio-cognitive relevance: A specific example. *Journal of the American Society for Information Science and Technology,* 53(11): 960-965.

Hopper, E. (1981). *Social Mobility*. London, UK: Blackwell.

Hyland, K. (2000). *Disciplinary Discourses: Social Interactions in Academic Writing*. Harlow, UK: Pearson Education Ltd.

Ingwersen, P. (1982). Search procedures in the library analysed from the cognitive point of view. *Journal of Documentation*, 38(3): 165-191.

Ingwersen, P. (1992). *Information Retrieval Interaction*. London, UK: Taylor Graham Publishing.

Ingwersen, P. (1999). Cognitive Information Retrieval. *Annual Review of Information Science and Technology*, 34: 3-52.

Jacob, E.K. & Shaw, D. (1998). Sociocognitive perspectives on representation. *Annual Review of Information Science and Technology*, 33: 131-185.

Jennings, C. (1999). Citation data: The wrong impact?. *Neuroindocrinology Letters*, 20: 7-10.

Johnstone, R.A. (1995). Sexual selection, honest advertisement and the handicap principle: Reviewing the evidence. *Biological Reviews,* 70: 1-65.

Kaplan, N. (1965). The norms of citation behavior: Prolegomena to the footnote. *American Documentation*, 16(3): 179–184.

Kessler, M.M. (1963). Bibliographic coupling between scientific papers. *American Documentation*, 14: 10-25.

Kirkpatrick, M. (1986). The handicap mechanism of sexual selection does not work. *American Naturalist*, 127: 222-240.

Knorr-Cetina, K.D. (1981). *The Manufacture of Knowledge*. New York, NY: Pergamon Press.

Knorr-Cetina, K.D. & Mulkay, M. (1983). *Science Observed: Perspectives on the Social Study of Science*. London, UK: Sage.

Kock, N. & Davison, R. (2003). Dealing with plagiarism in the information systems research community: A look at factors that drive plagiarism and ways to address them. *MIS Quarterly*, 27(4): 511-532.

Kuhn, T.S. (1962). *The Structure of Scientific Revolutions*. Chicago, IL: University of Chicago Press.

Kuhn, T.S. (1970). *The Structure of Scientific Revolutions*. 2nd edition. Chicago, IL: University of Chicago Press.

Kuhn, T.S. (1974). Second thoughts on paradigms. In: Suppe, F. (ed.), *The Structure of Scientific Theories*. Urbana, IL: University of Illinois Press: 459-482.

Kuhn, T.S. (2000). *The Road Since Structure: Philosophical Essays, 1970-1993, with an Autobiographical Interview*. Chicago, IL: University of Chicago Press.

Kvanli, A.H., Guynes, C.S. & Pavur, R.J. (1996). *Introduction to Business Statistics: A Computer Integrated Approach*. St. Paul, MN: West Publishing.

Lakatos, I. (1970). Falsification and the methodology of scientific research programmes. In: Lakatos, I. & Musgrave, A. (eds.), *Criticism and the Growth of Knowledge*. Cambridge, UK: Cambridge University Press: 59-89.

Lakatos, I. (1978). *The Methodology of Scientific Research Programmes*. Cambridge, UK: Cambridge University Press.

Larsen, B. (2002). Exploiting citation overlaps for information retrieval: Generating a boomerang effect from the network of scientific papers. *Scientometrics*, 54(2): 155-178.

Larsen, B. (2004). *References and Citations in Automatic Indexing and Retrieval Systems: Experiments with the Boomerang Effect*. Copenhagen, DK: Royal School of Library and Information Science. Ph.D. Thesis.

Latour, B. (1987). *Science in Action*. Cambridge, MA: Harvard University Press.

Latour, B. & Woolgar, S. (1986). *Laboratory Life: The Construction of Scientific Facts.* Princeton, NJ: Princeton University Press.

Laudan, L. (1977). *Progress and its Problems: Toward a Theory of Scientific Growth*. Berkeley, CA: University of California Press.

Laudan, L. (1984). *Science and Values: The Aims of Science and Their Role in Scientific Debate*. Berkeley, CA: University of California Press.

Laudan, L. (1996). *Beyond Positivism and Relativism: Theory, Method, and Evidence*. Boulder, CO: Westview Press.

Law, J. (1973). The development of specialties in science: The case of x-ray protein in crystallography. *Science Studies*, 3: 275-303.

Law, J. & Williams, R.J. (1982). Putting facts together: A study in scientific persuasion. *Social Studies of Science*, 12: 535-558.

Lawani, S. (1981). Bibliometrics: Its theoretical foundations, methods and applications. *Libri*, 31(4): 294-315.

Leydesdorff, L. (1998). Theories of citation?. *Scientometrics*, 43: 5-25.

Liu, M. (1993). Progress in documentation - the complexities of citation practice: A review of citation studies. *Journal of Documentation*, 49: 370-408.

Lockett, M.W. & Khawam, Y.J. (1990). Referencing patterns in C&RL and JAL, 1984-1986: A bibliometric analysis. *Library and Information Science Research*, 12: 281-289.

Lübcke, P. (1982). *Vor Tids Filosofi: Engagement og Forståelse*. Copenhagen, DK: Politikens Forlag.

MacRoberts, M. (1997). Rejoinder. *Journal of the American Society for Information Science*, 48(10): 963.

MacRoberts M.H. & MacRoberts, B.R. (1984). The negational reference: Or the art of dissembling. *Social Studies of Science*, 14: 91-94.

MacRoberts. M.H. & MacRoberts B.R. (1986). Quantitative measures of communication in science: A Study of the formal level. *Social Studies of Science,* 16: 151-172.

MacRoberts, M.H. & MacRoberts, B.R. (1987a). Another test of the normative theory of citing. *Journal of the American Society for Information Science*, 38: 305-306.

MacRoberts, M.H. & MacRoberts, B.R. (1987b). Testing the Ortega hypothesis: Facts and artefacts. *Scientometrics*, 12: 293-295.

MacRoberts, M.H. & MacRoberts, B.R. (1988). Author motivation for not citing influences: A methodological note. *Journal of the American Society for Information Science*, 39: 432-433.

MacRoberts, M.H. & MacRoberts, B.R. (1989a). Problems of citation analysis: A critical review. *Journal of the American Society for Information Science*, 40: 342-349.

MacRoberts, M.H. & MacRoberts, B.R. (1989b). Citation analysis and the science policy arena. *Trends in Biochemical Science*, 14: 8-10.

MacRoberts, M.H. & MacRoberts, B.R. (1996). Problems of citation analysis. *Scientometrics,* 36: 435-444.

Magee, B (ed.) (1978). *Men of Ideas.* London, UK: BBC.

Mahony, M.J. (1976). *Scientists as Subjects: The Psychological Imperative*. Cambridge, MA: Ballinger.

Martyn, J. (1964). Bibliographic coupling. *Journal of Documentation*, 20(4): 236.

Marius, R. & Wiener, H.S. (1991). *The McGraw-Hill College Handbook*. New York, NY: McGraw Hill.

Marr, R. (1944). *Fischerisms*. Springfield, IL: Charles C. Thomas.

Marshakova, I.V. (1973). A system of document connection based on references. *Scientific and Technical Information Serial of VINITI,* 6(2): 3-8.

Marx, K. & Engels, F. (1970). *The German Ideology*. New York, NY: International Publishers.

Masterman, M. (1970). The nature of a paradigm. In Lakatos, I. & Musgrave, A. (eds.), *Criticism and the Growth of Knowledge*. Cambridge, UK: Cambridge University Press: 59-89.

Mattelhart, A. & Mattelhart, M. (1998). *Theories of Communication: A Short Introduction*. London, UK: SAGE Publications.

Maynard Smith, J. (1976). Sexual selection and the handicap principle. *Journal of Theoretical Biology*, 57: 239-242.

Maynard Smith, J. & Parker, G.A. (1976). The logic of asymmetric contests. *Animal Behaviour*, 24: 159-175.

McCain, K.W. (1990). Mapping authors in intellectual space: A technical overview. *Journal of the American Society for Information Science*, 41(6): 433-443.

McCain, K.W. (1991). Mapping economics through the journal literature: An experiment in journal cocitation analysis. *Journal of the American Society for Information Science*, 42(4): 290-296.

McLaughlin, B.P. & Rey, G. (1998). Semantics, informational. In: Edward, C. (ed.), *Routledge Encyclopedia of Philosophy* [compact disc]. London, UK: Routledge.

Meadows, A.J. (1974). *Communication in Science*. London, UK: Butterworth.

Meadows, A.J. (1998). *Communicating Research*. San Diego, CA: Academic Press.

Merton, R.K. (1957). *Social Theory and Social Structure*. New York, NY: Free Press.

Merton, R.K. ([1938] 1973). The puritan spur to science. In: Merton, R.K. (ed.), *The Sociology of Science: Theoretical and Empirical Investigations.* Chicago, IL: University of Chicago Press: 228-253.

Merton, R.K. ([1942] 1973). The normative structure of science. In: Merton, R.K. (ed.), *The Sociology of Science: Theoretical and Empirical Investigations.* Chicago, IL: University of Chicago Press: 267-278.

Merton, R.K. ([1957] 1973). Priorities in scientific discovery. In: Merton, R.K. (ed.), *The Sociology of Science: Theoretical and Empirical Investigations.* Chicago, IL: University of Chicago Press: 286-324.

Merton, R.K. ([1963] 1973). The ambivalence of scientists. In: Merton, R.K. (ed.), *The Sociology of Science: Theoretical and Empirical Investigations.* Chicago, IL: University of Chicago Press: 383-412.

Merton, R.K. (1977). *The Sociology of Science: An Episodic Memoir*. Carbondale, IL: Southern Illinois University Press.

Merton, R.K. (1979). Foreword. In: Garfield, E. (ed.), *Citation Indexing: Its Theory and Application in Science, Technology and Humanities.* New York, NY: Wiley: vii-xi.

Merton, R.K. (1995). The Thomas theorem and the Matthew effect. *Social Forces*, 74(2): 379-424.

Metz, P. (1989). A statistical profile of College and Research Libraries. *College and Research Libraries*, 50: 42-47.

Mey, J.L. & Talbot, M. (1988). Computation and the soul. *Semiotica*, 72: 291-339.

Mill, J.S. (1843). *A System of Logic: Ratiocinative and Inductive.* London, UK: J.W. Parker.

Milton, J.R. (1998). Bacon, Francis (1561-1626). In: Edward, C. (ed.), *Routledge Encyclopedia of Philosophy* [compact disc]. London, UK: Routledge.

Mitroff, I.I. (1974). *The Subjective Side of Science*. New York, NY: Elsevier.

Mittermeyer, D. & Houser, L.J. (1979). The knowledge base for the administration of libraries. *Library Research*, 1: 255-276.

Mizzaro, S. (1998). How many relevances in information retrieval?. *Interacting with computers*, 10(3): 305-322.

Moed, H.F. & Garfield, E. (2003). Basic scientists cite proportionally fewer "authoritative" references as their bibliographies become shorter. *Proceedings of the 9th International Conference on Scientometrics and Informetrics*: 190-196.

Moed, H.F. & Vriens, M. (1989). Possible inaccuracies occurring in citation analysis. *Journal of Information Science,* 15: 94-107.

Moed, H.F. et al. (1983). *On the Measurement of Research Performance: The Use of Bibliometric Indicators*. Leiden, NL: University of Leiden.

Moravcsik, M.J. & Murugesan, P. (1975). Some results on the function and quality of citations. *Social Studies of Science,* 5: 86-92.

Moxley, J.M. (1992). *Publish don't Perish: The Scholar's Guide to Academic Writing and Publishing.* Westport, CT: Greenwood Press.

Mulkay, M.J. (1974). Methodology in the sociology of science: Some reflections on the study of radio astronomy. *Social Science Information*, 13(2): 107-119.

Mulkay, M. (1991). *Sociology of Science: A Sociological Pilgrimage*. Bloomington, IN: Indiana University Press.

Musgrave, A. (1998). Social relativism. In: Edward, C. (ed.), *Routledge Encyclopedia of Philosophy* [compact disc]. London, UK: Routledge.

Mustelin, O. (1988). Källhänvisningar och fotnoter i svenskspräkiga Åbodissertationer under 1700-talet. In Kolding Nielsen, E. et al. (ed.), *Bøger, Biblioteker, Mennesker: Et Nordisk Festskrift Tilegnet Torben Nielsen Universitetsbiblioteket i København*. København, DK: Det kgl. Bibliotek i samarbejde med Det danske Sprog- og Litteraturselskab: 105-126.

Møller, A.P. (1994). *Sexual Selection and the Barn Swallow*. New York, NY: Oxford University Press.

Narin, F. (1976). *Evaluative Bibliometrics: The Use of Publication and Citation Analysis in the Evaluation of Scientific Activity.* Cherry Hill, NJ: Computer Horizons, Inc.

Narin, F. (1987). To believe or not to believe. *Scientometrics*, 12(5-6): 343-344.

Nederhof, A.J. & Van Raan, A.F.J. (1987a). Peer review and bibliometric indicators of scientific performance: A comparison of cum laude doctorates with ordinary doctorates in physics. *Scientometrics*, 11(5-6): 333-350.

Nederhof, A.J. & Van Raan, A.F.J. (1987b). Citation theory and the Ortega hypothesis. *Scientometrics*, 12(5-6): 325-328.

Nickles, T. (1981). What is a problem that we may solve it?. *Synthese*, 47: 85-118.

Nicolaisen J. (2000). *Combination of Citation Analysis and Expert Judgement: A Methodology for Research Evaluation*. Copenhagen, DK: Royal School of Library and Information Science. Study Report.

Nicolaisen, J. (2002a). The J-shaped distribution of citedness. *Journal of Documentation*, 58(4): 383-395.

Nicolaisen, J. (2002b). The scholarliness of published peer reviews: A bibliometric study of book reviews in selected social science fields. *Research Evaluation*, 11(3): 129-140.

Nicolaisen, J. (2003), The social act of citing: Towards new horizons in citation theory. *Proceedings of the 66th ASIST Annual Meeting*: 12-20.

Niiniluoto, I. (1999). *Critical Scientific Realism*. Oxford, UK: Oxford University Press.

Nystrand, M., Greene, S. & Wiemelt, J. (1993). Where did composition studies come from?: An intellectual history. *Written Communication*, 10(3): 267-333.

Nørretranders, T. (2002). *Det Generøse Menneske*. Copenhagen, DK: People's Press.

O'Connor, J. (1967). Relevance disagreements and unclear request forms. *American Documentation*, 18(3): 165-177.

O'Connor, J. (1968). Some questions concerning "information need". *American Documentation*, 19(2): 200-203.

Osburn, C.B. (1989). The structuring of the scholarly communication system. *College & Research Libraries*, 50(3): 277-286.

Passmore, J. (1967). Logical positivism. In: Edwards, P. (ed.), *The Encyclopedia of Philosophy.* New York, NY: Macmillan: 52-57.

Peritz, B.C. (1981). Citation characteristics in library science: Some further results from a bibliometric study. *Library Research*, 3: 47-65.

Peters, H.P.F., Braam, R.R. & Van Raan, A.F.J. (1995). Cognitive resemblance and citation relations in chemical engineering publications. *Journal of the American Society for Information Science*, 46(1): 9-21.

Poehlmann, C. (2002). Software reviews. *Information Technology and Libraries*, 21(1): 38-40.

Polanyi, M. (1951). *The Logic of Liberty: Reflections and Rejoinders.* London, UK: Routledge and Kegan Paul Ltd.

Popper, K. (1974). Intellectual autobiography. In: Schlipp, P.A. (ed.), *The Philosophy of Karl Popper.* La Salle, IL: Open Court: 3-181.

Popper, K. (1995). *The Logic of Scientific Discovery.* London, UK: Routledge.

Price, D.J.S. (1963). *Little Science, Big Science.* New York, NY: Colombia University Press.

Price, D.J.S. (1964). Statistical studies of networks of scientific papers. Paper presented at the *Symposium on Statistical Association Methods for Mechanized Documentation.* Washington, DC: National Bureau of Standards: March 17.

Price D.J.S. (1965). Networks of scientific papers. *Science*, 149: 510-515.

Price, D.J.S. (1970). Citation measures of hard science, soft science, technology, and nonscience. In: Nelson, C.E. & Pollock, D.K. (eds.), *Communication Among Scientists and Engineers.* Lexington, MA: Heath: 3–22.

Pylyshyn, Z. (1980). *Computation and cognition: Issues in the foundation of cognitive science. Behavioral and Brain Sciences*, 3(1): 111-134.

Ravetz, J.R. (1971). *Scientific Knowledge and Social Problems*. Oxford, UK: Oxford University Press.

Reichenbach, H. (1938). *Experience and Prediction*. Chicago, IL: University of Chicago Press.

Robins, R.W., Gosling, S.D. & Craik, K.H. (1999). An empirical analysis of trends in psychology. *American Psychologist*, 54(2): 117-128.

Rowlands, I. (1998). *Mapping the Knowledge Base of Information Policy: Clusters of Documents, People and Ideas*. London, UK: City University. Ph.D. Thesis.

Sancho, R. (1992). Misjudgements and shortcomings in the measurement of scientific activities in less developed countries. *Scientometrics*, 23: 221-233.

Schafer, R. (1976). *A New Language for Psychoanalysis*. New Haven, CT: Yale University Press.

Schally, A.V. (1971). The amino acid sequence of a peptide with growth hormone-releasing activity isolated from porcine hypothalamus. *The Journal of Biological Chemistry*, 216(21): 6647-6650.

Schrader, A.M. (1985). A bibliometric study of JEL, 1960-1984. *Journal of Education for Library and Information Science*, 25: 279-300.

Schrader, A.M. & Beswick, L. (1989). The first five years of PLQ, 1979-1984: A bibliometric analysis. *Public Library Quarterly*, 9: 3-23.

Schubert, A. et al. (1984). Quantitative analysis of a visible tip of the peer review iceberg: Book reviews in chemistry. *Scientometrics*, 6(6): 433-443.

Schultz, D.P. & Schultz, S.E. (1992). *A History of Modern Psychology*. 5th edition. Fort Worth, TX: Harcourt Brace Jovanovich College Publishers.

Searle, J. (1992). *The Rediscovery of the Mind*. Cambridge, MA: MIT Press.

Shadish, W.R. et al. (1995). Author judgement about works they cite: Three studies from psychology journals. *Social Studies of Science*, 25: 477-498.

Shapere, D. (1964). The structure of scientific revolutions [book review]. *Philosophical Review*, 73: 143-149.

Shannon, C.E. (1948). A mathematical theory of communication. *Bell System Technical Journal,* 27: 379-423 & 623-656.

Shapin, S. (1995). Here and everywhere: Sociology of scientific knowledge. *Annual Review of Sociology*, 21: 289-321.

Skinner, B.C. (1974). *About Behaviorism*. New York, NY: Vintage.

Small, H. (1973). Co-citation in the scientific literature: A new measurement of the relationship between two documents. *Journal of the American Society of Information Science,* 24(4): 265-269.

Small, H. (1987). The significance of bibliographic references. *Scientometrics*, 12(5-6): 339-341.

Small, H. (1998). Citations and consilience in science. *Scientometrics*, 43(1): 143-148.

Small, H. & Griffith, B.C. (1974). The structure of scientific literatures 1: Identifying and graphing specialties. *Science Studies*, 4: 17-40.

Small, H.G. (1976). Structural dynamics of scientific literature. *International Classification*, 3(2): 67-74.

Small, H.G. (1977). A co-citation model of a scientific specialty: A longitudinal study of collagen research. *Social Studies of Science*, 7: 139-166.

Small, H.G. (1978). Cited documents as concept symbols. *Social Studies of Science*, 8: 327-340.

Smelser, N.J. (ed.). (1988). *Handbook of Sociology*. Newbury Park, CA: Sage.

Smith, A. ([1776] 1976). *En Undersøgelse af Nationernes Velstand, dens Natur og Årsager*. Copenhagen, DK: Rhodos.

Smith, E.A., Bird, R.B. & Bird, D.W. (2003). The benefits of costly signaling: Meriam turtle hunters. *Behavioral Ecology*, 14(1): 116-126.

Smith, L.C. (1981). Citation analysis. *Library Trends*, 30(1): 83-106.

Soergel, D. (2003). Annual review of information science and technology [book review]. *Library and Information Science Research*, 25(1): 111-113.

Song, M. & Galardi, P. (2001). Semantic relationships between highly cited articles and citing articles in information retrieval. *Proceedings of the 64th ASIST Annual Meeting*: 171-181.

Sperber, D. & Wilson, D. (1986). *Relevance: Communication and Cognition*. Cambridge, MA: Harvard University Press.

Sperber, D. & Wilson, D. (1995). *Relevance: Communication and Cognition*. 2nd edition. Cambridge, MA: Harvard University Press.

Sperber, D. & Wilson, D. (1997). Remarks on relevance and the social sciences. *Multilingua*, 16: 145-151.

Stephenson, M.S. (1993). The Canadian Library Journal, 1981-91: An analysis. *The Canadian Library Journal*, 18(2): 1-18.

Sternberg, R.J. (1996). *Cognitive Psychology*. Fort Worth, TX: Harcourt Brace College Publishers.

Storer, N.W. (1966). *The Social System of Science.* New York, NY: Holt, Rinehart & Winston.

Swanson, D.R. (1986). Subjective versus objective relevance in bibliographic retrieval systems. *Library Quarterly*, 56(4): 389-398.

Swanson, D.R. (1997). Information retrieval as a trial-and-error process. *Library Quarterly*, 47(2): 128-148.

Számadó, S. (2000). Cheating as a mixed strategy in a simple model of aggressive communication. *Animal Behaviour*, 59: 221-230.

Sztompka, P. (1986). *Robert K. Merton: An Intellectual Profile*. London, UK: Macmillan.

Talbot, M.M. (1997). Relevance. In Lamarque, P.V. (ed.), *Concise Encyclopedia of the Philosophy of Language*. New York, NY: Pergamon: 445-447.

Terrant, S.W. (1974). Letter to the editor. *Journal of the American Society for Information Science*, 25(1): 72.

Thagard, P. (2002). Cognitive science. *The Stanford Encyclopedia of Philosophy.* [Available at http://plato.stanford.edu/entries/cognitive-science/]. Visited June 28, 2004.

Thelwall, M. & Wilkinson, D. (2004). Finding similar academic Web sites with links, bibliometric couplings and colinks. *Information Processing and Management*, 40: 515-526.

Tolman, E.C. (1934). The determiners behavior at a choice point. *Psychological Review*, 45: 1-41.

Törnebohm, H. (1974). *Paradigm i Vetenskapernas Värld och i Vetenskapsteorin*. Göteborg, SE: University of Göteborg.

Toulmin, S. (1970). Does the distinction between normal and revolutionary science hold water?. In Lakatos, I. & Musgrave, A. (eds.), *Criticism and the Growth of Knowledge*. Cambridge, UK: Cambridge University Press: 39-47.

Trivison, D. (1987). Term co-occurrence in cited/citing journal articles as a measure of document similarity. *Information Processing & Management*, 23(3): 183-194.

Unisist (1971). *Study Report on the feasibility of a World Science Information System*. By the United Nations Educational, Scientific and Cultural Organization and the International Council of Scientific Unions. Paris, FR: Unesco.

Van Raan, A.F.J. (1998). In matters of quantitative studies of science the fault of theorists is offering too little and asking too much. *Scientometrics*, 43(1): 421-428.

Virgo, J.A. (1971). The review article: Its characteristics and problems. *The Library Quarterly*, 41(4): 275-291.

Virgo, J.A. (1977). A statistical procedure for evaluating the importance of scientific papers. *The Library Quarterly*, 47(4): 415-430.

Vladutz, G. & Cook, J. (1984), Bibliographic coupling and subject relatedness. *Proceedings of the American Society for Information Science*, 21: 204-207.

Walter, G. et al. (2003). Counting on citations: A flawed way to measure quality. *The Medical Journal of Australia*, 178(6): 280-281.

Ward, J.H. (1963). Hierarchical groupings to optimise an objective function. *Journal of the American Statistical Association*, 58: 236-244.

Webster, B.M. (1998). Polish sociology citation index as an example of usage of national citation indexes in scientometric analysis of social sciences. *Journal of Information Science*, 24(1): 19-32.

Weinberg, B.H. (1974). Bibliographic coupling: A review. *Information Storage and Retrieval*, 10: 189-196.

Weinstock, N. (1971). Citation indexes. In Kent, A. (ed.), *Encyclopedia of Library and Information Science*. New York, NY: Marcel Dekker: 16-41.

Weller, A.C. (2001). *Editorial Peer Review: Its Strengths and Weaknesses*. Medford, MJ: Information Today.

Whewell, W. (1840). *The Philosophy of the Inductive Sciences Founded upon their History.* London, UK: J.W. Parker.

Whiley, R.H. (1983). The evolution of communication: Information and manipulation. In: Halliday, T.R. & Slater, P.J.B. (eds.), *Animal Behaviour*. Oxford, UK: Blackwell Scientific Press: 156-189.

White, H.D. (1990). Author co-citation analysis: Overview and defence. In: Borgman, C.L. (ed.), *Scholarly Communication and Bibliometrics.* Newbury Park, CA: Sage: 84-106.

White, H.D. (2001). Authors as citers over time. *Journal of the American Society for Information Science and Technology*, 52(2): 87-108.

White, H.D. (2003). Author cocitation analysis and Pearson's r. *Journal of the American Society for Information Science and Technology*, 54(13): 1250-1259.

White, H.D. (2004a). Replies and a correction. *Journal of the American Society for Information Science and Technology*, 55(9): 843-844.
White, H.D. (2004b). Reward, persuasion, and the Sokal hoax: A study in citation identities. *Scientometrics*, 60(1): 93-120.
White, H.D. & Griffith, B.C. (1981). Author cocitation: A literature measure of intellectual structure. *Journal of the American Society for Information Science*, 32: 163-171.
White, H.D. & McCain, K.W. (1998). Visualizing a discipline: An author co-citation analysis of information science, 1972-1995. *Journal of the American Society for Information Science*, 49(4): 327-355.
White, M.D. & Wang, P. (1997). A qualitative study of citing behavior: Contributions, criteria, and metalevel documentation concerns. *Library Quarterly*, 67(2): 122-154.
Whitley, R. (1974). Cognitive and social institutionalisation of scientific specialties and research areas. In: Whitley, R. (ed.), *Social Processes of Scientific Development*. London, UK: Routledge and Kegan Paul: 69-95.
Wiener, J. (1974). Footnote - or perish. *Dissent*, 21: 588-592.
Wiener, J. (1977). The footnote fetish. *Telos*, 31: 172-177.
Wiener, N. (1948). *Cybernetics: Or Control and Communication in the Animal and the Machine*. New York, NY: John Wiley & Sons.
Wikipedia (2002). Wikipedia: The Free Encyclopedia. [Available at http://en.wikipedia.org/wiki/Main_Page]. Visited June 28., 2004.
Windsor, D.A. & Windsor, D.M. (1973). Citation of the literature by information scientists in their own publications. *Journal of the American Society for Information Science*, 25(5): 377-381.
Wittgenstein, L.J.J. (1922). *Tractatus Logico-Philosophicus*, London, UK: Routledge.
Wolfgang, M.E. et al. (1978). *Evaluating Criminology*. New York, NY: Elsevier.
Woolgar, S. (1988). *Science: The Very Idea*. London, UK: Tavistock Publications.
Worthern, D.B. & Shimko, A.H. (1974). Letter to the editor. *Journal of the American Society for Information Science*, 25(1): 72-73.
Woodward, J. (1998). Statistics. In: Edward, C. (ed.), *Routledge Encyclopedia of Philosophy* [compact disc]. London, UK: Routledge.
Wouters, P. (1998). The signs of science. *Scientometrics,* 41: 225-241.
Wouters, P. (1999). *The Citation Culture*. Amsterdam, NL: Universiteit van Amsterdam. Ph.D. Thesis.
Wright, K. (1998). Gadamer, Hans-Georg (1900-). In: Edward, C. (ed.), *Routledge Encyclopedia of Philosophy* [compact disc]. London, UK: Routledge.

Zahavi, A. (1975). Mate selection: Selection for a handicap. *Journal of Theoretical Biology*, 53(1): 205-214.

Zahavi, A. (1977a). The cost of honesty (further remarks on the handicap principle). *Journal of Theoretical Biology*, 67(3): 603-605.

Zahavi, A. (1977b). Reliability in communication systems and the evolution of altruism. In: Stonehouse, B. & Perrins, C. (eds.), *Evolutionary Ecology*. London, UK: The MacMillian Press: 253-259.

Zahavi, A. (1979). Why shouting. *American Naturalist*, 113(1): 155-156.

Zahavi, A. (1980). Ritualization and the evolution of movement signals. *Behaviour*, 72(1-2): 77-81.

Zahavi, A. (1987). The theory of signal selection and some of its implications. In: Delfino, V.P. (ed.), *Proceedings of the International Symposium of Biological Evolution*. Bari, IT: Adriatica Editrica: 305-327.

Zahavi, A. (1993). The fallacy of conventional signalling. *Philosophical Transactions of the Royal Society of London Series B – Biological Sciences*, 340(1292): 227-230.

Zahavi, A. (2003). Indirect selection and individual selection in socio-biology: My personal views on theories of social behaviour. *Animal Behaviour*, 65: 859-863.

Zahavi, A. & Zahavi, A. (1997). *The Handicap Principle*. New York, NY: Oxford University Press.

Ziman, J.M. (1968). *Public Knowledge: An Essay Concerning the Social Dimension of Science*. Cambridge, UK: Cambridge University Press.

Ziman, J.M. (2000). *Real Science: What it is, and What it Means*. Cambridge, UK: Cambidge University Press.

Zuckerman, H. (1977). Deviant behavior and social control in science. In: Sagarin, E. (ed.), *Deviance and Social Change*. Beverly Hills, CA: Sage Publications: 87-138.

Zuckerman, H. (1978). Theory choice and problem choice in science. *Sociological Inquiry*, 48: 65-95.

Zuckerman, H. (1987). Citation analysis and the complex problem of intellectual influence. *Scientometrics*, 12(5-6): 329-338.

Zuckerman, H. & Merton, R.K. ([1971] 1973). Institutionalized patterns of evaluation in science. In: Merton, R.K. (ed.), *The Sociology of Science: Theoretical and Empirical Investigations.* Chicago, IL: University of Chicago Press: 460-496.

Ørom, A. (2004). Knowledge organization in the domain of art studies: History, transition and conceptual changes. *Knowledge Organization*, 30(3/4): 128-143.

Appendix 1: Abbreviated journal names

Psychoanalytic journals

INT J PSYCHO ANAL
INT J PSYCHOAN 0801
INT J PSYCHOANAL
INT J PSYCHOANAL 1
INT J PSYCHOANAL 2
INT J PSYCHOANAL 3
INT J PSYCHOANAL 4
INT J PSYCHOANAL 5
INT J PSYCHOANAL 5 6
INT J PSYCHOANAL 6
INT J PSYCHOANALYSIS
INT J PSYCHOANALYSIZ
INT J PSYCHOANANLYSI
INT J PSYCHOANAYSIS
INT J PYSCHOANAL
INT JPSYCHOANAL

PSA Q
PSYCHOANAL Q
PSYCHOANAL QUART
PSYCHOANALYTIC Q

J AM PSCHOANALYTIC A
J AM PSYCHANAL ASSN
J AM PSYCHANALYTIC A
J AM PSYCHOALANYTIC
J AM PSYCHOANAL AS S
J AM PSYCHOANAL ASS
J AM PSYCHOANAL ASSN
J AM PSYCHOANAL JASS
J AM PSYCHOANALYTIC

CONT PSYAHOANALYSIS
CONT PSYCHO ANAL
CONT PSYCHONAL
CONTEMP PSYCHOANAL

Behavioral journals

J EXP ANAL BEHAV
J EXPT ANAL BEHAVIOR
J EXPT AN BEHAVIOR
J EXPT ANAL BEH
J EXPT ANAL BEHAF
J EXPT ANAL BEHAV
J EXPT BEHAV
J EXPTL ANALYSIS BEH

BEHAV RES THER
BEHAV RES THER S1
BEHAV RES THERAPY

J APPL BEHAV ANAL
J APPL BEHAV APPL
J APPL BHEAV ANAL

BEHAV THER
BEHAV THERAPY
BEHAV THERPY
BEHAVIOUR THERAPY
BEHV THERAPY

Cognitive journals

COGN PSYCHOL
COGNITIVE PSYCHOL
COGNITIVE PSYCHOLOGY

COGN
COGNITION

MEM COG
MEM COGN
MEM COGNIT
MEM COGNITION
MEMORY COGITION
MEMORY COGNIT
MEMORY COGNITIN
MEMORY COGNITION
MEMROY COGNITION

J EXP PSYCHOL LEARN
J EXPER PSYCHOL LEAR
J EXPT PSYCHOL LEARN
J EXPT PSCYHOL LEARN
J EXPT PSCYOL LEARNI
J EXPT PSYCH LEARNIN
J EXPT PSYCHOL LARNI
J EXPT PSYCHOL LARNI
J EXPT PSYCHOL LEARE
J EXPT PSYCHOL LEARI
J EXPT PXYCHOL LEARN
J EXPTL PSYCHOL LEAR

Neuroscientific journals

J NEURO PHYSL
J NEUOPHYSIOL
J NEUORPHYSIOL
J NEUROPHSYIOL
J NEUROPHYSIL
J NEUROPHYSILL
J NEUROPHYSIOL

J NEUROPHYSIUOL
J NEUROPHYSI8OL
J NEUROPHYSL
J NEUROPYSIOL
J NEUROPHYSIOLOGY

ANN REV NEUR
ANN REV NEUROCI
ANN REV NEUROSCI
ANN REV NEUROSCI D
ANN REV NEUROSCIENCE
ANNU REV NEUROSCI
ANNUAL REV NEUROSCIE

TREND NEUROSCI
TRENDS NEUROSCI
TRENDS NEUROSCI S
TRENS NEUROSCI
TRNEDS NEUROSCI
TRENDS NEUROSCIE MAY
TRENDS NEUROSCIENCE
TRENDS NEUROSCIENCES

J NERUOSCI
J NEUROCI
J NEUROSCI
J NEUROSCID
J NEUROSCIO
J NEUROSI
J NEUROSIC
J NEUSCI
J NEUROSCI 1
J NEUROSCI 2

Appendix 2: Raw co-citation matrix (psychology)[†]

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
2	612														
3	935	536													
4	338	236	270												
5	0	0	0	0											
6	69	21	50	7	94										
7	4	0	6	1	283	360									
8	27	7	34	4	101	2025	555								
9	18	3	16	1	98	113	42	45							
10	35	6	30	2	136	80	28	40	2817						
11	9	4	11	2	88	165	22	44	3086	2151					
12	20	11	26	5	95	192	20	42	3241	2172	4479				
13	10	5	14	2	94	83	7	30	787	589	371	430			
14	14	7	14	1	75	99	13	37	660	452	352	405	8213		
15	22	11	17	1	126	106	11	44	606	650	374	420	13327	9790	
16	23	13	29	1	290	242	30	79	1251	1006	808	994	28405*	18075	31330*

* Only SCI

[†] Table 5.11. translates the journal numbers.

Appendix 3: Raw co-citation matrix (communication theory)[†]

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	…
2	81																		
3	16	3																	
4	450	2	14																
5	12	2	3	109															
6	105	20	46	112	42														
7	5	3	5	27	117	13													
8	28	15	8	9	43	23	234												
9	14	1	6	8	16	8	72	581											
10	23	9	9	117	127	42	580	380	163										
11	56	6	24	38	26	57	11	14	6	60									
12	10	0	20	48	32	23	138	28	28	300	101								
13	22	8	34	28	19	46	8	12	5	18	41	26							
14	6	1	20	5	3	13	4	2	2	3	33	10	48						
15	53	11	17	96	32	87	4	8	10	18	31	15	19	8					
16	11	2	4	27	5	19	1	2	2	4	10	2	2	3	45				
17	12	2	6	4	7	12	4	8	0	15	9	3	5	4	7	0			
18	17	2	10	2	7	10	7	6	1	14	9	5	12	4	5	0	71		
19	14	2	8	26	28	39	71	34	8	248	117	107	15	4	18	0	32	32	
20	10	2	22	3	6	16	7	5	3	8	16	9	16	6	3	3	25	35	
21	2	0	7	3	16	11	12	20	5	44	39	69	5	5	1	2	0	4	
22	8	2	20	2	12	23	12	13	1	30	35	141	17	8	12	1	5	7	
23	2	2	11	5	8	15	4	33	13	19	11	16	10	5	7	1	4	4	
24	18	1	13	18	15	26	11	12	4	33	21	17	14	3	16	3	85	69	
25	5	0	5	28	39	26	37	8	6	96	60	292	20	8	19	0	2	1	
26	0	0	3	1	0	5	2	1	1	3	2	5	7	2	1	0	2	0	
27	3	0	11	0	2	13	0	1	0	3	18	15	12	6	10	1	0	4	
28	10	0	5	0	3	4	6	28	5	24	3	5	2	0	1	0	1	1	
29	4	1	2	1	15	1	1	8	3	11	2	3	0	0	7	2	2	2	
30	15	3	10	4	5	26	5	30	14	9	2	7	4	2	0	0	2	0	
31	10	1	1	4	2	6	4	8	0	2	1	5	0	0	0	0	0	1	
32	4	1	6	5	17	11	2	6	0	18	47	86	9	2	1	1	1	2	
33	5	1	7	5	5	13	6	14	4	16	16	37	2	2	1	1	1	0	
34	11	1	7	1	5	19	7	38	15	21	6	23	6	1	3	0	1	1	
35	6	2	11	4	60	12	10	11	4	24	11	56	5	3	6	3	0	1	

[†] Table 5.12. translates the theoretician numbers.

	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34
20	26															
21	24	21														
22	58	83	232													
23	31	60	54	125												
24	129	36	11	19	7											
25	208	20	185	268	60	231										
26	25	13	1	11	2	3	21									
27	68	13	4	28	14	16	54	19								
28	3	2	5	7	5	0	5	0	0							
29	4	0	7	7	6	2	8	0	0	106						
30	5	0	3	6	0	3	3	0	2	16	3					
31	4	0	0	3	0	5	0	0	0	2	0	44				
32	38	18	423	220	37	7	417	0	14	4	16	6	1			
33	12	7	49	42	5	6	61	1	6	22	15	21	7	147		
34	5	8	6	11	5	2	23	1	5	85	35	50	3	32	62	
35	19	9	27	62	23	11	263	9	26	41	22	12	0	308	81	97

Appendix 4: Correlation matrix (psychology)

	1	2	3	4	5	6	7	8
1	*							
2	0,951	*						
3	0,998	0,939	*					
4	0,84	0,822	0,818	*				
5	-0,598	-0,597	-0,582	-0,619	*			
6	-0,243	-0,267	-0,236	-0,284	0,046	*		
7	-0,316	-0,329	-0,309	-0,337	0,469	0,729	*	
8	-0,211	-0,239	-0,212	-0,253	0,064	0,889	0,724	*
9	-0,399	-0,405	-0,387	-0,418	0,019	-0,229	-0,342	-0,291
10	-0,394	-0,404	-0,384	-0,42	0,058	-0,242	-0,346	-0,297
11	-0,35	-0,352	-0,338	-0,362	-0,03	-0,18	-0,301	-0,247
12	-0,356	-0,358	-0,344	-0,37	-0,021	-0,183	-0,309	-0,251
13	-0,289	-0,283	-0,278	-0,302	0,285	-0,164	-0,283	-0,215
14	-0,304	-0,299	-0,294	-0,319	0,23	-0,175	-0,296	-0,226
15	-0,284	-0,279	-0,275	-0,301	0,313	-0,158	-0,279	-0,211
16	-0,314	-0,308	-0,303	-0,329	0,271	-0,181	-0,307	-0,232

	9	10	11	12	13	14	15	16
9	*							
10	0,934	*						
11	0,9	0,816	*					
12	0,909	0,821	0,995	*				
13	-0,036	-0,03	-0,146	-0,124	*			
14	-0,052	-0,051	-0,156	-0,135	0,809	*		
15	-0,05	-0,028	-0,148	-0,127	0,886	0,84	*	
16	-0,052	-0,039	-0,159	-0,138	0,955	0,884	0,97	*

* missing

Appendix 5: Correlation matrix (communication theory)

	1	2	3	4	5	6	7	8	9	10	11	12
1	*											
2	0,802	*										
3	0,515	0,276	*									
4	0,513	0,334	0,387	*								
5	0,22	0,049	0,087	0,588	*							
6	0,627	0,538	0,341	0,623	0,355	*						
7	0,044	0,063	-0,038	0,526	0,694	0,095	*					
8	-0,023	0,012	-0,103	0,233	0,419	-0,064	0,593	*				
9	0,074	0,146	-0,048	0,091	0,31	0,003	0,563	0,962	*			
10	-0,013	-0,006	-0,002	0,175	0,664	0,022	0,854	0,432	0,521	*		
11	0,155	0,239	0,381	0,459	0,298	0,338	0,332	0,018	0	0,275	*	
12	-0,059	-0,049	-0,003	0,344	0,612	0,073	0,613	0,359	0,148	0,38	0,542	*
13	0,501	0,247	0,777	0,489	0,198	0,507	0,101	-0,081	-0,004	0,053	0,516	0,174
14	0,272	0,091	0,645	0,196	0,005	0,363	-0,093	-0,13	-0,102	-0,051	0,24	0,052
15	0,655	0,38	0,463	0,669	0,377	0,793	0,018	-0,07	-0,058	0,072	0,32	0,014
16	0,594	0,272	0,389	0,609	0,308	0,703	-0,036	-0,072	-0,041	-0,016	0,154	-0,075
17	0,084	0,037	0,128	0,062	-0,013	0,078	0,045	-0,06	-0,01	0	0,114	-0,038
18	0,017	0,083	0,161	0,045	-0,033	0,044	0,034	-0,048	-0,027	0,023	0,152	-0,029
19	0,03	-0,025	0,11	0,42	0,523	0,08	0,638	0,289	0,17	0,29	0,561	0,868
20	-0,119	-0,04	0,261	-0,112	-0,134	-0,061	-0,089	-0,116	-0,108	-0,088	0,189	0,226
21	-0,183	-0,099	-0,006	-0,068	0,022	-0,127	0,02	-0,058	-0,037	-0,013	0,303	0,462
22	-0,245	-0,137	-0,004	-0,054	0,027	-0,157	0,011	-0,107	-0,076	0,045	0,442	0,514
23	-0,145	-0,089	0,19	-0,082	0,03	-0,097	0,081	-0,026	0,101	0,051	0,313	0,487
24	-0,043	-0,03	-0,019	0,097	0,124	0,046	0,085	-0,051	-0,04	0,116	0,475	0,581
25	-0,219	-0,16	0,044	-0,032	0,13	-0,128	0,086	-0,072	-0,088	0,103	0,502	0,435
26	-0,141	-0,126	0,174	-0,035	0,026	-0,032	0,003	-0,1	-0,101	0,067	0,505	0,411
27	-0,089	-0,107	0,157	0,02	0,016	0,027	-0,046	-0,178	-0,141	0,116	0,602	0,474
28	-0,145	0,004	-0,16	-0,111	0,047	-0,173	0,107	0,045	0,158	-0,027	-0,175	0,015
29	-0,124	-0,07	-0,153	-0,072	-0,06	-0,159	0,022	0	0,011	-0,043	-0,127	0,004
30	0,116	0,175	0,033	0,019	-0,014	-0,077	0,115	0,116	0,314	0,108	-0,08	-0,058
31	0,06	0,194	0,069	0,004	-0,014	0,094	0,025	-0,013	0,129	0,07	-0,011	-0,067
32	-0,232	-0,13	-0,038	-0,114	0,077	-0,15	-0,043	-0,108	-0,09	-0,022	0,241	0,506
33	-0,206	-0,112	-0,058	-0,131	0,064	-0,195	-0,008	-0,077	-0,031	-0,029	0,229	0,36
34	-0,128	-0,016	-0,043	-0,11	0,091	-0,217	0,088	0,035	0,167	0,021	-0,074	0,091
35	-0,239	-0,134	-0,154	-0,043	0,001	-0,156	-0,013	-0,094	-0,072	-0,014	0,254	0,478

	13	14	15	16	17	18	19	20	21	22	23	24
13	*											
14	0,764	*										
15	0,531	0,231	*									
16	0,412	0,153	0,946	*								
17	0,116	-0,015	0,037	-0,016	*							
18	0,071	0,035	0,011	-0,049	0,953	*						
19	0,277	0,097	0,046	-0,047	0,254	0,234	*					
20	0,152	0,135	-0,089	-0,164	0,341	0,324	0,227	*				
21	0,021	-0,042	-0,146	-0,15	-0,118	-0,109	0,294	0,407	*			
22	0,033	-0,004	-0,177	-0,169	-0,116	-0,078	0,4	0,386	0,82	*		
23	0,095	0,025	-0,118	-0,133	-0,024	0,043	0,293	0,755	0,625	0,81	*	
24	0,14	0,028	0,047	-0,09	0,339	0,373	0,597	0,246	0,231	0,47	0,333	*
25	-0,005	-0,07	-0,154	-0,157	0,121	0,114	0,363	0,392	0,756	0,738	0,527	0,14
26	0,149	0,141	-0,042	-0,16	0,082	0,173	0,427	0,318	0,176	0,353	0,496	0,658
27	0,201	0,158	0	-0,085	0,167	0,153	0,486	0,404	0,348	0,47	0,478	0,701
28	-0,255	-0,217	-0,199	-0,139	-0,157	-0,161	-0,062	-0,15	-0,049	-0,077	-0,038	-0,128
29	-0,227	-0,211	-0,179	-0,102	-0,134	-0,142	-0,074	-0,097	0,064	0,014	-0,014	-0,087
30	-0,076	-0,167	-0,01	-0,079	-0,167	-0,156	-0,128	-0,181	-0,07	-0,15	-0,079	-0,152
31	-0,053	-0,127	-0,008	-0,044	0,001	-0,037	-0,057	-0,115	-0,064	-0,123	-0,104	-0,083
32	-0,062	-0,025	-0,156	-0,154	-0,175	-0,153	0,309	0,289	0,772	0,848	0,618	0,434
33	-0,099	-0,133	-0,218	-0,179	-0,204	-0,187	0,181	0,141	0,818	0,684	0,427	0,134
34	-0,235	-0,205	-0,229	-0,16	-0,216	-0,222	-0,012	-0,158	0,156	0,129	0,063	-0,066
35	-0,057	-0,115	-0,16	-0,182	-0,179	-0,183	0,328	0,118	0,853	0,724	0,375	0,409

	25	26	27	28	29	30	31	32	33	34	35
25	*										
26	0,292	*									
27	0,526	0,899	*								
28	-0,036	-0,125	-0,12	*							
29	0,026	-0,13	-0,079	0,839	*						
30	-0,118	-0,183	-0,162	0,429	0,308	*					
31	-0,067	-0,135	-0,101	0,073	-0,018	0,548	*				
32	0,649	0,363	0,449	0,085	0,072	-0,038	-0,096	*			
33	0,747	0,113	0,281	0,274	0,279	0,202	0,014	0,799	*		
34	0,224	-0,02	0,03	0,578	0,674	0,389	0,301	0,322	0,51	*	
35	0,684	0,218	0,382	0,101	0,21	0,069	-0,064	0,691	0,881	0,31	*

* missing