# Dimensions of Citation Analysis

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*An analytical scheme that differentiates among the various types of cognitive and social functions of citations is used as the basis for an analysis of the results of a questionnaire designed to probe the citing behavior of a group of scientists who had cited one of four papers originating from a single biochemical laboratory. Even when papers fall within a relatively well-defined research area and are based on research conducted within a single lab, groups of scientists to which a given paper is of relevance can have quite distinct internal structures. We argue that the scientists' subjective reasons for citing a given work do not correspond with the actual argumentative uses of cited articles in citing texts. Scientists appear to regard the papers they cite as having a rather generalized significance even if in their citations they refer to very specific claims made in the original texts. The consequences of these findings for a theory of citation and for the use of citations in science studies are discussed.*

Citation analysis has conquered the world of science policy analysis. Aggregates of citations are commonly used in evaluation studies as indicators of the "impact" of publications, and as one of the measures of the "quality" of research groups or even of individual researchers. Co-citation maps of scientific specialties, and also, increasingly, citation patterns among journals, are used to describe the development of disciplines and specialties, and to identify emerging areas of scientific inquiry (Small and Sweeney 1985; Narin 1976; Leydesdorff 1987a; Todorov and Glänzel 1988).

The fact that publication and citation behavior vary among disciplines was noted shortly after the appearance of the first *Science Citation Index* in 1961 (Hagstrom 1970; Price 1970; Garfield 1979). Differences in the citing practices of scientists from different disciplines and specialties (Cozzens 1985), and even systematic differences in citing patterns within specialties (Moed 1989), have led some analysts to the conclusion that comparisons of citation counts are meaningful indicators of variation in performance only

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when one compares groups working within a single field and within similar institutions. The call for comparisons of "like with like" (Martin and Irvine 1985), however, did not eliminate all criticisms of the policy uses of citation studies. Most recently, such criticisms have been presented by MacRoberts and MacRoberts (1986, 1987a, 1987b), who argue that citing practices are incomplete and biased. Controversies surrounding the uses of citation analysis for policy purposes have also led to calls for a theory of citation (Cronin 1981).[1] In response, various functions of citations have been distinguished and discussed (Cozzens 1981). Citations have been analyzed in terms of their institutional functions as part of the reward system in science (Kaplan 1965). It has also been suggested that citations should be understood as concept-symbols (Small 1978), as rhetorical tools (Gilbert 1977), or as textual elements fulfilling specific cognitive functions in arguments presented in the citing papers (Moravcsik and Murugesan 1975; Chubin and Moitra 1975; Amsterdamska and Leydesdorff 1989).

In our opinion, the difficulties in arriving at a systematic understanding of the citing behavior of scientists and of aggregate citation patterns stem not only from theoretical differences between the proponents of the various theories of citation, but also from the relative lack of attention given to the inherently multidimensional character of citation, and thus also to the complex significance of aggregate citation patterns. First, it is necessary to recognize that providing an answer to the question "why do scientists cite?" will not necessarily furnish an adequate explanation of aggregate citation patterns. Second, an analysis of the significance of citations has to specify whether citations are to be studied as indicators of links among texts, links among authors of texts, or links between texts and authors. Distinct theoretical and methodological problems arise in each of the perspectives (Leydesdorff 1989).

In a recent review article, Cozzens (1989) emphasized the dual nature of citations, which, in her view, operate within "two systems": the rhetorical system (which we would prefer to call more traditionally the cognitive system of science) and the reward system (which could be more broadly seen as the social system of relations between scientists). When citations are seen as establishing links of whatever nature between two *texts*, what is being analyzed is the *cognitive* content of this relation; when citations are regarded as establishing links between *authors*, what is being studied is the *social* organization of scientific communities. In practice, the two aspects of citations are, of course, thoroughly intertwined. Their analytical separation, however, is necessary if we want to appreciate clearly the underlying assump-

tions about the relations between the social and cognitive aspects of science in various forms of citation analysis.[2]

| | Citing Author | Citing Text |
|---|---|---|
| Cited Author | Professional relation | Reward |
| Cited Text | Cognitive resource | Discursive relation |

The particular interest that citation analysis has had for science studies and for the evaluation of the performance of groups or individuals stems from the fact that citations appeared as a particularly useful indicator of links between the social and the cognitive dimensions of science. For example, the number of times an article was cited could be seen as an indicator of the performance of the cited author(s), and thus a translation was made from the cognitive use of citations in a text to the social system of rewards operating in the scientific community. Alternatively, since the citing authors, who were competing for recognition within their communities, were said to use cited texts as a means of convincing others, citations could be analyzed as a means of persuasion; translation here was from the social organization of scientific communities, in which reputations are unequally distributed, to the cognitive organization of science, which was seen as an outcome of social processes. It was precisely these translations between the social and the cognitive aspects of citations that were always seen as the most analytically problematic. Do citations say something about the cited authors? Do they reflect some form of strategic behavior on the part of the citing author? How can we tell when citation reflects a *cognitive* debt and when it is mainly a reflection of a *social* hierarchy within the scientific community? Is it legitimate to equate the two dimensions of citations, to treat the social and the cognitive organization of science as isomorphic? How can such questions be addressed empirically?[3]

In recent research, we studied citations as relations among texts, arguing that if scientific texts are treated as attempts to integrate new knowledge claims into a fabric of shared knowledge, citations can be shown to fulfill a number of distinct argumentative functions in the citing articles (Amsterdamska and Leydesdorff 1989). The citing texts now only modify the knowledge claims made in the cited papers, but also use these claims in their own arguments. Thus, the cited papers can be used to supply premises or supporting evidence for further conclusions, to help in the formulation of a research agenda, or to assist in the placement of the citing text in the context of other research. By comparing these textual functions of citations to four different biochemistry articles, we have also come to the conclusion that the

uses to which the cited papers were put were systematically related to the manner in which the original paper integrated its own claims into the body of knowledge in the field. This manner of integration, as well as the later uses of the papers when they were cited, was quite distinct for the four biochemistry articles we examined.

However, the analysis of the textual links between citing and cited papers tells us nothing about the citing authors' perceptions of the papers they cited, about their reasons for citing a particular paper, or about the social determinants of citation behavior. Accordingly, this study cannot be regarded as an attempt to validate the results of textual analysis. Relations among texts do not necessarily shed light on relations among texts and authors, or among cited and citing authors. How do scientists who cite a particular paper perceive its significance? Are the differences in the argumentative uses of these papers related to differences in scientists' perceptions of their significance? Do the scientists who cite a given paper perceive it similarly? How are perceptions of the cited paper related to the personal links between the cited and the citing authors? What is the relationship between textual links between papers and the social organization of the groups of scientists who use the paper in their own texts? In order to explore these dimensions of citation behavior further, we circulated a questionnaire among scientists who have cited one of the four biochemistry articles we used in our previous analysis.

## Methods

In April 1987, we mailed a questionnaire to the 239 authors of articles that contained one of the 91 citations to one of the four most highly cited articles published between 1979 and 1982 by one research group in the Department of Biochemistry at the University of Amsterdam.

The cited research group, as a whole, studied energy transfer processes across bio-membranes. The area is well-known for the so-called Mitchell debate concerning the role of proton transfers across membranes in oxidative and photophosphorylation.[4] Although Peter Mitchell, the originator of the chemiosmotic theory, received the Nobel Prize for chemistry in 1978, the debate on the chemical and physical principles involved in this type of energy transfer continues to this day.

The professor in this laboratory, together with several doctoral and postdoctoral students, was involved in an attempt to develop and test a non-equilibrium thermodynamic model of chemiosmotic energy transfers in oxi-

**Table 1. Citation Rates**

| | *Citations (Publication Date to December 31, 1985)* | *After Correction for In-Group and Self-Citation* | *Citations Per Year* |
|---|---|---|---|
| Westerhoff et al. 1981 | 38 | 30 | 7.5 |
| Sips et al. 1980 | 30 | 28 | 5.6 |
| Scholte et al. 1981 | 27 | 17 | 4.3 |
| Bernstein et al. 1981 | 21 | 16 | 4.0 |
| | 116 | 91 | |

dative phosphorylation in mitochondria and sub-mitochondrial particles. The second senior scientist, together with his graduate students, was studying the problem of the structure and regulation of the phosphoenolpyruvate:sugar phosphotransferase system in *Salmonella typhimurium*. The third line of research was being pursued by a doctoral student working in collaboration with some members of a different laboratory in the same institute. Their subject was the transport of amino acids across rat liver membranes and its role in metabolism. Finally, a new permanent member of the lab, together with a postdoctoral visitor, were studying cell differentiation processes in *Dyctostelium discoideum*, which is a problem at the cellular level intrinsically related to transport processes at the membrane. During the four years in question, the group as a whole published some 57 papers, 47 of which were full-length articles. We studied these articles in detail, interviewed the director of the group extensively, and were granted access to the files of referee reports, etc.

The fact that four distinct and yet closely related research lines were being pursued in our laboratory meant that, in our study, cognitive context varied while the immediate institutional environment remained constant. For our analysis, we chose the four most frequently cited papers, each of which conveniently came from a different research line.[5] Table 1 gives the citation rates before and after correction for self-citation and "in-group" citation, and indicates the citation rate per year.

The questionnaire (Appendix) contained 30 questions exploring the citing author's perception of the cited article, the place of the article in the citer's research program, the citer's perception of the research field(s) to which the citing and the cited articles belong, personal links between the cited and the citing authors, and the citing author's evaluation of the work of the group from which the cited article originated. The responses can be classified in terms of approximately 80 variables. Chi-square was used for significance testing when the variables were nominal, Kendall's Tau-B when they were ordinal.

**Table 2. Number of Authors and Response Rates**

| | *Citations* | *Authors* | *Responses* | *Response* |
|---|---|---|---|---|
| Westerhoff et al. 1981 | 30 | 75 | 38 | 50.7% |
| Sips et al. 1980 | 28 | 67 | 31 | 46.3% |
| Scholte et al. 1981 | 17 | 55 | 24 | 43.6% |
| Bernstein et al. 1981 | 16 | 42 | 14 | 33.3% |
| | 91 | 239 | 107 | 44.8% |

Because the first nine questions referred specifically to the citing paper, we asked the respondents who cited the original paper more than once to answer these nine questions separately for each paper in which the citation appeared. The remaining questions were of a more general nature and were to be answered only once by each citing author independently of how often he or she had cited the paper.[6]

To contact citing authors, we used all institutional addresses of the articles, and we did not attempt to trace authors who had moved, since that would necessarily have introduced another specific bias. All authors were included as mentioned on their published articles. After 6 weeks, having received only a 19% response, we mailed another copy of the questionnaire.[7]

Seven authors indicated that they were replying also for their co-authors; six of them had cited Sip, Groen, and Tager's (1980) paper. Since students and technical staff occasionally co-author papers on biochemistry, we think that 45% is a reasonable response rate. With the exception of the Bernstein et al.'s (1981) case, we received an average of more than one response per citing article.[8]

Of course, the survey method also introduces bias. First, there was a bias in favor of those with tenured positions, since they were more likely than their non-tenured colleagues and students to remain working at the same institute for several years and were therefore more likely to receive the questionnaires. Second, it should be clear that by polling only those scientists who cited one of our papers we did not reach the entire population of scientists for whom the claim made in the original paper might have been relevant. For example, there might be some who used the paper in their research but did not cite it in their work, or some who were simply not aware of the existence of the article when they published on a closely related topic. We could not correct for these kinds of biases with our material. However, we added an additional question about the three best research groups in the area, which allowed us to draw a sociogram and to estimate the extent to which our questionnaire has covered the specialty group.

## The Scientific Identities of the Citing and Cited Authors

Although the four papers whose reception we studied all originated from the same research group, were all published in recognized biochemistry journals, and all dealt with problems relating to transport processes in biological membranes, each of the papers belonged to a different line of research and addressed a different scientific audience. To what extent can we talk of the cited and citing authors as members of the same communities, specialties, or even disciplines? How do they identity themselves and what sort of group do they form?

When asked directly whether their work belongs to the same speciality as the research presented in the cited papers, most of the respondents answered in the affirmative. Only in the case of those who cited the paper of Sip et al. was there a substantial number of respondents (30%) who believed that their own work belongs to a different specialty. We asked our respondents to identify the specialty in question, but unfortunately very few took the opportunity to do so. Only in the case of those who cited the paper by Westerhoff, Simonetti, and Van Dam (1981) was the response large enough to allow us to draw a conclusion. Of the 17 respondents who fully answered the specialty question, 16 believed that their work belongs to the same specialty as that of Westerhoff et al. and generally identified the specialty as bioenergetics. The interpretation of the results in the other cases is far more problematic. In the case of those who cited either Sips et al. or Scholte, Schuitema, and Postma (1981), no conclusions can be drawn at all. In the case of Scholte et al. there were only four responses, but they were too diverse to justify even a tenuous guess.[9] The seven respondents who cited Sips et al. indicated biochemistry (3 times), alanine metabolism, hepatocytes, biochemical physiology, and liver chemistry.

We also asked our respondents about their *disciplinary* identities (see Table 3). Not surprisingly, the majority of our respondents identified themselves as biochemists (69% of the 61 respondents who answered this question). Relatively few of them, however, chose biochemistry as their unique disciplinary affiliation (18 respondents, or less than 30%). Fifteen respondents (36%) combined biochemistry with one other field (molecular biology, physiology, medical sciences, or microbiology), and nine chose three or more labels to describe their disciplinary identity (one respondent needed as many as 6 categories).

The proportion of respondents who chose biochemistry differed in the four cases, as did the choice of the combinations and alternatives. Respondents who cited Sips et al., for instance, identified themselves as physiologists

**Table 3. Disciplinary and Specialty Identities of Citing Authors**

| *Disciplinary Identity* | *Westerhoff N = 19* | *Sips N = 22* | *Scholte N = 15* | *Bernstein N = 5* | *Total N = 61* |
|---|---|---|---|---|---|
| Biochemists | 15 (79%) | 15 (68%) | 8 (53%) | 4 (80%) | 42 (69%) |
| Molecular Biologists | 2 | - | 10 (67) | 1 | 13 (21) |
| Physiologists | 5 (26) | 8 (36) | 2 | - | 15 (25) |
| Medical Scientists | - | 9 (41) | 1 | - | 10 (16) |
| Biologists | 3 (16) | - | 2 | 1 | 6 (10) |
| Microbiologists | 2 | - | 6 (40) | - | 8 (13) |
| Biophysicists | 2 | 1 | - | - | 3 (5) |
| Other | 3 (16) | 1 | 3 (20) | 1 | 8 (13) |

NOTE: The total does not add to 100% because of the large number of multiple selections.

(36%) and medical scientists (49%) as well as biochemists (68%). A small proportion of those who cited Westerhoff et al. (26%) saw themselves as physiologists, but biochemistry was mentioned by an overwhelming majority (79%) of the respondents. Among those who cited Scholte et al., the most popular identity was not biochemistry (chosen by 53%), but molecular biology (67%).

Our data on "specialty" and "disciplinary" identities suggests two conclusions:

1. The fact that only about half of our respondents (33) chose a unique disciplinary affiliation and that only 18 (30%) regarded themselves simply as biochemists gives additional support to the claim that disciplinary identities in the biological and biomedical sciences are rather fluid (Studer and Chubin 1980; Whitley 1984). At the same time, however, with one important exception (those who cited Sips et al.), those who cite these papers believe that their work belongs to the same specialty as that reported in the paper being cited. This is so, even when the identification of this specialty "by name" is problematic. Obviously, "specialties" cut across disciplinary identities.

2. Despite the fact that the cited papers originated from the same research lab, the combinations of disciplinary identities chosen by the citing authors were quite distinct for each citing group and can be easily understood if one considers the content of the original papers. For example, the presence of a

**Table 4. Number of Groups Mentioned**

| | *Once* | *2 Times* | *> 2 Times* | *Number of Respondents* |
|---|---|---|---|---|
| Westerhoff et al. | 11 (50%) | 5 (23%) | 6 (27%) | 19 |
| Sips et al. | 16 (69%) | 3 (13%) | 4 (17%) | 15 |
| Scholte et al. | 8 (53%) | 1 (7%) | 6 (40%) | 13 |
| Bernstein et al. | 3 | 2 | 2 | 5 |

large number of "molecular biologists" and "microbiologists" among those who cite Scholte et al. makes perfect sense when we consider the fact that Scholte et al. study genetic control of a sugar transport system in a bacterium. Similarly, the presence of substantial numbers of medical scientists and physiologists among those who cite Sips et al. is perfectly congruent with the fact that their paper examines the regulation of amino acid metabolism in the liver, a physiological process related to various metabolic disorders.

## Speciality Structures

Given, on the one hand, the fact that, except in the case of Sips et al., the overwhelming number of our respondents located their work within the same specialty as the work they cited, and, on the other hand, the fact that their disciplinary identities were far from homogeneous, we might wonder about the extent to which such "citing" groups of scientists can be regarded as coherent and cohesive communities. To what extent do these scientists agree about the importance of the various research groups contributing to the field in question? Did the original papers make reference to the work of the "most important" groups? And how were these papers received by those groups? We can gain some insight into these queries by examining the responses we received to questions asking our respondents to identify the three most important groups working in their area, and by examining the extent to which those groups were identified as the most important in the field cited or were cited by the original papers.

We asked each respondent to list the three most important groups working in their area of research (see Table 4). There were 52 usable responses to this question. Since, as expected, there was no overlap among the responses of authors citing different papers,[10] we will consider the cases separately.

Despite the small number of cases, we believe we can draw some tentative conclusions about the differences among the four cases. Although in every case a substantial number of groups were mentioned only once, there are

significant differences among the four cases; the respondents who cited Scholte et al. reached a relatively high level of agreement about which groups are the most important, as shown in the number mentioned more than twice.[11] A consensus about which groups are important seems lowest in the case of those who cited Sips et al.; not only were 69% of all the groups mentioned only once, but only 17% were mentioned more than twice. In the case of Westerhoff et al., half of the groups considered "most important" were named only once, and a substantial percentage was named twice, so that the six groups mentioned more than twice accounted for only 27% of all the named groups. The small number of responses in the case of Bernstein et al. makes it difficult to draw any conclusions in terms of percentages of groups mentioned only once, twice, or more than twice, though the degree of consensus about important groups was clearly greater than in the case of Sips et al.

The differences among the cases are even more apparent if we consider the percentage of respondents who identified the first three most frequently mentioned groups; the two groups mentioned most often by those who cited Scholte et al. are both named by 85% of the respondents, and the third group is mentioned by 69%. In the case of Sips et al., the most frequently listed group was recognized by only 47% of the respondents, the second-ranking group by 40%, and the third by 33%. In the case of Westerhoff et al., one group was mentioned by 53% of the respondents, one by 47% and one by 26%. In Bernstein et al.'s case, the most frequently mentioned group was selected by 4 of the 5 respondents and another by 3 of the 5.

These differences among the four cases are also apparent when we consider the citations of the most important groups which appeared in our original papers, and the citations of these four papers in the work of those groups listed as most important in the particular research area. The original papers cited the work of *all* the groups listed at least twice as "most important" by those who cited either Scholte et al. or Bernstein et al., and authors from *each* of these groups were among those who cited the original paper from the Amsterdam laboratory. This complete reciprocity observed in Bernstein et al.'s and Scholte et al.'s cases breaks down in the case of Sips et al. and Westerhoff et al. Although Westerhoff et al. cited 9 of 11 groups that were nominated as the "most important" by at least 2 of our respondents, his own paper was cited in the work of only 4 of these 11 groups. In the case of Sips et al., the relation is reversed. While the citations in the original paper included references to the work of only 3 of the 7 groups listed at least two times as "most important," Sips et al.'s paper was cited by scientists from 5 of these 7 "most important" groups.

Although this data justifies the conclusion that the scientists who cited our papers belong to separate and differently structured communities, we should be rather careful in interpreting the meaning of these differences. In the case of Scholte et al., the relatively high degree of consensus about the most important groups, together with the fact that all these groups are both cited by and cited the original paper, seems to warrant the conclusion that the research area to which Scholte et al.'s paper contributed is being pursued by a fairly well- delineated community that is headed by several broadly recognized "elite" groups. Interpretation of the results in the other three cases seems more problematic. A lack of consensus in listing the three most important research groups working in the area might testify to the fact that those who cite a given paper do not belong to a single coherent research area, or might indicate the relatively egalitarian reputational structure within this specific research area, of the presence of some other divisions (such as a lack of cognitive consensus) within the field. In the case of Sips et al., it is the first of these interpretations which is most likely, because, as we already reported, a full 30% of those who cited Sips et al. and responded to our questionnaire felt that they did not belong to the same specialty as the authors of the cited paper.[12] In the case of Westerhoff et al., however, the relatively low degree of consensus about the identity of the most important groups working in the area cannot be explained by the fact that the citing authors did not work within a unique specialty, because, as we have seen, an overwhelming majority of the respondents regarded themselves as working in the same area as the authors they cited, and identified this area as bioenergetics.[13]

A comparison of the disciplinary and specialty affiliations of the citing authors, and of the structure of the communities to which they belonged, suggests the following.

1. The audience of scientists citing a particular scientific paper can be either a part of a fairly homogenous and relatively well defined "specialty group" (Scholte et al., Westerhoff et al.), or alternatively it might constitute a rather heterogenous collection of scientists who address their work to different reputational communities (Sips et al.).
2. Within some specialty groups one can find a relatively high degree of consensus about the reputational ranking of different groups working within the field (Scholte et al.); but the reputational hierarchy in other specialties — even when they are recognized as such by the scientists working in the area — appears to be far less consensual and more diffuse (Westerhoff et al.). These kinds of differences in the organization of research communities are not apparent from the examination of citation data alone, and yet we might well expect them to affect citation rates and patterns.

## Social Links Between Cited and Citing Authors

What is the position of the Amsterdam laboratory and its work within these different communities of scientists? Are the citing authors personally acquainted with the authors of the articles they cited? Do the different structures of the communities citing a particular paper correspond to differences in the social interactions between the cited and the citing authors?

An overwhelming majority of our respondents knew of other work originating from the Amsterdam laboratory (91.1%) and cited it often or very often (87.5%). Only in the case of Sips et al.'s paper did it appear that some of our respondents had never cited another paper from this group (29.2%), or did not know of other research performed by the Amsterdam group (21.1%).

Not only did the citing scientists know and frequently cite the work of the researchers from this laboratory, but many of them also knew the Amsterdam scientists personally (59.3%). Nearly all contacts consisted of meetings at conferences (48.3%) and exchanges of papers or letters (37.9%), although 19% of the respondents had also at some point collaborated with the authors they cited, and more than 22% reported visits to each other's laboratories. Here again, the case of Sips et al. stands somewhat apart; while Scholte and/or his co-authors were personally known by 78.6% of the respondents at the time they were cited, Westerhoff et al. by 68.4%, and Bernstein et al. by 80%, only a third of those who cited Sips et al.'s paper and answered our questionnaire personally knew either him or one of his co-authors. Those who cited Sips et al.'s paper were also the least likely to exchange papers or letters with the original authors, engage in collaborations with them, or visit each other's laboratories.

In the previous section, we showed that the audience of Sips et al.'s paper did not belong to an easily demarcated "specialty group," and that the audience of Scholte et al.'s paper corresponded most closely to an "ideal type" of such a specialty group. This conclusion receives additional support from the fact that Scholte and his co-authors, as well as their work, were known to a very large proportion of those who cited them; that very many of them cited other work from the Amsterdam laboratory either often or very often; and that they were the group most likely to send each other preprints and to visit each other's laboratories. In other words, not only does Scholte's group appear to belong to a well-defined community, but the Amsterdam laboratory seems to be well-integrated and quite central within this community. In contrast, Sips and his co-authors and their other work were the least familiar to those who cited them. A substantial number of those who cited

**Table 5. Respondent Evaluation of Amsterdam Research**

| | *+3* | *+2* | *+1* | *0* | *−1* | *−2 or −3* | *N* |
|---|---|---|---|---|---|---|---|
| Quality | 46.9% | 34.7% | 12.2% | 6.1% | 0% | 0% | 49 |
| Relevance | 44.0 | 28.0 | 22.0 | 4.0 | 2.0 | 0 | 50 |
| Influence | 22.2 | 37.8 | 31.1 | 8.9 | 0 | 0 | 45 |
| Originality | 20.4 | 42.9 | 24.5 | 10.2 | 2.0 | 0 | 49 |

NOTE: For quality: +3 = excellent, +2 very good, +1 good, 0 average, −1 below average, −2 bad, −3 very bad; for relevance, originality, and influence: +3 very high, +2 high, +1 quite high, 0 medium, −1 quite low, −2 low, −3 very low.

Sip et al.'s paper never cited any other work of this group; not one respondent in this case remembered sending a preprint of his paper to Sips or his co-authors, and none of them reported visiting the Amsterdam laboratory or being visited by one of the authors of Sip et al.'s paper.

The data on social links between the cited and the citing authors also supports our earlier conclusion that the audience of Westerhoff et al.'s research was a distinct specialist community, although its reputational structure was more difficult to characterize than in the case of Scholte et al. As in the case of Scholte et al., we find relatively high levels of social interaction between the citing and the cited authors. Westerhoff and his co-authors, as well as their work, are well-known; they are just as likely as those who cited Scholte et al. to exchange papers and letters and to collaborate with those who cited them. Although the respondents in Westerhoff et al.'s case are somewhat less likely than in the case of Scholte et al. to exchange laboratory visits, they appear to meet at conferences much more often than any other group (73.7%). Thus, despite our difficulty in establishing a clear reputational hierarchy within this specialty, the group around Westerhoff, like that around Scholte (and probably also Bernstein), seems to be socially well-integrated into the community of scientists working in this area.

## Evaluations of the Amsterdam Laboratory

Personal acquaintance with the researchers from the Amsterdam laboratory was not correlated with the evaluation of the quality of this work; there was no difference between the evaluations of those who knew the Amsterdam researchers and those who did not. Scoring on a 7-point Likert-type scale for the "quality," "relevance," "originality," and "influence" of the research performed in Amsterdam, our respondents evaluated the research done in

Amsterdam very highly, particularly in terms of its quality and relevance, although the laboratory also scored high on influence and originality.

Correlations (Kendall's Tau-B) among these measures are highly significant (all $p < .01$), and rank from .77 between influence and originality, to .40 between quality and relevance.

However, if we disaggregate these results according to the four cited papers, we again find significant differences in the evaluation on the four scales. Furthermore, the correlations between the indicators sometimes disappear at subgroup level.

While the work of Sips and Scholte, and their co-authors, is rated very highly in terms of its quality, the work of Westerhoffet al. is rated highest in terms of its relevance. Because of its low rating on influence and its high rating on relevance, the correlation between these variables disappears in the Westerhoff et al. case, ($T = .21213$; $p = .1773$). Correspondingly, the correlations between influence and quality, in the case of Sips et al., and between relevance and quality, in Scholte et al.'s case, are smaller than .3 and not significant ($p > .05$). (In the case of Bernstein et al., we again have a rather low rate of response, but our few respondents judged the Amsterdam research in this area lower than judgments of the other groups of citing authors.)

Although the disappearance of correlations is enhanced by the smaller group sizes when the cited authors are assessed separately, since some of the other correlations are not affected—for example, quality and originality correlate for all groups significantly—these results indicate that there is underlying heterogeneity among the respondent groups. The convergence at the aggregate level is to some extent an artifact of the grouping of papers assessed for the most part positively. The next question, therefore, is: what makes the reception of the work of these four research groups so different?

## Reception of the Paper and Its Impact

Using discriminant analysis, with the cited reference as the grouping variable and the other variable as discriminating,[14] group-membership is correctly predicted in 104 out of the 107 cases (97.2%). Classification is 100% correct for the responses by authors citing Sips et al. or Scholte et al.; in the case of citations to Bernstein et al.'s paper only one of the 14 cases is misclassified (as a citation of Scholte et al.), and in Westerhoff et al.'s case two of the 38 cases are misclassified (see Table 6).

This result suggests that the cited reference is a major source of variance. For 28 of the 72 variables (39%) that can be relevantly assessed in this way,

**Table 6. Classification Results using Discriminant Analysis**

| Actual Group | Number of Cases | Predicted Group Membership 1 | 2 | 3 | 4 |
|---|---|---|---|---|---|
| Group 1 | 38 | 36<br>94.7% | 0 | 1 | 1 |
| Group 2 | 31 | 0 | 31<br>100.0% | 0 | 0 |
| Group 3 | 24 | 0 | 0 | 24<br>100.0% | 0 |
| Group 4 | 14 | 0 | 0 | 1 | 13<br>92.9% |

NOTE: Percent of "grouped" cases correctly classified: 97.2.

the chi-square of the relation with the cited article is significant. These questions include most of the cognitively most interesting questions of the survey, such as whether the reading of the cited article "affected the way you did your research" (question 7A) or "modified the argument you wanted to make" (7D). On the other hand, differences were not significant when questions focused on how the citing author proceeded with the text of the cited article, for example, whether he or she had read the entire article or only the abstract; whether a draft or reprint was sent to the cited authors (only exceptionally); or whether the citing author thought that the cited authors should have noticed his or her work for their own research (almost always).[15]

What did the scientists mention as their cognitive reasons for citing these articles? In question 9, we asked the citing authors whether they cited the paper because it offered them a useful comparison, provided support for their conclusions, allowed them to broaden their claims or arguments, was a contribution to the discussion, and so on (see Appendix). Since we wanted to be able to compare our ratings of the uses of these texts in arguments to the respondents' cognitive reasons for citing them, the range of options we offered in the questionnaire corresponded to our classification of textual functions. Generally speaking, we distinguished among citing the paper as a means of contextualization, referring to it for agenda-building purposes, or citing it as a warrant for a particular conclusion.

Since the reasons why someone cites do not necessarily have to correspond to the actual use of the citation in the text, the comparison we performed cannot be regarded as a kind of "validation" of our previous study, or vice versa. Instead, we were interested in the extent to which the author's motives and reasons for citing would correspond to the functions of citations in the textual discourse.

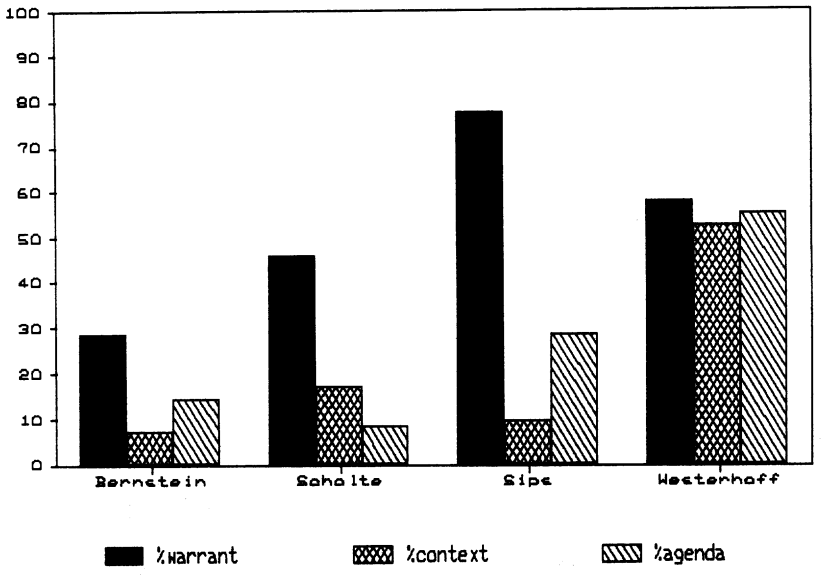


**Figure 1. Various reasons for citations given by citing authors.**

As might be expected, we found both similarities and differences.

First, it appeared that the citing authors themselves were far more likely than we had expected to mark the relatively diffuse "contribution to the discussion," which we had intended only for those cases in which the citation was chosen merely for the demarcation of the context to which the citing paper properly belongs. This was, however, clearly not the interpretation given to this phrase by most of the respondents. Many of them claimed to have cited the paper as a "contribution to a discussion," even when in the text of their articles they very clearly used a specific bit of information from that paper as a warrant which allowed them to draw a conclusion or advance a specific claim (such as using the molecular weight in order to identify a protein).[16] It appears that, in identifying their reasons for citing a particular paper, the citing authors were more likely to focus on the overall importance of that paper for the area in question and on the kind of significance it had for them in general, rather than on their specific use of the claims made in the cited article in the arguments presented in their own papers.[17]

However, the distribution of the answers to options other than "contribution to the discussion" in question 9 are all significantly related to the "cited reference" involved, and this suggests specificity. If we leave the one non-

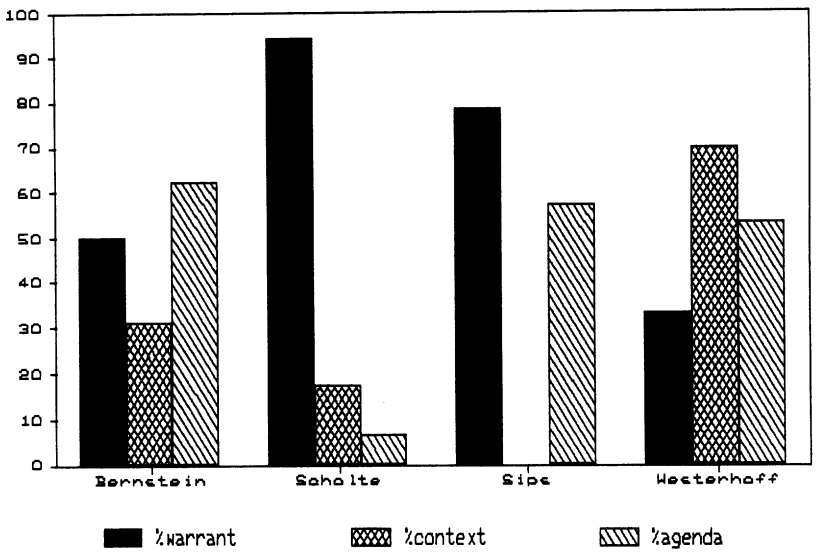


**Figure 2. Various functions of citations in text.**

discriminating option out of consideration, we are able to construct Figure 1, which can be compared to Figure 2 from our previous study. The agreement between the results of our analysis of the textual uses of the citations and the reasons for citing reported by the citing authors is, however, qualitative rather than quantitative. For example, in both cases, citations of Westerhoff et al.s paper ar often categorized as providing a summary or background ("context"), while references to Sips et al.'s paper are mostly classified as "warrants." However, there are important differences as well.[18] The citing authors rarely regard either Sips et al.'s or Bernstein et al.'s papers as setting a research agenda, while a textual analysis suggests that they have often been cited precisely in order to raise research questions. Similarly, while, textually, Scholte et al.'s paper was often cited as a warrant for specific conclusions, the citing scientists claimed to have cited it for this reason only relatively rarely. Moreover, if we pursue the analysis not at the level of the four groups but on a case-by-case basis, the results of the two types of analysis no longer correspond.[19]

More detailed statistical analysis has suggested that the major reason for these differences is related to more complex patterns of interaction with other "cognitive" variables. For example, answers to question 7A ("whether the cited article affected the way you did your experiments") are strongly

correlated with responses to some of the options in question 9. Those who claimed that the cited article affected the way they performed their experiments were also likely to claim the article served as a warrant or a critical agenda-building device. Apparently, varying relations between papers and research practices underlie the differences in scientists' perceptions of why they cite a particular paper. Textual analysis alone is obviously not a good source of information about research practices and the significance of scientific articles in these practices.

The cited paper was rarely reported as affecting the conduct of experiments. Only Sip et al.'s and Bernstein et al.'s papers were said to have affected experiments in a substantial number of cases (29% and 21%, respectively). Nor were the cited papers believed to have had a great impact on the focus of the argument made in the citing paper. A more substantial number of the citing authors claimed that the paper they cited affected the interpretation of their own results (26.2%, not significantly different for the four groups). A large proportion of the respondents to this question (which aimed at distinguishing the influence of the cited papers on the construction of the discursive argument from their impact on the research process) checked the option "other," and many of them took the opportunity to comment. The comments did not, however, refer to the impact of the article on either research or interpretation (one respondent specifically said that the cited paper "had no effect on research or interpretation") but rather to its *use* in the citing paper. For example, many respondents referred to the fact that the article served as a support or confirmation of some point the author wanted to make, or that it reinforced his or her conclusions (comments read, for example: "it substantiated our interpretation of the data," it was a "good confirmation of my supposition," "it supported a point," or it "confirmed our results"). A few respondents claimed that the cited article provided a context or a background for the citing paper.

In summary, despite the fact that our own textual analysis did not agree with the reasons for citing provided by our respondents, both types of analysis demonstrated that the perception of the significance of the four papers and their textual uses in argumentation were distinct and directly related to the manner in which the claims made in the original paper were given significance by being integrated into specific evidential contexts.

Second, although both our textual analysis and the analysis of the reported reasons for citing clearly reveal that the four papers were perceived as having different kinds of significance, it is a mistake to treat textual functions of citations in arguments as equivalent to reasons for citing.[20] Reported motives and the subjective reasons given by authors for their citing should be

distinguished from the various cognitive functions which citations play in argumentation presented in scientific articles. We will return to these points in the conclusions.

## Evaluations of the Cited Paper

Just as the differences in the reasons for citing the four papers were not reflected in the numbers of citations these papers received, so also citation counts do not reflect the citing authors' judgments about the validity and importance of these papers either when they were cited or later. The papers we have selected were cited relatively frequently, but were the claims made in them judged to be correct? How well have they withstood the test of time? Did they continue to be important? Would they still be cited today?

We asked our respondents whether they believed the claims in question to be correct, in need of modification, or incorrect. Although only two respondents believed that the claims made in any one of the papers were simply wrong, there were significant differences in the evaluation of the four papers in this respect. While the paper by Westerhoff et al. was the most heavily cited of all the papers we studied, only a minority (36.8%) believed that the claims made in this paper could stand in their original form, 52.6% believed that they needed modification, and 10.5% believed they were incorrect. The papers of Bernstein et al. and Scholte et al, despite the fact that they were cited somewhat less frequently, were regarded as correct by all respondents; and although nobody claimed that the paper by Sips et al. was incorrect, 20% believed that the claims made in this paper needed some modification.

Moreover, it appears that most of those who believed the cited paper to be correct (and thus the majority of our respondents) also continued to regard it as important and believed that it would also be cited today. Assessment of importance is indeed significantly related to the estimated likelihood that other scientists writing on a similar topic would cite the article today. However, we found no significant relation between the questions of whether the knowledge claim was thought to be correct and whether it was still recommended for citation.

Of course, it should not be surprising that an important paper might need modification, or that a paper the claims of which are believed to be correct, might no longer be considered new enough to be cited, or even that some might continue to cite papers that they believe need modification. But, just as different papers are used for different purposes when they are cited, so also

these complicated relations among importance, validity, and citing behavior suggest that any interpretation of simple citation counts as indicators of impact, quality, or importance must be considered problematic.

## Conclusions

In the introduction to this article, we presented a scheme of citing versus cited and authors versus texts. We argued that the various dimensions of citations that can be distinguished on this basis do not necessarily have to coincide, or even exhibit, congruence. Indeed, we have shown in our limited sample that the relations established by citations are more complex.

The first conclusion our material allows us to draw concerns the use of citations in science studies as indicators of institutional performance. Although the four articles whose citations we studied were written by scientists working within a single laboratory of biochemistry at one university, although all of them belonged to the same general area of bioenergetics and transport, and although each was cited approximately the same number of times, we found systematic differences in the structure of the communities to which these papers were of relevance, in the social relations between the citing and the cited authors, in the perception of each paper's importance, and in the reasons scientist gave for citing them. Though we were systematically comparing "like with like," we found nothing but differences.

At the same time, these differences could be related systematically to the contribution the cited paper made to a specifically structured problem area on which a group of scientists have been working.[21] Some of these groups formed rather clear specialty communities with definite cognitive and social identities, high levels of interaction among their members, and extensive agreement on the reputational structure within the specialty. Others were more heterogeneous, their identity was far less clearly defined, or they appeared to be substantially less integrated, and in some there was also less agreement about the reputational structure. Obviously, the reception of a scientific paper will be influenced in some measure by such differences in the structure of the communities to which a paper is of relevance.

The recognition of such differences between different kinds of fields was the main reason for the insistence of many citation analysts on the point that citation rates can be meaningfully compared only among groups working in the same areas. Our analysis suggests, however, that the criterion of what constitutes sufficiently similar groups is itself problematic. Despite the fact that the four papers we compared originated from the same small laboratory,

they were clearly addressed to four distinctly structured communities. The local institutional identity of the papers' origin is not sufficient to assure the similarity of their audiences, nor does it provide clues for explaining the papers' perceived significance and reception.

A second question in the sociology of science that we may address with some of our material is that of how to conceptualize the organization of research at the level of scientific fields. Some have contended that this organization should be analyzed as a reputationally controlled work organization, organized at the level of scientific communities (Whitley 1984). Again, we did not find one scientific community of biochemists with a shared pattern or work organization, but a variety of social structures at a lower level than that of the specialty involved ("bio-energetics"). While one or two of these communities were indeed firmly integrated, and therefore probably controlled reputationally, integration and reputational hierarchy remained more problematic in the other cases. In the case of the authors citing Westerhoff et al., we notice a state of continuous crisis at the research front, a state which is reflected in a lack of clear reputational hierarchy in this area. In the Sips et al. case, we found an instance of a knowledge contribution that indicated interdependencies among researchers who did not work in precisely the same research area, and who therefore, according to several indicators, could not be considered as members of the same specialty group. We think that in this case the paper's significance is not even mediated by organization at the level of communities.

Therefore, we would like to speculate that rather than the social organization of science explaining the cognitive organization, the social organization is part of, and perhaps even constituted by, the specific problems that are addressed at the research front. When research problems are stable—one might say "paradigmatic"—they give rise to stable communities that sometimes manage to agree on standards and past performance in the area. However, in other cases, contributions can be acknowledged almost purely for cognitive reasons.[22]

The results of our questionnaire also suggest that the impact of a cited paper on the conduct of research or on the interpretation of results is often slight, that it differs from case to case, and that it is not related directly and unequivocally to how the citing authors themselves evaluate the results of the paper they cite. The citing authors appeared to cite often for rather diffuse reasons (such as "it was a contribution to the discussion"), even if the sentence that was actually cited was fairly specific and played a definite role in the argumentation of the citing paper. This difference between the scientists' reasons for citing and the textual role of the cited text in the argumen-

tation of the citing paper is an important analytical distinction which cannot be ignored in the analysis of citation behavior on the one hand and of the cognitive role of citations in scientific literature on the other (Mulkay et al. 1983; Leydesdorff 1987b; Cozzens 1989).

Our results suggest that papers are perceived as having different kinds of significance, and these perceptions do not correspond directly to the functions that the cited texts play in the argumentation of citing texts. Scientists appear to regard the papers they cite as having a definite, though rather generalized significance, to treat them in a sense as "concept symbols" of particular types of contribution (Small 1978). These perceptions are distinct for different papers and reflect the scientist's perception of the field's cognitive organization, its research agenda and background. These perceptions of the paper, however, do not correspond clearly to the distinct textual uses to which the papers are put when they are cited. The disjunction between these two levels of analysis—of the cognitive organization of the field as evidenced by textual analysis and of the actors' perceptions of this organization—has important methodological consequences not only for the use of citations as indicators of the quality or impact of scientific publications, but also for various approaches adopted in sociological analyses of science.

Finally, in relation to citation analysis as an instrument for science policy, we are able to narrow the formulation of our main conclusions down to the following points. First, whether or not a scientific contribution will be cited seems to depend, in the first instance, on whether *citing* authors can *use* the reference in their texts.[23] Whether they, in turn, will be able to do so depends on the current development of the field; hence, citations are not a valid indicator of the quality of cited papers at the moment they are published. Second, when using citations as indicators of social structure (i.e., reputational hierarchies or group networks), one is not always allowed to infer from relations among aggregates of documents to attributes of authors or institutions (MacRoberts and MacRoberts 1986, 1987a, 1987b).

## APPENDIX

*Your Article*

1. Did your article constitute

( ) a report on a part of a more extended project you were involved in
( ) a sideline with respect to your main research interest
( ) a part of your doctoral dissertation
( ) a part of a dissertation you were supervising

2. Did you send a draft or a preprint of the article to any one of the authors of this reference?

( ) yes, ( ) no, ( ) unsure

3. Did you consult anyone else about this reference?

( ) yes, ( ) no, ( ) unsure

4. Do you think that the authors you cited should have noticed your article for their own research?

( ) yes, ( ) no, ( ) unsure

5. To which journals did you submit the article before having it accepted for publication? (In historical order)

_____
_____
_____

*The Reference*

6. Did you read the cited article

( ) before you performed the experiments reported in your paper
( ) before you wrote your first draft
( ) after the first draft was finished
( ) during the publication process
( ) don't remember

7. Did reading the cited article

( ) affect the way you did your experiments
( ) affect the interpretation of your results
( ) force you to change the emphasis of your paper
( ) modify the argument you wanted to make
( ) other _____

8. Did you cite this article for

( ) its theoretical contribution
( ) its methodology
( ) its empirical data
( ) other _____

9. Did you cite the article because

( ) it gave additional support for the claim you were trying to make
( ) it allowed you to broaden your claim or your argument
( ) it raised possible objections to your claim or argument
( ) it served as a useful comparison
( ) it raised important questions
( ) it gave a summary of a certain sort of argument or position
( ) it was a contribution to the discussion
( ) any other reason _____

10. Did you ever try to replicate any of the experiments reported in the paper you cited?

( ) yes, ( ) no, ( ) unsure

If yes, did you succeed?

( ) yes, ( ) no, ( ) unsure

11. How often do you cite the work of this author(s) or his group

( ) very often (in most of your papers?)
( ) occasionally (in several papers)
( ) this was the only time

12. Do you think other scientists writing on a similar topic would be likely to cite this article?

| | |
|---|---|
| at that time | ( ) yes, ( ) no, ( ) unsure |
| today | ( ) yes, ( ) no, ( ) unsure |

If so, could you specify why?

*You as an Author and Your Field*

13. Did you ever publish in the journal in which the cited article was originally published?

( ) yes, ( ) no

14. Do you regard this journal as central for work relevant to your research interest at that time?

( ) yes, ( ) no, ( ) unsure

15. Do you see yourself as a

( ) biochemist
( ) molecular biologist
( ) microbiologist
( ) biologist
( ) chemist
( ) physiologist
( ) medical scientist
( ) other _____

16. Do you work at a research group which is primarily engaged in

( ) biochemistry
( ) molecular biology
( ) microbiology
( ) biology
( ) chemistry
( ) physiology
( ) medical sciences
( ) other _____

17. If you don't regard yourself as a biochemist, is biochemistry one of the knowledge sources for you?

( ) yes, ( ) no, ( ) unsure

18. Do you think that your work is relevant to the authors of the cited article so that, for example, they would be likely to cite it?

( ) yes, ( ) no, ( ) unsure

19. Could you list the three most important groups working in the research area to which your article belongs

_____
_____
_____

20. Could you list three journals in which the most important work in your field is likely to be published?

_____
_____
_____

*The Cited Article*

21. How did you learn about the existence of this article?

( ) from a preprint sent by the author
( ) by reading the journal
( ) from an index or documentation service
( ) from a colleague
( ) at a conference
( ) by reading another paper in which this article was cited
( ) from a remark of a referee
( ) other _____

22. When you cited the article did you consider it important as

( ) a theoretical contribution
( ) a methodological or instrumental development
( ) an empirical contribution
( ) a summary of important work in this area
( ) other _____

23. Do you regard the article as belonging primarily

( ) to your specialty, i.e. _____
( ) to a different specialty, i.e. _____

24. With hindsight, do you still consider the article to be important?

( ) yes, ( ) no, ( ) unsure

Why (not?)

25. Do you think that the main knowledge claim in the cited article was

( ) correct
( ) needed modification or specification
( ) wrong

26. Did you actually read

( ) the entire article
( ) the relevant sections
( ) only the abstract
( ) the reference came from one of the co-authors
( ) it was a cross-reference

*The Author and His Institute*

27. Did you personally know any of the authors of the article when you cited the work?

( ) yes, ( ) no, ( ) unsure

28. Have you met any of the authors of this article since that time?
( ) yes, ( ) no, ( ) unsure

29. Did you ever have more extensive contact with one of the authors?

( ) meet at conferences
( ) exchange papers/ letters
( ) collaborate
( ) visit each others laboratory
( ) other _____

30. Do you know other work of this Amsterdam group?
( ) yes, ( ) no, ( ) unsure

If so, how do you judge this work with respect to

a. its *quality?* ( ) can't judge

excellent . . . very good . . . good . . . average . . . below average . . . bad . . . very bad

b. its *relevance* for your specialty ( ) can't judge

very high . . . high . . . quite high . . . medium . . . quite low . . . low . . . very low

c. its *influence* on other work ( ) can't judge

very high . . . high . . . quite high . . . medium . . . quite low . . . low . . . very low

d. its *originality* ( ) can't judge

very high . . . high . . . quite high . . . medium . . . quite low . . . low . . . very low

## Notes

1. "Behind the practical achievements (of citation analysis) lies an unresolved epistemological question, which is responsible for the hairline cracks in the intellectual superstructure."

2. Note that at the structural level, aggregates of citations can correspondingly be used for various forms of mapping:

| | Citing Groups/Institutes | Citing Document Sets |
|---|---|---|
| Cited groups or institutes | sociometric networks | hierarchies |
| Cited document sets | e.g., co-citation maps | e.g., journal mapping |

The question of how individual citation behavior is to be conceptualized in relation to networks of citations as indicators of structure merits a separate, more methodologically oriented study. See also Burt (1982).

3. The substantive validation of scientometric constructs is, of course, in itself an important subject in science studies. However, we have found in the literature no systematic attempts to evaluate how and why a particular citation relation plays a role in the four distinct dimensions, i.e., as relations between cited and citing author, and cited and citing text.

This lacuna was also noted by Cronin (1981, 21). In the field of information retrieval, citations have been reviewed extensively as carriers of information for practicing scientist (Cronin 1984). The focus in these studies has mostly been on the reasons why citing authors select references. The first study to our knowledge was by Westbrook (1960). For recent studies see Brooks (1985, 1986) and Vinkler (1987). The emphasis on "practicing scientists," i.e., citing authors, is also fundamental to co-citation maps; see e.g., Small and Sweeney (1985); McCain (1986). In addition, research has focussed on the validation of co-citation studies and of other classificational schemes (e.g., Mullins et al. 1977.) Bibliometric links by citations are the core of a set of studies, which are usually labeled "citation context analysis" (Moravcsik and Murugesan 1975; Chubin and Moitra 1975; Latour and Woolgar 1979; Cozzens 1985). Sociometric links between cited and citing researchers have been studied by Murray and Poolman (1982).

4. For an earlier sociological study of this debate, see Gilbert and Mulkay (1984).

5. The four articles are: Sips et al. (1980), Westerhoff et al. (1981), Scholte et al. (1981), and Bernstein et al. (1981).

6. This happened in 33 cases, and in 4 more cases we thought it appropriate to handle the response in this way. There were 19 partial responses on Westerhoff et al.'s article (50%), and 8 (out of a total of 14) partial responses from authors citing Bernstein et al.'s article.

7. In one case, we received notice of the death of the respondent; in several cases the questionnaire was returned as undeliverable. One Dutch respondent, who had cited the Bernstein et al. article five times, angrily refused to respond. In one case, an Italian respondent added to the margin of his questionnaire: "Despite the letter of recommendation by Prof. Van Dam, this has to stop!"

8. While in the other cases citations by Dutch colleagues are exceptional (zero in the case of Sips et al., 1 in the case of Scholte et al., and 2 in the case of Westerhoff et al.), a substantial percentage of the citations in the Bernstein case (8 out of 16) was provided by members of the Cell Biology & Morphogenesis Unit of the Zoological Laboratory at the University of Leiden. Although we do not believe this to be the case, we cannot exclude the possibility that the results in this case have a national bias.

9. "Transport," "molecular biology," "bacterial phosphotransferase systems," and "sugar uptake systems" were each mentioned once.

10. The original Amsterdam laboratory, mentioned often by the respondents, could be considered an exception, but even here the names of the principal researchers were different: Van Dam and/or Westerhoff were mentioned in the case of Westerhoff et al.'s article; Postma in the case of Scholte et al.'s article, and Sips and/or Tager in the case of Sips et al.'s article.

11. This high degree of consensus becomes even more pronounced if we disregard the answer of a single scientist who listed nine groups instead of the three we asked for. In that case, the number of groups mentioned is reduced to 10, of which 2 are mentioned twice, and 4 more than twice.

12. It was also this group of respondents that felt least sure as to whether the cited authors would notice the citing paper for their own research or cite it in their work. Whereas 100% of the respondents in the case of Bernstein et al. and Scholte et al. felt that the authors they cited should have noticed the citing paper for their own research, only 74% of the respondents who cited Sips et al. expected this kind of reciprocity.

13. On the basis of the data from the questionnaire, however, it is impossible to ascertain whether the dissensus about the "most important groups" is in this case a result of the lack of a pronounced stratification within a relatively large field or a result of some other – cognitive or social – divisions, which account for the relative lack of agreement among bioenergetists concerning the three "most important groups" working in the field.

Both the literature in the field and the interviews we conducted suggest that bioenergetics is indeed in a state of continuous crisis, and that the lack of consensus about the most important groups might be a result of cognitive disagreements within bioenergetics. Although the chemiosmotic theory proposed by Mitchell serves as a framework for most of the work being done in the area, a number of alternative interpretations of this theory are being pursued, and the scientists involved adopt different and not always consistent experimental approaches and formal interpretative methods.

14. Although discriminant analysis is only mathematically defined for interval variables, in the case of dichotomous variables most evidence suggests that the linear discriminant function may perform reasonably well (e.g., Gilbert [1968], Moore [1973], SPSS [1976]).

15. For example, if we take as discriminating variables only the replies to the first nine questions, i.e., the ones in which we asked respondents to reply for each case separately because of their cognitive character, the percentage of correctly classified cases is already 81.25% (approximately 3%).

16. For example, in Waygood et al. (1984, 144). The reference to Scholte et al.'s article appears in the subsection entitled "Molecular Weight Assessment" and reads: "Molecular weight standardization of the frozen gel was made using enzyme . . . III glc, 20K (ref. to Scholte et al., and to another paper reporting the weight) . . . as phosphorylated standard." In the text, the reference obviously serves the specific function of validating the identification of a protein as enzyme IIIglc, and thus as a warrant for the conclusion "this is IIIglc," whereas at least one of the respondents in this case saw its function as a "contribution to the discussion."

17. In one of the papers citing Sips et al. (Hayes and McGivan 1982, 365), the reference to Sips et al. occurs in an introduction and clearly serves to establish the importance of the problem the paper will address: "It has subsequently been shown that the transport of alanine into hepatocytes can limit alanine transport under certain conditions (Sips et al., 1980), *so it is important* that the effect of starvation on alanine transport be quantified." Despite the fact that the paper is in part an attempt to provide just such a quantification, so that the citation of Sips et al.'s claim seems to serve as a legitimation of the problematics raised in the text, one of the authors of the article claimed to have cited that statement in this paper not because it "raised important questions" but because "it gave a summary of a certain sort of argument or position."

18. The underlying matrices for the two pictures correlate at .48 (not statistically significant).

19. It is not possible to classify a majority of the cases using only the answers to questions 9A to 9F as discriminating variables in a discriminant analysis: when the Scholte et al. article is the grouping variable, 75% of the cases are misclassified, against only 29% for the Sips et al. article.

20. See also Mulkay et al. (1983).

21. See also Collins (1985).

22. See also Vinkler (1987), who estimates from his data that 81% of the citations are given for exclusively professional reasons, 2% for exclusively connectional reasons, and 17% as a result of interaction between the two factors.

23. Using citations as an indicator for citing practices is in accordance with the philosophy behind co-citation analysis. See also Small, Sweeney, and Greenley (1985).

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