BLAISE CRONIN

the citation process

THE ROLE AND SIGNIFICANCE OF CITATIONS IN SCIENTIFIC COMMUNICATION

TAYLOR GRAHAM

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Author : Blaise Cronin

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Introduction

This essay takes as its starting point the view that science* needs to be looked at as a large social system. If we are to understand how science works, then we have to know something about the norms and values which guide and constrain the actions of individual scientists. In other words, it may be necessary to look beyond the 'storybook' image of science as a selfless and dispassionate search after truth; a search in which the individual scientist subjugates personal gain and vanity to the greater communal good. Recent sociological investigation has provided us with a 'relativist' account of science derived from observed behaviours, which is far removed from the 'storybook' or mythopoeic conception (e.g. Knorr-Cetina and Mulkay, 1983).

Science is a social process. The actions and behaviours

*The term science will be used throughout as a convenient shorthand for the formalized and institutionalized process of systematic investigation, knowledge creation and research dissemination, both in relation to the natural ('hard') sciences and the social ('soft') sciences. The undifferentiated use of the term does not mean that some kind of unifying structure is ascribed to science. As Ellis (1969) notes, science is a complex, fragmented and amorphous activity — technically, politically and socially. There are, in addition, different types of scientific research subsumed under the rubric: basic and applied; theoretical and problem-solving; academic and techno-commercial. The existence of different occupational roles and motivations is therefore implicit in the use of the term science.

of scientists are context-dependent. To understand this process we need to understand settings and personalities, whether we are looking at the ways in which scientists 'do science', or the ways in which they communicate the results of their professional endeavours.

The primary communication system is the principal mechanism whereby the scientific establishment records and 'rubber stamps' the individual and collective achievements of its members. One function of the system is to disseminate knowledge; a second to ensure the preservation of standards; and a third to distribute credits and recognition to those whose earlier work has contributed to the development of ideas in different fields. In some respects the scholarly journal is the scientific establishment's ledger of achievement and roll of honour wrapped in one.

The most common means of bestowing credit and recognition in science is via citations. With the emergence of commercially available citation indexes in the course of the last two decades, the significance of citation in the professional lives of career scientists has taken on a new dimension. Citation indexing rests squarely on the assumption that citations can be treated as units. Aggregate these units, so the theory goes, and one can arrive at a measure of an individual's or group's contribution to the growth of knowledge in a particular subject field. Implicit in this is the assumption that authors' citing habits display conformity and consistency.

In the 'storybook' account of how science works, citation is seen as a serious acitivity, governed by a tacit understanding of how and why authors should acknowledge the works of others. That is to say: an author's reasons for citing in a particular way at a particular time are controlled by an internalized set of norms. This essay sets out to test and explore the validity of this assumption.

Writing in the 1950s, Merton commented wryly on the fact that very few sociologists or historians of the

twentieth century could bring themselves to treat science as 'one of the great social institutions of the time' (Merton, 1957 : 635). In the intervening years the situation has improved with the gradual emergence of Social Studies of Science as a speciality in its own right (MacLeod, 1977).

Perhaps the simplest and most economical way of introducing and explaining the notion of science as a social system is to quote Merton's summing up from his paper *Priorities in scientific discovery: a chapter in the sociology of science*:

> 'Like other social institutions, the institution of science has its characteristic values, norms and organization. Among these, the emphasis on the value of originality has a self-evident rationale, for it is originality that does much to advance science. Like other institutions also, science has its system of allocating rewards for performance of roles. These rewards are largely honorific, since even today, when science is largely professionalized, the pursuit of science is culturally defined as being primarily a disinterested search for truth and only secondarily a means of earning a livelihood. In line with the value-emphasis, rewards are to be meted out in accord with the measure of accomplishment. When the institution operates effectively, the augmenting of knowledge and the augmenting of personal fame go hand in hand; the insitututional goal and the personal rewards are tied together.' (Merton, 1957 : 659)

Several writers, scientists and sociologists of science have used the 'Game' metaphor to describe the practice of science and to explicate the underlying rules and regularities which govern the players' (*sic* scientists') actions (e.g. Mitroff, 1974A). In Mitroff's view:

> 'Current written formal accounts of the Game (whether by scientists in scientific papers or by philosophers of science in treaties on the Game) bear as little relation to the actual conduct (plays) of the Game as high school civics texts bear to the actual workings of the political system. Personal elements are integral parts of the Game. As such they deserve to be incorporated into philosophical accounts of the Game to a far greater degree than

they have previously been recognised and acknowledged.' (Mitroff, 1974A : 255)

More specifically, Leopold (1973) identified the 'Citation Index game' as one of the stratagems employed by scientists to increase their visibility among their peers. The 'Game' metaphor has usually been invoked to counter the 'storybook' idea of science as an idealised, dispassionate and selfless quest after truth and knowledge, in which personal feelings and motivations are held in check by institutional imperatives. The metaphor is more than a gratuitous irreverence; its use is intended to inject a measure of realism into the prevailing conceptions of what science entails. The problem with the 'storybook' version of how science operates is that it does not easily accomodate the typical scientist's striving after peer group recognition — one of the trophies of the 'Game' (Mahoney, 1976). Taking the 'Game' metaphor one step further, Gaston compared competition in science with 'a race between runners in the same track and over the same distance at at the same time' (Gaston, 1971 : 472). The difference between track events and science, however, is that in the former there are medals for the runners-up. In science this is not so: originality alone is rewarded. Many run, but few are acclaimed.

Those who propound normative theories of science are faced with the problem of reconciling the idealised conception of science with the behavioural reality, and of articulating the relationship between institutional and personal norms. Merton, for example, has spoken in terms of value clashes and internal contradictions (Merton, 1957; Merton, 1963). Our aim will be to consider the extent to which authors' citation practices are norm-regulated, and to what extent they are influenced by extra-scientific factors. This objective relates directly to ideas put forward by Kaplan in the mid-1960s, when he proposed that citation should be

viewed as a social control mechanism (Kaplan, 1965).

Anecdotal evidence seems to favour the idea that many authors cite in reflexive fashion, without necessarily dwelling on the implications of the practice. To put it another way: authors may not be clear in their own minds why it is that they cite the way they do, or how ciatation relates to the ideology of science — 'the norms and values presupposed in the conduct of science' (Tranøy, 1980 : 191). Citation is therefore a potentially useful starting point for an investigation into the degree of congruence between the empirical reality of science and the 'storybook' conception of science as a dynamic, norm-regulated activity. To quote West:

> 'It is commonly believed that persons engaged in scientific research adhere to a set of moral values representing ideal types of behavior which facilitate the production of new knowledge. However, the many listings of these which may be found in the literature range from the intuitive to, at best, speculative. Few attempts have been made to discover whether a majority of scientists actually holds them as they are stated.' (West, 1960 : 54)

In a recent paper, Gilbert and Mulkay (1982) put forward the thesis that the language used by scientists is largely context-dependent. Focusing specifically on formal research reports, they noted that when scientists are in what might be termed systematic accounting mode, the style and character of the language is deliberately impersonal and restrained ('storybook' language). In a series of in-depth interviews they found a wealth of evidence to support their hypothesis that scientists move from informal to formal accounting as experimentation leads to publishable findings. The authors note that this stylistic transition is hallmarked by the absence of reference to the dependence of experimental observation on theoretical speculation, to the degree to which experimenters are committed to specific theoretical positions, and to the influence of

social relationships on scientists' actions and beliefs. Elsewhere, they characterise these two interpretative repertoires as the 'contingent' and the 'empiricist', respectively (Mulkay and Gilbert, 1981).

Citation, to use their terminology, is part of the formal accounting process of science. Yet, like the language of the scientific paper, it is contingent upon unrecorded and unexpressed attitudes, relationships and experiences. In their view, the actions and behaviours of scientists are shaped by contexts, and can only be properly comprehended through a process of social accounting (Gilbert and Mulkay, 1980; see also Mulkay, 1969).

It was Kaplan (1965) who pointed out that, although many readers and writers of scientific papers had at least partial awareness of the technical functions of citations, little was known about either the operating norms of actual practice, or the sociological functions of such norms. It is no exaggeration to say that Kaplan's early speculations gave impetus and direction to a debate which has matured greatly since then. Before considering further the views of Kaplan, it may be helpful to set his remarks in their historical context.

It is quite conceivable that citation would not have emerged as a serious 'academic' issue for sociologists and historians of science had not the commercial development of citation indexing proved so successful (Garfield, 1979; Hall, 1970; Narin, 1976)*. The practical utility of citation-based information retrieval systems has meant that citation has in a short time become a topic of great

*'A citation index is an ordered list of cited articles, each accompanied by a list of citing articles. The citing article is identified as a source, the cited article as a reference. The Science Citation Index (SCI), published by the Institute for Scientific Information is the only regularly issued citation index in science. It is prepared by a computer and provides an index to the contents of every issue published during a calendar year of more than 2,000 selected journals. Journals covered by the index are chosen by advisory boards of experts in each of the topics represented and by large-scale citation analyses.' (Garfield, 1970 : 669)

interest and importance to the scientific community at large. Scientists have begun to look more closely and critically at a practice which, previously, had hardly been deemed worthy of serious attention. Prior to this, citation was only mentioned when a serious breach of etiquette or a priority dispute arose. As with many codes of behaviour, or social rules, it is only when an infringement occurs that the underlying issues and assumptions are called into question. However, if citations are to be used as aids to performance evaluation and research policy formulation, then the scientific community will naturally take a keener interest in the matter. In fact, Garfield was at an early stage warning against indiscriminate or unqualified use of quantitative citation data for sociological evaluations, including personnel and fellowship selection (Garfield, 1963).

An exchange of letters (one of but many over the years) which appeared in the pages of *Scientometrics* (Chubin, 1980; Garfield, 1980A) illustrates the nature of the polarisation which has tended to characterise much of the debate of the past two decades. In one corner, we find Chubin, representing the views of the sociology camp, who refers to 'ambiguities in level and interpretation of measurement' and a 'meaningless numerology' (Chubin, 1980 : 91). Basically Chubin's plea for greater caution is an echo of Kaplan's (1965) opinion that citation findings should be related to other kinds of social data on science and scientists. In the other corner, there is Garfield, concerned that citation indexing should not be unfairly dismissed, simply because of certain, recognised limitations inherent in the raw data, or because of the irresponsible use made of citation databases by some practitioners and policy makers.

The launch of the Institute for Scientific Information's (ISI) *Science Citation Index* in the international scientific community was accompanied by a flurry of

correspondence in journals such as *Nature* and *Science*, concern and cynicism being mixed with cautious interest and welcome (e.g. Goudsmit, 1974). It was as if the scientific establishment had not previously recognised the full import of one of its most frequently exercised conventions — citation. The development of citation indexes for science turned the spotlight on a little-thought-about practice. It was as if a *frisson* had run down the collective spine of the scientific establishment. Authors were for the first time being obliged to reflect on the deeper implications and significance of a practice which had belonged to the penumbral world of professional ritual. Attention was now being focused on the 'why' as well as the 'what' of citation. Davies was prompted to wonder whether reference-giving had not become 'an absurd convention which scientists unthinkingly adhere to' (Davies, 1970 : 1356), while May (1967) drew attention to the flawed and idiosyncratic manner in which authors selected and applied citations.

Citation is a subject of interest to various constituencies involved in the practice and management of professional science. The quotation below, from a brief but illuminating review of the central issues by Morman, summarizes the impact of citation indexing upon various communities engaged in the analysis and evaluation of scientific activity.

> 'Citation analysis in the history of science results from the convergence of the needs of policy makers of the post-war era, the maturation of bibliographic citation practice in the scientific literature during this century, the availability of electronic data processing, and a school of sociology concerned with the internal dynamics of the scientific community. It presupposes a model of science which is simultaneously rational and literary; the scientist is regarded above all as the producer of scientific inscriptions, and he is expected to credit the source of each idea, method or datum which he uses in consistent fashion.' (Morman, 1981 : 12)

Having provided the essential historical framework, he then goes on to identify the central problem:

> 'And since citation techniques share no common methodological assumptions beyond acceptance of quantification and regard for the citation as a usable primary datum, it is appropriate to look to interests which reside outside the cognitive aspects of the scholarly disciplines to which the techniques are applied, for much of the reason of their application.' (Morman, 1981 : 12)

The key phrase here is 'it is appropriate to look outside the cognitive aspects of scholarly disciplines', a fact which was first clearly grasped by Kaplan. The scaffolding within which much of the most illuminating sociological research and discussion has subsequently taken place was effectively erected by Kaplan. Recognising that citation could no longer be treated as an inconsequential aspect of the scientific process, he provided a conceptual framework within which to pose certain fundamental questions about scientists' habits and motivations. At an early stage he concluded that citation practices were in large measure a social device for coping with the problems of property rights and priority claims. This led him to think of citation as an expression of the communistic ideal in science.

> 'The citation is probably among the more important institutional devices for coping with the maintenance of the imperative to communicate one's findings freely as a contribution to the common property of science while protecting individual property rights with respect to recognition and claims to priority (Kaplan, 1965 : 181)

In his view, and in the view of many sociologists who have since been attracted by the subject, the most important question concerns the degree to which citation practices are reflective of elements of the normative and value systems of science. This, of course, is precisely the sort of question Mitroff (1974A) was raising in respect of the relationship between the total corpus of scientific

norms and the existential reality of scientists' daily lives. But for Kaplan's initial questioning, and the resultant growth in cross-disciplinary interest, it is conceivable that citation would have remained a peripheral issue, and that citations would continue to be treated as 'necessary trivialities' (Kaplan, 1965 : 183). Kaplan's achievement was to set citation in its social context, and to elevate the issue to one of serious sociological concern for those interested in the internal dynamics of science.

To conclude this brief introductory statement of intent, here is a first quotation from Ravetz's seminal work, *Scientific knowledge and its social problems*, which warns against absolutism in analysing scientific activity:

> 'Analyses of the social behaviour of scientists, and of external influences on scientific research, have assumed the products of that research to be absolute, and unconditioned by the peculiar circumstances of their achievement. On the other hand philosophical analyses of the nature of scientific knowledge have either been completely abstracted, or have invoked a model of a working scientist isolated from his environment and traditions. But a proper analysis of the social activity of science must be based on understanding of the very special goals of the scientist's task; and an analysis of achieved scientific knowledge must comprehend its character as a social possession, the product of an historical process.' (Ravetz, 1971 : 71)

In Ravetz's opinion, scientific knowledge is the product of an historical process, and is shaped by the 'peculiar circumstances' of its achievement. The aim of this essay is to explore the 'peculiar circumstances' of the citation process.

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Citation and the primary communication system in science

The journal article with its accompanying list of citations is, and is likely to remain, the universally accepted medium whereby the scientific establishment records and reports the results of its investigations. The appearance of the electronic journal (e.g. Bezilla, 1982) may herald the eventual demise of the print-on-paper journal as it is known today, but even if the form is changed it does not mean that the basic concept of the refereed journal article will be any less central to the publication system of the future.

To understand the significance of citation it is first of all necessary to understand the nature of scientists' communication behaviour. The costs and efficiency of journal publishing have been much discussed in recent years (e.g. Meadows, 1979; Moravcsik, 1980). However, despite attempts to introduce change, such as the American Psychological Association's (APA) proposal to establish a multi-million dollar computerized system for

the exchange of pre-prints (Boffey, 1970), the appeal of the traditional approach remains unweakened. The attractiveness of the medium resides largely in the system of peer review. Consequently, rather than the scientific journal being the *raison d'être* of the peer review system, it is almost as if the roles have been reversed and the refereeing system has itself become the *raison d'être* of the primary publication process. Scientists may be less than totally satisfied with the scholarly journal as a dissemination mechanism, but they are deeply attached to it as a means of preserving a faithful and reliable account of scientific progress; as a repository of accepted ideas and beliefs (Royal Society, 1981).

The principal shortcoming of the scientific journal is its slowness in communicating news of the latest developments in science. It has long been recognised that for the majority of scientists and researchers the primary communication system is an inadequate current awareness system (Garvey and Griffith, 1972). This is particularly true as far as eminent or highly visible scientists are concerned, as they, invariably, are 'plugged into' a number of informal information exchange networks. A recent review of the role and importance of 'invisible colleges' within the social sciences noted that virtually all attempts to take advantage of the news-switching capability of inter-personal networks in order to build improved dissemination systems had proved unsuccessful (Cronin, 1982B). There is plenty of evidence to suggest that most scientists are content to operate, and participate in, a two-tier system, with informal networks being used to channel preliminary notifications of research findings, and the formal system being used as an archive of findings which have been vetted and approved by the scientific establishment.

The perceived importance of peer review is considerable. Its primary purpose is to ensure the preservation of standards and to screen out (or at least delay the

introduction of) ideas which are antithetical to dominant paradigms. Maverick ideas, or notions which are, scientifically speaking, revolutionary, are thus effectively debarred from the official record of science — the journal archive. It is within this context that citation plays a crucial role (see, for example, Ziman, 1968).

One of the most lucid expositions of the relationship between citation and scientific communication practices has been provided by Whitley:

> 'By virtue of its peculiar links with the reward system in science, the communication system plays a central part in the maintenance and growth of science. It is the means by which the individual scientist relates to the social system: he publishes his work to gain recognition, and reads the publications of others to maintain his knowledge. The formal communication system also forms the basis for the allocation of rewards: instrumental and consumatory. Thus it is a means of exercising social control . . . Publication of an article in an archival journal signifies a degree of recognition for the author, while legitimising the object of research and methodology.' (Whitley, 1969 : 219)

The publication process combines reward and recognition. The scientist is rewarded for his efforts by having publication status conferred on his work, i.e. he receives the seal of approval of the scientific establishment, and those whose work he has cited in turn receive recognition for the part they have played in the development and furtherance of the citing author's theories. On the basis of this interpretation, Ravetz (1971) introduced the idea of intellectual property and intellectual property rights.

By describing briefly how the scientific journal came into being (e.g. Knight, 1976; Houghton 1975), it should become clear in what sense the notion of intellectual property and its protection can be applied to the products of individual scientists' research efforts. A short, but particularly good, account of its genesis has been provided by Ravetz (1971), and is summarized below.

The development of a proprietary attitude to research findings was first witnessed in the sixteenth century. Prior to then, debates on the acceptability and admissability of atoms of knowledge tended to be conducted as battles of books between conflicting traditions. In this way, disputes were not seen as conflicts over intellectual ownership rights, but as clashes between rooted and favoured traditions. It was in the seventeenth century that the scientific academies and societies came into being (the formalization of the earliest 'invisible colleges'), which in turn led to the precursor of the scholarly journal as we know it today. Up until this development, however, plagiarism was a very real and worrying problem. In fact, it was not unknown for results to be expressed anagrammatically in order to prevent intellectual theft. It was only when communication of results via Transactions became commonplace that there could be a relaxing of caution over the protection of intellectual property rights.

With the foundation of the earliest specialist journals (which grew out of the German university tradition), the process of authentication or quality control was combined with that of claim-staking. The flourishing of the scientific journal can be explained in terms of four important functions it fulfilled for the scientific community: (1) it provided a means of communicating with interested colleagues; (2) it went some considerable way towards guaranteeing the quality of results by instituting the peer review system; (3) it also allowed individual authors to demonstrate the originality and value of their thinking; and (4) it facilitated the distribution of credits among the scientific community.

This gradual institutionalization of scientific publishing managed to combine utility (the promulgation of ideas and information) with a social function (controlling the professional conduct of the scientific fraternity) (Zukerman and Merton, 1971). In Ravetz's elegant

phrase, the scientific journal allows for the 'harmonizing of collective goals and private purpose' (Ravetz, 1971 : 300)

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Science as a social system

Scientists are occasionally, and not without some justification, accused of working in ivory towers. But however scientists choose to see their role, they cannot realistically claim to work in a social vacuum. To understand the actions and motivations of scientists, as individuals and as members of a socio-cultural elite, it is necessary to think of science, not in purely operational terms (or as an inert aggregation of intellectual accomplishments), but as a social system, and of scientists, not as individual actors on a public stage, but as members of a closed community (subject, of course, to scrutiny and accountability), with certain shared prospects, attitudes, beliefs and conceptual schema. As Tranøy put it:

> 'Scientific and other cognitive activity necessarily presupposes an identifiable set of norms and values, an ideology, required to guide and justify, to judge and evaluate such activity. It is reasonable to demand that this set should be as far as possible internally consistent.' (Tranøy, 1976 : 145)

As with any community, science has a more or less coherent and articulated system of values, which define, constrain and infuse both individual and group behaviours. These rules of association and conduct confer on science its particular moral character.

> 'The normative framework within which the scientist works contains components pertaining to cognitive and technical standards, to everyday practice, and to extra scientific dealings.' (Robbins and Johnston, 1976 : 353)

That said, however, it seems not unreasonable to suppose that individual scientists, if questioned on the nature of science's normative system, would differ in their interpretations of how they, collectively and individually, do and should behave. Merton is widely recognised as having being the first to elaborate the norms which underpin science. He identified four guiding principles: Universalism; Organized Scepticism; Communism; and Disinterestedness (Merton, 1973). Universalism refers to the internationality and independence of scientific findings. Organized Scepticism requires that conscientious scientists check and question, challenge, and hunt falsity wherever it exists. Communism relates to the need for openness, honesty, and the willingness to share the results of scientific research. Disinterestedness can be summed up in the dictum 'science for science's sake'. Others, notably Barber (1952) and Storer (1966), have commented on, refined, or added to these norms. In fact, Merton (1957) himself subsequently added the norms of Humility and Originality to his original quartet. The credit, however, remains with Merton for opening up new vistas on the social dynamics of science and effectively creating a new sociological paradigm.

In an expansion of Merton's initial list, Mitroff (1974A) identified a corpus of eleven norms. Allowing for the inevitable variations in nomenclature which arise in any discussion of norms, this list probably consitutes a comprehensive inventory of the norms which, in theory or actuality, guide scientists' actions. In enumerating the normative foundations of 'pure' science, Mitroff was not attempting to endorse the 'storybook' image of science. Quite the reverse: his inventory is the necessary prelude

to a critical assessment of the logical status of science's norms. In his view the evidential bases from which these norms have been inferred are suspect. Personal experience, folk wisdom and the collected testimonies of scientists themselves, he seems to be saying, suggest that an image of science predicated on these eleven criteria would be less than accurate. In other words, the stringency of the norms is such that their exemplification in actual practice would be somewhat unlikely. They constitute, in effect, an essentially Platonic conception of how science works. Mitroff argues that we would be better advised to regard these norms as inspirational, rather than instrumental, in the modulation of an individual's behaviour. A similar view has been expressed by Rothman (1972).

Storer's explanation of how and why scientists abide by the norms of science does not require that we see scientists as a morally upright elite. He concedes that scientists' understanding of science's normative framework may be ill-formulated and based upon the principle of self-interest.

> 'Scientists subscribe to the norms of science first of all because of their importance for the continued, adequate circulation of the commodity in which they are mutually interested. That is not to say that all scientists are aware of this relation, but that through a combination of perceiving in a general way the necessity for such patterns of behavior, of training and mutual reinforcement, and of their congeniality with the personalities of many scientists they have become institutionalized. It is the occasional reinforcement given these norms by the scientist's awareness of their relevance to his work rather than to the general goal of science which . . . accounts for their potency . . . ' (Storer, 1966 : 84)

If the exchange system of science is to operate properly, then, according to Storer, normative control is essential. Without norms, science would suffer from a lack of internal coherence. Just as Ravetz (1971) spoke of 'intellectual property', Storer speaks here of knowledge

The norms of science (after Mitroff, 1974)

1. FAITH IN RATIONALITY
2. EMOTIONAL NEUTRALITY: an instrumental condition for the achievement of rationality.
3. UNIVERSALISM: in science all men have morally equal claims to the discovery and possession of rational knowledge.
4. INDIVIDUALISM: expresses itself in science as anti-authoritarianism.
5. COMMUNITY: priority rights are reduced to credit for priority of discovery; secrecy thus becomes an immoral act.
6. DISINTERESTEDNESS: men are expected to achieve their self-interest in work satisfaction and prestige through serving the community interest.
7. IMPARTIALITY: a scientist concerns himself only with the production of new knowledge and not the consequences of its use.
8. SUSPENSION OF JUDGEMENT: scientific statements are made only on the basis of conclusive evidence.
9. ABSENCE OF BIAS: the validity of a scientific statement depends only on the operations by which evidence for it was obtained and not with the person who makes it.
10. GROUP LOYALTY: production of new knowledge by research is the most important of all activities and is to be supported as such.
11. FREEDOM: all restraint or control of scientific investigation is to be resisted.

as a 'commodity', and of its place in the 'exchange system' of science. The commodity which scientists traditionally exchange is knowledge or information, and in drawing on the intellectual property of their peers, scientists have to enter the exchange system and 'pay the going rate', so to speak. The currency, to maintain the economic metaphor, is the 'coin of recognition' (Merton, 1968B : 56).

The exchange on which the social system hinges is information for recognition. The formal record of these transactions is the scientific establishment's traditional ledger, the scholarly journal. The most common form of currency is the citation. If science appears to display stable characteristics and to follow predictable patterns in its internal workings, then, so the argument goes, this is largely due to the existence of the primary communication system. If the norms of science are to be taken seriously, then, as both Storer and Ravetz seem to be saying, it is only because the peer review system has been built into the process whereby scientists offer their 'goods' to the marketplace. This happy fusion of the dissemination and quality control functions in a single system seems to allow individual scientists to be at once dispassionate and communistic, and at the same time to be able to give vent to basic, though not necessarily base, human urges such as the desire to be recognised and acclaimed. The bonding effect of science's normative values has been summarized in the following manner by Merton:

> 'Science, as is the case with the professions in general, includes disinterestedness as a basic institutional element. Disinterestedness if not to be equated with altruism and interested action with egoism. Such equivalences confuse institutional and motivational levels of analysis . . . For once the institution enjoins disinterested activity, it is to the interest of scientists to conform on pain of sanctions and, in so far as the norm has been internalized, on pain of psychological conflict.' (Merton, 1968A : 612–613)

Appealing though this interpretation is on the surface (it does, after all, have a most plausible internal logic), it does not satisfy all sociologists of science. In his book, *The subjective side of science*, Mitroff (1974A) counters by saying that normative versions of how science works fail to give adequate attention to the question of normative ambivalence; something, he argues, which observation and inquiry would suggest more accurately characterizes the scientific process. A much sharper distinction between institutional and motivational levels of analysis is required if the whole picture is to be painted. Furthermore, he sees emotional commitment (the antithesis of disinterestedness) as being a 'necessary condition for the development of science', and goes on to say that 'if disinterestedness deserves to be considered as a norm of science, then commitment and bias play a positive role in science and also deserve to be elevated to the regal status of norms of science' (Mitroff, 1974A : 73). His thesis is that science (and indeed all social systems) are characterized by ambivalences, by normative tensions. As far as he is concerned, the popularly accepted norms of science (e.g. universalism) can be matched, norm for norm, with counter-norms, whose existence is no less conceptually or empirically valid than the positive or pure norms. Those who favour the positive norms (to the exclusion of all others) are supporting an image of science, and of scientists, in which scientific investigation consitutes the apotheosis of emotional detachment and neutrality; something which, in his view, is not borne out by the evidence to hand (Mitroff, 1972; Barber, 1971).

Mitroff's critique takes in three aspects: (1) the empirical or factual status of the norms; (2) the 'regulative status' of the norms as the embodiment of ideal standards; and (3) the 'epistemic status' of the norms. With regard to (1) he is concerned to establish the degree of fit, or correspondence, between the norms-as-assumed and the norms-in-use; with regard to (2) he

wishes to establish the extent to which the norms describe a to-be-aimed-at level of group performance and behaviour; while in (3) he is casting doubt on the argumentative and evidential bases that are used to infer the existential status of the norms. The third of these is for him a particularly juicy bone of contention. As far as he is concerned, the norms have been identified and selected in the first instance by inductively generalizing from the cases of individual scientists; from the highly select writings of the rare, great scientists who were psychologically motivated enough to write glowingly of science. An essentially similar point has been made by Law:

> '. . . there is a case for arguing that in our normative, and conceptual studies of scientific knowledge, we have gone some way to creating a self-validating methodological and theoretical system. We look for norms, we choose certain types of data — those where we expect to locate the norms, and we go on to interpret that data normatively. If we fail to find shared norms we take it that our methods are not good enough, or that the area has not been institutionalized properly.' (Law, 1974 : 168)

Mitroff's objective in exploring the subjective side of science was to challenge the 'bad metaphysics' (norms without empirical foundation) by marshalling coutervailing evidence in the form of the actual attitudes and behaviours of 'real' scientists.

If the normative position is accepted without conducting some sort of survey or field study among working scientists, then it is of necessity based on assumptions, or theoretical premises, which Mitroff would presumably label 'metaphysical'. In the absence of explicit and universally recognised consistencies in individuals' citation practices, it is difficult to see how citation can be defined as a norm-regulated activity. Ravetz (1971) finds himself in the position of having to employ very 'unscientific' terms such as 'tacit' and 'unselfconscious'

to provide an explanation of why and how scientists cite as they do.

> 'Since citations must convey some very subtle messages by a very crude device, the etiquette of each field will impose a code for their interpretation, whereby the entries and their possibly brief comments will convey the requisite meanings to those in the field. Each such node will depend on the character of the problem in the field, on the types of mutual dependence, and also on the ruling conception of the right division of intellectual property. In every case it will be a purely informal, perhaps tacit and unselfconscious, craft knowledge shared by members of the field. Thus, in the last resort, this aspect of the system of the protection of property depends like others on an informal etiquette as well as on a formal system of rules.' (Ravetz, 1971 : 257)

It may be that on the surface things happen more or less as Ravetz has chosen to describe them. Apart from the occasional flare-up on a question of priority, it is probably true to say that the majority of scientists see citation as a low priority issue by comparison with the many other professional and extra-professional matters which are competing for their time and attention. Thus, the views of Ravetz constitute a cosy and convenient account of what happens, and why. If science, viewed as a social system, works well as far as the majority of scientists is concerned, for the majority of the time; and if the profession has its own tailor-made system of sanctions; and if there is some evidence that there exists an inchoate code of citation conduct, then why, the question might go, should the citation process be subjected to such close scrutiny?

There is no one, best answer to this question. One might, however, evince a desire to explicate what is something of a muddled issue; to test theoretical assumptions against concrete behaviours, or to identify classes of activity or practice, which if in some way altered, could contribute either to greater professional understanding of what citation entails, or to the

increased effectiveness of the primary communication process. Returning to a point made earlier, it is mildly ironic that science, founded on traditions of quantification and verification, should be content with an explanation of citation, an activity central to the scientific process, which emits a whiff of the metaphysical. It may be that prevailing interpretations of citation are the best that can be hoped for (or that are required by the majority of scientists), but that is no reason to discourage further investigation.

There is, in addition, another (rather more pragmatic) reason why citation warrants further study as a social phenomenon. This relates to the fact that citations (due to the development of citation indexing systems) have become an integral part of the information support facilities used by scientists on a day-to-day basis. The commercialization of the citation means that those who generate citations (publishing authors), those who make use of them (other scientists), those who process and package them (the information industry), and those who mediate in their delivery (librarians and information scientists) need to have a sound grasp of what citation entails and signifies. Discussing the quest for universals in sociological research, Turner drew a distinction between variables and concepts. As far as citation is concerned, this distinction has become hopelessly blurred, variables being treated as concepts.

> 'A variable is any category which can be measured or identified and correlated with something else. A concept is a variable which is part of a theoretical system, implying causal relations . . . correlations among variables, of themselves, do not provide a basis for theory, or even for anticipating future correlations.' (Turner, 1953 : 610)

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The need for a theory of citing

Metaphorically speaking, citations are frozen footprints on the landscape of scholarly achievement; footprints which bear witness to the passage of ideas. From footprints it is possible to deduce direction; from the configuration and depth of the imprints it should be possible to construct a picture of those who have passed by, whilst the distribution and variety furnish clues as to whether the advance was orderly and purposive. So it is with citations in respect of the growth and development of scientific knowledge; they give substantive expression to the process of innovation, and, if properly marshalled, can provide the researcher with an analytical tool of seductive power and versatility.

The relatively objective data (Mulkay, 1974) provided by citations have been imaginatively exploited in a variety of applications. By treating citations as 'a quantitative and 'computer manipulable' measure of something or other' (Singleton, 1976 : 260) sociologists and others have acquired what Lindsey (1978), somewhat disparagingly, describes as a quality sensor machine, which can be used, with varying degrees of confidence, to estimate the quality, impact, originality, penetration or visibility of individual and corporate

performance within and across disciplines (see also: Lawani and Bayer, 1983).

In any scientific field the existing 'body of knowledge' is an accumulation of distilled insight, theoretical constructs, experimentally derived data and empirical observations. The published literature of a subject field is a selective, edited and approved inventory of that knowledge, and if intelligently schematised it can display the geneaology of achievement within the field. Citation indexing, drawing on the inter-document linkages which authors reveal in their choice of citations, has made this possible in a way that has opened up new horizons in the sociology of science. For the historian of science, citation analysis can be employed to establish the pedigree of ideas, and to unravel networks of scholarly interaction. ISI's multi-disciplinary citation indexes are the most eloquent articulation of the principles of citation analysis: they are to scientists what Debrett's is to the genealogist.

Citation indexing rests firmly on the premise that a bibliographic citation is an expression of a relationship between two documents, the citing and the cited. The technique does not make explicit the nature of the inter-document relationship, merely exploits it to index the literature of science (Blackwell and Kochtanek, 1981). Apart from this inspired connection, citation indexing is nothing other than a straightforward, though commercially sophisticated, repackaging of publicly and freely available data. The technique and all derivative applications are based on the assumption that the citation data can be treated quantitatively. Citation indexing operates on the assumption that citations have *prima facie* equal value (Voos and Dagaev, 1976). In reality, of course, some citations are 'more equal' than others. That is to say, the presence of a citation *may* signify that author A has been influenced by the work of author B, but it cannot, on its own, say anything about

the extent or strength of the influence (Martyn, 1964). However, if the assumption (each citation is a micro unit with a fixed value of one) is conceded, then the benefits flow freely.

There is a considerable body of evidence to suggest that citation counts correlate with a variety of subjective and objective performance measures. Clark's (1957) study of American psychologists was one of the first to use a battery of quality indicators which included citations. The most important contributions have been made by the Cole brothers, who showed that high citation counts correlated positively with recognised quality indicators such as honorific awards, Nobel laureateships and reputational ability (Cole and Cole, 1967; Cole and Cole 1968; Cole and Cole, 1971). Support for these findings came from Hagstrom (1971), who correlated citation counts with such variables as quality of graduate faculty and grants awarded to departments. In the area of science policy, the National Science Foundation (NSF) in the United States uses citations as one indicator of the effectiveness of its funded research programmes (e.g. National Science Foundation, 1981). Yet, behind the practical benefits lie unresolved epistemological and methodological questions, which are responsible for hairline cracks in the conceptual superstructure (e.g. Edge, 1977; 1979).

The need for more secure epistemological foundations has been touched on by a large number of writers (e.g. Sullivan *et al*, 1977). More specifically, however, Crane (1972) has argued that the use of citation linkages between scientific papers is an approximate rather than an exact measure of intellectual debts; Porter (1977) that the correct functional form of this linkage is unknown; Whitley (1969) that it is difficult to say how much of the difference in citation rates is due to the intrinsic quality of the work and how much is due to other factors; Hodges (1978) that what one knows about a writing,

simply because it cites or is cited by another, is almost content-neutral in terms of the nature of the relationship; Gilbert (1977) that we do not as yet have a clear idea about what we are measuring when we analyse citation data; Dieks and Chang (1976) that the impact of a paper is not only determined by its scientific significance but also by such extrinsic factors as locale of the author, prestige of the journal in which the article is published, and so on; Meadows (1974) that the evidence concerning the selection of citations is inadequate; Chubin (1973) that citation counts are not linear in relation to the quality of an individual's research; Dewitt *et al* (1980) that uncritical use of citation data as a sole, or even major, criterion could do a great deal of harm; and Szava-Kovats (1982) that formal-numerical citation analysis does not provide a true picture. More particularly, Gottfredson and Garvey (1980) have noted that the majority of studies seeking to establish correlations between citedness and other quality indicators have generalized from samples of eminent scientists to the total population of scientists — a criticism endorsed by Bensman (1982).

The welter of criticism has not gone unnoticed by the Institute for Scientific Information (ISI). In a 'reply' paper, Garfield (1980B) indicated that ISI might begin to shift from counting citations to counting 'authors influenced by'. It remains to be seen whether public opinion will be affected by the objections raised by sociologists and concerned scientists, or whether the considerable practical benefits of citation indexing will continue to outweigh the residual misgivings concerning the construct validity of the principle upon which the system rests.

Ultimately, citation is a private process (Chubin and Moitra, 1975), albeit a private process with a public face. The essential subjectivity of the act of citing means that the reasons why an author cites as he does must remain a

matter for conjecture. What is lacking, in Swanson's (1977) view, is a rapid and convenient measure for discovering the nature of the relevance link which the citing author has established. This conjectural element is worth pursuing, precisely because the end-product of the private process (the citation) acquires the status of a public commodity. It is this privateness which invests citation relationships with frequent biases (Zunde, 1971). Assumptions are made about the nature of this commodity, despite the fact that its real significance is obscured by the secretiveness of the production process. Textual analysis of the citing paper cannot reveal why an author has chosen to cite in a particular way, though it may suggest very plausible reasons. In Mulkay's (1974) view, there has been no clear demonstration of the way in which citations reflect the process of scientific influence. Further, according to a review study commissioned by the NSF, there has been little empirical work undertaken to explore and understand the norms of scientists' citation behaviour (Gottfredson *et al*, 1977).

The absence of a satisfactory theory of citing in part explains why writers on the subject (see, for example, the opening paragraph of this chapter) often resort to a metaphor in an attempt to clarify what is not entirely self-evident. But metaphor, like analogy, has an unfortunate tendency to increase awareness at the expense of understanding. Logically, the use of citations as a basis for value judgements should imply that there is a universally recognised convention among authors. However, this convention, in so far as one can be said to exist, displays a remarkable resistance to standardization. It is for this reason that metaphor is so popular, and useful, in citation exegesis. Ravetz (1971), as already mentioned, chose to interpret citations as a form of reward or income, while Gilbert (1977) has preferred to view them as tools of persuasion. Small (1978), on the other hand, speaks of citations as markers or symbols.

Because the citation process is subjective and inhospitable to standardization it is necessary to rely on ostensible reasons for citing or reasons which can be adduced from the context of the citing work (Frost, 1979). It is no historical accident that studies of citation have shied away from an analysis of the generative stage.

There are many reasons why authors cite the works of others. In some cases it may well be possible to adduce the motive, but this is an attributive exercise, and cannot make the author's intentions explicit. There will invariably be a gap between why the author cited and why we *think* the author cited. The reasons why an author cites in a particular fashion may be serious or frivolous. Weinstock (1971) has identified fifteen specific functions of citations, all of which could be classed as 'serious'. These are listed below.

Reasons for citing: Weinstock's list (1971)

1. Paying homage to pioneers
2. Giving credit for related work
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 researchers 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 the original publication describing an eponymic concept or term as, e.g., Hodgkin's disease . . .
14. Disclaiming work or ideas of others
15. Disputing priority claims of others

The difficulty with attributing motivation using the list of reasons drafted by Weinstock is that what at first glance seem like a perfectly good explanation of an author's intentions may in fact be no more than a surface explanation. At best, the attributive process can only offer face validity. For instance, a particular citation in the text may alert the reader to a forthcoming work (one of Weinstock's reasons), but unless the reader is *wholly* familiar with the subject to which the paper relates, he cannot *know* whether the cited paper was the most appropriate choice bearing in mind the theme and orientation of the citing paper. Furthermore, in most cases he will not be able to tell whether personal bias has played a part in the author's selection process. In citation, as in life generally, there are sins of omission and commission. Full knowledge of the factors which influence authors in their citation practices would require omniscience on the reader's part. For those interested in the exploitation of citations for information retrieval purposes this can, in the opinion of Griffith *et al* (1977), be either an object of study or a bother. All too often it is the latter.

Citation is coloured by a multitude of factors, not all of which have to do with the accepted conventions of scholarly publishing. Social and psychological factors play a part, along with subconscious remembering and forgetting (Aaronson, 1975). Then there are what might be termed extrinsic factors: the target audience (more precisely, the author's perceptions of the readership's requirements, capabilities and expectations); the character and status of the journal in which the article is to appear; the scope, format, aims and length of the article itself; the author's knowledge of the area in which he is writing; and his ability, not to say willingness, to use the appropriate information services and sources associated with the subject field. There is a bewildering array of variables to be taken into account, and, as Aaronson

concedes, not all of these can be identified or quantified.

May (1967) challenged the orthodox view that citations give an accurate or exact picture of the intellectual links between publications. He contends that there is a considerable amount of 'deviation', which results from 'memory failures, lack of self-awareness, carelessness, plagiarism of other people's citations without having actually used them, the widespread custom of not citing 'obvious' sources, and many other causes — all consequences of the simple fact that the author selects citations to serve his scientific, political and personal goals and not to describe his intellectual ancestry' (May, 1967 : 890). Protagonists of the orthodox view, such as Martino (1971), counter by saying that the force of the statistical/empirical evidence in support of citation analysis outweighs the objections. However, even Garfield (1979) concedes that there are ambiguities associated with the use of citation counts that prevent them from being completely definitive.

Recognising the social significance of citation counts, not least as arbiters in tenure or promotion disputes, Weiner (1977) offered advice on how the aspiring author could avoid the 'dustbin of the uncited', and noted that his (not entirely tongue-in-cheek) suggestions had already been practiced with a vengeance by Eugene Garfield (founder and president of ISI), whose self-citation rate (self-citation being one of the ploys recommended by Weiner) was calculated to be 79%. However, the 'footnote fetish' (Weiner 1977) can perhaps be excused as a not-too-serious manifestation of basic human vanity. But, even if this is granted, there remains the question of deliberate citation abuse.

An example of this is the so-called 'Burt scandal' in the world of psychology. In launching his case against Burt, Gillie (1978) raised numerous questions concerning the probity of much of Burt's published research, and in a subsequent article posed the following questions.

> 'As it is we must ask whether Burt's pathological streak began earlier. His attempts to steal credit from Spearman for inventing factor analysis seem to go back to at least 1937. Burt's earlier work on IQ closely shadowed that of H. W. Winch although it was inferior in quality — was Burt plagiarizing? Why does a 1951 bibliography approved by Burt contain references to non-existent publications, supposedly published much earlier when he was with LCC?' (Gillie, 1980 : 15)

In attempting to answer Gillie's question one has to assume that Burt's objective was to pre-empt criticism, and to push his 'knowledge claim' (Gilbert, 1976) further towards acceptance status. Burt, presumably, recognised the psychological fact that citation, at least affirmative citation, constitutes an appeal to disembodied authority. The references to non-existent publications were included to add spurious credibility and weight to his argument. Had Burt's work been scrutinised at the time it was written, without there being any expectation or suspicion of malpractice, then, presumably, it would have been possible to classify the various citations using Weinstock's typology. This, of course, is an extreme illustration of dishonest citation practice (but see also Zirkle, 1954). However, the dividing line between deceit and what Ravetz (1971 : 257) refers to as the ability 'to under-cite without stealing results, or over-cite with the effect of inflating the value of the property of a colleague', or what Merton (1973) refers to as 'cryptomnesia' (unconscious plagiary), is somewhat blurred at times. It is difficult to conceive of all bias and subjectivity being removed from citation behaviour, although advances in what is loosely referred to as Artificial Intelligence (AI) research may ultimately make it unnecessary for authors themselves to decide where and what to cite in writing formal research papers. As early as 1964, Garfield had discussed the possibility of citations being generated automatically by computer without any involvement on the part of the author (Garfield, 1965).

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For the present, however, the questions raised by Jevons concerning the use of quantification in quality measurement have still not been answered to everyone's complete satisfaction.

> 'Numerology . . . is interesting but should be used only with the utmost circumspection. In particular cases it could be wildly misleading. Is it in fact likely to be misused? Is there a danger that it might supplant better criteria for determining growth rates, allocating grants and deciding promotions? If so, should work on these lines be discouraged? Or will it do to adopt the ethical neutralist position, doing the work just to get the information and leaving it to others to decide how, if at all, it is to be used?' (Jevons, 1973 : 45)

Note: part of this chapter appeared as The need for a theory of citing, *Journal of Documentation*, 37(1), 1981, 16–24.

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Context and content analysis of citations

Over the years a number of noteworthy attempts have been made to peel away some of the ambiguity and vagueness associated with citations and their use. Basically, two approaches have been tried: context and content analysis. In a comprehensive review, Small (1983) noted that the development of citation classification schemes had not been a cumulative endeavour, with each classifier regarding his problem as unique. The result has been as assortment of classification schemes, each addressing the same problem yet designed in virtual isolation. Nevertheless, Small did find that, despite the range of approaches adopted and differences in the literatures studied (from physics to German scholarship), the results displayed certain regularities.

The studies assessed by Small had at least one important feature in common: they all sought to illuminate the inter-document relationship implied by the presence of a citation by devising a classification or taxonomy based on an analysis of the text surrounding a citation. Hence his choice of the phrase 'citation context studies'. However, the specific objectives of these studies differed in certain respects. In some cases the aim was to improve retrieval performance by throwing extra light on the actual functions of citations, while in others the goal was to arrive at a deeper understanding of the citation process in general. Broadly speaking, the studies were

either of the applied information science variety, or sociological in character.

As it happens, both the earliest and most recent studies in this area are characteristic of the information science category. Lipetz's (1965) work was an attempt to improve the selectivity of existing citation indexes to the scientific literature. One of the major drawbacks associated with citation indexes (then and now) has been what information scientists refer to as the 'noise' factor. A feature of citation-based information retrieval systems (from the user's point of view) is that a large number of unproductive or irrelevant 'leads' are included in the output of a search run on a citation index. Ironically, citation indexing's greatest advantage is also the cause of its major shortcoming. Citation indexes are based on connections between documents supplied by the citing authors. There is no need for the intervention of trained subject indexers. Citations are, in effect, used as descriptors or indexing terms in their own right. It is the primacy of these linkages which confers on citation indexing its uniqueness over other secondary information services. But, because authors cite in different ways and for different reasons, not all the connections prove to be useful. As was mentioned earlier, some citations are 'more equal' *and* more useful than others.

Lipetz's pioneering work was an attempt to compensate for the variability in the quality of the output generated by searches conducted on citation databases. Being an information scientist Lipetz was keen to reduce the 'noise' level, and (to continue with the jargon of information science) to eliminate 'false drops'. Noise may, as Johansson (1976) feels, be an intrinsic factor of referencing practice, but given the cost of using on-line information retrieval systems (such as the *Science Citation Index*), most information intermediaries and system users would strongly support research designed to improve system cost-effectiveness. This was precisely the thinking

behind Lipetz's work. His aims were as follows:

> 'To introduce the means for higher selectivity in a citation index requires that the bare citing reference be supplemented with additional information which, in one way or another, reflects the content of the citing reference as it relates to typical search problems. In other words, each entry in the citation index must be expanded to include one or more additional data categories.' (Lipetz, 1965 : 82)

As others were to discover subsequently, it was often necessary to include more than one qualifier (or relational operator) to fully describe the function or character of a given citation. The fact that there are in Lipetz's view no absolute and exclusive categories to adequately and comprehensively describe the relationship between two documents (linked via a citation) means, in practical terms, that the enhancement effort is not slight. In the event, Lipetz settled for four principal relationship categories, with numerous sub-divisions. His twenty-nine-item classification is reproduced below. Lipetz actually applied these operators in the Continuity Index of *Information Science Abstracts*, but because of the labour-intensive nature of the work, his ideas were not adopted by commercial database producers.

Relational Indicators: Lipetz's list (1965)

GROUP 1. ORIGINAL SCIENTIFIC CONTRIBUTION OR INTENT OF CITING PAPER

1. Description of observed phenomena
2. Data transformation
3. Explanation
4. Hypothesis or theory
5. Calculation from theory
6. Prediction
7. Definition or notation
8. Statement of experimental technique

GROUP 2. CONTRIBUTION OF CITING PAPER OTHER THAN ORIGINAL SCIENTIFIC CONTRIBUTION

9. Review article
10. Bibliography
11. Data cumulation

GROUP 3. IDENTITY OR CONTINUITY RELATIONSHIP OF CITING PAPER TO CITED PAPER

12. One or more authors in common
13. Same text
14. Abstract or condensation
15. Erratum
16. Continuation
17. Precursor
18. Inclusion

GROUP 4. DISPOSITION OF THE SCIENTIFIC CONTRIBUTION OF THE CITED PAPER TO THE CITING PAPER

19. Noted only
20. Distinguished
21. Reviewed or compared
22. Applied
23. Improved or modified
24. Replaced
25. Changed the precision (plus or minus)
26. Changed the scope of applicability (plus or minus)
27. Questioned
28. Affirmed
29. Refuted

Recognising from the outset that the need for intellectual effort in applying the operators would be a very real barrier to widespread implementation, he advocated that authors should, ideally, be encouraged to include the relationship indicators themselves, thereby not only making the scheme more commercially attractive, but also inducing authors to reflect rather more carefully on their reasons for including citations in the first place.

An approach very similar to that employed by Lipetz has been tried by a research team financed by the Scottish Education Department (Duncan *et al*, 1981). The aim of this project has been to establish a prototype on-line qualified citation index to the literature of educational technology. The thinking behind the idea is remarkably similar to that which motivated Lipetz. The goal has been to create a citation index in which data obtained from cited articles are amplified by including descriptions of the reasons for citing, or of the relationship between the cited and citing paper, with a view to improving retrieval effectiveness. A longer term aim is to explore the feasibility of context searching (i.e. using the surrounding text in which citations are embedded to provide additional information at the retrieval stage). In passing, it is worth noting that O'Connor (1982) has, independently, been exploring the possibility of using computer-selected citing statements (i.e. using the text immediately surrounding the reference to the cited document) to improve retrieval performance.

The approach adopted by the Scottish team is essentially the same as that used by Lipetz. First, a more or less randomly assembled list of relational operators was tested and refined for subsequent use in a series of simulation exercises. A prime concern has been to come up with a list of operators based on users' needs and perceptions, rather than those of the citing authors. At the time of writing, feedback trials are being conducted with a 500-item document collection and an initial

corpus of twenty-six qualifiers (see list below).

Qualified Citation Index Project: list of operators (1981)

1. Paying homage
2. Background reading
3. Historical
4. Bibliographical leads
5. Narrative
6. Definition
7. Clarification
8. Illustration
9. Example
10. Experimental detail
11. Theory
12. Data
13. Methodology
14. Description
15. Current concerns
16. Development of ideas
17. Disputing
18. Criticism
19. Corroboration
20. Disclaiming
21. Substantiation
22. Similar research
23. Contradictory research
24. Further detail
25. Same paper
26. Statistics

Unlike the two classification attempts just described, Frost's (1979) study was centred on humanistic rather than scientific scholarship. In selecting German literary scholarship as her testbed, she was hoping to discover whether citation usages in the humanities were affected by the greater tendency to rely on non-empirical or circumstantial evidence.

A scheme was developed in 'an *a priori* way', and, as with the Scottish project, was then piloted to test its reliability. Her final classification (see below) comprised three broad categories which reflected: (a) whether a cited work was used as a primary or secondary source; (b) whether the work was used as a basis for a statement of fact or opinion; and (c) whether, in the case of secondary works, the disposition of the cited work was positive or negative. The scheme was applied to a small sample of articles and monographs dealing with German literary scholarship, but, as with the two previous schemes, the task required considerable intellectual effort in analysing the citing work and the nature of the

corpus of twenty-six qualifiers (see list below).

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citation. Her findings confirmed the hypothesis that the documentation of *opinion* is commonplace, and that factual information is less heavily used than in the scientific literature, No less interesting was her observation that two key reasons for using citations in the scientific literature, to acknowledge previous work or indebtedness, and to build on previous ideas, were conspicuously lacking in the humanistic literature.

List of Citation Classifiers: Frost (1979)

A. DOCUMENTATION OF PRIMARY SOURCES — REFERENCES TO LITERARY TEXTS, LETTERS, ETC.

1. To support an opinion or factual statement on the specific literary author(s) or work(s) discussed in the citing work
2. To support an opinion outside the central topic of the citing work
3. To support a factual statement outside the central topic of the citing work

B. DOCUMENTATION OF SECONDARY SOURCES — REFERENCES TO PREVIOUS SCHOLARSHIP

1. Independent of approval or disapproval of the citing author
2. Representing the approval of the cited scholar
3. Representing the disapproval of the citing author

C. DOCUMENTATION OF SOURCES EITHER PRIMARY OR SECONDARY

1. To refer to further reading
2. To provide bibliographical information on a specific edition

One of the features common to the three classification schemes described is the underlying (and apparently unquestioned) assumption that all citations are of the

kind referred to earlier as 'serious'. None of the available classifications or operators is capable of indicating whether the function of the citation is serious, essential, discretionary or trivial in relation to its textual and intellectual environment. Frost, at least, does give a nod in this direction, before moving quickly on.

> '. . . while recognizing motivations for citation which are non-scholarly in nature, such as window dressing, I will be concerned either with the ostensible reasons for citation or with reasons which can be adduced from the context of the citing work.' (Frost, 1979 : 401)

This objection also applies to the classification scheme devised by Hodges (1978). She 'intuitively' identified ten relationship indicators (see below) on the basis of analysing a small sample of papers from a range of subject fields, but did not attempt to demonstrate the reliability of her list by asking others to apply the indicators. Another putative classification has been devised by Peritz (1983) for the social sciences and related fields, which is, essentially, an elaboration and refinement of Hodges' scheme.

Citation Classification Scheme: Hodges (1978)

1. Evidential
2. General informational
3. Historical
4. 'Sibling'
5. Oppositional
6. Corroborative
7. Specific informational
8. Documentary
9. Methodological
10. Corrective

In a series of studies, Moravcsik and Murugesan (1975; 1979; Murugesan and Moravcsik, 1978) attempted to go one step further with this type of approach by analysing the *quality* of and *contexts* in which citations were made. As a physicist studying the literature of his own field, Moravcsik felt that he would be better able to understand the subtleties or organic nature of citation

than researchers with backgrounds in sociology or library science. Their classification consisted of eight paired categories (see below). A citation could belong to more than one of the four groups, but not to both categories in any one group.

Citation Categories: Moravcsik and Murugesan (1975)

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

The first of the four groups (Conceptual or Operational) specifies what was cited — whether a theory, concept or idea, or, alternatively, a tool, method or technique. As Small (1983) notes, this is more a content than a relationship indicator. Group 2 (Organic or Perfunctory) was included to distinguish between essential and non-essential citations — the aspect ignored by the information science research community in its classificatory efforts. With Group 3 (Evolutionary or Juxtapositional) citations could be categorised in terms of whether the citing paper was building on previous ideas, or proposing an alternative viewpoint. Group 4 (Confirmative or Negational) focused on the citing paper's view of the 'correctness' of the cited work.

This conceptually more elastic, yet simpler, scheme advanced thinking by highlighting the quality factor in citation. For instance, the function of the Organic vs Perfunctory classification is to help identify 'also rans' — papers which get cited because of their mere existence and not necessarily because they contribute to the progress of science. Group 4, on the other hand, would enable the reader to identify papers which are held to be

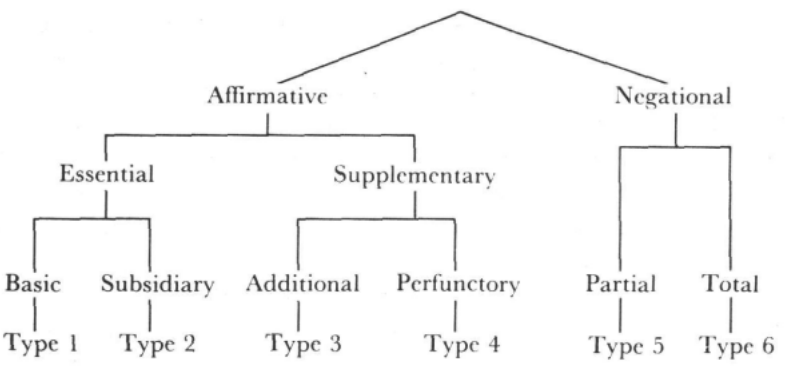
wrong, or whose status is disputed (Moravcsik and Murugesan, 1975).

The authors also introduced the concept of redundancy (e.g. several citations to papers all of which make more or less the same point, but included to keep everyone happy in the game of priority hunting). Significantly, Moravcsik and Murugesan found that a large number of citations fell into the Perfunctory category — 41% in an analysis of thirty articles drawn from *Physical Review* on theoretical high energy physics. In their follow-up studies, the authors made a number of modifications to the initial scheme, and carried out additional research to determine the degree of inter-classifier reliability for their scheme. A refinement of their thinking on the nature of the distinction between Redundant and Perfunctory citations is worth noting here.

> 'There is a difference between redundant and perfunctory citations. The latter could just stand by itself, and still be judged perfunctory because it does not contribute to the development of the citing paper except that it stands in a group with other papers cited, all of whom make the same contribution to the citing paper.' (Moravcsik and Murugesan, 1979 : 168)

The work of Chubin and Moitra (1975) was a more or less direct response to Moravcsik and Murugesan's 1975 paper. Although they recognised the value of an approach to citation analysis based on an inspection of content and quality, they had certain reservations about the eight category typology, and recommended that it should be reworked as a set of mutually exclusive categories, and then applied to a larger literature sample. Their alternative was a six-class scheme which allowed citations to be uniquely classified.

Citation Typology: Chubin and Moitra (1975)



Analysing citations in a sample of articles in high energy physics, they found that citation practices varied by form, content and outlet of the article (their sample included journal articles and letters). More specifically, they reported a 20% incidence of Type 4 citations (Perfunctory: corresponding to the Group 2 category of Moravcsik and Murugesan). Despite the difference between the two sets of figures (20% as opposed to 41%), Chubin and Moitra felt that they had replicated the spirit, if not the letter, of the Moravcsik-Murugesan analysis. Further support for these findings has come from an analysis of the literature of business administration by Prabha (1983), which found that less than one third of the works cited by his sample were considered (by the citing authors) to be essential to their works.

The two types of approach reviewed thus far (the information science and the sociological/philosophical) have features in common, but they have somewhat different objectives. Lipetz and the Scottish project had as their goal the improvement of citation indexing systems as information retrieval tools. Moravcsik and Murugesan and Chubin and Moitra, on the other hand, wished to answer rather more fundamental questions

about the nature and purpose of the citation process. This divergence of approach (which must not, however, be over-played) is mirrored in the literature of this rather specialised, and at times incestuous, field. At the risk of oversimplifying, it may be useful to distinguish between the Pragmatists (those whose principal aim is to improve the quality of existing information retrieval systems) and the Positivists (those, who, though, not necessarily decrying the utility of citation indexes, would like to see the fundamental questions relating to the construct validity of citation tackled in thoroughgoing fashion.)

Apart from the studies already alluded to, there have been a number of other attempts at citation classification (e.g. Finney, 1979; Oppenheim and Renn, 1978). The typology created by Oppenheim and Renn was devised to help explain the reasons why certain old papers are still heavily cited many years after their publication. Although the authors acknowledged the usefulness of the classification scheme proposed by Chubin and Moitra (1975), they felt that there was a need for a typology which would provide a separate classification for those articles cited as historical background. For the purpose of their study 'old' was defined as pre-1930. The elaborated classification, which was applied to a sample of 28 papers dealing with the literature of physics, is reproduced below.

Classification of historical papers: Oppenheim and Renn (1978)

A. Historical background
B. Description of other relevant work
C. Supplying information or data, other than for comparison
D. Supplying information or data for comparison
E. Use of theoretical equation
F. Use of methodology
G. Theory or method not applicable or the best one

Of all the classification schemes mentioned, possibly the most interesting is that produced by Finney (1979). Her approach consisted of classifying the citations attached to fifty-one medical articles (containing a total of 1,115 citations) into seven groups (specified below) on the basis of their lexical content and their location in the text, and then searching for differences, variations and regularities in patterns of use depending upon the type and character of the citing paper.

Citation Classifiers: Finney (1979)

1. Assumed knowledge
2. Tentative
3. Methodological
4. Confirmation
5. Negational
6. Interpretation/developmental
7. Future research

As with earlier experiments from the information science 'stable', Finney began with the assumption that all citations were necessary to the author and to the subject of the research being reported. In a sense, therefore, her classification scheme represents a retrograde step: it ignores the reasons why authors cite some papers in preference to others, and does not seek to identify perfunctory or redundant citations. On the credit side, she feels that on the basis of her analysis of the relationship between a citation and its lexical context it should be possible (in principle) to automate the process of assigning the seven categories.

Classification, though a popular avenue of exploration, has not been the only approach tested. Herlach (1978) attempted to find a way round the problem of the heavy intellectual input required to assign qualifiers and relational operators. Her research suggested that the multiple mention of a citation is an indicator of a close or

serious relationship between the citing and cited documents. She also argued that this link could be mechanistically identified. Thus, if multiple mention of a citation is used as a selection criterion for document retrieval, there could be an increase in the precision level (i.e. a reduction in the number of peripherally relevant items retrieved), but a fall-off in the recall level (i.e. fewer items will be identified *in toto*). This sting-in-the-tail effect is explained by the simple fact that there may very often be an appreciable yield of important and relevant papers which have not been cited more than once in the course of an article.

More recently, Bonzi experimented with another technique in the hope of improving the predictive capability of citation links. As before, the aim was to find a way of selectively identifying strongly paired papers using citations and structural features of the citing paper. She likened her approach to a fishing expedition:

> '. . . an exploration of which characteristics of both cited and citing works may lead us to the development of a reliable tool to aid in the retrieval of relevant documents through citation indexes. The hypothesis of the study, if there is one, is that characteristics of the citing articles are generally better indicators of the extent to which a citation adds information to the article than are the characteristics of the cited work.' (Bonzi, 1981 : 211)

This in itself is not an unreasonable objective, and the rationale is superficially appealing. However, the definition of citation relevance is based on the extent to which the cited work is treated in the citing article. There may be a positive correlation between the frequency and/or extent to which a cited paper is referred and the contribution or influence it has upon the development of the citing author's ideas, but this presupposition is not explored. Instead, Bonzi selects four categories with which to measure citation relevance (see below), and then proceeds to link these to a variety of factors relating

to the characteristics of the journals, articles, authors and citations.

Categories of Citation Relevance: Bonzi (1981)

1. Not specifically mentioned in the text (e.g. 'Several studies have dealt with . . .')
2. Barely mentioned in the text (e.g. 'Smith has studied the impact of . . .')
3. One quotation or discussion of one point in the text (e.g. 'Smith found that . . .')
4. Two or more quotations or points discussed in the text.

All of these studies (from Lipetz to Bonzi) have essayed to increase our understanding of the relationships which exist between citing and cited documents in the scientific literature (and, in one case, the literature of the humanities). In each case *one* aim has been to improve the reliability of citation indexing, both as an evaluative tool and as a conventional information retrieval facility. Yet, each of the methodologies employed depends on inference rather than motivational analysis. None of the approaches mentioned is, or could have been, capable of providing us with privileged insights into the cognitive processes employed by citing authors. Kaplan's (1965) comment that little is known about the norms and behaviour surrounding citation still stands, despite the combined efforts of the researchers just referred to. That, in itself, is not (nor is it intended to be) a criticism of the quality or integrity of the research carried out in the area of citation context and content analysis, merely an observation on the intractability of the problem. On the basis of the findings outlined above, it is difficult to see how we can go beyond saying that citation is a presumptive indication of influence, or that the work of a cited author has been influential inasmuch as it has been noticed and reacted to (Bayer and Folger, 1966).

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Perspectives on citation

To understand why an author cites in a particular way at a particular time we would need, to put it crudely, to step inside that individual's head. The complex of factors which characterize an author's approach to citation belong to his phenomenal field, and not to the public domain. Consequently, direct questioning might, at first glance, appear to be the most sensible and productive method of trying to gain access to this private world. However, because an author's reasons for citing may not always be 'pure' or 'scientific' [Mitra (1970) refers to malpractices of window dressing, padding and the practice of sprinkling a few citations as an afterthought as a means of enhancing the respectability of a paper] this approach may prove to be self-serving (Chubin and Moitra, 1975). Direct questioning and empathic understanding could, conceivably, result in a portfolio of reasons, strategies and motivations about the citing process, derived not from textual analysis, but from the recorded experiences of publishing authors (Hedges, 1978).

Citation needs to be thought of in terms of the citing author's own constructs if the prevailing functionalist interpretations are to be expanded and enlarged upon,

and our understanding of citation in the communication process in science is to be improved. Edge (1977) has argued that citation analyses of communication patterns in science have to take as their starting point the 'participants' perspective', because every decision (how and what to cite) is particular, and because citation and co-citation analysis, in striving to accumulate and average, destroy the evidence we need to account for individual variation. This conception of things is rather different from the 'storybook' image (less uniform, less glamourous), which sees author citation practices as being a controlled expression of a particular normative position. Ravetz's (1971) belief that citation is a professional etiquette maintained by 'an aggregation of self-interested policies' is therefore unlikely to entirely satisfy those who choose not to assume cognitive consensus (e.g. Law, 1974).

If our understanding of the citation process is to be broadened, then it will be necessary to find out more about what Bavelas (1978) calls the social psychology of citation. This is an important area for investigation because it could, ultimately, lead to the development of more efficient and economical citation indexes. As has been stated, many writers in the literature of information science have commented on the 'noise' factor in citation-based information retrieval, which, at heart, is a direct by-product of authors' tendencies to include trivial, perfunctory, redundant, or wayward citations. If authors can be educated as to the social *and* informational role of citations, and can be encouraged to show greater restraint and care in their choice and placement of citations, then it may be possible to arrive at a point where increased standardization and consistency in citation habits will result in better quality information retrieval systems.

Despite the wealth of literature on citation there have been few convincing attempts to explore citation 'in the

round'. Sociologists have for some time questioned the usefulness and validity of citations as definitive measures of influence and connectedness in the scientific communication process, and in the past few years there has been a perceptible shift from the 'aggregationist' approach to citation analysis (i.e. treating citations as things-in-themselves or units which can be combined or totalled to produce a faithful picture of impact and scholarly interaction) to integrated social accounting (i.e. viewing citation as but one element or activity in the social construction of scientific facts). The sociology camp would argue that citation can only be comprehended by examining the social conditions which predispose publishing scientists to cite as they do. *Ergo*, to understand the significance of citation it is necessary to understand the social reality of citation.

In their highly original anthropological account of the operations, functions, structure and inter-personal dynamics of a large research laboratory, Latour and Woolgar put forward the notion that scientific activity should be equated with a continuous process of literary inscription.

> 'A laboratory is constantly performing operations on statements; adding modalities, citing, enhancing, diminishing, borrowing, and proposing new combinations. Each of these operations can result in a statement which is either different or merely qualified. Each statement in turn provides the focus for similar operations in other laboratories. Thus members of our laboratory regularly noticed how their own assertions were rejected, borrowed quoted, ignored, confirmed, or dissolved by others. Some laboratories were seen to be engaged in the frequent manipulation of statements while elsewhere there was thought to be little activity. The problem for participants was to persuade readers of papers (and constituent diagrams and figures) that its statements should be accepted as fact. To this end rats had been bled and beheaded, frogs had been flayed, chemicals consumed, time spent, careers made or broken, and inscription devices had been manufactured and accumulated within the laboratory.' (Latour and Woolgar, 1979: 86–87)

In this interpretation of scientific activity and purpose, the published paper with its quota of citations is the final stage in establishing the 'facticity' of the statements and ideas forged in the laboratory. In essence, the micro-sociological view is that citations do not exist *in vacuo*, and that a proper comprehension of the citation phenomenon and its surface manifestations will only be achieved by moving the critical gaze from the formal communication mechanisms (the superstructure) to the social reality (the infrastructure) which supports the primary communications system. Hence, the relatively new-found interest in the 'work-a-day' life and personal motivations of individual scientists. Citation indexing (applied aggregationism) is of indisputable practical value to a great many professional scientists, but the nature of the technique is such that it cannot take account of this other (social) reality. As Martyn (1965) observed, citation is not a unit, but an event. What sociologists demand is a fuller understanding of this event.

Despite the apparent naturalness with which authors cope with the matter of citation, the fact remains that authors do not cite in a standardized fashion. Always, there remains the question: 'Well, why did so-and-so cite so-and-so's paper in such a way at such-and-such a point in the text?'. As we have seen, functionalist interpretations lack the flexibility to account for the human element, while textual analysis (i.e. looking at citations in relation to the surrounding text), though illuminating in certain respects, does not help close the interpretative gap.

Recently, Small (1978; 1980) have been advancing the idea that citations are markers or symbols which *denote* particular theories, concepts, proofs, ideas or methodologies. The great attraction of this approach is its pragmatism, but the limitation, in some people's eyes, is that it still does not account for an individual's motiv-

ations. To come to grips with the social psychology of citation there is a need for wide-angled investigation. Chubin and Moitra (1975) have talked in terms of a phenomenology of citation, but no research along these lines has been reported.

Writers such as Merton and Ravetz seem to favour the notion that citation behaviour is norm-regulated; that the scientific community adheres to an implicit code of professional conduct, which guides individuals in the crucial and delicate matter of dispensing credits. This communistic interpretation is not to everyone's liking. Many sociologists are reluctant to accept the normative viewpoint — echoes of the 'ghost in the machine' (Ryle, 1949) — because, as Cozzens (1981) has observed, the mechanisms involved in linking actions to particular positions are difficult to lay bare. Is there a middle ground between the pragmatism of the aggregationists' position and the stringency of the microsociological view? In the final analysis, the answer may be 'no', but it may nonetheless prove instructive to look again at the various strands of the debate before abandoning the idea that reconciliation is possible. Indeed, it may also be instructive to break out of a mode of thinking which encourages an 'either-or' option. A proper (pluralistic) explanation of what citation entails may mean that we accept aspects of both (or all) perspectives. It may, therefore, be counterproductive to think in terms of 'competing' theories or perspectives.

As indicated, it is possible to introduce a somewhat artificial, though revealing, dichotomy into the voluminous literature on the subject of citation. On the one side there are those whose attitude and approach can best be described as pragmatic, or naively rationalistic. The social reality of citation is not a topic they dwell upon, for the good reason that they feel questions pertaining to underlying motives and needs are unlikely to result in answers or insights which can be readily converted into

practical improvements in existing citation indexing systems. On the other side of the divide are those (the positivists) who are unwilling to turn a blind eye to the more fundamental questions, even if the answers they seek are not immediately forthcoming.

The way in which the pragmatists deal with (side-step?) the niggling question of intention is worth noting, as is the language used. Smith, for example, summarizes the epistemological problem in the following manner:

> 'Although citation linkages do not necessarily reflect social contacts, it is *probable* that there is a certain amount of congruence between documental and social structures.' (Smith, 1981 : 95) (*italics added*).

And Small, though echoing these sentiments:

> 'The reasons and motivations for citing appear to be as subtle and as varied as scientific thought itself, but *most* references do establish valid conceptual links between scientific documents.' (Small, 1976 : 67) (*italics added*).

attempts to show that the difficulty has been exaggerated by arguing that citations operate as symbols representing identifiable and traceable concepts in the literature of a given field.

> 'The concept symbol interpretation of citation practice does not contradict the functional, social or political interpretations, but is complementary to them. Whether the motive for citing a work is politically conditioned or merely haphazard (for example, adding references to a paper after it is written, where they 'fit in'), the work must be associated with specific language in the text and cannot be appended without some explicit or implicit context'. (Small, 1978 : 337)

In his most recent work Small claims that co-citation links can be 'rewritten in sentence-like form' with the result that citation-derived networks can be 'translated into patterns of interlocking sentences' (Small and Greenlee, 1980 : 300; see also Cozzens, 1982). This idea

has been taken up by McInnis, who see the citation as a metaphor:

> 'A bibliographic citation, as a symbol for a concept, functions as a metaphor for the cognitive content of a specific publication. That is, the relationship between the cited document and the concept it symbolizes is metaphoric.' (McInnis, 1982 : 56)

If particular citations are associated with identifiable concepts, ideas or methods, then it is possible, in Small's view, for authors to engage in a meaningful dialogue because the cited works, within the context of a particular paradigm, have acquired a standard or conventional interpretation. It is the putative existence of such group vocabularies, or semaphores, which gives citation its objective character. Nevertheless, even though this approach gets close to eliminating some of the long-standing uncertanties surrounding citation, it does not, as Morman has noted, resolve the central issue:

> 'Small presents the concept symbol thesis explicitly in response to demands for a theory of citing behaviour. But by dismissing other possible explanations for choice of citation (e.g. the desire to persuade, to curry favour, to publicize, or to avoid offending) as inadequate Small avoids dealing with interactions between symbolization and other factors. He is thus left with a wide gap in his explanation of citing behaviour — why do particular authors choose particular cited works as symbols for particular concepts?' (Morman, 1981 : 11)

The 'queries and caveats', to use Porter's (1977 : 257) phrase, remain unanswered, despite Small's attempt to root citation in a theory of interactive symbolism. In Porter's view, citations are:

> '. . . measurable indicators, logically linked to interesting theoretical variables (e.g. scientific productivity, communication units or whatever), but the correct functional form of this linkage is unknown. At best, the errors in measurement thus introduced may not be serious, but at worst the indicator may appear inappropriate — 'construct validity' is lacking.' (Porter, 1977 : 263)

It is this lack of construct validity which has united a substantial body of sociologists of science to contest and probe some of the claims made on behalf of citation indexing. The split between the naive rationalists and the positivists is quite starkly revealed in the writings of two of the principal protagonists already mentioned, Small and Edge, respectively.

At the heart of the interpretative rift is the question of unknown motivations. Citation is not a transparent activity: the process is not amenable to scrutiny. To quote Miller (1962 : 71): 'It is the *result* of thinking, not the process that appears spontaneously in consciousness'. And so it may be with authors when it comes to citing the works of others.

Interpetation is impeded by our virtual ignorance of authors' motivations. Also, if we accept Miller's thesis, then attempts to expose personal motivations are likely to founder, for the simple reason that it is the results, not the process leading up to citation selection, which authors are likely to recall if questioned directly on their practices. We are, in fact, forced into a position where we have to accept Wittgenstein's aphorism: 'What we cannot speak about, we must pass over in silence' (1961 : 151). In some respects this appears to be the conclusion reached by Ravetz (1971), who was forced to view citation as an instance of tacit knowledge — a craft skill osmotically acquired. It may be, as we said earlier, that this is the most satisfying solution available, and in this respect the following quotation from Hudson is worth bearing in mind.

> 'Far from being a porridge-like and irrational mess, the mental processes of which we are unaware often prove to possess a high degree of *structure*. Indeed, some of the most precise thinking we do seems to be thinking to which we have little access, and over which we exert a minimum of control. Secondly, the distinction between conscious and unconscious is in any case far from clear-cut. There exist massively influential forms of thinking that are

strictly neither conscious nor unconscious, but *tacit*.' (Hudson, 1975 : 72)

The dichotomy referred to, naive realism versus positivism, does not only refer to participants' perceptions of citation. By implication, it reflects the different conceptions of science held by the two groups. For the naive realists science is likely to be thought of in terms of institutionalized standards and purposes, whilst for the positivists science is more likely to be conceived of in terms of particularistic accounts of behaviour. Krohn has captured the essentials of the alternative *Weltanschauungen* most effectively:

'Several authors have used the economic metaphor to describe current science. At least one finds the use of economic metaphor frequent among scientists themselves. 'Production and reproduction', 'investment of resources', 'symbolic capital' etc. have made insights available into relations among resources, the exchange and conversion from one kind of research resource, such as scientific 'credit', to another, research or publication in journals, etc. That is, the economic language has allowed the juxtaposition and interpretation of elements otherwise segregated into 'intellectual', 'professional', 'institutional', 'cognitive', etc., categories. Less explicitly, economic language has allowed the transition of the semi-mystified or sacred language of institutionalized science into a secular language. Thus behind noble motives, 'love of truth', or 'curiosity' can be detected the down-to-earth motives of seeking recognition, prestige and career advancement.' (Krohn, 1980 : xviii)

Looking at the issue from a slightly different angle, how is it that the subjective, intensely personal activity of creative science can be transmuted into objective, impersonal knowledge? How is it that citation (a private process) can be accepted as science's principal accounting mechanism, upon which so much depends? The most successful attempts to answer questions of this kind have been made by a number of the 'new wave' sociologists of science (e.g. Edge, Mulkay, Gilbert, Latour and

Woolgar, to mention some whose work we have already referred to). The microsociological approach breaks with scientometric and quantitative traditions by preferring to focus on the quotidian reality of science as it is practised. Science, in other words, is taken off its pedestal, and the process of knowledge creation viewed, less as a mechanistic, well-oiled sequence of actions, than as a fortuitous harmonizing of individual and institutional objectives. The new conception of science has been described in the following way.

> 'A high/distant/formidable image is replaced by a low/close/everyday one. An abstract/spiritual/perfect image is replaced by a physical/imperfect image. And an impersonal/universal/permanent image is replaced by an image of personal work carrying the marks of a craftsman and his time and location of work, which is assumed to be of temporary utility and value.' (Krohn, 1980 : xii)

Although an interpretative dichotomy exists, some degree of harmonization may, nonetheless, be possible. As Cozzens (1981) says, there are a number of perspectives which can be adopted (she speaks of the normative, the interpretative and the symbolic), but there is no necessary reason why one should be preferred at the expense of the others. It is her contention that the various theories need not be viewed as being in competition, and that in the course of writing a paper a scientist's actions may be consistent with aspects of the various perspectives. Furthermore, to refer again to Cozzens (1981 : 10), there is a need for research into citation which 'cuts across the traditional theoretical orientations of the sociology of science and involves cooperation among people with different methodological inclinations'.

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Citation 'in the round'

Citation needs to be considered in relation to the habits, attitudes, experiences and expectations of four loosely defined groups: the Quality Controllers; the Educators; the Consumers; and the Producers. Together, these four groups shape, define and exemplify through practice the norms of citation, in so far as these can be said to exist. Yet, citation tends not to be discussed in terms of the behaviours and perceptions of these four constituencies. Instead, citations are spoken of as if they were things in themselves (*Dinge an sich*). Imperceptibly, the humble citation has undergone a process of reification. The four-fold categorisation proposed above may prove helpful in developing a more rounded and balanced appreciation of what citation entails.

The Quality Controllers are those who act as the scientific community's invigilators, i.e. journal editors, referees and editorial board advisers. It is this select group which vets submitted manuscripts and, by extension, the attached lists of citations. The attitudes and practices of this group *vis à vis* citation are little known. Both Garfield (1977) and Price (1964) have commented on the need for, and desirability of, greater control and consistency in refereeing practices as far as citation is concerned.

It may be that citation cannot be defined more

satisfactorily other than in terms of tacit knowledge (a skill which is acquired and exercised largely without conscious reflection), and it may be that fledgling authors model their citation behaviour on that of established authors in their particular field, but this 'learning through modelling' theory remains untested. Likewise, the role played by university teachers and mentors in socializing students into the conventions of the field is something of a 'black box'. The Educators are, therefore, an important group to consider in any analysis of citation.

The Consumers are those at whom citations are directed; in other words, the readers of the scientific literature and members of the attentive scientific community. In some respects, little is known about their requirements, expectations or reactions to citations.

The fourth group, the Producers, are those who are actively involved in the scholarly publication process. The practices and habits of this group effectively define, shape and exemplify the prevailing (and presumably accepted) standards in a given domain.

It is not for a moment being suggested that these four groups constitute mutually exclusive populations; membership of one does not necessarily preclude membership of another, and in many cases members of one group are *de facto* members of a second or third, or may after successful apprenticeship become so. Nevertheless, this crude classification provides an articulated structure for a more sensitive analysis of the citation process.

In an article entitled *The outlook of journal editors and referees on the normative criteria of scientific craftsmanship*, Lindsey and Lindsey (1978) examined the criteria used by editorial board members in a number of social science disciplines in appraising submitted manuscripts. Their aim was to lay bare the technical and artistic norms against which authors' submissions were assessed. Res-

pondents were asked to react to a twelve-item list of criteria (see below) so that the relative importance of the various dimensions could be established. Citation was not one of the criteria used, though it could perhaps be swept under 'scholarship' or 'empirical evidence'.

Journal editors' normative criteria: Lindsey & Lindsey (1978)

Value of findings	Grasp of design
Theoretical relevance	Scholarship
Creativity of ideas	Empirical evidence
Sophistication of methods	Relevance to journal
Ethical sense	Value to social life
Entertainment quality	Reputation of author

In reviewing the role and evolution of the scholarly journal, we made the point that the conventions and procedures for reporting research findings were so designed as to minimize the possibility of suspect or fraudulent data being accepted into the journal archive and dispersed throughout the scientific community. Within this scheme of things journal editors and the supporting ranks of advisors ('the institutionalized interpreters of normative criteria' Lindsey, 1978 : 17) operate as quality controllers. Senior members of the scientific establishment undertake quality control work because they see this as a necessary consequence of their belief in the norm of communality. In Meadows' view:

> 'This will bind them to the negative task of ensuring that a paper contains no demonstrable deviations from normal scientific standards . . .' (Meadows, 1974 : 41)

But he also states that detailed criticism over and above this is somewhat rarer. Indeed, the refereeing process by its nature is not entirely objective or consistent. An ingenious study by Peters and Cecci (1981) of inter-referee reliability, which involved the re-submission of accepted articles to the same journals, clearly demonstrated the problems associated with manuscript eval-

uation, and called into question some of the basic assumptions made about the journal refereeing system in general (see also Whitley, 1970).

It may therefore be legitimate to see citation as the 'Achilles' heel' of the journal refereeing system: one particular area in which an author's judgement and habits are perhaps not always subjected to close scrutiny. Price (1964), for instance, recommended that journal editors and referees should summarily reject bibliographies that are either insufficient or padded. Others, notably Cleverdon (1970) and Garfield (1977), have expressed essentially similar views. In practice, matters are neither so simple nor so easily remedied. One very good reason for this is that the literature, even in a narrowly specialized field, is too voluminous for a single author to utilize fully (Kochen and Perkel, 1978). If this is true for authors, then it is even more likely to apply to editors and referees. Lawani (1977A) also makes the point that it can be a problem finding enough good referees.

One of the sharpest criticisms of citation practices was delivered by Thorne (1977), an academic with thirty years editorial and publishing experience in psychology. He concluded his article, *The Citation Index: another case of spurious validity* with the following comments:

> 'In my tenure of editorial office, I came to the conclusion that reference citations were idiosyncratic and non-evidential. I deliberately limited authors to citing only a few directly relevant citations in reference to the specific points at issue. Such editorial standards cannot avoid influencing citation rates, and such factors should be considered in evaluation studies.' (Thorne, 1977 : 1161)

His experience suggested that authors employed a variety of stratagems to manipulate citation frequencies.

These are summarized overleaf:

Authors' citation stratagems: Thorne's list (1977)

1. Serial publication (division of a single research project into many parts, each reported separately)
2. Multiple publications (minor variations of a project report submitted to different journals)
3. Hat-tipping citations (acknowledgement of eminent figures)
4. Over-detailed citations
5. Over-elaborate reporting
6. Evidentiary validity (citations can be selected to support any point of view)
7. Self-serving citations
8. Deliberate premeditation (conscious playing of the citation game)
9. Searching out grant funding (identifying currently popular research trends)
10. Funding support for publications (the publication of luxurious research reports to attract attention)
11. Editorial preferences (authors seek to identify preferred topics and styles of journals to which they submit)
12. Citations as projective behaviors (citations as reflection of author biases)
13. Conspiratorial cross-referencing (the 'you scratch my back and I'll scratch yours' syndrome applied to citation)
14. Pandering to pressures (citing works because it is felt that the reading public requires, or expects, them to be cited)
15. Editorial publication policies (discriminatory biases in editorial policies re. selection and rejection)
16. Non-recognition of new authors
17. Intra-professional feuding
18. Obsolete citations
19. Political considerations (citing the 'party line')

It is instructive to think of Thorne's list as the obverse of that produced by Weinstock (1971). Both lists offer a variety of explanations as to why authors cite as they do, but whereas one (Weinstock's) is closely aligned with the 'storybook' image of science, the other (Thorne's) is less reverential and closer to the thinking of the micro-sociologists.

In an effort to explore the views of Quality Controllers, Cronin (1982A) carried out a survey of journal editors and editorial board members in a sample of English-language psychology journals. The results obtained tend to support Cozzens' view that the different perspectives on citation (e.g. the normative and the interpretative) are artificial. Respondents' replies to Cronin's questionnaire did not reveal an uncomplicated split along this notional binary line. To some extent this lack of consensus suggests that editors have widely differing outlooks on the significance, importance and functions of citations. In part this can be explained by the lack of explicit guidelines for authors on how and why they should cite; it may also, to some degree, be explained by the fact that citation, alongside other factors, such as the presence of demonstrable errors, insignificant results, lack of originality, or lack of clarity, plays a relatively small part in manuscript evaluation (Gordon, 1979). Although there has been some discussion of ways to improve manuscript evaluation (e.g. Bowen *et al*, 1972; Wolff, 1973; Scott, 1974) the question of citation standardization has received little explicit attention.

Entrants to the world of career science and research have to learn to accept that there are certain ways of doing things (such as citing the works of others), and that certain rules, traditions, etiquettes and codes of conduct determine the limits of acceptability for individual actions. The newcomer is a little like the traveller in a strange land who has to adjust to different cultural sets and expectations. 'Doing science' is not just a matter

of having an enquiring mind or high academic ability: it also has to do with playing the 'Game' according to the written and unwritten rules. Training and preparation for a career in science goes beyond the acquisition of technical skills and competencies; it requires that a student be socialized to the mores and behaviours of the discipline he is preparing to enter. There are, so to speak, certain rites of passage to be undergone before the transition from apprentice to craftsman can be considered completed. To quote Fisch:

> 'Becoming a scientist involves not only an educational process and an accumulation of knowledge and skills but also in part a filtering process through which one goes in the course of professional development.' (Fisch, 1977 : 289)

Much of the socialization process, what Bourdieu (1972 : 45) terms 'pre-reflexive adherence to the tacit pre-suppositions of the field', will take place on-the-job, but university provides an early and extended introduction to the academic and research ethos. The extent to which university prepares neophytes and encourages enculturation will depend on the attitude of faculty, and how they see their role as socializers.

This process has been most aptly described by Hagstrom in his book *The scientific community*:

> 'The effects of scientific socialization are reinforced by a highly selective system of recruitment. Of the fraction of the population who enter college, only fractions of those interested are permitted to graduate in the exact sciences and enroll in graduate school. Attrition in graduate school tends to be high, and only the more competent and highly motivated students obtain the doctorate. Among those who do obtain doctorates in science, only a fraction are permitted to enter careers in basic research; the rest become teachers, administrators, and applied scientists. Basic scientists, then, are a highly selected and socialized elite group. The entire socialization and selection process tends to produce scientists who are 'self-starting' and 'self-controlling'. A common view of the organization of science, held implicitly or explicitly by most

> scientists, is that these individual characteristics are sufficient to account for conformity to scientific values and norms.' (Hagstrom, 1965 : 11)

However, he also notes that the effectiveness of the socialization process will depend, to some extent, on the concreteness of the norms which operate in a particular community of students and scholars. In his view, there is less likelihood of deviation from norms when those norms are specified for a concrete set of practices. Thus, we would expect physical scientists to be less likely to deviate from the norms of science and scholarship than social scientists or humanists.

Cronin (1983) has reported a study of current citation training practices in British and American university psychology departments. His findings suggest that the approaches used differ considerably from department to courses on research methods and the ethics of citation) to the highly informal (e.g. co-authorship involving faculty and students). Overall, however, Educators appear to be concerned to instill an appreciation of correct bibliograpic format, but less concerned with explaining the underlying principles. Cronin concluded that much more could be done by Educators to improve the general quality and consistency of students' citation practices, a view echoed by a number of other writers (e.g. Basefsky, 1982; Lawani, 1977A) including Voverene (1981) who advocated that the ethics of scientific work (in particular the underpinning rationale of citation) should be taught as a matter of course in scientific training programmes.

We do not know why an author cites in a particular way at any given moment. We may be able to guess (even guess correctly), but we cannot be certain that we have adduced the real reason or motivation. It may be a truism to state that citation is private event, but the point needs to be made, if only because it serves as an antidote to the infectious appeal of the normative position, which, if accepted at face value, would commit us to the view

that the actions, behaviours and statements of scientists are governed and directed by a (largely unseen and unspoken) set of universalistic and communistic principles. In an ideal world, citations *would* be taken as objective markers or symbols; *would* be seen as being employed in consistent fashion, and *would* be susceptible to consistent analysis and interpretation. As we have tried to show, this view is, if not suspect, at least incomplete.

Theoretically, if citation were dependent solely upon the character of the citing paper and its objective relationship to existing literature and scholarship, and in no way dependent upon the perceptions, needs, attitudes, prejudices, background and erudition of the author, then the occurrence of citations within a paper should be predictable. Recognising this fact, Garfield (1965) discussed the possibility of citations being generated automatically by computer without any direct involvement on the part of the citing author. He was, in effect, speculating on how the citation process might be 'desubjectivized'. The intuitive hypothesis suggests that even if two equally well-informed individuals were presented with a citation-less paper and asked to 'dress' or 'prime' it with citations, that the two sets of recommendations would differ. Even if only asked to indicate *where* citations were required, without specifying *what* they should be, it is highly improbable that there would be a perfect match between the two lists. However, even if this supposition could be demonstrated, it would not necessarily invalidate the normative position. The normative view does not have to be rejected if anything less than 100% adherence to the supposed norms is registered. As Mitroff (1974A) observed, the norms of science can have regulative status, insofar as they embody the *ideal* standards of rationality. The fact that prevailing standards fall short of the ideal does not mean that we cannot, or should not,

continue to aim for the ideal. The standards remain the rational ideal, even if our behaviours do not always exemplify them.

Bibliographic citations have been described variously as symbols, markers, metaphors and signposts. Generically, citations act as signs: they denote particular works and seem to imply some sort of relationship between the citing and cited work. The study of signs is known as semiotics, and can be thought of in terms of three levels (Cherry, 1978 : 223).

Syntactics: signs and their relation to other signs
Semantics: signs and their relations to the outside world
Pragmatics: signs and their relations to users

As a rule, citation analyses are concerned with the first of these; that is to say, they explore and articulate patterns of connectedness between signs (citing and cited document elements). Small's (1978) concept marker theory is perhaps the closest to the second of Cherry's categories, in that it seeks to establish equivalence between a sign and a theory, model, concept or methodology. Cronin (1983) has described a number of experiments which attempted to explore the relationship between signs (citations) and users (readers) of the literature — what Cherry calls pragmatics.

Cronin's strategy was to distribute unpublished journal articles denuded of their original citations to carefully selected samples of readers, asking them to suggest where citations were required. The innovative feature of this approach lies in the fact that it shifts the focus of interest from the author (producer) to the reader (consumer). Traditionally, citation studies have taken as their starting point the lists of citations attached to published papers and proceeded to establish networks and connections based on such data sets. This means, in practice, that little attention is given to what the reader

expects or requires. The result, inevitably, is generally a selectively biased picture of the citation process. And yet, citations are a public commodity (and scholarly journals their common carrier) used by various communities of interest.

The results of Cronin's infill experiments suggest that the ways in which different groups perceive the need for citations display an underlying regularity. Although the response levels in his study were poor, there did nonetheless appear to be some evidence of a shared understanding as to how and where citations should be affixed to a scholarly journal article. However, Cronin was unable to show that certain trigger words or kernel phrases were unambiguously associated in the readers' minds with the need for citation (the idea put forward by Finney, 1979. Had this been demonstrated, then it would have been possible to think of citations being generated automatically without any direct involvement on the part of author (Garfield, 1965).

The great mass of the literature on citation is producer-oriented, i.e. it consists of quantitative analyses of the citation lists generated by individual authors, by particular research communities or networks of scholars, or of the citation lists attached to particular journals or clusters of journals (Hjerppe, 1980). Viewed as a social system, science is international and cosmopolitan. Adherents of the normative view would argue that, despite the many differences between individual nations and individual scientists, the actions of scientists in citing are guided by supra-national and supra-personal considerations.

However, Cronin's (1981) study of transatlantic citation patterns in educational psychology suggests that this may not be the case. Even though two or more groups of readers of a particular paper might propose the same number of citations, and agree on the locations for those citations in the text, it would not follow that their

choice of works (and the nationality of those works) would necessarily be the same. For instance, British authors/readers might propose a preponderance of British citations, and American scholars a preponderance of American works, or individuals within either group might show favouritism towards colleagues or fellow ideologues.

Citation analyses of communication within psychology (Cronin's field of study) are not uncommon. Since Cason and Lubotsky's (1936) pioneering study of journal interactions there have been several attempts to evaluate journal impact and connectedness, using both subjective and quantitative approaches (e.g. Jakobovits and Osgood, 1967). One of the best known quantitative studies was that of Xhignesse and Osgood (1967), which established a matrix representation of flow and interdependence within a 21 journal network. More recently, Pinski and Narin (1979) refined Xhignesse and Osgood's matrix display idea to produce a set of citation influence measures for a variety of psychological sub-fields. This did not, however, include educational psychology, the area studied by Cronin. Cronin analysed citations linked to papers appearing in two educational psychology journals (one British, the other American) and found that the profiles of the two journals differed markedly, with US authors displaying a heavily ingrown citation tendency (i.e. on average US authors cited 95% American works, while British authors cited rougly 40% American authors).

There are many reasons why authors display inward-looking citation tendencies: proximity or sense of loyalty to local colleagues; institutional affiliation; ease of access to home-produced literature; vague political or cultural pressures; linguistic isolation. However, as far as educational psychology is concerned, there are in the main shared concepts, experimental practices and a common knowledge base, all of which would be expected to

facilitate healthy interaction across national boundaries. Cronin's findings of citation insularity (based, though they were, on a small sample) are not idiosyncratic. Inhaber and Alvo (1978) found that the US literature attracted by far the greatest number of citations from within and without. They found, *inter alia*, that US journals were approximately seven times as likely to cite themselves, or other US journals, than journals from the UK, while UK journals, by way of contrast, divided their attention almost equally between the US and the UK literature. Further evidence of the global dominance of US literature in science has been provided by the American Psychological Association (1965), Lawani (1977B), Bath University (1979) and Jagodizinski-Sigogneau *et al* (1982).

To allow that bias is possible is not quite the same thing as imputing base or mischevious intent to authors whose citation practices deviate from the expected norm (Broadus, 1983). However, recognition of the fact that citation may be coloured by extra-scientific factors is a useful caution to the extremes of the normative position. In his article *The disinterested scientist: fact or fiction*, Mitroff (1973 : 765) argues persuasively that science derives great strength from its 'contaminating subjective elements'. To support his case, he provides a wide variety of quotations from eminent scientists reflecting on the presence of, and need for, subjective biases in science. Contrary to what might be expected, many scientists seem opposed to the simplified interpretation of scientific behaviour implied in the normative view. The following comment from one of the Apollo moon scientists interviewed by Mitroff should illustrate his point.

> 'The disinterested scientist is a myth. Even if there were such a being, he probably wouldn't be worth much as a scientist. I still think you can be objective in spite of having strong interests and bias. If you make neutral statements, nobody really listens to you.

> You have to stick your neck out. The statements you make in public are actually stronger than you believe in. You have to get people to remember that you represent a point of view even if for you it's just a possibility. It takes commitment to be a scientist. One thing that spurs a scientist on is competition, warding off attacks against what you've published.' (Mitroff, 1973 : 765)

In Mitroff's view (1973; 1974B), bias and commitment are not necessarily negative features. Without them, he maintains, science would be an impoverished undertaking. If Mitroff is right, then we have to consider the implications as far as authors' citation practices are concerned. We have, to use Merton's stylish phrasing (1968B : 271), to take account of the interaction between normative imperatives and 'ethnocentric particularism'.

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Towards a synthesis

We began this essay by presenting two broad approaches to the analysis of science and the professional activities of scientists. Traditionally, the analysis and interpretation of science has favoured a normative view of how science is organised and how scientists' conduct is shaped and controlled. This perspective has been described by Mulkay (1977) as having a plausible descriptive rhetoric. In certain respects this view was confirmed and encouraged by the writings of sociologists and historians of science such as Merton, Barber and Storer. The great appeal of the normative view is its apparent ability to reconcile individual ambition with collective concerns. Thus, acceptance of the normative position made it possible to preserve the 'storybook' image of science, even when the totality of available evidence might have pointed to the need for theory modification or revision. In Mulkay's view, however, the lack of direct data on scientists' commitment to social norms is nothing less than astonishing.

With the emergence of a new wave of interest in the social processes of science came a challenge to the hegemony of the normative view. The iconoclasm of the interpretative approach to the sociological analysis of science required that the taken-for-granted assumptions upon which the normative view of science was posited

be re-examined. In some quarters the static view of scientific behaviour was replaced by an on-going process of empirical investigation and cautious theory construction. Science, it was contended, did not in fact display the imagined or hoped-for regularities ascribed to it by Merton and his followers: instead it was seen as being subject to continual negotiation. Interpretative sociologists preferred not to think in terms of a high degree of cognitive and normative consensus, but in terms of negotiated and fluid interactions between individuals. That is not to say that the interpretative sociologists rejected out of hand the idea that science was in part normatively regulated, merely that this assumption should only be accepted if empirically validated in terms of the behaviours, actions and testimonies of individual scientists.

Thinking specifically in terms of citation it is not hard to see why the normative view should have proved so popular. An all-embracing normative framework meant that it was possible to interpret citation as yet another instance of scientists' shared appreciation of how and why they should act in a manner conducive to the common good. Consequently, there was no need to seek a fundamentally fresh or original explanation of citation behaviour in general, nor any need to undertake detailed and laborious analyses of individuals' approaches to citation. Citation was seen as being locked into the prevailing view of science as a normatively governed system, in which individuality played second fiddle to communism. The distinguishing feature of interpretative sociology was its reluctance to accept generalizations which were not behaviourally or experimentally derived, however convenient the alternative. It preferred to take as its starting point the often mundane, but nonetheless relevant, daily realities of the working life and relations of individuals, and from these to fashion models (perhaps narratives would be a better term) of scientific behaviour.

By far the greatest attraction of the normative position, as far as citation is concerned, is that it offers an all-in-one account of a complex phenomenon. Within the normative framework the strength of the norms and the power of the associated sanctions are deemed sufficient to check idiosyncratic and individualistic behaviour. In a sense, the normative position 'straitjackets' behaviour in a coat of rationality and enlightened group concern.

The normative view has been described as 'bad metaphysics' (Mitroff, 1974B). This may be something of an overstatement, but in the writings of Polanyi (1966A; 1966B) there is a tendency to veer towards a metaphysical account. It may be difficult to resist the arguments put forward by Polanyi, if only because of the subtle way in which he shepherds us towards the desired conclusion. In preparing the reader for the unorthodoxy of his view (citation as an instance of 'tacit' knowledge) Polyani engages in a process of low-key desensitization. The following two extracts illustrate the approach and the language used.

> 'The process of formalizing all knowledge to the exclusion of any tacit knowledge is self-defeating.' (Polanyi, 1966A : 20)
>
> '. . . an unbridled lucidity can destroy our understanding of complex matters.' (Polanyi, 1966A : 18)

That being the case, one can easily begin to see the attractiveness of placing citation among that class of activities whose exercise and acquisition are not readily amenable to exposure and analysis. If we were to adopt Polanyi's position, then we might choose to think of citation as a skill like bicycling or swimming.

> 'If I know how to ride a bicycle or how to swim, this does not mean that I can tell how I manage to keep my balance on a bicycle, or keep afloat when swimming. I may not have the slightest idea of how I do this, or even an entirely wrong or grossly imperfect idea of it, and yet go on cycling or swimming merrily. Nor can it be said that I know how to bicycle or swim

> and yet do *not* know how to coordinate the complex pattern of muscular acts by which I do my cycling or swimming. I both know how to carry out these performances as a whole and also how to carry out the elementary acts which constitute them, though I cannot tell what these acts are. This is due to the fact that I am only subsidiarly aware of these things and our subsidiary awareness of a thing may not suffice to make it identifiable.' (Polanyi, 1966B : 4)

As with many analogies, its usefulness is skin-deep. To view citation in this way would mean glossing over a number of critical differences (between citation and swimming). Swimming is a motor activity — one is either able to swim or not (to put it crudely). For the would-be swimmer there is immediate, unequivocal performance feedback. If he sinks, then he has not suceeded in swimming; if he remains afloat in swimming mode, then he has been successful. These are baseline measures (sinking or staying afloat) against which to assess performance. As measures, they are both objective and definitive. Such performance measures do not exist so far as the evaluation of citation behaviour is concerned. Secondly, to rely on an analogy of this kind would be to overlook the matter of intentionality. The notion of subsidiary awareness which Polanyi proposes as an explanation of how citation is correctly practiced does not take into account the potentially enormous variability of individuals' motivations for citing in a particular fashion. In some respects, this echoes Merton's (1972) distinction between 'acquaintance with' and 'knowledge about'. Thus, we might think of authors as being acquainted with the protocols of citation, but not always or necessarily knowledgeable about its significance. What both Polanyi and Merton, in their different ways, seem to be saying is that there are levels of knowledge and appreciation.

Perhaps we need to think of citation along similar lines: a process which is residually subjective, but which,

through the application of a set of more or less agreed techniques, aspires to be a vehicle of universal communication within the scientific information exchange process.

A failing of normativism is that it takes no heed of the 'soft underbelly' of science (Edge, 1979 : 115). The deficiency of Polanyi's position is that it eliminates the motivational component in citation. A frustrating aspect of the interpretative perspective proposed by contemporary sociologists of science is that it denies us a simple and satisfying account or theory of citation generally. What we now need to do is consider the extent to which it is possible to develop an integrated account of citation behaviour from the evidence to hand.

We began by saying that science should be thought of as a social system. The participants in this social system, career scientists and researchers, enter of their own volition. In so doing they presumably recognise that there will be constraints on individual actions, and responsibilities towards others operating within the system. The stability of the social system is maintained by a delicate process of complementary interaction. To quote Storer:

> 'The patterns of interaction among two or more participants in a social system are maintained because the reaction of the participants to one another's behavior is mutually rewarding. Implied here is the assumption that each participant has some standards by which to judge whether the other's actions are appropriate or not, and also that the other participant knows of these standards so that he can choose the behavior that will elicit rewarding behavior from the first.' (Storer, 1966 : 32)

In reality, things are rather more harried; rather more complicated. The normative view of science tends to play down, or gloss over, the more robust and venial aspects of scientific life. Even in 1902, Rutherford was writing to

his mother saying:

> 'I have to keep going, as there are always people on my track. I have to publish my present work as rapidly as possible in order to keep in the race.' (quoted in: Crowther, 1952 : 54)

In the intervening eighty or so years the pressures and stresses associated with science have increased greatly. The competitiveness of science has attracted investigation, and there is evidence to suggest that the pressures on scientists substantially affect their social interactions (e.g. Gaston, 1971; Watson, 1970).

Science is institutionalized competition, and scientific knowledge is a cultural artifact (Collins, 1975). In academic life (particularly) the main reward or intangible reinforcer is peer group recognition. Mitroff's (1974A) deep-felt conviction of this led him to propose a set of counter-norms to explain the actions of scientists. As a result, much modern sociological investigation has concentrated on stripping away the romance associated with professional science. Bernal (Goldsmith, 1980) feels that the scientific paper functions as a sketch map to the truth, while Medawar is of the opinion that it is a fraud:

> 'The scientific paper is a fraud in the sense that it does give a totally misleading narrative of the process of thought that goes into the making of scientific discoveries.' (Medawar, 1964 : 43)

According to Meadows, this fraudulence has been cultivated intentionally:

> 'One motive behind this development has clearly been the desire to transmit research results in a stylized and impersonal form so as to transfer both information and priority claims with the greatest possible efficiency.' (Meadows, 1974 : 82)

If the scientific paper is a fraud, then one needs to ask how this might affect authors' approaches to citation. Gilbert (1977) has argued that citation should be thought of as a process of persuasion. How, in that case, does an author maintain the necessary 'delicate balance'

(Kaplan, 1965) between the desire to make priority claims and the requirement that recognition and credits be distributed equitably?

Perhaps the single most important criticism of citation analysis is that it ignores this tension. As Cozzens (1981) noted, many citation analysts view their measures as direct manifestations of certain social constructs, without visualizing at the same time the scientists who create the patterns. Citation is part of the social process of science, but this fact is over-looked in the majority of applied citation analysis studies.

Citation is a complex issue. Earlier, we outlined a number of interpretative perspectives which could be applied to citation, but it may be, as Cozzens (1981) has proposed, that there is no need to settle for one of these at the expense of others. In her view, the action of scientists in writing a scholarly paper are consistent with the various perspectives. It is also consonant with Maslow's perceptions of how professional scientists behave.

> 'It is possible for healthy scientists to enjoy not only the beauties of precision but also the pleasures of sloppiness, casualness, and tight and/or loose, sensible and/or crazy, sober and/or playful, seems to be a characteristic not only of psychological health but also of scientific creativeness.' (Maslow, 1966 : 31)

If Maslow is correct in his assessment, then it is only to be expected that sloppiness, casualness and veniality will be exhibited occasionally in the citation practices of scientific authors.

In a thought-provoking paper, Wartofsky (1982) proposed a social model of medical knowledge, i.e. a model of medical knowledge as a social product and of medical practice as a social activity. In doing so he argued that neither the product nor the activity could be fully comprehended other than in terms of social relations and the ideological framework within which members of the medical community operated.

In defining the nature of medical knowledge, and the nature of possession of this knowledge, he invoked the knowledge-as-property metaphor. This metaphor is also used in relation to citation, the latter being seen as one means of protecting intellectual property rights in the scientific marketplace. Wartofsky's idea is noteworthy because it posits three forms of property right. These forms he defines in the following manner:

> 'The first form of possession may be seen as akin to slave systems in which the slave has no possession . . . The second form is that of communal or feudal property in which individual use . . . is on the condition of certain obligation to the commune or to the feudal lord, and the right to use of one's possession is a fiduciary right . . . The third form of private or individual ownership with the concomitant right to use . . . is in fact the classical model of the free-exchange system of capitalism, where property exchanges for property through the medium of money, by the consent of exchangers . . . knowledge or rather disposition over its use in this context is a commodity, exchangeable for a fee, in the market-place.' (Wartofsky, 1982 : 126)

However, having identified three forms, or evolutionary phases, he found that no one seemed to apply to medical knowledge.

> 'The peculiar thing about medical practice is that it is caught up in all of these three forms of property-right and represents a complex historical palimpest of these modes.' (Wartofsky, 1982 : 126)

Now, as we have attempted to show, there is no single, all-embracing theory of citation. We have also seen that the appropriateness of the various analytical perspectives is situationally or contextually dependent. Citation, however we care to view it, ostensibly involves the expression of a relationship between two participants in the social process of science — the citing and cited authors. The coordinates of this relationship are the atoms of knowledge (intellectual property) defined by the citing and cited works. Using Wartofsky's model we

might like to think of citation in relation to the three forms of property possession mentioned above. Better still, we should perhaps think of citation in terms of a 'complex historical palimpest'. The advantage of such a move would be to rescue us from the invidious, if not impossible, position of having to explain citation in terms of one, and only one, of the available interpretations. In Wartofskian terms the 'logic' of citation is etched on a complex palimpest, which seeks to acknowledge the three forms of knowledge possession.

The first of these, the slave image, corresponds to the normative view of science (and by extension citation) as a socially responsible activity devoid of personal or selfish considerations. Knowledge is universalistic and scientists are fired by the principle of communism. Ownership and intellectual property rights are therefore antithetical to this world view.

The second, or feudal, form of knowledge possession is a somewhat less severe version of the first. The individual scientist or researcher, though having to acknowledge the ultimate authority of the scientific establishment (and the general public, for that matter), is nevertheless entitled to some degree of recognition and some claim-staking, provided personal interest does not get in the way of the greater scientific and communal good. There is, after all, a difference between possession and possessiveness.

The third form corresponds in many respects to the interpretative approach espoused by the microsociologists. In other words, the actions of scientists are expressions of personal motivations and ambitions (not least the search for peer group recognition and acclaim), which may on occasion be moderated by 'purer' considerations. In this view of things scientists see their atoms of personally discovered knowledge as belonging unequivocally to themselves. These they can exchange for a particular sort of currency — citations from other

members of the scientific community. Knowledge is thus a property which can be exchanged or traded for public recognition. Citation behaviour has to be seen in relation to an individual's background, professional training and experience, and belief system. Differences in ideology or in the processes of socialization to the norms of scientific conduct; or differences in the practices, structures and social dynamics of subject fields and research communities, will also effect citation behaviour.

Because citation is a private act, and because there are many shades of interpretation as to what constitutes correct or acceptable citation, it is particularly difficult, if not impossible, to speak of a theory of citation. At both the individual and group level differences in approach to citation are widespread. Such inconsistency does not facilitate the construction of predictive theories. And yet, one *senses* (and the word is used advisedly) that authors do have some shared perceptions and understanding of what is called for and why (Cronin, 1983). It may be this vague feeling that the practice is not entirely haphazard or whimsical, which explains why writers on the subject have resorted to terms such as 'osmosis', 'tacit', 'cultural transmission', and 'craft skill'. Hence, too, the appeal of the notion of 'self-coordination by mutual justification' (Polanyi, 1966A : 71), which conveniently sweeps the interpretative difficulties under a metaphysical carpet.

Citation needs to be thought of as a process. The outcomes of this process (on a recurring basis) are lists of citations attached to scholarly papers. The character and composition of the lists reflect authors' personalities and professional *milieux*. The elements in the chemistry of citation are almost infinite, and it is this fact which necessitates particularistic accounts of citation. We need, in effect, to move into the 'unexamined psychology of science' (Mahoney, 1976) if further progress is to be made.

One of the issues which we have alluded to, though not

attempted to resolve, is that of the relationship between cognitive style and an individual's personality (Mitroff, 1977). Future studies will have to devise novel methods of defining and articulating the nature of this relationship, if our understanding of what citation behaviour entails is to be expanded. There is no single theory of citation capable of telling us why authors cite in a particular fashion. Singer has provocatively suggested that, if asked for an explanation of the ultimate *raison d'être* of science, few scientists would be able to reply with confidence and assurance. On the other hand, 'they are fairly sure they know one when they see one' (Singer, 1971 : 1012). Perhaps the same applies for citation.

We cannot say that citation is an activity governed by adherence to a specific and universally recognised set of norms. By the same token, the evidence does not permit us to conclude that the practice is characterised by randomness and inconsistency. The interplay between institutional norms (even if only vaguely grasped by authors) and personal considerations is extremely complicated. We may not be able to champion the normative view, but by the same token we are under no obligation to subscribe wholly and uniquely to the literalism of the interpretative approach. As Mitroff says of science in general:

> 'It is too simple to say that the process or system of science is either totally objective or totally subjective.' (Mitroff, 1972 : 615)

So indeed, it may well be as far as citation is concerned. To seek and expect to find a single, theoretically integrated and undimensional account of authors' citation behaviour may be as unprofitable an activity as it is unrealistic. The 'storybook' account of science may be a fairytale (Mitroff, 1972), but the 'warts-and-all' version would itself appear to veer occasionally into the realms of fantasy.

Kuhn has argued that there are two sorts of history of

science, the internalist and the externalist (see below), and it may be that the same can loosely be said to apply to citation.

> '. . . there seems at times to be two distinct sorts of history of science, occasionally appearing between the same covers but rarely making firm or fruitful contact. The still dominant form, often called the 'internal approach', is concerned with the substance of science as knowledge. Its newer rival, often called the 'externalist approach', is concerned with the activities of scientists as a social group within a larger culture. Putting the two together is perhaps the greatest challenge now faced by the profession . . .' (Kuhn, 1968 : 76)

That is to say: most citation analyses have been internalist rather than externalist in character, in that they have concentrated on quantities and frequency distributions rather than on the contexts within which, and processes by which, authors employ citations. The reservations which exist about the use and validity of citations as indicators of impact and performance are unlikely to evaporate simply because further quantitative studies are carried out, or because more refined citation mapping and clustering techniques are developed. Citation analysis requires the sort of firm and fruitful contact between these two approaches which Kuhn feels is needed in the history of science.

Although his research was not specifically concerned with scientists' appproaches to citation, Mitroff (1973; 1974A; 1974B) has perhaps succeeded in furnishing us with a richer and more plausible interpretation of how scientists view their own actions and attitudes, and those of their peers. If novel insights on citation are to be mined, then an approach similar to that used by Latour and Woolgar (1979) in their 'anthropological' account of life in the laboratories of the Salk Institute will be required. Investigators will need to mix with and participate in the work-a-day life of authors and scientists if they are to grasp fully the significance of citation in the

social process of science. As Latour and Woolgar state:

> 'The production of papers is acknowledged by participants as the main objective of their activity. The realisation of this objective necessitates a chain of writing operations from a result first scribbled on a sheet of paper and enthusiastically communicated to colleagues, to the final registering of published literature in the laboratory archives. The many intermediary stages (such as talks with slides, circulation of preprints, and so on) all concern literary production of one kind or another. It is thus necessary carefully to study the various processes of literary production which lead to the out-put of papers.' (Latour and Woolgar, 1979 : 71)

Citation is not something which happens in a void, and citations are not separable from the contexts and conditions of their generation. Citation, to use the words of Latour and Woolgar, is part of the various processes of literary production. If we are to comprehend the significance of citation, then we need to lay bare these processes.

Future studies of citation should therefore concentrate of the content of citations, and the conditions of their creation and application. As far as Law and French are concerned:

> If science is viewed as a social process, then there is as much sociological reason to be interested in it from the point of view of the laboratory technician or the student as there is from the research scientist or journal editor. Another area of interest is the production of scientific papers. These are written in situations that are peopled by such significant others as administrators, professors, anticipated audiences, recalcritant research assistants, typists, colleagues, husbands and wives. These situations refer to laboratories, promotions, salaries, research grants, equipment, computer time, and mortgages. Thus, for example, any study which uses scientific papers as data should take cognisance of the situations in which they are written.' (Law and French, 1974 : 589)

This view is widely held by contemporary sociologists of science (e.g. Knorr-Cetina and Mulkay, 1983;

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Woolgar, 1980), but it has not penetrated the thinking of the information science community. There is, arguably, a need to explore what Star (1983) calls the 'dialectical relationship' between resource constraints and research results. In her study of the 'simplification' process in scientific work she identified several types of constraints: (1) intersection; (2) clinical; (3) technical; (4) conclusion pressures; (5) formatting; (6) editing rules; (7) specialization. Along with these constraints there are social, institutional and ideological pressures bearing on scientists' behaviours and actions. If we are to claim to be able to understand the purpose and significance of citation in the social process of science, then some attention has to be given to these pressures and constraints. As Star says, scientific work involves the representation of chaos in an orderly fashion. If we accept this interpretation, then we are obliged to consider the part played by citations in conferring orderliness and acceptability on published research findings. It is to be hoped that future studies of citation will at least take note of the microsociological viewpoint, and use it to enrich our appreciation of what citation signifies in the knowledge construction and dissemination process.

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