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PART I

ACKNOWLEDGEMENT:
A REVIEW OF THE ISSUES

COLLABORATION

Michel Eyquem de Montaigne, reflective and reclusive in his tower, provides a powerful and enduringly appealing image of the scholarly ideal: dedication of purpose, a fierce commitment to the pursuit of truth and knowledge, and independence of spirit coupled with a dauntingly ascetic lifestyle. But a great deal has changed since the 16th century French moral philosopher and essayist wrote his *magnum opus* and in the process helped immortalize the image of the unworldly and isolated scholar. Of course, the lone wolf scholar is not yet on the endangered species list; he is alive and well, both in popular lore and in academic reality. Long before and after Montaigne, many lone scholars have plied their trade with great distinction and occasionally with great public celebration of their avocational single-mindedness. Indeed, this is the popular image of writers in general, not just scholars, as Brodkey (1987, pp.54-55) has argued tellingly: '... when we picture writing we see a solitary writer ... the image privileges only one event in writing, the moment when the writer is an amanuensis, making that one event a synecdoche of writing.' This 'picture post-card of writing' (p.57), to use her phrase, blinds us to the fact that 'writing is a social act' (p.54), akin to a conversation with 'rules for conversing' (p.4).

The mini publishing industry which has grown up around the late Austrian philosopher, Ludwig Wittgenstein, bears testimony to the public's fascination with the biographical membrane of scholarship. Wittgenstein's tortured life, his intense interactions with Cambridge colleagues, such as Bertrand Russell, his sporadic withdrawal from the academy and search for solitude, in Norway, Ireland, and an Austrian monastery, and his constant agonizing over the meaning of life and the utility of philosophical inquiry have given rise to an almost obsessive public interest in his extra-scholastic activities (most recently in the form

of Derek Jarman's eponymously titled movie) and, in the process, reconfirmed the lone (better still, lonesome) scholar stereotype. However, while the great legacies of both Montaigne (the *Essays*) and Wittgenstein (the *Tractatus* and *Philosophical investigations*) are unabashedly works of independent scholarship, not of formal collaboration (though anyone who has seen Jarman's film will, of course, realize that Wittgenstein's thinking was deeply influenced by the social and intellectual *milieux* in which he moved) and while the single author treatise continues to be the norm in philosophy (Russell and Whitehead's *Principia mathematica* being a rare and notable exception), that is no longer the case in many other fields, least of all in the domain of Big Science where collaboration at times verges on caricature.

With the advent of Big Science's mega projects, such as the building of the atomic bomb at Los Alamos during World War II, NASA's (National Aeronautics and Space Administration) Mercury, Gemini and Apollo programmes which culminated in the successful landing of a manned space flight on the moon, or more recently the stillborn multi-billion dollar superconducting supercollider project in Texas, large-scale inter-disciplinary teamwork and formal collaboration have become inescapable components of the strategic research process. The enormity and complexity of the problems typically associated with Big Science extend well beyond the capabilities of any one individual, or even gifted groups of like-minded disciplinary specialists. Major laboratories, such as CERN (Conseil Européen pour la Recherche Nucléaire) in Geneva or Fermilab outside Chicago are staffed by teams of theoreticians, experimentalists, and engineers recruited from within and across core disciplines who are having to pool their knowledge, perspectives, and skills to master problems of unprecedented complexity. But collaboration in science (and scholarship in general) is by no means peculiar to Big Science, even if the Manhattan Project, in part no doubt because of the forceful and colourful personalities of its principals (e.g., Edward Teller, Richard Feynman, Robert Oppenheimer), remains one of the most frequently invoked examples of successful inter-disciplinary collaboration.

Of course, collaboration can manifest itself in myriad ways, of which the Big Science examples above, and the major clinical trials which sometimes involve dozens, if not scores, of hospitals and medical centres in a single country, or even around the world, are merely among the most obvious and most elaborate. Collaboration between scholars can be overt (e.g., formalized in a joint bid, via a memorandum of agreement or strategic partnership between institutions) or covert (casual and

unaudited in a formal sense); simple (a one-on-one interaction whether at the individual or institutional level) or complex (many-to-many); organic (fundamental, as in, say, high energy physics research) or optional (nice to have). Notwithstanding, it would be erroneous to treat collaboration (in the sense of 'freely chosen associations of current teamwork' (Beaver and Rosen, 1978, p.72)) as being co-extensive with Big Science, or to assume that it is a recent phenomenon, as, indeed, Beaver and Rosen (1978, p.69) have shown in their historical and sociological analysis of the origins of collaboration in European science. Collaboration, they maintain, is a logical and predictable response to the progressive professionalization of science: it is, in their phrase, 'a mechanism for gaining and sustaining access to recognition in the professional community' which 'provides avenues of mobility for those who seek recognition.' Collaboration, in other words, can provide competitive advantage in career terms.

One of the notable findings to emerge from the landmark APA (American Psychological Association, 1969) *Project on scientific information exchange in psychology* carried out in the 1960s was that less than one in seven ideas for research projects originated from (formal) sources such as journal articles or presentations at national meetings. The most fecund sources of inspiration were informal networks of exchange, reflecting the fact there exists a small but disproportionately productive minority of psychologists who are 'highly organized into small clusters of researchers who maintain continual contact with one another's work.' (Garvey and Griffith, 1972, p.128) The idea that workers in scientific specialties are organized into social circles ('distinguished by the greater density of relations between ... members than between members and non-members' (Collins, 1974, p.166)) has been popularized most effectively in Price's (1963) idea of the invisible college, a small, possibly evanescent, and essentially self-identifying group of researchers with a common or overlapping set of intellectual interests at any given moment. The point to be made, though, is that the greater the intensity of informal communication between scholars, the greater the likelihood of collaboration occurring among connected members of specialty groups (e.g., Crane, 1970; Cronin, 1982). Collaboration, then, can provide researchers with an edge both in terms of productivity and career development. There are, of course, many reasons why scholars choose to collaborate, and Beaver and Rosen (1978, p.70) have developed a helpful typology of motives which is reproduced in Table 1. Eaton (1951) identified four principal reasons to account for scientific/professional collaboration: i) teamwork is a means

of dealing with the increasing complexity of the knowledge base; ii) collaboration promotes intellectual stimulation, cross-fertilization, and a multiplicity of perspectives; iii) it facilitates the synthesis of theoretical assumptions from different disciplines; iv) it encourages division of labour where a single researcher might be handicapped. However, this generic list needs to be expanded in the light of developments which have occurred over the last three or so decades.

Table 1: Motives for collaboration: a summary

access:	to special equipment or facilities
	to special skills
	to unique materials (e.g. chemical compounds)
	to visibility
	recognition
efficiency in:	use of time
	use of labour
to gain experience	
to train researchers	
to sponsor a protege	
to increase productivity	
to multiply proficiencies (thereby increasing access to source of support, visibility, recognition)	
to surmount intellectual isolation	
need for additional confirmation of evaluation of a problem	
need for stimulation or cross-fertilization	
spatial propinquity	
accident (serendipity)	

First, there is increased availability of funding to support research and development activities. Building on Patel's (1973) study, which found a relationship between funding and levels of assistantship, and on an unpublished study by Hirsch and Singleton (1965), Heffner (1981) looked at the relationship between funded research, multiple authorship and sub-authorship collaboration in four disciplines — political science, psychology, biology, and chemistry — and found that funding had a greater impact on technical than on theoretical collaboration at the sub-authorship level. Second, in some 'hot' research arenas (e.g., cognitive science; development economics; AIDS research; biotechnology; environmental protection), progress is now really only possible by assembling multi-disciplinary teams and by creating *agora* (physical and virtual) in which scholars from historically separate academic tribes can congregate, share experiences and trade ideas. Third, success in

attracting external research funds can depend enormously on the ability to establish multi-disciplinary/multi-professional and multi-institutional research teams: collaborative intent may thus be a precondition of credibility with certain funding agencies, or even a *sine qua non* of grant awards (e.g., the European Commission's pre-competitive, collaborative research programme in information technology, ESPRIT).

Fourth, collaboration may be the only way in which groups of researchers/scholars can ensure effective access to the necessary physical, conceptual, or symbolic resources to support their research endeavours (e.g., tools, expertise, data sets, laboratory equipment, primary literature, distributed computing infrastructure, political capital). Fifth, global developments in information and communication technologies (e.g., computer conferencing; wide area networking; groupware; collaboratories) have dramatically increased the potential for intra-mural, inter-disciplinary, trans-institutional, and cross-national collaboration in virtually every academic specialty. As a result of advances in telecommunications in particular, strategic partnering and time-based competition, as predicted by Davenport and Cronin (1990), have become two of the key bases of comparative advantage in the increasingly competitive world of academic grantsmanship.

Sixth, there is a growing body of evidence which suggests that collaboration, when operationalized as co-authorship, results in both increased productivity (e.g., Harsanyi, 1993) and higher citation scores (Rousseau, 1992). A less flattering, though widely held, explanation lays the blame with 'gratuitous authorship for supplies of drugs, clones, viruses, and so on' (Croll, 1984, p.402), a practice which is a 'clear violation of the principle that authorship is a measure of credit and responsibility for original work reported in an article.' The idea that authorship can be obtained as a result of isolating a cell line, virus, or clone has also been criticized implicitly by Gallo (quoted in Broad, 1981, p.1137).

The trend to collaboration has spawned a burgeoning research literature on the phenomenon. Recent studies have demonstrated changing patterns of international collaboration between universities and research laboratories. Miquel (1991, p.141; p.149), in an analysis of scientific interaction between the United States and Greece and the United States and Denmark, captures the naturalness and underlying incrementalism of collaboration: 'Every scientist would like to work with another scientist who could contribute to his or her work. Scientists know each other's work through various networks: articles, seminars, conferences, among others. Thus, when they meet, for example at an international conference, they may not only exchange information, but

may also plan to initiate a collaborative project. Cooperation may start by the exchange of doctoral and post-doctoral fellows between laboratories. And through facsimile machine and electronic mail, they may exchange data on everyday experiments. Later they may co-author their results in an article. This kind of story repeats itself at an increasing rate ... The accumulation of the output of such research — a quantitative data set of coauthored articles — reflects the activities of science in the international community.'

Interest in mapping and measuring international scientific collaboration continues to grow, whether the motive be pragmatism (e.g., Vinkler, 1993) or intellectual curiosity (e.g., Lancaster, *et al.*, 1992). Despite some of the methodological difficulties in using bibliometric indicators to articulate cross-national collaboration (e.g., the coverage limitations of ISI's (Institute for Scientific Information) citation indexes; the problems of determining an author's true national affiliation/allegiance) data on co-authorship patterns are unquestionably the most accessible, quantifiable, and popular measures of formal collaboration, and can be used to inform science policy debate and to help funding bodies develop a sense of the relative effectiveness of collaborative research programmes. For example, Lewison and Cunningham (1989) applied bibliometric data on international co-authorship to evaluate research programmes funded by the Commission of the European Communities (CEC). More recently, Moed and de Bruin (1990) conducted a bibliometric analysis of the agricultural research literature on behalf of the CEC to determine: (i) whether there had been an increase in the number of articles co-authored by scientists from two or more EC member states; (ii) if so, how this compared with the proportion of papers co-authored by an EC scientist and one or more scientist from a non-EC country, and (iii) whether papers by EC scientists cited work by scientists from other member states proportionately more frequently than non-EC authors.

In Australia, the government has recognized formally the economic and other benefits to be derived from international cooperation in scientific research. The Department of Industry, Technology and Commerce (1987, p.198), in its report on science and technology indicators, *Measures of science and innovation*, mentions explicitly the value of sharing expertise and resources, reduced research costs, and increased awareness of foreign research. The report provides a detailed break-down of Australian scientists' international co-authorship by both country and field.

Co-authorship is arguably the most institutionalized and visible

expression of functional interdependence between scholars, but it is by no means a recent phenomenon in the primary communication process: indeed, it has a rather respectable pedigree, as Beaver and Rosen's (1978) unearthing of a co-authored chemistry article dating from 1665 demonstrates. Since the 17th century, however, the incidence of multiple authorship has increased dramatically across many disciplines (between 1960 and 1980 the average number of authors per paper rose from 1.67 to 2.58 (Broad, 1981)), such that the late Derek de Solla Price (1963, p.89) was inclined to predict that 'by 1980 the single author paper will be extinct.' While that has not happened, the point has been reached where editors of reputable journals in, for instance, the field of high energy physics, can expect to receive papers with 200 or more named co-authors (McDonald, 1995).

The overall direction of the trend line is clear, even if the gradient is still to some degree field dependent. Garfield (1979, p.242) cites an unpublished (and, admittedly, somewhat dated) study by Lindsey and Brown (1977) which showed that multi-author papers accounted for only 17-25% of samples of papers in the fields of economics, social work, and sociology, but 47-81% of samples of published papers in gerontology, psychiatry, psychology, and biochemistry. Broad (1981) cites figures of 67% for *The Astrophysical Journal* and 95% for *Cancer Research*, and an average of five authors per article in the *New England Journal of Medicine*. In some quarters this trend is being viewed with mounting disquiet: according to Tilley (1990), the National Institutes of Health (NIH), in an effort to contain the growing practice of listing sometimes dozens of researchers as authors of a paper, has suggested 'a tiered system that would credit only the primary researchers as authors, with other authors *acknowledged* (italics added) as contributors.' If such a proposal were to be adopted by the NIH, and subsequently endorsed by other national and international agencies, acknowledgements could take on a fresh significance in the context of the scholarly publication process.

Collaboration between scientists and scholars can assume a variety of forms. Mullins (1973, pp.18-19), in his network stage model of theory group communication, has identified four principal kinds of interpersonal relationships: (i) communication in the sense of serious discussion about ongoing research; (ii) co-authorship; (iii) the apprenticing, or mentoring, of a student by his or her teacher, and (iv) colleagueship (at the most basic level, two scientists working in the same laboratory). Each of these relationships can inform and influence the research process, but in only one (i.e., the second) will formal testimony of collaboration necessarily be provided: in each of the other three instances, the fact and nature of

the collaboration *may* either be acknowledged explicitly, expressed tacitly, or remain unstated in any research report arising.

The centrality (and potential utility) of trusted assessorship to an understanding of both the formal and informal dimensions of scholarly communication has been underscored forcibly by Chubin (1975, p.367), who argued that 'networks of trust constitute the informal social organization of science' and that the 'potential use of trusted assessorship ... in modelling the relations that scientists initiate, sustain or terminate (as research and organizational constraints dictate) seems unbounded.' He goes on to suggest that trusted assessorship should be explored in greater depth, and makes a convincing case for turning the spotlight on acknowledgements, which function as a 'worthwhile indicator' of the phenomenon, and as a 'first approximation of the relation' (p.366). This idea was picked up by another sociologist, Patel (1973, p.81), who employed 'sub-authorship collaboration' in conjunction with co-authorship to create a compound assistantship index spanning six decades of professional research in North American sociology. Most recently, Edge (1994, p.371), in a witty address on the occasion of his being presented with the 1993 Bernal Prize by the Society for the Social Studies of Science, observed with justifiable irony that scholarly societies tend 'to honour those who have published a lot, when the society's' members believe that reading those publications has 'influenced' them. It's a hierarchical, 'one-way flow' and *non*-'tacit' model — precisely the model that we keep having to remind ourselves we have abandoned as a way of thinking about technical and scientific innovation. And yet, every time we give a prize of this kind we perpetuate it.'

Patel's index was based upon an analysis of four widely circulated sociological journals, *American Sociological Review* (*ASR*), *American Journal of Sociology* (*AJS*), *Social Forces* (*SF*) and *Rural Sociology* (*RS*). Each journal was examined from its date of initial publication to the last issue of 1965. The unit of analysis was the article. Data on the number of authors per paper and on the number of persons whose assistance was otherwise acknowledged were plotted for 7,908 articles. The increase in multi-purpose assistance, as revealed through the analysis of footnotes and acknowledgement statements, was even more dramatic than the increase in multi-authorship. In the case of *ASR*, the number of persons required to produce a sociological article from its inception to final product increased almost threefold over the period under review. Not unreasonably, Patel (p.86) concludes that collaboration has been stimulated by 'expanding research facilities, availability of

student assistants, technical personnel, electronic equipment, funding agencies and other such institutional patronage' and that it makes sense to regard sub-authorship collaboration, especially by single authors, as a 'compensatory measure to stave off their individualistic handicap in the growing complexities of the research enterprise.' It also seems plausible that the increasing specialization and machine-dependence of contemporary science (and of scholarship in general) trigger a need for collaborators who can provide linkages and connection with the larger corpus of literature and related research results, not to mention sometimes highly sophisticated tools and techniques.

Of course there are exceptions to this tendency, as Pandit (1992, p.107) noted in her qualitative survey of the information seeking behaviours of humanities scholars: 'The conduct of research in humanities is not through the use of machines of common inquiry ... The machines, the cyclotrons and the betatrons and all these high speed particles machines in physics, all this is as if they were all children playing in the same kind of playground ... in philosophy all you have to do is have a pad, a pencil and a wastebasket ...'. This view is endorsed by Tibbo (1994, p.608) who notes that the 'typical humanist' works by him or herself, and that as a group they 'differ from scientists in their information gathering techniques and the work they produce.' Which brings us full circle to Montaigne.

On a more general level, Patel (1973, p.92) argues that the lone scholar, or 'lone wolf', to use his preferred term, is most likely to be in evidence in the formative or ground-breaking stages of a discipline's evolution when boundaries are being negotiated and proto-conceptual frameworks are vying for dominance. Later, a 'pack' of scholars working in collaborative mode will emerge in the wake of the wolf to undertake 'large research projects under conditions of a cumulative body of knowledge, access to specialized skills and technical facilities, methodological sophistication, and institutional support.' This is an intuitively attractive hypothesis as far as some disciplines are concerned, but one in need of empirical testing prior to generalizing. Indeed, its evidentiary base has been challenged by Beaver and Rosen (1978, p.71), who cite a lack of historical perspective in the Anglo-American tradition of research as the reason for the attachment to the independent actor theory.

Given the considerable investigative effort which has been devoted to understanding the dynamics and social significance of informal information flows within the scholarly communication system over the last few decades, and given recent interest in developing more robust

indicators of scholarly collaboration at different levels of aggregation (e.g., work group, institution, specialty, nation state, sector), it is surprising how little resolution has been brought to bear on existing, formal records of sub-authorship collaboration. Acknowledgements, though they may register essentially personal or behind-the-scenes interactions, gift giving, or trades between scholars, do nonetheless exist in the public domain, whether as meta-textual elements appended as codas to peer-reviewed articles, or as forewords to monographs and doctoral dissertations, and could provide a potentially revealing window onto trends in collaboration, particularly if used conjointly with other measures of scholarly interdependence and interaction.

REWARDS AND RECOGNITION

Even body builders seem to fret less about their vital statistics than the professorate. Scholars are forever sizing up one another; evaluating *résumés,* reviewing dossiers, ranking courses and institutions, gauging productivity, assembling measures of public esteem, rating scholarship, recording teaching evaluations, tallying grant awards, and counting citations to their work. Vita building, or what Wernick (1991, p.168) has termed 'the promotionally mediated contest for career credits', has become the academic equivalent of the workout. Virtually all measurable by-products of the academic enterprise have become fair game in the statistical ranking process and this is nowhere better illustrated than in the at times obsessively detailed mass of materials assembled for promotion and tenure committees — the 'productivist scramble'(p.169) *par excellence.*

Scholarly publishing is thus as much about rewards as texts. The texts crafted by scientists and scholars are not merely vehicles for the dissemination of new knowledge, but an established means of allocating credits and auditing accomplishments (dutifully bound up in the academy's principal ledger and roll of honour, the scientific journal), such as in the case of academic promotion and tenure (P&T) decisions, where various facets of the typical peer-reviewed article can be factored into the evaluation process. Authorship is, of course, the most obvious cut: how many articles has Professor Smith authored; how many were co-authored? Another source of valuable data are the lists of bibliographic references which accompany virtually every published, refereed paper. How often has Professor Smith's work been cited by his peers, and where (e.g., in the top-ranking journal set or in second tier journals; by eminent scholars or by mediocrities)? The former (authorship) is an approximate measure of Smith's productivity; the latter

(citation) an approximate measure of Smith's productive impact. Even seemingly sedate fields such as English have been smitten with the citation bug, with the American Modern Language Association (MLA) building databases which are especially useful for young high fliers because 'they document the extent to which the cited academic author has actually been incorporated into the professionally sanctioned intertext' (Wernick, 1991, p.170).

But authorship and citation do not tell the whole story, and the emphasis on quantitative marks of quality means 'banishing to the shadows such difficult questions as how to evaluate orally transmitted influence, the interdependence of one person's thoughts and achievements with another's' (Wernick, p.169). More generally, as Kennedy (1983, p.48) notes, 'matters of authorship, attribution, and acknowledgement have become more complex; responsibility for work in which we are less personally involved has become more common; and the customs prevailing in different fields have diverged.' Staying within the textual realm, there is another vector which could be utilized in the P&T evaluation process, namely, the acknowledgement statement. Acknowledgements record the multifarious contributions of immediate colleagues and sundry others to the reported research. They reflect a rich mix of personal, moral, instrumental, financial, technical, and conceptual support received from institutions, research councils, government agencies, co-workers, peers (including competitors), mentors, family members, and even (sometimes anonymous or pseudonymous) experimental/survey subjects. These expressions of gratitude can range from ritualized genuflection in the direction of a funding body, through detailed expressions of thanks to long-suffering co-workers, to a sincerely felt acknowledgement of intellectual stimulation provided by a professional mentor. A close cousin of the acknowledgement is, of course, the book dedication, which reflects 'an author's 'citation' of both personal and professional impact and development' (Gifford, 1988, p.225), though, typically, it is a much terser and more personalized expression of gratitude than an acknowledgement.

The practice of acknowledgement is well-established in works of literature and scholarship. Epstein (1983, p.43) feels that footnoting 'seems to resemble nothing so much as an assembly line', though he concedes that 'giving acknowledgements seems to be a highly pleasurable activity. Once begun, it is not easily brought to a close, for it is something akin to handing out gratuities with play money.' A common view seems to be that in matters of style and format there is great diversity. Hamilton (1990, p.25) has the sense that acknowledgements

'are treated as if they were naughty activities best left out of polite conversation ... a curious achievement of pretension, hyperbole and banality.' While that may hold true for certain genres of non-fiction writing, and, on occasion, doctoral theses, where baroquely personal and stylistic flourishes sometimes invite parody, a universal stereotype should not be assumed. But, this, too, is an area deserving of closer investigation.

In an essentially discursive analysis of acknowledgement styles in approximately 200 ethnographies (and also partly on the basis of conversations with primarily British and North American anthropologists) Ben-Ari (1987, p.75) has illustrated, using many engaging examples, the fondness among anthropologists for richly textured public documentation of their gratitude to mentors, family and friends (from the late sixties onwards, for instance, he detects a greater diversity of forms in acknowledging the contributions of partners and, in fact, in the nature of the spousal relationship) and also to those who were studied in the field and became the lifeblood of the resulting ethnographies. He goes on to explore possible reasons for the juxtaposing of the stylized professional prose of the text proper and the often intimate and highly subjective wording of the acknowledgement section which typically follows (but sometimes precedes) the main body of the text, concluding that acknowledgements are used to establish ethnographic authority and authenticity: 'the absence of professional parlance, along with the personal appeals and individual statements, tales of rapport, confessions of fallibility, and use of fictive kin terms, all serve to create the impression of a human, concrete, intimate — and *therefore* believable and genuine — experience.' Of course, as he concedes, acknowledgements can sometimes be used for more venal reasons such as 'name dropping' (p.72), 'manipulative strategizing' (p.72) or 'careering' (p.70). Overall, Ben-Ari (p.79) characterizes acknowledgements as 'special textual constructs' whose 'formulation is governed by conventions which are different from those of the main text.' Note, however, the assertion that this is a regulated, and not an entirely individualistic or whimsical activity.

Naturally, what holds for certain groups of anthropologists may not hold for all anthropologists, even less so for scholars in other disciplines with different cultures, research methods, modes of argumentation, and writing styles. McCain's (1991, p.496) study of acknowledgement behaviour among geneticists suggests a more straightforwardly transactional model, one which assumes that 'information-recognition exchange functions in the dissemination of research-related information

as well as in the communication of research results.' That is to say, the same rules apply in the shadow or informal economy as apply in the formal information economy where citations are the coin of the realm. This virtual marketplace, which brings together colleagues, peers, and competitors, has become a prerequisite of leading-edge scientific research. For instance, in the 1990 volume of the journal *Cell*, each of the 421 research articles contained an acknowledgement statement, most of which were compound in character (see Appendix 2 for an example). For more dramatic evidence of the practice's significance, one need only reflect on the bitter dispute relating to Robert Gallo's use of the HIV virus developed by Luc Montagnier and his team at the Pasteur Institute in Paris to realize just how central and institutionalized these informal trading networks have become, and, also, how economically and politically important the allocation of full and just credit is in an age of patentable discoveries and significant commercial revenue generation opportunities (Rawling, 1994).

McCain's approach involved scanning both the methods and materials and acknowledgements sections of 241 experimental papers in the 1988 volume of the journal *Genetics*. She found the acknowledgement of gifts (e.g., a stock, strain, or phage; plasmids, bench techniques, innovative instruments) to be commonplace, reflecting the field's long history of open communication and the expectation of reciprocity among its members. She left open the possibility, however, that the format of acknowledgement might well vary from field to field and from journal to journal. Acknowledgement practices are not simply a function of a field's social structures and research ethos, but also of the historical period. Bazerman (1984, pp.182-183) notes in his study of experimental reports in physics that the acknowledgements of the 1890s were personal testimonials to friends and mentors, but that those which re-emerged in the 1920s were 'more spare, sharing limited forms of credit and recognizing institutional dependencies' such that 'the acknowledgement of intellectual fellowship lost personal effusiveness.' Acknowledgements come in all shapes and sizes, ranging from the cryptic or whimsical, through the clinically detailed, to the garrulously personal. (A youthful Umberto Eco, while doing his military service, wrote a book on art and beauty in the Middle Ages, the acknowledgements in which were to his general and corporal (mentioned in *The European*, 1991)). That which is acknowledged can be as cosmically significant as Allah, as rudimentary as library facilities, as quotidian as secretarial support, or as subtle and formative as intellectual influence. Whatever the local variations in literary style and structure, the practice of acknowledging co-workers,

trusted assessors, mentors, graduate assistants, and various others has become a fact of life in the world of scholarly publishing, one deserving of more serious and systematic scrutiny than has heretofore been the case.

TEXTS AND TOKENS

Cultural analysis is 'essentially a study of texts of all kinds including the printed word, graphic images ... and even the messages 'given off' in social settings by such behaviours as bodily movements and positions.' (Wuthnow, 1992, p.1) Words and actions become the very stuff, or constitutive features, of cultural analysis. By analyzing texts we are better able to understand social relationships and power structures within communities. The questions which cultural analysis invites are typically of the kind: What regularities can be observed in particular modes of social discourse? How can the 'vocabularies of public life' (Wuthnow, 1992) be deconstructed to expose underlying rules and conventions? What can be gleaned from an analysis of the internal structure of texts about external behaviours and social structures? Which symbolic codes operate in which domains and to what ends?

According to Wuthnow (p.10), public discourse is an 'intentional construction' and must therefore 'follow certain rules, employ certain devices, and utilize certain strategies for it to accomplish its purpose.' A significant and concerted body of research in the area of textual and discourse analysis (including micro analysis at the level of the citation) was carried out in the 1970s and 1980s by sociologists of science and others. Their findings challenged rudely the storybook version of science, and exposed the often contentious ways in which novelty was created, facticity fabricated, and consensus engineered. The messiness, ambiguity, and occasional venality of scientific life that relativistic inquiry laid bare contrasted sharply with the highly stylized, sanitized and resolutely impersonal by-products popularly associated with the scientific enterprise (e.g., Latour and Woolgar, 1979; Gilbert and Mulkay, 1984). Orthodox assumptions were challenged further as sociologists, literary critics, and sundry constructivists dissected surgically the superstructure, internal features, syntax, and rhetorical characteristics of the conventional scholarly paper, which, to use Wuthnow's (1992, p.9) language could conceivably be viewed as another instance of a 'seemingly simple act of public discourse, such as a sermon, a direct-mail solicitation, or a popular song' containing a 'vast inner structure of form and content.'

As far as Bazerman (1984, p.164) is concerned, the overall conclusion

to be drawn from the many sociological and linguistic studies of scientific texts is that 'scientific writers indeed write their texts to serve their own scientific interests.' That being so, it is reasonable to assume, as Ben-Ari (1987) has implied, that acknowledgements, one element in a specialized discourse repertoire, play a part in serving those interests, in mobilizing the author's goals and in constructing meaning. Ben-Ari (p.67) also recognized that acknowledgements were 'special textual constructs that relate both inwards toward the main parts of ethnographic texts and outwards toward the social contexts within which these ethnographies are produced.' In cultural anthropology, textual phenomena such as acknowledgements convey 'meta-messages' which are 'part of the processes of 'management of meaning' and thus warrant examination as constitutive of social action.' (p.66) In a sense, Ben-Ari confirms Bourdieu's (1991, pp.66-67) view that 'it is rare in everyday life for language to function as a pure instrument of communication' since the 'pursuit of maximum informative efficiency is only exceptionally the exclusive goal of linguistic production and the distinctly instrumental use of language which it implies generally clashes with the often unconscious pursuit of symbolic profit.' Bourdieu's notion of 'symbolic profit', at least in the context of acknowledgement behaviour, can be translated into 'strategic choice involved in careering' (Ben-Ari, 1987, p.68). The importance of rhetorical forms, prescriptivism and incremental encyclopedism in scholarly writing have been described in detail by Bazerman (1988) in his book, *Shaping written knowledge.*

Despite the fact that many scholarly articles routinely carry personal acknowledgement statements of one kind or another, there has been scant formal investigation of their social and cognitive significance in the context of the scholarly publication system. As I have noted, acknowledgements can serve a variety of purposes, functional and/or symbolic. They can be construed as measures of trusted assessorship (Mullins, 1973), indicators of hidden influence, gifts (McCain, 1991), tokens of esteem, credits or rewards, fixing or ordering devices (like references, footnotes or synopses (Ben-Ari, 1987)), ritualistic appendages. Bazerman (1984), for instance, stressing human/social relationships, talks principally in terms of 'institutional dependencies' (p.183), 'intellectual fellowship' (p.183) and 'personal apprenticeship loyalties' (p.189). Brodkey (1987, p.23), on the other hand, would maintain that acknowledgements, like citations, are part of 'the cultural repertoire of all academics' (p.23); that they are 'formal properties of scholarly texts' — one component of the 'rules of discourse' (p.18) or

'cultural knowledge' (p.7) which make writing a social or collective as much as an individual act. McConnell (1993), in fact, uses a case study in acknowledgement failure (an example of moral shortcomings in the conduct of scholarship) as the *entrée* to a wide-ranging treatise on the nature of gratitude.

Functionally, acknowledgements convey public gratitude for often private gestures of assistance which contributed in some meaningful way to the research or scholarship being reported. A simple or composite acknowledgement statement at the end of a multi-author paper is no less an expression of functional interdependence between scholars than is the list of co-authors at the top of the article's title page. Who is to say whether the least significant co-author's contributions were greater or less than those of the most helpful acknowledgee? In addition, it is doubtful whether there exists universal consensus among primary authors or principal investigators as to the criteria for awarding assistantship or co-authorship status to collaborators (e.g., Patel, 1973, p.86). For Heffner (1979), co-authorship implies active partnership, whereas being acknowledged suggests assistantship and occupation of a subordinate position in academic science. Stratification is assumed tacitly within an article's architecture, despite the fact that *locus* (co-author at top; acknowledgee at bottom/end) may well be locally negotiable and determined by local conditions and *mores* rather than an established etiquette.

Symbolically, however, acknowledgements may serve a broader purpose. In fields which tend to exhibit a high degree of intellectual or ideological sectarianism (e.g., political philosophy, literary criticism, linguistics) they can be used to send out meta-messages, such as: 'I am a member of this tribe', 'I subscribe to this dogma', or 'I belong to this movement'. In a more general fashion, acknowledgements can be employed just like the star-studded bibliography or reference list attached to a published article to locate the author(s) in a particular cognitive or social *milieu* (celebrity endorsement, or coat-tailing, so to speak). In this case, the meta-message is: 'Look at the company I'm keeping.' As with other kinds of social exchange, acknowledgements may be used 'to cement peer relations and sometimes to produce differentiation of status, contradictory as these two consequences appear to be.' (Blau, 1967, p.89) Group identity may also be a reason: Brodkey (1987, p.47) speculates that many feminists value collaboration 'out of all proportion' judging by the intensity and detailedness of the acknowledgements they grant to collaborators, irrespective of their academic status or affiliations.

The issue of acknowledgement can be viewed from yet another angle:

'Where formal acknowledgements are made, they can be treated as if they were citations ('super-citations', perhaps) and subjected to similar quantitative analysis.' (Edge, 1979, p.106) Since it is *a priori* possible that the contributions of some acknowledged colleagues could be of greater instrumental value than the contributions of some cited authors, Edge's suggested reconceptualization of acknowledgements as 'super-citations' is logically appealing, since even the most trivial, perfunctory, self-serving, not to mention negative, citation counts for more in the P&T accounting game than the most glowing and substantive acknowledgement. To count one but not the other is anomalous, as Mackintosh (1972, p.70) pointed out more than twenty years ago: ' ... lack of interest in acknowledgements does not necessarily indicate their complete irrelevance as rewards in science, or, if it does, then citations of one's published work by others must fall at the same stroke.'

CITATION AND ACKNOWLEDGEMENT

Until the development of commercial citation indexing tools in the 1960s, citations were an essentially taken-for-granted (meta-textual) feature of the typical scientific paper. Their broader import was largely unrecognized. It was really only with the development of sophisticated co-citation mapping and clustering techniques that the potential of citations to illuminate the internal dynamic and structure of the scholarly communication system began to be appreciated. One consequence of three or so decades of citation research and development has been the highlighting of, to use again the language of Wuthnow (1992, p.9), the 'complex codes, distinctions, rules and interplay between form and content that permit a public utterance to articulate with its social environment' — the social significance of citation, in other words.

Citation analysis deals with data which describe formal patterns of scholarly interaction and consumption: publications are referenced, frequency of citation recorded, and a variety of citation impact measures derived. Citation counting provides analysts and evaluators with a convenient, if not quite perfect or comprehensive, means of calibrating influence and eminence. Citation is, of course, a social process (Cronin, 1981; Cronin, 1984): a largely institutionalized mechanism for dispensing credits and recognition to peers in the public domain. The citing of one author by another is assumed to be a significant event, and the more significant events recorded in favour of a given scientist/scholar, the greater that individual's presumed influence or prestige in his or her peer community. However, citation counts are an incomplete register of social and intellectual influence, a point which has

not gone entirely unnoticed: 'Science has evolved no mechanisms for acknowledging influence in papers except for the occasional 'pers. comm.' and 'acknowledgements' which are usually not included in the data from which citation analysts work.' (MacRoberts and MacRoberts, 1989, p.344) For such reasons, it would be instructive to know to what extent the exclusion of acknowledgements from the academic audit process discriminates against those whose influence is either primarily, or supplementarily, registered in ways other than publication and the (hoped-for) subsequent citation. Or, to put it another way; is there within the academy a population of hidden influencers whose contributions to scholarship are under-recognized and accordingly under-valued?

Both citations and acknowledgements declare a relationship: both testify to some kind of cognitive, social, or procedural interaction (involving another individual, an agency, or facility) which may, of course, be profound or superficial. One, the citation, has objective status in that a third party can refer to the cited document and corroborate the citing author's interpretation, pursue an intellectual lead, or chain backwards or forward through the related literature; the other, the personal acknowledgement, describes an inherently private interaction, or debt, which, by definition, cannot have the same commodity status. It is this distinction which perhaps best explains why citations, but not acknowledgements, are traditionally factored into faculty assessment exercises and evaluations of research programmes.

Most acknowledgements hint at, articulate, or describe in detail a personal (unwritten and perhaps unspoken) contract between two or more individuals. The nature of the contract is, however, vague; an example of what Blau (1967, p.93) would term a social exchange which entails unspecified and diffuse future obligations. Citations, on the other hand, are highly formalized modes of acknowledgement ('collective, standardized symbols as opposed to private nonce symbols' (Sandstrom, 1994, p.422)), which can be traded in consistent fashion in open markets. There are widely adhered to citation norms and stylistic conventions which regulate scholars' citation practices, and the existence of those norms creates the conditions for today's competitive academic marketplace where talk is of gypsy scholars, star hires and head-hunting. In this marketplace, citations have acquired convertible currency status: to be highly cited raises one's stock. While codification is reasonably clear-cut as far as citation behaviour is concerned, the norms of acknowledgement at first sight appear much less clearly articulated, though the following extract from Wolcott's (1990, pp.72-73) *Writing up*

qualitative research captures nicely the author's embarrassment on failing to acknowledge adequately his mentor, George Spindler. One suspects that many scholars would be able to empathize with either Spindler or Wolcott in this vignette — if not both.

> I received an early lesson about acknowledging others from my mentor George Spindler. The Spindlers were houseguests visiting me after I took a full-time academic appointment upon completion of doctoral studies. I eagerly shared an early draft of a chapter I had been invited to write, tentatively titled 'Concomitant Learning.' Spindler was up early the next morning, but to my disappointment I found him looking through materials he had written (my library contained many of them) rather than reading my new draft. He had already read and enjoyed my article, he explained, but he expressed disappointment at my failure to credit him as a source or inspiration for the concept that provided my title and rationale. He had been searching for the citation I should have made. 'But you've never written about it,' I explained, reaffirming what I already knew and he was beginning to suspect. 'I got the idea from you, but you only suggested it in a seminar. There was no publication to cite.'
>
> Technically (and luckily) I was correct, as his search revealed. That wasn't the entire lesson, however. 'No matter where or how you encounter them,' he counseled, 'always give credit for the sources of your ideas. It's so easy to do; so appropriate to good scholarship ... and so appreciated.' Never again have I limited my acknowledgements only to people whose ideas are in print. And I, too, have 'so appreciated' that courtesy when extended to me!'

In certain cases there is a high degree of isomorphism between citation and acknowledgement behaviours. I can consult a statistics primer on the use of factor analysis techniques and duly *cite* the section which supports my particular methodological approach (thereby triggering token exchange), or I might consult directly with a colleague (who could conceivably be the textbook's author) and receive advice identical to that culled from the textbook. In the latter case I would presumably *acknowledge* my colleague's personally transmitted advice, but he would not receive a convertible token of appreciation in the shape of a formal bibliographic citation. The fact that one exchange is embodied in a publicly available (and tangible) commodity (the cited work) which can be consulted and used independently by a third party, and the other is often defined by a unique (non-replicable) personal interaction, does not necessarily mean that the latter is in any way a less sociometrically valid or ultimately less revealing measure of scholarly communication and influence. Nevertheless, acknowledgements at present have no cash-in or commutable value.

In the case of acknowledgements to tangibles (e.g., grant support, equipment, specimens) it is clearly possible to verify, measure, and, in principle, apply accounting procedures in a way that it is not for intangible contributions. Practicably, one of the reasons why acknowledgements are not incorporated into formal assessment/

evaluation programmes may be the lack of an acknowledgement database comparable with, say, the ISI's citation indexes which would be capable of furnishing machine readable, easy-to-use datasets. On a deeper level, it may have to do with the fact that citations have an artifactual character (they exist 'out there', independent of the citing author) making the written conversation they refer to accessible to the inquiring reader, and allowing the reader 'to recapitulate the writer's reading as well as read and evaluate the text.' (Brodkey, 1987, p.16) Acknowledgements to intangible contributions (e.g., advice, guidance), however, do not offer the opportunity for 'recapitulation or for entry by the reader into that part of the academic discourse signified by the acknowledgement' (Brodkey, p.29) nor can they be re-used in the way that a cited reference can. In that sense, they have the attributes of a private rather than a public good.

Still, this does not necessarily invalidate their sociometric significance, nor does it mean that they could not be fed into evaluation exercises as a complementary indicator of merit or impact. Deployed alongside already operationalized measures (the partial converging indicators approach to research evaluation urged by Martin and Irvine (1983)), acknowledgements could enrich potentially our understanding of how 'extrinsic benefits' such as advice or assistance (Blau, 1967, p.98) are traded and how influence is exercised within and between communities of scholars.

Arguably, the historically neglected acknowledgement adds an extra dimension to publication and citation-based impact assessment, and thus warrants greater recognition as a meaningful unit of measure in both the social studies of science literature and in the classic promotion and tenure process. Both citation and acknowledgement provide a degree of contextualization and coloratura to studies of scholarly communication: both reveal webs of interaction and connectedness; both record intellectual debts; both can be viewed as, and reduced to, units of symbolic capital. Citations have been interpreted variously in the literature as rewards, tokens, or gifts (as, indeed, have acknowledgements) which can be exchanged in the marketplace of ideas according to a tacitly held set of norms. Others, however, have referred to them disparagingly as 'coloured glass beads in a game that values glass beads.' (Anderson, 1992, p.104) Citations have also been viewed as a kind of tax which is levied to permit the use of another's idea/method/theorem — a royalty payment, so to speak, for use of proprietary/original intellectual property (Becher, 1989; Cronin, 1991). However, Kaplan's (1965, p.179) early comments about citation, namely,

that little is known about the 'operating norms with respect to the more sociological functions' still very much apply in the case of acknowledgement behaviour.

In many instances, the contributions of a trusted assessor, anonymous referee, or doctoral advisor are not logged via citation, but through an acknowledgement statement. Yet, in some respects, the acknowledgement which accompanies the published work may be at least the equivalent of a citation in terms of what is signified, even though it does not have the latter's hard currency status: tenure, after all, was never granted on the back of acknowledgements alone. Given the importance sometimes attached to publication and citation counting in the context of salary determination (e.g., Cronin and Overfelt, 1994; Toutkoushian, 1994) and also promotion and tenure decisions (e.g., Cronin and Overfelt, 1995), it is somehow paradoxical that acknowledgements are excluded entirely from scholarly accounting. At present, being mentioned in the acknowledgements section of a scholarly article is, in effect, to be consigned to limbo. The precise status of an acknowledgement is murky. In the reward system of the academy, a mere acknowledgement, no matter how influential the acknowledged intellectual contribution is perceived to be, is not treated as equivalent to even the lowest form of citation. A citation of whatever kind is presumed to be more significant (in terms of measurable intellectual debt) than even the most fulsomely worded acknowledgement received from a mentor, colleague, or peer.

And yet, a 'hall of fame' built on citations alone is a questionable edifice, since scholarly influence is exercised and exhibited in a mix of ways, some subtler and less public than citation: nurturing, mentoring, tutoring, apprenticing, counselling, for example. Kochen's (1987, p.56) question,' How big a debt must be incurred to warrant citation?' should in all fairness be asked in respect of acknowledgements, since it is abundantly clear that citation counts on their own lack the requisite variety to capture the diverse bases of scholarly influence.

There are other reasons, both particular and general, why acknowledgements have been accorded relatively little attention in the literature on scholarly communication. Specifically, it has been suggested that physical scientists may exchange information through informal networks 'without doing it in such a structured way to make acknowledgement appropriate' (Suls and Fletcher, 1983, p.578), thereby diminishing the broad significance of the practice. Generally, there is evidence of a reflexive tendency to relegate the acknowledgement to the category of essentially non-substantive phatic communication, 'a type of

speech in which ties of union are created by mere exchange of words.' (Malinowski, 1946, p.162) If this is the case, then acknowledgements would indeed be more ceremonial than substantive (MacRoberts and MacRoberts, 1986, p.162), but, to take a specific example based on a close reading of the texts of some experimental reports, an acknowledgement may actually reflect a specific contribution by a colleague to the author's critical conceptualization of a problem (Bazerman, 1984, p.188). It calls for a kind of double-take to realize that the 'use of politeness forms in acknowledgement' (Ben-Ari, 1987, p.70) goes beyond closed or private exchange to public signalling of the importance of sub-authorship collaboration and of the scale of structural interdependence that undergirds contemporary scholarship.

Although the case for applying similar analytic methods to acknowledgements as to citations was argued compellingly by Mackintosh (1972), the degree of correspondence between the two behaviours has been little recognized in the information science or other literatures. This is ironic in that the information science field is centrally involved with issues relating to scholarly communication and has for many years been a pioneering force in theoretical and applied bibliometric research. Both citation and acknowledgement are well-established facets of the typical scholar's rhetorical repertoire, first cousins in the realm of gratitude: both describe networks of influence and exchange, and both declare a relationship between the author and other actors on the academic stage. In the analysis of scientific discourse, therefore, both deserve serious scrutiny.

Like citation, acknowledgement appears at first sight to be governed, to a greater or lesser extent, by an implicit code of professional conduct. What is interesting is that many authors choose to acknowledge the contributions and assistance of others: acknowledgement is a voluntary act of reciprocation. Given that the practice is well-established, why should we not accept that it has real social, cognitive and instrumental significance? The fact that an individual's underlying motive for granting an acknowledgement must necessarily remain the stuff of speculation does not mean that the process should be discounted, any more than citation should be disparaged because individual citer motivations cannot be laid bare in every case (it may, however, be possible to infer intentionality from an analysis of either content or the surrounding context (e.g., Small, 1978)). Although citation remains a residually 'private process' with a 'public face' (Cronin, 1980, p.311), that does not invalidate the use of co-citation techniques to map networks of scholarly communication and interaction and to reveal 'intertextuality' and,

possibly also, 'interpersonal ties' (White, 1990, p.98). In fact, as White observes pointedly, both political analysts and psephologists are usually content to monitor voting patterns and swings in the aggregate, without exploring individual voter motivations, so why not 'believe that there is a norm in citing — straightforward acknowledgement of related documents — and that the great majority of citations conform to it' (p.91), and thereafter focus on the big picture in terms of mapping and clustering? And why not, to paraphrase White, believe that there is also a norm in acknowledging and that the great majority of acknowledgements conform to it?

RULES OF THE GAME

The proliferation of acknowledgements attached to scholarly papers is beginning to cause headaches for editors of some major scientific journals. Kassirer and Angell (1991, pp. 1511-1512), writing in the *New England Journal of Medicine,* note that 'traditionally, authors use acknowledgements to identify those who made special contributions to a study that are not sufficient to qualify them for authorship'. In reports of multicentre clinical trials, however, acknowledgements are often made to everyone who had anything to do with the study, 'including those who were merely carrying out their jobs, such as technicians.' They describe a manuscript with an acknowledgement section that listed 63 institutions and 55 physicians, the number of patients contributed by each participating institution, the 51 members of the seven different committees, and the secretaries in the trial office. Of the manuscript's twelve pages, five were devoted to the acknowledgement statement. That is what I meant earlier by caricature.

These comments seem to imply two things: first, some individuals make contributions, which though not felt to be deserving of co-author listing, do warrant an explicit acknowledgement somewhere in the text. A tacit knowledge on the author's part of what justifies one modality rather than the other is assumed. Second, the practice is being devalued progressively as, apparently, trivial or routine (as opposed to special) contributions are being acknowledged. In fact, Kassirer and Angell's comments call to mind earlier debate within the bibliometric literature as to what constitutes a serious as opposed to a perfunctory or redundant citation (e.g., Moravcsik and Murugesan, 1975).

If Kassirer and Angell are correct in assuming that many acknowledgements reflect 'special intellectual or technical contributions', it is all the more perplexing that these records of influence should be effectively sidelined in the context of evaluation exercises. In

assessing scholarly performance, two principal measures are used: productivity and impact. As I noted earlier, the limitations of both measures are widely recognized (e.g., Anderson, 1992), but they continue to be used, largely for want of anything better. To set the reward register ringing, all a researcher has to do is feature as an author or co-author and, ideally, have his work cited by another. Whether he is the least significant co-author of a multi-author paper, or whether the paper is excoriated by those who cite it, does not ultimately matter. Within the prevailing reward system, a measure of kudos will be conferred, however exiguous or tendentious the actual contribution.

On the other hand, a glowing acknowledgement, even one from a Nobel laureate, will fail to activate the reward register. If authorship and citedness are to be included in the academic audit process, so, too, it might be argued, should acknowledgements. Currently, the acknowledgement functions as a kind of 'closet' citation: it almost begs to be taken out, dusted down and put to good use. By admitting acknowledgements, the Reward Triangle (authorship, citation, acknowledgement) would be closed. A rare, if not in fact unique, example of bibliometric triangulation in the research literature is provided in Murray's (1982) case study analysis of classical ethnoscience. In charting the evolution and dissolution of this *élite* specialty, he developed a matrix which combined co-authorship and trusted assessorship data on those individuals most closely identified with the major internal clusters (e.g., Yale component analysis; Ethnographers of communication), with trusted assessorship being 'operationalized as a recurrently listed commentator among a scholar's published acknowledgements.' (p.173) When citation data were factored into the analysis, it emerged that patterns of trusted assessorship and citation paralleled one another (p.167; p.174). It is not enough to argue that because some acknowledgements may be trivial, or, because unlike citations, they lack tradeable commodity status, that they should be debarred from consideration. The limitations and inconsistencies of citation analysis have not prevented the approach from achieving a reasonably broad measure of acceptance (albeit sometimes begrudging) among both the research and science policy communities, and there are no compelling, *prima facie* reasons why a different set of standards should be applied to acknowledgements. Indeed, if fear of trivialization is the primary concern, then there is a number of practical quality assurance measures which can be implemented.

The International Committee of Medical Journal Editors (1988, p.260) has agreed a set of procedures to be followed by authors. The

Uniform requirements for manuscripts submitted to biomedical journals includes a section devoted to acknowledgements, which concedes that there may be 'contributions that need acknowledging but do not justify authorship.' More specifically, it states that authors 'are responsible for obtaining written permission from persons acknowledged by name because readers may infer their endorsement of the data and conclusions.' The document also recommends that 'technical help should be acknowledged in a paragraph separate from those acknowledging other contributions.' In 1991, the *New England Journal of Medicine* (Kassirer and Angell, 1991, pp.1511-1512) announced that it would 'leave to the authors the choice of those acknowledged, but limit the space devoted to acknowledgements ... If acknowledgements fill more than a column of *Journal* space ... we shall deposit them with the National Auxiliary Publications service. At the authors' request we shall consider publishing fuller acknowledgements ... in reprints of the paper.'

Neither set of recommendations suggests to me that journal editors consider acknowledgements as rank textual trivia, undeserving of their attention. If anything, the reverse. The evident concern is to put in place guidelines which will ensure that authors explicitly, yet parsimoniously and correctly, acknowledge the contributions of their peers and co-workers: 'It is not in our readers' interests to permit unlimited lists of authors and acknowledgements, and it undermines the meaning of authorship and the *value of an acknowledgement* (italics added).' Other professional associations are more or less specific as to what constitutes correct behaviour in this regard. According to the American Association for Counseling and Development (AACD) due credit should be given 'through joint authorship, acknowledgement, footnote statements or other appropriate means to those who have contributed *significantly* (italics added) to the research and/or publication.' (Callis, *et al.*, 1982, p.12) The American College Personnel Association (ACAP, 1981, p.187) recommends that major contributions be acknowledged through joint authorship, while 'minor contributions of a professional or technical nature are acknowledged in footnotes or introductory statements.' If the 'invigilators' (Cronin, 1984, p.67) of the primary communication system can agree that acknowledgements are not without value, then there is a logically and pragmatically compelling case for folding them into the academic audit process.

The phenomenon of trusted assessorship is well established within, and long recognized by, the scientific community. According to Mullins (1973, p.18), 'every active, productive scientist has a few trusted assessors, usually not in his own department and often not in the same

specialty or even discipline who act as friendly critics of his work. These scientists may or may not reciprocate the service. In fact, the people in one scientist's group of assessors do not necessarily share the same set of trusted assessors; often there is not even one assessor in common.' In the absence of documented evidence showing that the practice of acknowledging trusted assessors (and any significant others who contributed to the conduct of science) is either frivolous, or in some sense flawed or discreditable, it is reasonable to ask why this particular public expression of gratitude is downplayed and undervalued alongside citations.

The question of the relative value (and utility) of the two indicators becomes a substantive one for individuals whose careers may be impacted by the ways in which their superordinates, peers, mentors, or co-workers choose to record the nature of their professional collaboration. Since co-authorship (even for the *n*th author of a multi-author paper) and citation (even negative citation) can be entered in the academic performance ledger, and despite the fact that the contributions thus recorded may have been less than those reflected in an acknowledgement, collaborators, not least those with limited experience, leverage or social power, may well be concerned that merit is not always rewarded according to strictly scientific criteria. In the absence of consistent guidelines across institutions and disciplines for determining the basis on which co-authorship versus an acknowledgement is awarded, junior colleagues, in particular, may sometimes feel that their contributions are given less weight than they deserve.

It is instructive that the *Uniform requirements for manuscripts submitted to biomedical journals* spells out the criteria for awarding co-authorship status (and by default the criteria for acknowledgement): 'Each author should have participated sufficiently in the work to take public responsibility for the content. Authorship credit should be based on substantial contributions to (a) conception and design, or analysis and interpretation of the data; and to (b) drafting the article or revising it critically for important intellectual content; and on (c) final approval of the version to be published. Conditions (a), (b), and (c) must all be met.' If there is credible evidence of systematic withholding of credit from status subordinates, (or the 'buying of authorship with power or funding' (Croll, 1984, p.406)) then the inconsistency in the way in which a group or discipline handles co-authorship, citation and acknowledgement becomes a significant issue for the scientific community at large, whatever the local basis for withholding.

In a study of women in professional *élites,* Epstein (1982, p.66)

attempted to show that systematic withholding of credit was related to gender: 'The rewards of the female professionals may also be more closely linked to their female status than to their status as professionals ... Bright female graduates students are often satisfied with gifts of approval and attention from professors for whom they work, and are content with the vicarious pleasures of contributing ideas as an ancillary partner.' She went on to address the spousal dimension in collaborative research, presenting a somewhat different picture from that painted by Ben-Ari (1987) in his study of acknowledgement trends in cultural anthropology. Epstein maintains that female graduate assistants are akin to wives who act as research assistants for their husbands: 'Like the wives, they are given florid acknowledgements in prefaces', leading her to conclude that 'professional reputations are built on publication and citation, not on dedication.'

Tentative empirical support for this view has been presented by Heffner (1979). He surveyed more than 200 individuals who had received a footnote acknowledgement for their contribution to research published in twenty eight social and natural sciences journals. Respondents were asked whether in the previous five years they had ever been excluded from authorship on a paper for which they believed their contributions to the published research deserved co-authorship status. His findings (p.379) suggest that credit in collaborative research 'is not always allocated according to universalistic criteria.' As far as gender bias is concerned, Heffner's results suggest that 'perceived exclusion from co-authorship recognition' (p.381) is more likely to occur among female researchers, which highlights one of the central difficulties in social exchange, that of determining in advance the appropriate return for a favour and ensuring that others can be trusted to discharge their obligations according to the prevailing norms.

The ethical issues associated with the assignment of credit have been addressed by Spiegel and Keith-Spiegel (1970), and, in an essentially replicating study, by Bridgwater, *et al.* (1981), while Moore (1984) has looked at the effect of authors' gender on acknowledgements. The first of these studies is especially interesting, both because of its methods and of the breadth of issues covered. The authors surveyed a random sample of: (i) 1,000 APA members and associates listed in the 1966 APA *Directory;* (ii) all psychologists who had five or more publications listed in the 1967 index of *Psychological Abstracts,* and (iii) 260 psychologists employed by the Veterans Administration. Respondents were asked to give their opinions concerning the assignment of publication credit in a range of (hypothetical) collaborative research situations (e.g., professor and

student collaborative research; ordering of credits based on functions performed; professional status of contributors). Each of the twenty eight situations was presented in *vignette* form (i.e., as a mini case study), and respondents were asked to choose from a variety of mechanisms for allocating credit equitably.

In each of eleven cases, 82.2% of the psychologists agreed on the form of credit that was most appropriate, which led the authors of the study to suggest a number of tentative guidelines, based upon the high levels of agreement. For example: 'When a psychologist assists a colleague with a small portion of a study by performing a service that involves some professional skills, *a footnote credit* (italics added) is the appropriate credit to be given.' (p.746) An example of a more detailed guideline would be the following: 'In the planning phases of a study, it is sound policy for the head of a research team to insist that decisions be made about who shall be responsible for each aspect of the project and to delineate explicitly *the form of acknowledgement* (italics added) that shall be given to each contributor, *if* he allows for changes to be made in credit assignments on the basis of the *actual* contributions that are made during the conduct of the project.' Spiegel and Keith-Spiegel (p.747) conclude that 'publication credit should be given only to persons who are very actively involved in contributing to a project' and that footnotes should be 'relatively concise indications of minor contributions and should be as explicit as possible concerning the nature of the contribution.'

Bridgwater, *et al.* (1981) conducted a partial replication of Spiegel and Keith-Spiegel's multiple choice survey in an effort to update attitudes on authorship assignment. They found that a majority of respondents (this time a sample of exclusively academic psychologists) 'agreed on solutions for most of the situations, with ten *vignettes* yielding solutions that were endorsed by 70% or more of those surveyed. Data collection (even if time-consuming) was deemed worthy of only footnote credit, along with supervision of data analysis, providing a research idea without being involved in the actual project, designing and building equipment, interviewing subjects, literature searches, running subjects, and archival data collection.' (p.525) Overall, Bridgwater, *et al.* noted a striking similarity between their findings and the results of Spiegel and Keith-Spiegel's earlier survey, leading them to conclude that 'despite a resurgence of the publish-or-perish syndrome, the interpretation (and ideally, the application) of ethical standards relating to publication credit has remained fairly consistent over the decade.' (p.525)

Reassuring though these findings are, it is important to remember that

they relate to hypothetical situations, and thus the possibility of discrepancies between idealized behaviour and actual practice should be taken into account. Second, what may obtain for psychology, may not apply in, say, the publishing ethos of high energy physics or biblical exegesis. Nonetheless, these two studies at least suggest that some (perhaps many) scientists have a shared understanding of the norms which govern the allocation of credits, whether it be a by-line or an acknowledgement nestling at the tail of a journal article. However, some empirical testing will be necessary if the (unwritten) rule base of acknowledgement behaviour is to be laid bare.

Although citation analysis has its occasionally severe critics (e.g., 'Citations form just a thin but glistening band, sandwiched between the rock of eons. And it is this highly limited, highly unrepresentative, yet alluringly available band of rock that the ISI has fetishized and turned into a highly desirable and marketable commodity' (Hicks and Potter, 1991, p.483)), most scholars have at least an osmotically acquired sense of the why, when, and what of citation behaviour (e.g., Cronin, 1984). Should an author fail glaringly to cite prior work or give credit where credit is due, or should he be found guilty of plagiary, then condemnation, contumely and sanctions in various measure may reasonably be expected, though, as a number of recent, high visibility cases have demonstrated, the scientific establishment does not always react to cases of alleged or actual malpractice with quite the degree of the rigour or consistency that a majority of its members have come to expect (e.g., Broad and Wade, 1982; Chubin and Hackett, 1990, Sarasohn, 1993).

In fact, as Chubin (1990, p.151) has noted, the limits of acceptable behaviour are being recalibrated subtly: 'On the one hand, plagiarism, fabrication, falsification, and misrepresentation of data are apparent wrongs; on the other, careless record keeping, unconscious bias (experimenter effects, in psychology lingo), and data hogging are almost-wrongs.' To this laundry list he adds the following examples of questionable or undesirable practice: 'attributing 'honourary' coauthorship to superiors, co-workers, or distant colleagues whose familiarity with, much less contribution to, the research report submitted for publication is dubious, even to them.' (The role of non-contributing authors has been well reviewed by Croll (1984)). If nothing else, Chubin's comments challenge the rather sanguine view presented by Spiegel and Keith-Spiegel and also Bridgwater, *et al.* as far as the normative bases of acknowledgement behaviour and etiquette are concerned. His conclusions are certainly unpalatable, but thereby all the

more deserving of close investigation: 'False authorship or redundant publication is no solitary act. It occurs in an organizational setting that either elicits such behaviour or does nothing to prohibit it. Such a setting creates a climate for malpractice: the combination of silence, lack of monitoring and unknown rules equals a conspiracy of normlessness.' (p.153)

ACKNOWLEDGEMENT INDEXING

Despite lingering criticisms relating to construct validity (e.g., Edge, 1979; MacRoberts and MacRoberts, 1989), citation analysis (facilitated in great measure by ISI's large-scale online citation indexes) has become an increasingly popular tool for measuring research productivity and impact at the individual, unit, programme, institutional and national levels (e.g., Borgman, 1990, Braun, *et al.*, 1985; Cronin and Overfelt, 1994; Evered and Harnett, 1987; Oppenheim, 1995; Snyder, *et al.*, 1995; White and McCain, 1989). Given the functional and symbolic similarities between citations and acknowledgements, there would seem to be a reasonable case for developing a complementary product, namely, an online acknowledgement index. Even some critics of citation indexing can see the logic for indexing acknowledgements: in a recent posting to a number of listservs, Fuller (1994) expresses surprise that from the outset the scientific community ever tolerated, let alone appeared to accept, the commercialization of citation indexing by the Institute for Scientific Information. In highlighting the limitations of citation analysis, he makes the point that scientists owe a great deal to 'informal networks, close ties to teachers, students, referees, *etc.* that are not adequately (or at least not proportionally) represented in the average article's reference list' and goes on to add that the 'problem has not been remedied by, say, indexing 'Acknowledgements' sections of articles.'

This idea has, in fact, been considered seriously by ISI (Garfield, 1994), particularly as far as acknowledgements to funding agencies are concerned, on the assumption that such information would help sponsors pinpoint the publication impact of their programmatic investments. Further, this information could be combined with citation data to assess the relative impact of funded *versus* unfunded research (e.g., Peritz, 1990; Harter and Hooten, 1992). At present, though, no major commercial secondary service markets online access tools to acknowledgements of any kind, structured or unstructured, and ISI remains sceptical that the additional production and quality control effort could be cost-justified.

Nevertheless, a number of related, essentially non-commercial

initiatives deserve mention. In the US, the National Library of Medicine (NLM) routinely records funding acknowledgements to the National Institutes of Health (NIH) and the US Public Health Service, while The Wellcome Trust in the UK (Jeschin, 1995; Lewison, 1995) has recently established a biomedical research outcomes database (which includes acknowledgement data relating to institutional support) for organizations interested in evaluating the success of the research they fund. Of all the elements in the acknowledgement spectrum, expressions of thanks to funding bodies are probably the easiest and least ambiguous to tally in a formal sense. Acknowledgements to family members, friends, co-workers, technicians, mentors, and graduate assistants, on the other hand, are messier, in that such a wide range of contributions, at variable levels of intensity, is possible, while the necessarily subjective and imprecise nature of the language employed would make weighting or scaling of individual contributions a virtual impossibility.

The closest equivalent to a commercial online acknowledgement index is an amateur (in the true sense of the term) undertaking by Dima Verner, an astrophysicist until recently affiliated jointly with the Kapteyn Astronomical Institute, Groningen in the Netherlands and the Space Research Institute in Moscow, Russia. His starting premise (Verner, 1994) is straightforward and uncontentious: 'Every astronomer knows that ordinarily many people (not only the authors of the resulting paper) contribute to a scientific research project.' Since almost all astronomical papers include a list of acknowledgements, in addition to references, even a one-year index derived from only seven leading journals produces an impressive set of statistics. Verner's *Astronomy Acknowledgement Index 1991* (Verner, 1992) includes 11,375 personal acknowledgements to 5,605 individuals, with a mean of 2.93 acknowledgements per article (Appendix 3 shows the top page from the index, all of which was made available over the Internet at no cost). The 21 most frequently named scientists have from 15-23 acknowledgements: all of these individuals are well known scientists who work in large astronomical centres and have an active scientific life. Most of the acknowledgements (79%) were for useful discussions and comments (what McCain (1991, p.512) has termed 'peer interactive communication'), and about a quarter were for providing access to data, theoretical models, and computer codes. The *Astronomy Acknowledgement Index 1992* extended the coverage to nine journals, generating 12,955 acknowledgements to 4,221 individuals with a mean of 3.07, slightly higher than the preceding year (Verner, 1993).

What has been the effect within the astronomy community of this prototypical acknowledgement index? According to Verner (1994), the

reactions and requests from fellow astronomers increased significantly after the publication of the 1992 index. Overall, reactions varied: some felt that it was a very interesting and useful development; others deemed it a waste of time (This is reminiscent of the early scepticism and antagonism directed against ISI's citation indexes by certain sections of the scientific establishment in the 1960s and 1970s.) Some astrophysicists requested their personal data, including one of the most highly ranked individuals who was interested specifically in tracking acknowledgements for the use of a federally funded computer code.

But acknowledgement counting is not only about the big names in a field. In reviewing normative and interpretative sociologies of science, Law and French (1974, p.589) make the point that if science is to be viewed as a social process, then there is good 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.' This resonates with another of Verner's (1994) observations: he has remarked on the 'unexpected but pleasant reaction' received from telescope operators and night assistants who, apparently, had long felt the need for such a register of contributions, since, as a group, they tend not to feature in author lists or be cited in papers. Consequently, acknowledgements from astronomers at the end of research papers are the only published proof the technicians have of their input to the scientific enterprise.

Verner believes that there are other compelling reasons for compiling online registers of acknowledgements. For instance, many astronomical databases (e.g., NASA/IPAC Extragalactic Database (NED)) are in the public domain, and can be used free of charge. Since the database constructors are funded from the public purse, they are understandably keen to capture any formal acknowledgements of use made of their products in published papers, as such evidence of value-in-use may help secure grant extensions or at the very least demonstrate to federal agencies the effectiveness of their efforts.

What, then, might a generic, commercial acknowledgement index look like? Using the *Science Citation Index* (SCI) as an exemplar, there is a number of design approaches which could be used to create an equivalent product covering acknowledgements of whatever kind. Three which suggest themselves are the compliance, free-trade, and no-frills models. In the compliance model, publishers and/or editors take on the role of quality controllers, requiring that submitting authors conform procedurally and stylistically to a given (journal-specific? field-dependent?) acknowledgement convention (just as is the case with referencing behaviour). The obvious drawbacks are the additional burden on both authors (yet another hoop to be jumped through before a paper can be submitted to the peer review process) and editors, and, of course,

the inevitable difficulty of settling on a universally acceptable standard. And yet, as is clear from the *New England Journal of Medicine's* advice to submitting authors quoted earlier, there are already intimations of such a trend. The attraction of this approach is that the database producer would work from a tightly pre-structured, meta-textual element which would be machine-processable like any other.

Table 2: Qualified Citation Index Project: relational operators

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

The free-trade model shifts the focus (and onus) onto the database producer. Here, the acknowledgement statement would be treated as a searchable textual element (just like the author name, institutional affiliation, or reference list attached to the typical scholarly paper) which has a number of pre-determined categories (e.g., peer interactive communication; funding source; technical assistance) and it would be left up to authors to decide whether or not to conform to the normative language and stylistic conventions. This is a quintessentially free-market approach to trading credits. Users of such an acknowledgement index would be able to search for specific categories of acknowledgement. A similar approach (as far as citations are concerned) was proposed some years ago in the context of the *Qualified citation index project* (1982, p.17), the idea being that authors could be encouraged by publishers/journal editors to affix one of 25 relational operators (see Table 2) to each bibliographic reference so that the reader would have a better sense of its specific purpose, and how the text related functionally to the citing article. A by-product of the proposed refinement would be more effective information retrieval. Interesting though the proposal is, it has never been built into in a commercial system.

In the no-frills model, the entire acknowledgement statement is treated as a separate, searchable field. Free text searches could be run using an

individual's, institution's, or entity's proper name (AU=SMITH in a cited author search would become ACK=SMITH where Smith is the individual acknowledged, or ACK=NSF, where the National Science Foundation is the funding agency being thanked for its support; or ACK=plasmid, where receipt of a plasmid from another scientist is being acknowledged). The key factor here is that the user has to specify in advance the appropriate search term(s).

The development of an online acknowledgement index should pose few technical problems for a commercial database producer, particularly given recent advances in digital imaging technology. The short to medium-term viability of the database, however, would hinge upon the producer's ability to stimulate awareness and, crucially, to build product credibility among both the academic and research communities. Whether and how that might be achieved remain valid concerns for any would-be developer interested in this area.

OUTSTANDING QUESTIONS

All of the foregoing raises a host of interesting issues and questions relating to acknowledgement behaviour. How widespread is the practice of acknowledging peers, co-workers and others whose actions, resources, inspiration, and good nature make possible the effective conduct of research? To what extent is the practice institutionalized? How consistent are acknowledgement behaviours across disciplines? Is acknowledgement style a function of research scale, technical complexity, local culture, styles of argumentation? Is acknowledgement behaviour governed by a set of formally stated or tacitly held rules? How consistent, in terms of literary style and affect, are acknowledgement statements from one discipline or field to the next? Are acknowledgements typically made to one individual/institution, or to multiples of either or both? What do composite (or compound) acknowledgements reveal about the dynamics of interdependence within/between communities of scholars/researchers? Does the frequency with which individuals are acknowledged in a given domain conform to a generalized distribution curve (e.g., with a small number of individuals being acknowledged frequently and a minority mentioned infrequently?).

Is there within the academic community a hidden population of influential teachers and/or researchers whose contributions (real and substantive) as mentors, stimulators, and nurturers are not revealed adequately through traditional publication and citation counts? What (if any) is the degree of correlation between cited and acknowledged authors: are we dealing with co-extensive, partially overlapping, or quite

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discrete populations? What are the arguments for and against treating acknowledgements as explicit records of intellectual debts akin to citations? Why do acknowledgements typically not feature in the academic auditor's *armamentarium*; why are they not included in the promotion and tenure process? What are the norms and etiquette which govern acknowledgement behaviour? How are these taught, inculcated, and monitored within the primary communication process? What (if any) sanctions exist and how are they invoked, if ever, to deal with demonstrable breaches of acknowledgement etiquette? What expectations exist as far as the giving and receiving of acknowledgements are concerned? Could the protocols and practice of acknowledgement behaviour be standardized (the equivalent of a referencing house style, in a sense) so that acknowledgement counting, like citation counting, would be amenable to mechanization? What are the bases on which co-authorship as opposed to an acknowledgement is granted, and how consistent are credit allocation practices within and across domains? So far, these questions remain largely unanswered.