Electronic Journals and the Transformation of Scholarly Communication. Constraints and Technical Possibilities

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In some ways, the fate of science parallels that of bourgeois democracy: both were born as exuberant forces for liberation against feudalism, but their very successes have turned them into caricatures of their youth. The bold, antiauthoritarian stance of science has become docile acquiescence; the free battle of ideas has given way to a monopoly vested in those who control the resources for research and publication. Free access to scientific information has been diminished by military and commercial secrecy and by the barriers of technical jargon; in the commoditization of science, peer review is replaced by satisfaction of the client as the test of quality. The internal mechanisms for maintaining objectivity are, at their best - in the absence of sycophancy toward those with prestige, professional jealousies, narrow cliques, and national provincialism - able to nullify individual capricious errors and biases, but they reinforce the shared biases of the scientific community. The demand for objectivity, the separation of observation and reporting from the researchers' wishes, which is so essential for the development of science, become the demand for separation of thinking from feeling. This promotes moral detachment in scientists which, reinforced by specialization and bureaucratization, allows them to work on all sorts of dangerous and harmful projects with indifference to the human consequences. The idealized egalitarianism of a community of scholars has shown itself to be a rigid hierarchy of scientific authorities integrated into the general class structure of the society and modeled on the corporation. And where the pursuit of truth has survived, it has become increasingly narrow, revealing a growing contradiction between the sophistication of science in the small within the laboratory and the irrationality of the scientific enterprise as a whole.

Richard Levins and Richard Lewontin (1993: 315-16).

Couldn't sociology aspire to the moving effects/affects of music? I would like sociological writing to weep, and to introduce us, readers and writers, into a weeping. "Let the music express joy or grief, pity, or love, every moment we are what it expresses".

Ann Game

We are talking, then, about a self that is open to the other, not without borders, but with moveable borders that allow a relation with the world. This is a state of being that requires discipline and training in, for example, letting go of a Hegelian desire for identity and mastery, letting go of a desire to be closed, without emotion.

Ann Game

Well, art is art, isn't it? Still, on the other hand, water is water! And East is East and West is West and if you take cranberries and stew them like applesauce they taste more like prunes than a rhubarb does.

Groucho Marx

"I'd explain it to you, but your head would blow up."

Steven Wright

List of Tables

Introduction

Information specialists, [1] scholars, [2] and governments [3] have been warning of a crises in scholarly communication for years. For decades, academic libraries, have seen their ability to maintain adequate collections of journals and monographs slowly but inexorably eroded by the predatorial pricing policies of commercial publishers capitalising on their virtual monopoly of high prestige academic titles. Certainly students suffer both in terms of declining access to books and monographs, and also because at least a portion of the rising costs are transferred onto their shoulders via tuition fees. But academics and authors pay the price as well as their ability to publish manuscripts declines as more and more of library budgets are taken up with journal subscriptions. It is not atypical these days to find publishing houses, because of the declining ability of the market to bear monograph publication, rejecting manuscripts that do not have direct relevance to university courses.

For a time, it was hoped that electronic publication would bring some much needed relief by reducing the cost of distribution of the primary journal literature. This was thought to be so because the electronic journal allowed much (as much as 80%) cheaper distribution of scholarly material than paper publication. Electronic publication circumvented the high cost of paper, printing and mail distribution with no, or very little, loss of quality. It was expected that paper publications would, as a result of their numerous weaknesses and inefficiencies, eventually go the way of the dinosaur and be replaced by more efficient electronic publication.

However despite hopeful statements in the early years, it now appears increasingly unlikely that electronic publication will bring the required relief. Not that there will be no electronic publication. Rather the opposite. Already there are thousands of electronic journals available. The problem is simply that the commercial publishers, who have moved into the field in force, rather than seeing epublication as an opportunity to reduce costs to the academic libraries which they *serve*, have chosen to exploit the opportunities for **increasing** profit presented by the new technologies. In some ways this shouldn't be a surprise. The opportunities for profit provided by a closed, monopoly system of production are simply too attractive to resist. From our 1998 perspective, the 1996 vision of a future communication system where scholarly skywriting is conducted in a low cost, collectivist manner by the scholars themselves now seems largely a hopeless fantasy. Andrew Odlyzko, [4] for example, notes that despite the early predictions for a rapid quantum shift away from paper and

towards electronic forms of distribution of scholarly information, no dramatic shifts have materialised. Even Steve Harnad, untiring advocate of revolutionary change in the primary communication system, has given up predicting the apocalyptic demise of the traditional publication system and now simply continues his advocacy of alternative publication models in the face of continuous resistance to change. [5]

A recent statement issued by The International Coalition of Library Consortia (ICOLC) [6] confirms the worst fears of scholars and dramatically and unambiguously politicises the issues. As the ICOLC statement indicates, publishers are using their growing monopoly position and control over the scholarly communication system, to force libraries to purchase **both** paper and electronic versions of their journals at rates that are already higher than the standard print cost and at rates that the coalition fears will eventually add as much as 40% or more to the cost of scholarly material in journals. This, coupled with the failure of independent scholarly publication, has led to the current desperate situation. This is certainly not the dream of independent scholarly publication. It is the nightmare of unregulated monopoly control. In a press release which introduced the statement, the ICOLC note:

> The explosion in electronic licensing, the wide variance in publisher practices, rapidly escalating prices, and a concern about the reduction in the number of independent scholarly information providers all served as the impetus for the statement. The Statement calls for developing multiple pricing models, separating charges for electronic licenses from those of paper subscriptions, and lowering the cost for the electronic information below that of print subscriptions. ICOLC expresses its concern over the growing practice of publishers that levy initial surcharges on electronic information, which is compounded by significant multi-year inflation surcharges and prohibitions against libraries canceling print versions of journal titles. As a result, while libraries may receive access to a larger array of titles by paying the "print price plus electronic subscription cost plus inflation," the total base price for electronic access over the print subscription could increase by 40% or more within as little as three or four years (ICOLC, 1998).

Should this current trend be allowed to continue, the *crises* in scholarly communication, which has been on a slow boil for a number of decades, will boil over. Commercial publishing houses will continue to squeeze out independent publishers on the net. And presumably, with their growing power,

society publishers and independent consortia will be the next targets. Monograph purchases will further decline (cutting into an important source of income for scholars), library collections will dwindle, and user fees will eventually be implemented to support the ongoing profiteering of the large commercial presses. This, coupled with the ongoing commercialisation of higher learning [7] bodes ill for the financial, intellectual, and professional health of academic institutions.

There seems to be little reason for hope. The big publishing houses are currently positioning themselves in order that they may exploit "attractive" opportunities in the scholarly communication market. For example, Reed Elsevier has recently announced that it will divest itself of IPC Magazines (a distributor of consumer magazines). This divestiture would allow Reed Elsevier to focus on developing a strategy of increasing its ability to exploit the "high value-added areas of 'must have' information" at the same time that it reduces its "exposure to consumer markets." As the cited press release indicates, "The proceeds [of the divestiture] would be used for future development of and acquisitions within Reed Elsevier's core Scientific, Professional and Business Divisions and would provide the company with greater flexibility to respond to attractive growth opportunities as and when they arise." [8]

The language seems clear enough and the intent obvious. And if this isn't worrisome enough, Reed-Elsevier had also proposed a merger with Wolters Kluwer which would, by combining the resources of two major scientific journal publishers, make a publishing megalith worth 17.5 billion pounds. [9] And even though this merger has fallen through [10] there will no doubt be future attempts to consolidate strength in order to eliminate competition and exploit opportunities. There should be little doubt that business sees profit opportunities in the university market. Even Microsoft - a company known for its profiteering, anti-competitive history, and arrogant desire to control the Internet, [11] and higher education [12] - has moved into the field. [13]

So what are we to make of this interesting turn of events? Certainly, the current state of affairs is a far cry from 4 years ago when pundits confidently proclaimed a revolution was nigh. But in some ways the current shift is very surprising. As argued in the body of this dissertation, there is powerful potential available in the new technologies to solve the scholarly communication crises and bring efficiencies to the system of scholarly communication. Because the most costly aspects of paper production are circumvented in the electronic realm (i.e., press costs, postage, etc.), and because the full electronic processing of text brings other efficiencies not possible in the paper realm, electronic communication seems a sensible way

forward. Yet none of the potentials inherent in the new technologies have been actualised. The question is why?

There are a number of related issues which impact on the electronic communication and which have precipitated its failure. In the first place, the discourse on electronic scholarly communication has essentially assumed that just because there is a technological potential, we will necessarily see an actualisation of this potential. The assumption, often implicit but sometimes directly stated, that technological potential leads directly to revolutionary outcome is based on a naïve technological determinism that fails to interrogate the linkages between technology and agency, technology and social structure, or technology and systems of hierarchy and prestige both inside and outside the academy. It is the worst form of utopian prognostication not only because in misrepresents social reality but also because it encourages a depoliticisation of the relevant issues and thereby pushes those most interested in the system to accept inactivity as a viable response to the journals crises. Given a basic analysis of the contours of the crises (high cost of paper and production) for example, and given a technologically determinist analysis of the powers of information technology, the conclusion was quickly reached by authors that electronic journals would reduce the cost of scholarly publication and bring much needed relief to the system - just like that, and without any deeper analysis of causal antecedents or sustained, and political, effort.

A second reason for the failure, directly related to the tendency to adopt technologically determinist arguments, it a tendency to individualise the problem by attributing the failure to the lack of initiative on the part of scholars and libraries. But like the deterministic analysis which saw revolutionary change flowing automatically from technological potential, this approach is not altogether fruitful (although inactivity is a problem). As we will see, failure of the revolution has as much to do with the structural constraints placed on the system than it does on individual failure. In fact, individual failure to act can largely be attributed to structural constraints. Of course we do not wish to replace a naïve technological determinism or misplaced reliance on human agency with an equally sterile structural determinism. There is no need to argue deterministically concerning the social and political structures which impede revolutionary change in the scholarly communication system. However it is to suggest that any potential revolution awaits a much more sociologically informed analysis of the scholarly communication system. And it is also to suggest that any meaningful change will meet with various forms of resistance - resistances which will have to be identified and dealt with if forward motion is to be maintained. Otherwise we joust at windmills.

The question now becomes just what are the contours of a more sociologically informed analysis. The first step towards such an analysis involves moving beyond traditional analysis which imputes the cause of the current fiscal crises in libraries simply to the high cost of journals. While it is technically true that the high cost of primary journals is putting strain on library resources, it is also true that there are underlying factors which have themselves caused the high prices. This reification of cause fails to move behind the empirical fact of high journal prices to theorise and examine the social conditions which contribute to high cost, or which (more appropriately) prevent the progressive potential of new technologies as applied to electronic journals from being realised. It is a profound oversimplification. As this dissertation attempts to argue, the crises of scholarly communication system goes much deeper and in fact participates in a much wider crises of liberal democracy in advanced capitalist nations, than a narrow focus on technology or agency (or even the monopoly characteristics of the primary journal) would suggest.

Just what is this crises of liberal democracy? It is, essentially, the abdication of social responsibility by representatives of the ruling classes and the shift towards forms of capitalism characterised by an overarching concern for the generation of private profit, by corporate control of the economy, by hegemonic manipulation of the population, [14] and by the increasing use of surveillance and the police state to back the corporate project (Gandy, 1993). This shift, variously termed neoliberalism or neo-conservatism, was initiated as a result of a crises of accumulation in the advanced capitalist nations in the mid seventies and early eighties. As Teeple writes, the economic and political conditions which supported the Keynesian welfare state and the associated possibility for reform and redistributive justice evaporated by the end of the 1970s as a result of the "internationalization of capital, new means of production, and declining national growth." [15]

> The 1980s was a watershed decade, a turning point in the history of capitalism. It was a period that witnessed the beginning of the end of a vast system of collective or state property in the so-called socialist countries, the establishment of computer-aided modes of production and distribution, the arrival of the global economy, and the adoption around the world of neo-liberal policies whose principle was the unrestrained economic power of private property. The decade signified the beginning of what has been called the triumph of capitalism. [16]

Much of the analysis surrounding the demise of the Keynesian state and the emergence of neoliberal politics and ideology do not concern us in this work.

However there are certain elements of the new ideologies and politics which directly impact not only on the extant crises in the scholarly communication system, but also on the ability of stakeholders to reform the system. As various authors have noted, a critical feature of the new politics is the replacement of public spaces with private opportunities for accumulation and the valorisation of capital. [17] Practically, this has meant the downsizing of governments (in areas of social spending), the decline of social reform and the welfare state, and the erasure of public space and its replacement with for-profit alternatives. In Canada the shifts are well documented and include reduction in social programs, decline of corporate taxes, and the gradual replacement of "profitable" public services like healthcare and education with private alternatives. [18]

The result of this neoliberal push is a profoundly reconfigured system of capitalist production. Dramatically reduce is the post WWII capital/labour accord. Gone is the potential for progressive reform and the gradualism characteristic of a system in need of accommodating to its citizens. In its place comes a system of predatorial capitalist production concerned primarily with reducing the cost of doing business as much as possible. If this means increasing poverty and hunger (to ensure an adequately disciplined labour force) even in the context of incredible wealth (witness Canada's high child poverty rate, our homelessness, and our high unemployment), then so be it. As we are constantly reminded by neoliberal ideologues, we (meaning anyone but the elite) must all sacrifice to remain globally competitive.

Like many other public arenas, education has not escaped the neoliberal assault unscathed. Rather than seeing universities and other educational institutions as public spaces under the tutelage of public officials and serving the public, universities and schools are being "re-imagined" as spaces appropriate for profit generation and private sector service. As a result of the neoliberal push, universities are being colonised, physically and intellectually, by capital, its representatives, and its ideologies. [19] This has meant the importation of market discourse, [20] including discourses of efficiency, accountability, and consumerism, the shift away from public funding towards private contribution, and the imposition of "market discipline" through various forms of tied financing. [21] In all this the state as representative of capital has played a critical role in undermining the autonomy of the university by eliminating state funding. This has essentially forced the universities to move "out into the market." As Kachur [22] notes of this marriage of convenience, their move out into the business world has been welcomed.

> The reality of the corporate agenda has a dual linkage. Universities are attempting to move out into the market in

> search of profits and corporations are attempting to penetrate the university boundaries in order to utilize university research. The window to ongoing research influences the direction of such research and gives access to marketable products based on that research. The role of the state, as perceived by the New Right and liberal technocrats, is to facilitate this new economic relationship.

We shouldn't develop the sense that universities as a site for the generation of private profit is new. It is quite clear that the status quo and universities have a long history together and that valorisation of capital has played a role in that relationship. [23] So current trends continue many past practices. However, recalling the two decade shift in political ideology and the realisation of the erasure of public space, there is a new dynamic and renewed vigour in the assault, made possible by the elimination and/or harnessing of alternative discourses that might counter the hegemonic ones of Weberian rationality and economic efficiency. It is these changing circumstances which make the realisation of progressive change in the universities so difficult. Like the demise of the post WWII labour/capital pax, the "equitable" working relationship between scholars and capital has been eroded. Scholars are now expected to willingly accept the supporting myths of capitalism and willingly subordinate their own intellects to the needs of the system. Indeed, with the introduction of new technologies, there is even the sense that scholars will very shortly become victims of hegemonic manipulation themselves. [24]

So where does the scholarly communication system, and the primary journal, fit into capitalist political economy? In a number of ways. As part of the system of scholarly communication, the primary journal has historically been a site for the generation of private profit. Private enterprise has seen significant commercial opportunity in the monopoly like journal system. As is well understood, prestigious journal titles have virtually no competition since libraries and scholars **require** the provision of access to these titles. This lack of competition has allowed commercial publishing houses to prey on the public institutions of higher learning by setting prices well above what is justifiable given the requirements for professional scholarly communication and reasonable private profit.

However there are deeper linkages between capitalism, neoliberalism, and the communication system and it is the task of this dissertation to explore in some detail what these linkages are. That is, this dissertation will contribute towards a more adequate theorisation of the primary journal and the scholarly communication system and the blocks towards progressive reform or revolution placed in the way by the current cultural configuration. To put this in

other words, it is the task here to expose the interests which are implicated in the system of scholarly communication both historically and in contemporary society. It is only by exposing these interests, and by understanding how these interests shape the scholarly communication system and, more importantly, resist change in the system in order to protect vested interests, that we can develop a theoretically informed praxis capable of transforming the system. Failure to develop an adequate theorisation will leave us nodding impotently in the direction of reform or revolution - keeping us always a dozen or so steps away from actual realisation of a progressive future.

Exposing these interests takes a number of forms. As part of the analysis, we examine business interests and their stake in the scholarly communication system and we embed this analysis in the context of wider political shifts. However business is not the only stakeholder in the system. Scholars also play a role in directing the evolution of the system and, ironically, resisting attempts to reform the system. To understand this resistance we have to understand that for scholars, the primary journal is not just a repository of knowledge (as much as empiricists would have us believe). Primary journals function equally as gatekeeping mechanisms, tools for the distribution of scarce rewards, and legitimaters of scholarly and professional prerogatives. These functions (many of which protect vested interests in the academy) need to be exposed, examined and accepted or rejected based on informed scholarly debate if authentic transformation is to be accomplished. Otherwise we can expect ferocious resistance on the part of the scholarly establishment to any attempt to tamper with or modify the system.

Should we fail to theorise the true nature of the scholarly communication system, and should we fail to partner successfully with the scholarly establishment, most of us (scholars, small journal houses, small independent publishers, libraries, the universities as public space) will pay a price since resistance and rejection of new forms of scholarly communication will ultimately support the further penetration of commercialism in the academy, the extension of neoliberalism and the (re) entrenchment of scholarly hierarchies based on race, gender, and nationality. Failure will also remove competition and support further commercial monopolisation and consolidation. It is clear that all of these are areas potentially amenable to progressive reform. Yet all of these will be sacrificed if alternative forms of scholarly communication are seen to challenge in a significant way scholarly prerogatives. This is a significant irony and challenge, and one not to be sniffed at since resistance on the part of the scholarly established has, in the past, been responsible for scuttling innovation.

There are very good reasons to attempt to work together. If current trends

towards the commercialisation of scholarly communication and the academy continue, it is reasonable to think that even the scholarly establishment faces a long term threat from neoliberal shifts. There has been concern expressed that scholars and their societies have been losing control over scholarly production over the years as titles are given over to commercial houses for production. This trend will no doubt continue and accelerate if cuts to funding continue and if no viable alternatives are developed to the commercial houses. In the long run, this means transference of skill outside of the academy and the loss of the ability to choose to develop journal titles. In the context of this transference (and this can include transferring to large university presses who have commercialised their operations) the long term result is the imposition of the logic of the market on journal selection. In other words, losing control over journals may, in the long run, mean that only those journals that are deemed "profitable" will be published. Why would a commercial house publish an unprofitable title after all? Or course they might if there was a subsidy. But governments and institutions continue to cut back support for projects which cannot attain "self sufficiency." As many have noted, significant areas of scholarship could be lost as neoliberalism penetrates the academy.

There are other potential threats. As noted earlier, a significant component of neoliberalism is the creation of a high-tech surveillance state and economy. In the new world order, surveillance is used as a commercial tool to target consumers (American Express for example owns two supercomputers that enable them to "profile" their card users) and also as a disciplinary device which more and more approaches a universal electronic panopticon. Surveillance, in one form or another, seems an almost essential component of the new capitalism. It is a means for securing the conditions for ongoing accumulation and also, perhaps for ensuring the long-term victory of capitalist production in the face of crumbled ideological alternatives. The question we raise here is whether or not this component of the new capitalism will penetrate the academy as other aspects of neoliberalism have (e.g., commodification).

The question is a reasonable one to ask. And others have been asking similar questions in attempt to explain lack of significant resistance to the neoliberal agenda. Indeed, Kachur expresses consternation that there has been no sustained resistance to the neoliberal assault at any level and asks the question, why has there been no significant counter hegemony? One important part of Kachur's answer to this question is his argument that, in a Canadian context at least, this lack of effective intellectual resistance to the agenda of neoliberalism is the result of the legacy of intellectual colonisation. Kachur writes that in Canada, intellectuals play the game of scholarship not by staking out an independent cultural or intellectual space, but by accumulating

"credentials" and "data" as signs of their acceptance into the scholarly fold. It is the intellectual version of Calvinist salvation. Work hard, struggle against adversity, don't complain too much, collect (intellectual) capital, and your access to the rarefied world of the philosopher's peak is secured. Kachur also writes that part of the process of colonisation is to leave the really important decisions outside of the academic sphere. Thus value judgements, morality, and other questions which lead directly towards social policy are strictly excluded from the purview of the academic. The result, for the Canadian academic, is a peculiar form of intellectual poverty that leaves them incapable of developing sustained and theoretically useful critiques or options to current hegemonic systems.

Certainly there is some sense of truth to this. But in all fairness, analysis should be extended to the intellectual class in general. There seems little reason to single out Canadian academics especially since it would seem that the distinction between "fact" and "value," hermeneutics and positivism, scientific and un-scientific is deeply embedded in the tradition of western empiricism and positivism. [25] If we are to argue that intellectuals have been colonised by a discourse that hamstrings their ability to participate and live in the world and develop other than sterile intellectual edifices, we should generalise this to most intellectuals in the western tradition (this is of course, not a new insight. Others have been quite willing to trash western intellectual traditions and the rarefied, specialised intellectual). It is not just Canada's intellectuals who have been colonised. We all participate, to one degree or another, in the modernist scientific project.

However, Kachur was on the right track I think. Intellectuals are implicated in structures of domination not only as purveyors of that hegemony but potentially as its victims. And while it is true that there has been very little research into the ways intellectuals become colonised, it is also true that it is only with the decentering of the western subject and the delegitimation of the western enlightenment project (the postmodern theoretical project) that space has been opened for a more sustained consideration of how scholars themselves are implicated in structures of domination and desire. With the decentering of the western subject, it is no longer possible to claim that scholars are above the messy psychopathologies, oppressions, dominations, and psychological caverns characteristic of the life of the commons. This shift has been critical for legitimating critical inquiry into scholarly practice.

Any in-depth treatment of the project opened up by the postmodern turn in theory is ultimately beyond the scope of this work. However, we can contribute to this project one important piece. That is, we can develop a theorisation of one aspect of the processes of intellectual colonisation as it emerges out of

the scholarly communication system. Our concern here is prefigured by Kachur's suggestion that scholars themselves might be victims of strategies to mobilise and direct scholarly inquiry and desire through "Cybernetic feedback systems of information processing…" [26]

This dissertation will explore these linkages in the context of the potential of new electronic technologies to create a cybernetic panopticon in the academy capable of extremely subtle means of disciplining scholars and controlling scholarly discourse. This work will examine what it is about technology which makes a system of hegemonic control of scholarly discourse possible, and also unpack how the selection of specific tools for increasing visibility functions to embed and systematise bias into the core of the system. What we essentially theorise here is how intellectuals are victimised by mechanisms designed to narrow the field of acceptable intellectual debate - much like the closure of public debate and public spaces which seems a component and a priority for neoliberalism. This closure, if I am correct in my assessment of the future of scholarly communication, will be effected through cybernetic technologies that mobilise scholarly desire (for credentials, for prestige, etc.) at the same time that they help limit avenues whereby these desires might be obtained. This is already how discipline and regulation are conducted in the spaces outside the ivory tower. So it seems reasonable to examine whether these new forms of hegemonic control are not also being imported into the academy. As Kachur notes... [27]

> The second element missed by most analyses is the changing nature of cultural regulation brought about by the revolution in new communication technology and the applications of the science of consumer management. Not only has cultural regulation become more anonymous, it is increasingly used to mobilize people's desires rather than develop their intelligence. Through this "science of desire," managerial elites secure their political agenda.

At the outset it is probably worthwhile to note that our focus on the scholarly communication system forms only part of a much needed analysis of neoliberalism and scholarly discipline that would also include the imposition of other forms of hegemonic control on scholarly desire and discourse. This extended analysis would include an examination of how market discipline is effecting research selection, how class room surveillance and the replacement of student culture with consumer culture is eliminating critical pedagogy, and how colonisation and commodification are modifying scholarly practice. Although much needed, such an extended analysis is beyond the scope of this work.

Chapter Outline

The analysis begins with a look at the history of scholarly communication and the extant systems of scholarly communication. In chapter one I demonstrate that from its inception, science and the primary system of scholarly communication has had links with the capitalist bourgeoisie. The links have arguably been tenuous at times, but nevertheless science and the scholarly communication system have served the needs of the production system upon which it depends. This includes science in the service of industrial expansion (through research and development) and also science in the service of cultural hegemony. On the latter, intellectuals have, to one degree or another, provided the hegemonic props for a system of private accumulation by their propagation of the myths of empiricism, value neutrality, and epistemic privilege. Support for the capitalist system and the (sometimes) extreme inequality which goes along with it has also been provided by the simple reflection of bourgeoisie hierarchies in the university system. Scholars and the education system are also implicated via the transmittal of cultural capital. The contribution of chapter one to the dissertation is to remind us that a) science participates in class domination and b) the mystification of the role of science has not gone unnoticed or unchallenged. In this context, we note that the scholarly communication system is more than simply a mechanism for distributing rarified and purified knowledge. It is a contested site where systems of domination and exploitation interpenetrate with more progressive tendencies.

Chapter Two is an empirical examination of the current difficulties faced by papryocentric system of scholarly communication. Here we look at the traditional analysis which pegs publication delay, high cost, and journal proliferation as the primary dysfunctions of the current system. However here we extend beyond the usual "functional" analysis of the scholarly communication system to unpack some of the deeper "dysfunctions." In the process, we explore the anglocentric and male bias in the system and note how traditional problems like publication delay may in fact support structured inequality and stratification in the academy. We also explore the political economy of the system and note that peculiar monopoly nature of the primary journal system has led to a degree of commercial control that is financially unhealthy. In addition to surveying the difficulties faced by the current paper centric system, this chapter is also useful for providing a deeper analysis of the problems in the scholarly communication system that may contributes towards the development

of alternatives journal models not plagued by the various "dysfunctions" identified in the chapter.

Chapter Three will examine what the electronic journal can potentially contribute to solving the scholarly communication crisis. In this chapter we will see that the electronic journal can solve, at least in part, problems of delay and access. We will also learn how, when handled by those that are not motivated by profit, the electronic journal can immediately and effectively solve the cost crisis for libraries. In order to highlight the potential impact of electronic journals on the financing of the scholarly communication system, we will examine and evaluate various alternative models of delivering scholarly communication in the electronic environment. It will be useful here to contextualise the discussion on the potentials of electronic communication in the discussion in chapter two on "surface" and "deep" pathologies in the system in order to take a step towards an informed re-imagining of the scholarly communication system. However despite the potentials inherent in new technologies to overcome some of the traditional limitations and problems in the system, I close the chapter with a caveat about the resistance likely to be faced by the commercial publishing sector.

It is the task of chapter four to explicate this deeper struggle between a public academy and a privatise education system. Here we will examine how traditional commercial presses are resisting a truly revolutionary outcome and attempting to claim the new frontier of electronic scholarly communication as their own. In this chapter we will outline the threat to an alternative system and unpack the discourse being utilised by the commercial presses and their representatives for de-legitimating alternatives to the traditional system of scholarly communication (which they largely control). It is in chapter four where the significance of the neoliberal agenda to the scholarly communication system becomes most obvious. Here we see clearly the importance of demonstrating how the scholarly communication system is embedded in, and a part of, the capitalist institutional structure. That is, it is only by understanding the political economy of the scholarly communication system that we can move beyond a narrow focus in order to understand the implications of neoliberalism for the scholarly communication system as a whole.

In Chapters Five and Six we broaden our analysis in order to examine in more detail the potential dark side of information technology and electronic publication. In Chapter Five we prepare the groundwork for a

critical analysis of information technology by introducing and critically analysing the premier method of evaluating scholarly output - citation analysis (CA). Here we examine CA in considerable detail in order to unpack the peculiar characteristics (i.e., biases) embedded in this form of measurement. It is a fact that different measuring instruments are better, or worse, at measuring different aspects of social reality. At the risk of sounding trite, multiple choice exams are less successful at assessing understanding than are essay exams. It is the same with citation analysis. Its peculiar structural characteristics make it particularly useful for extracting certain features of the scholarly communication system while obscuring others.

Chapter six rounds out the analysis of CA by examining the intersection of the scholarly communication system, citation analysis, and neoliberalism. Here we argue that given the neoliberal push to colonize the university and erase its public spaces will consist, in part, of a need to exert administrative control over scholarly output. This desire to bring a form of employee accountability will necessarily, I argue, intersect with the CA and information technology, to create a system for the control of scholarly discourse capable of mobilising the scholars desire for recognition. One this desire has been mobilised (something that new scholars will be most vulnerable to), it will then be possible anonymously regulate scholarly discourse.

I call this anonymous regulation through information technologies the "cybernation of the academy." It is in chapter's five and six that we confront fully the potential future for scholarly discourse in the face of neoliberal shifts and the attempt to create a hegemonic consensus. Systems of panoptic control can be seen to be developing in such a way as to secure hegemony over the intellectual through consensus at a deep level. Of course, one type of hegemonic control over scholarly discourse already exists and this is the insistence on empiricism, value neutrality, and unemotionality which, as many scholars are beginning to recognise, play into patriarchal, racist, and capitalist structures of domination. However where as before there has always been space for alternatives, now we seem threatened, in the long term, with the virtual elimination of space for criticism.

Chapter One:
Scientific Communication

> ...the traditional image of the scientist-as-writer, an image common both within science and without, is... a peculiarly humble and constricted one. Gone are the creativity and daring of the Titan. In their place is the self-effacing toil of the amanuensis, for, unlike poets, who are free to create the world anew as they write of it, scientists must be totally circumspect, must expunge every trace of their own personalities from their work, as they record only what is there. The scientist must view the world through some impersonal ocular device, the spectacles of objectivity, rather than through the eyes of a person. The scientist must weigh the world not in a hand but in a balance; must measure it not with a stride but with calipers. The Promethean scientist may uncover the secrets of the gods but must whisper them - like someone with an artificial larynx - in a voice devoid of emotion so as not, as the Fiesers put it for their fellow chemists, to "divert attention from the story we are trying to tell." Above all, scientists dare not run naked through the streets shrieking "Eureka!" or they will, as the Fiesers say, "violat[e] principles of good usage."[1]
>
> We will obtain a totally misleading view of science if we infer its social attributes from the formal characteristics of the claims presented in articles, reviews and textbooks. Formal knowledge claims have meaning only when they are interpreted by the members of the actual social groupings. The way in which their interpretations are realised depends on the outcome of contingent negotiations among those members. (Mulkay, 1979, *Science and the Sociology of Knowledge,* George Allen & Unwin, London)

Introduction

In this chapter we will take it as our main task to provide a descriptive account of the scholarly communication system. Our first task will be to examine the emergence of the primary scholarly journal in 17th century France and Britain. [2] We will examine some of the reasons why a journal communication system emerged in the first place. Further, we will also follow the development of the primary system of scholarly communication as it expanded and evolved through what some see as increasingly desperate and ineffective attempts to

manage the veritable explosion of scholarly inquiry and scholarly communication.

In addition to briefly discussing the history of the scholarly journal, we will also outline the extant systems of scholarly communication. Traditionally, the system of communication has been conceived of as divided into two parts: a **formal** realm (encompassing the written and archival literature of science); and an **informal** realm (encompassing everything else that goes on in science). Even though we are properly concerned with the formal communication system of science and the scholarly journal, we will look at both the formal and informal system of scholarly communication. The reason for this it twofold. On the one hand, the informal system has been largely misrepresented in previous discussions of the scholarly communication system. The traditional practice has been to clearly demarcate the formal and informal systems and privilege the formal system of communication as the *sin qua non* of the scientific enterprise. However recent work in the Sociology of Scientific Knowledge (SSK) has made maintaining this demarcation and privileged position difficult. It is in order to contribute to a wider dissemination of the newer understanding of the informal communication system that I review the research here.

A second, and perhaps better, reason for considering the communication system in toto is that we cannot develop a sophisticated understanding of the formal system without also understanding its relationship to the informal system. Those that have ignored the informal system, or those who hold a traditionalist conception of its role, have imported assumptions, myths, and biases into their analysis of the scholarly communication system that cannot be supported. A better understanding of the informal system of communication gives us a better understanding of the dynamics of science and helps check against reification of the primary communication system.

A Brief History of the Scholarly Journal

> Whereas there is nothing more necessary for promoting the improvement of Philosophical Matters, than the communication to such, as apply their Studies and Endeavours ... it is therefore thought fit to employ the Press, as the most proper way to gratifie those, whose engagement in such Studies, and delight in the advancement of Learning and profitable Discoveries, doth entitle them to the knowledge of what this Kingdom, or other parts of the World, do, from time to time, … To the end, that such Productions being clearly and truly communicated, desires and solid and usefull knowledge may be further entertained, ingenious Endeavours and Undertakings cherished, and those, addicted to and conversant in such matters, may be invited

> and encourage to search, try, and find out new things, impart their knowledge to one another, and contribute what they can to the Grand design of improving Natural knowledge and perfecting all Philosophical Arts, and Sciences. All of the Glory of God, the Honour and Advantage of these Kingdoms, and the Universal Good of Mankind. [3]

The history of the scholarly journal begins with the foundation of the *Philosophical Transactions of the Royal Society* in London on March 6, 1665 by the Royal Society of London (founded just three years earlier in 1662) [4] and the *Journal des Sçavans* in France on January 5, 1665 by the private scholar Denis de Sallo. The former journal was founded by members of the *Royal Society of London,* in particular their secretary Henry Oldenburg. The latter by a French dilettante by the name of Denis de Sallo. Following the emergence of the first two scholarly journals, a number of other journals were founded. [5] The first scientific periodical to be published in Germany, modelled after the *Transactions*, was the *Miscellanea Curiosa Medico-Physica,* in 1670. Like most other journals of the time, it was devoted to scientific papers, book reviews, communications and obituaries. Another German periodical entitled *Acta Eruditorum* was modelled after the *Journal des Sçavans*. Other important and influential periodicals included the *Raccolta d'opuscoli scientifici e fililogici* published from Venice from 1728 to 1757, *Gottingische Zeitung von Gelehrten Sachen* published from 1739 to 1752, and *Der Naturforscher* published at Halle from 1774 to 1804. [6]

At the close of the 17th century, there were about 30 scientific and medical journals. Thereafter there followed a century and a half of relatively slow growth. Between 1725 and the end of the 18th century, for example, 74 new journals were started. [7] However towards the middle of the 19th century journal starts began to accelerate so that by the end of the 19th century, 700 titles were listed. Since that time, the scholarly journal has grown steadily and exponentially [8] to a world wide body of periodical literature consisting of over 147,000 journals of which 25,000 are listed as scholarly periodicals. [9]

Emergence

Various factors are usually offered up as explanation for the emergence and subsequent proliferation of the scholarly journal. These include, obviously, the re-invention of the printing press, the emergence of an "enlightened" mode of scientific thought which required a more suitable organ for distributing empirical findings, the general growth of the scientific enterprise during the 18th and 19th centuries, the growth of a literate and informed market, and the growing need to create a scientific discourse that appeared objective and a-political. All of these factors intertwined to spur the growth of the scientific periodical as we know it today.

One of the preconditions for the emergence of the scholarly journal was the rapid and accelerating growth of the scientific enterprise. Not that scholarly activity has been unimportant during the Medieval era. [10] Indeed, there was a considerable intellectual activity (scholarly and otherwise) at all levels of society prior to the Enlightenment. This is reflected in the activity of the clergy, the growth of a literate public (which latter became an important support of the scholarly communication enterprise), and the growing demand for books and other materials in the centuries preceding the invention of printing. [11] However scholarly learning before the Enlightenment had primarily focused on retrieving and translating the classic works received from the Roman and Byzantine civilisations, [12] whereas after the emphasis shifted to acquiring new and "positive" knowledge.

This shift was key. Frances Bacon has been taken as the herald of the new intellectual order. In his utopian novel, *New Atlantis,* [13] he criticised the Scholastic emphasis on the retrieval of ancient learning and outlined a new program of empirical observation designed to move beyond what he saw as the limitations of ancient knowledge through the gradual and co-operative accretion of *new* and positive scientific knowledge. There was thus an intellectual shift away from philosophical thought and exegesis to a more empirical approach with greater emphasis on observation, description, and the accumulation of verifiable (or positive) knowledge.

This did not mean that controversies of the status of knowledge dissipated following the introduction of the journal, nor that the new empiricism supplanted all classic knowledge. The shift was more about the development of "positive" and "practical" knowledge, after Bacon, than it was about rejecting the entire ancient corpus. Indeed, in the first few years that the *Transactions* published, Henry Oldenburg found it continually necessary to defend the new modes of scholarly activity against charges that they ignored the old learning. It was not that they ignored the knowledge of the ancients, rather, the new scientific attitude emphasised that knowledge be verifiable and positive. If the ancient wisdom could be verified through observation, then surely it deserved to be incorporated into the new sciences.

> And here I earnestly implore … to bring into publick Light the Treasures of Libraries, before they be sacrificed to works and putrefaction; and to examine; and to examine Herodotus and Pliny, Theophrastus and Dioscorides…both with candour and equal integrity; to remark what is manifestly false, or with great reason to be suspected; to confirm what may be by Parallels be confirmed, & what may be thence discarded and what…may be adopted. [14]

As Harry Oldenburg pointed out, the scientists had respect for the church, respect for ancient patriarchs like "Noah, Moses, Solomon, Daniel and others." [15] However, they rejected uncritical Stochastic exegesis which led, in the

opinion of Oldenburg, to "Eternal Controversies…" [16] An interesting rhetorical battle waged in the first few years of the journal as the new "positive" knowledge slowly gained ascendancy over the old forms of learning.

> If Aristotle had been so happy, as to have enjoyed our optocks, and other Instruments of Arts, as such Engins as we now employ, He would have been quite of another spirit than these are; and would have acknowledged a greater variety and more curious contexture,….and would have confest the production of our Pyrotechnical Furnaces to excell all, that could be reasonably expected form his own fast Fiery Region. [17]

As this new enlightenment mode of knowledge acquisition grew, both before and after the birth of the journal, those holding to the belief that empiricism and positivism were the way forward began to see scholarly communication in terms of the *public* communication of short observations or experiments rather than fully worked out theoretical or philosophical treatises. [18] This shift was important to the birth of the journal since the old forms of scholarly communication were increasingly seen to be inadequate for the rapid communication of the fruits of science. Some scholars eventually concluded that effective and efficient communication required a new form of scholarly literature. [19]

What were the earlier methods of distributing scholarly knowledge that were no longer adequate? Prior to the advent of the journal, the primary methods for distributing scholarly communications were the learned letter, books, and gazettes. However in the context of the new enlightened approach to knowledge acquisition, these were seen to be inadequate vehicles. The book could no longer function adequately as a purveyor of scholarly research for two related reasons. One the one hand, the book was clearly not an efficient method of publishing the results of short experiments or observations simply because it took too long to accumulate enough material to justify a book length tome. Waiting the extra year or two that would be required for development of sufficient material for a book placed an unnecessary delay on the distribution of the early experimental material. The book also became inadequate because scholars who made contributions always sought priority for discoveries. Timing here was of course critical and as the pressure to publish quickly grew in the frenzy of scholarly development during the enlightenment, a new outlet was required that could move material rapidly into the public eye at the same time that it dated work closer to the actual discovery.

For a time, the scholarly letter functioned as the preferred means of communicating and establishing priority. Indeed, the letter was much closer in spirit to the new journals since it provided a medium for fairly rapid exchange of ideas and experimentation, was a vehicle for providing news concerning the activities of the scholars in distant parts of the world, and was a method of

ensuring priority through, albeit limited, distribution (sometimes in encoded form) of the work of scholars. However like the book, the letter also suffered a number of inefficiencies and deficits that were mostly related to the growing pressure to make the results of research publicly available in order to support further development of scientific knowledge. Scholarly letters were, after all, mostly private communications. And as such, they enjoyed limited distribution. According to Bacon science benefited no one, and was slowed in its progress, if scholars all over the world could not access the new knowledge. Letters were obviously limited in their capacity to make research public since they had only a limited field of distribution.

Initial solutions to this dilemma involved quick-fix additions to existing systems of communication. The learned societies of the time, for example, often solicited and collected the letters of scholars in order to facilitate their wider distribution. These collected communiqués were subsequently read before society meetings and archived for posterity. As the importance of scholarly communication grew, the academies and societies began to devote resources specifically for the purpose of soliciting, receiving, responding, reading, and archiving letters. They also designated a special membership category known as "correspondent." The society correspondent was responsible for collecting information on the progress of the sciences at home and abroad. They would then relay the information they collected back to the society where it would subsequently be distributed. [20]

As the volume of scientific material grew, these activities became a major burden on the resources of the learned societies. This led, by stages, to the development of labour saving devices and attempt to standardise the communication process. The printing press was solicited, shorthand systems were developed, and there was general a decline in the use of abbreviations and the creation of a simplified and standardised writing style. [21] Unfortunately, none of these solutions proved adequate to handling the steadily increasing volume of scientific material. So in the end, a new form of scholarly communication, the scholarly journal pioneered by the secretary of the Royal Society and Denis de Sallo, was needed. This new format for distributing scholarly work, the journal, "solved" the crisis of communication - at least for a time.

Taking the *Philosophical Transactions* as representative of the journals at the time, it is instructive to examine their early content. Not surprisingly, the new journals were devoted to a wide range of empirical sciences - including but not limited to navigation, botany, geography, and astronomy. Their role in the public communication of scholarly material is evident and the early volumes included contributions formally entitled *experiments* like "An Experimental History of Cold" and also *observations* which were just that, observations of some natural or biological phenomenon, and *letters* like the "Extract of a Letter, written from Paris, containing some Reflections on part of the precedent Roman Letter." In addition, and much like today's journals, there

were notices of new books, summaries of books, and book reviews.

Our discussion of the factors leading up to the introduction of the scholarly journal so far is summarised eloquently by Barnes: [22]

> The period from the invention of printing to the invention of the learned journal was a period which the Republic of Letters was handicapped by inadequate facilities of intellectual communication and publicity. In proportion to the increase in authorship and in the multiplication of books the need for communication and cohesion among savants expanded beyond the power of the learned letter, with its defects of privacy, loss of time and irregularity, to fulfill. This need of scholarship combined in the seventeenth century with the rise of the sciences and of intellectual curiosity among the upper classes to produce the learned journal.

Emergence and Subsequent Proliferation

Barnes, and others who have contributed to the above *traditionalist* account of the growth of the scholarly journal, were mostly correct in their assessment of the prevailing social and intellectual conditions which contributed to creating a niche requiring an innovative vehicle for distributing scholarly material. However there are political and social pieces missing from this idealised account. One particularly important factor missing from the picture is the role the middle classes played in the emergence of the scholarly journal.

The middle class, which emerged as a function of the growth of merchant and industrial capitalism, was a key factor in the growth of the scientific enterprise in general, and of the scholarly journal in particular. At one level, their role is simple - they provided the financial support for the scholarly journal. In the early years of the scholarly enterprise, the scientific press could simply not have survived without access to a market larger than could be provided by the small group of scholars and upper class patrons that Barnes suggests were responsible for the journal. Publishing was then, and is now, a capital intensive endeavour. Because of the high initial cost of publication, publishing houses depended upon a larger market in order to remain solvent. The middle classes provided this market.

What created the large middle class market? The standard causes can be enumerated - the industrial revolution and technology. As part of an expanding means of production, technology contributed in an unprecedented fashion to the economic advance of the European countries. The economic and technical expansion that occurred prior to the end of the 18th century acted indirectly on the scientific periodical by creating the conditions for the extension of the bourgeoisie and the creation of a leisured class anxious to distinguish

themselves from their peasant past. The new bourgeoisie had the time, the resources, and the motivation to devote to dabbling in scientific activity.

Indeed the pastime became quite popular in the 17th and 18th as the newly created middle classes became faithful devotees of scholarly activity. They organised royal societies and Masonic lodges for the express purpose of collecting and disseminating new knowledge, organised numerous lecture series for visiting scholars, attended society meetings, and generally poured forth their new found wealth into the scholarly enterprise. Thus by the time the journal first appeared on the scene in the latter part of the 17th century, a hungry public already existed that was capable of supporting a scholarly press that, lacking any kind of formal structure for institutional or governmental support, and lacking a captive audience (i.e., academic libraries), would have found it impossible to survive.

It was not just intellectual curiosity and social adjustment that drew the new middle classes to support the scholarly enterprise. Scientists themselves contributed to legitimising and increasing demand for their work through their efforts to convince the middle classes of the practical and social utility of the new scholarly endeavours. Scientists had been involved in creating a market for their work since at least the time of Galileo. [23] Galileo directed his arguments towards the aristocracy and the clergy, of course. But as the aristocracy was replaced with the bourgeoisie, scientists focused their arguments on the new emerging elite. The argument for science was multi-pronged. First of all, science provided practical knowledge. Science with its mechanical principles and its emphasis on the discovery of empirical laws of nature, could provided capitalists with an ongoing source of knowledge which could be used to good effect in industrial and colonial enterprises.

This feature of the scientific enterprise was formalised very early. As Margaret Jacob points out, "The propensity to link science with application, with trade and industry, was part of the ideology that created the Royal Society in 1662." [24] This ideology, i.e., the desire to link science with practical application and industry, is clearly evident in the early volumes of the *Transactions*. In the very first issue we find an article entitled "Of the New American Whale-Fishing about the Bermudas." This is an account of how to kill large whales (2 old-females and 3 Cubs in this article), how to extract oil, how much oil could be expected, and some qualities of that oil. [25] Clearly of practical import to the fishing industry. Similarly, the following "advertisement," is clearly directed towards those industrialists who would most benefit from the work of the scholar Sir Robert Boyle.

> Hearing of great complaints of the Rot of Sheep in many parts of England; we thought, it would not be unwelcome to the Reader, to be, on such an occasion, directed, for a good and cheap way of preventing the disease, to what the Honourable Robert Boyle hath publisht in this second Tome

of the Usefulness of Natural Philosophy, printed at Oxford A.1671. p. 15. [26]

The strongest statement on the coincidence of industry and science came from the editor himself, Henry Oldenburg, in his "Dedicatory" to the "Right Honorable Sir Joseph Williamson Knight, principle Secretary of State." This thinly disguised appeal for ongoing royal support is a veritable outpouring of the contributions of natural philosophy to English trade, the fishery and navigation. Oldenburg recounts how the natural philosophers have allowed the fertilisation of barren lands, the cultivating of waste grounds, the draining of bogs and fens, the increasing of the linen trade, and "the reducing … [of the] numbers of idle persons and vagrants to work for their relief and public good." (ii). This last comment clearly puts the scholars in league with the emerging bourgeoisie

All of this was essentially an appeal for continuance, fatuous by today's standards, of Royal support. While feigning that this was not his intention, Oldenburg reminds Joseph Night of the contents of the Royal Charter, suggesting that in the end he will make the appropriate inferences from the letter. "I also scruple not to sollicite continuance of your favour to advance the Designs of the Royal Society, according to his Majesties Intentions expressed in the Royal Charter…..tis unnecessary, I should say more to you, either for the one or the other, for the more Sublime, or for the more Useful Arts. Your own Genius, without any impressment, will make Inferences leading to further advantages. Mean while, I am full of hopes…" (iii) (all this from Transact, 9: I-iv, 1674, "The Epistle Dedicatory."

Another factor proselytised by the founding parents of the new sciences, and implicated in the willingness of the middle classes to support the enterprise, was science's ability to perform ideological and hegemonic tasks. Here it played a number of roles. As legitimiser of the bourgeoisie social position it dethroned the aristocracy and the church through new models of nature (i.e., heliocentricity). [27] it justified, then as now, inequality and rank colonialist exploitation and enslavement in the colonies. [28] And, most importantly it could perform a hegemonic function through education. As Thomas Campanella said of scholarly inquiry, "people's minds will be diverted from creating … any trouble, and will be incited to bend their studies that way which may be useful to the king." [29] By the 18th century, this latter "social utility" argument had become "the most important justification for the promotion of scientific inquiry." [30]

It was these conditions, the growth of the middle classes, the growing demand for ways to create profit, and the need to justify the status quo, that came together to create a demand for not only the works of science, but for those works presented in the form of the scholarly journal. The scholarly journal quickly came to be a rather unique vehicle for transmitting scientific information. On the one hand, it met the needs of the industrialists and

scientists for rapid distribution of the latest in scholarly activity. On the other, it provided an almost perfect hegemonic vehicle for scientific evidence that supported the status quo. We can understand the hegemonic function of the journal by considering intellectual changes that occurred during the enlightenment. The key change was the *masculinisation* of thought initiated by Descartes in his M*editations,* and brilliantly described by Susan Bordo

> Here "masculine" describes not a biological category but a cognitive style, an epistemological stance. Its key term is *detachment:* from the emotional life, from the particularities of time and place, from personal quirks, prejudices, and interests, and most centrally, from the object itself. The masculine orientation toward knowledge … epitomized in the modern scientific ideal of objectivity, depends on a clear and distinct determination of the boundaries between self and world…[31]

This masculinisation became formalised into the widely accepted canons of neutrality, objectivity, and the strict separation of the subject from the object. The scholarly journal was a formal expression of this new mode of approaching reality. In its very structure, the journal *expressed* this mode of thought. Short, pithy, to the point articles demanded that scholars present only "the facts." There was little room for metaphysical complexities or elaborations, emotional appeals or politicking. The journal became the sin qua non of this and, some would argue, alienated and alienating form of thought and communication.

Of course, this was all just a myth, widely accepted even to this day, but never standing up to the stream of criticism levelled at the pretensions to objectivity and neutrality. But this was of little concern. As Gross has pointed out, the journal and the scholarly article contained therein became a profoundly rhetorical device. Journals supported, by their pretensions to neutrality and objectivity, the social and political claims of the capitalist enterprise. Scientists and those who used scholarly research could easily justify their political and social beliefs vis a vis, for example, gross violations of human rights and dignity by reference to the purported objectivity, and utility, of the findings presented in scholarly journals.

There were other effects, beneficial from the perspective of those who profited most from the status quo, but profoundly alienating for those who suffered. The canonisation of the ideals of neutrality and objectivity, and their ridged ossification in the structure of the scholarly article, created a new discourse capable of not only supporting scientific claims, but also of marginalising the voices of those impacted by racist, sexist, and classist discourse. The new discourse became a closed universe, accessible only to the credentialled, which placed strict limits on expansion of critical discourse.

> The formation of the scientific *text* as a new, standardized cultural genre, [replaced] the more open, varied, metaphorically porous, literary forms of science….and the possibilities of multivalent meanings being created out of scientific language were thereby curtailed. [32]

With the creation of this new discourse, the political functions of science became obscured. It was a double thrust of the pitard. Science was used in the service of the new ruling classes. In order to protect the political discourse of science, the new language of science cloaked itself in claims to objectivity and neutral supported by the emerging structure of scholarly communication. Always profoundly rhetorical and always capable of being used in the service of the status quo, science came to have the appearance of value-neutrality. It is no coincidence that one of the first scholarly journals, *The Philosophical Transactions,* was initiated by an organisation who, from the very first, wished to detach experimental science from its association with radical or reactionary social and political critique. [33] "Just the facts" became the rallying cry of those wishing to silence all who had been run over and mangled by the steam engine of capitalist and colonialist expansion. Much like today the phrase "you are just being politically correct" is used to strangle feminist, anti-racist, or anti-classist discourse.

This should come as no real surprise. As Kachur writes "The members of the intelligentsia share a common interest; they depend on the direct producers of material goods and they need to sustain their power in a gift economy of symbolic sharing." [34] In other words, our class position has always been defined by a fundamental contradiction. Although we seek intellectual independence in the name of "truth," we remain firmly fixed in a specific relationship to the ruling classes in society. The survival of our enterprise is, in fundamental ways, linked to the survival of whomever holds the purse strings.

And so the survival and unprecedented success of the scholarly journal was secured. With the help of the literate middle classes, the production of scholarly journals exploded into a pattern of exponential growth that continues even to this day. It was so successful, in fact, that the pattern of exponential growth of scholarly material which emerged after the birth of the journal almost immediately caused problems. The proliferation of scholarly literature facilitated not only the fragmentation of the scholarly disciplines, but also contributed to the emergence of a secondary literature (in the form of abstract, index, and review journals) designed to help ease the task of access, assimilation, and control of the literature. [35] These abstracting journals began to emerge in the first decades of the 18th century. Since that time they to have also expanded at exponential rates [36] and have in there own turn contributed to what scholars and librarians have for decades been calling a crisis in the scholarly communication system.

Extant Communication Systems

Formal Communication and its Functions

At this point it will be useful to turn our attention to the scholarly communication system as it exists today. We begin our examination of the extant communication system of science with a look at the formal system of communication. This pen and paper based communication system includes the publication of research in journals, individual research reports, monographs, and other similar forums that cater to the initial *public* dissemination of research results. [37] Public here is the key word because word of research activity and preliminary results are often circulated in the informal communication system far in advance of its formal publication. However despite the early availability of research and results in the informal realm, distribution of these early results is limited to a select group of researchers. Hence the formal system remains extremely important to non active researchers, advanced undergraduates, beginning graduate students, people outside of specific disciplines seeking to gain insight or develop a rounded intellectual purview of the world, and indeed, anyone else not directly plugged into the informal communication system.

Within formal communication channels, authors [38] distinguish three separate types of communication channels; these are the *primary, secondary,* and *tertiary* distribution systems. Primary distribution channels include periodicals devoted to the publication of original research (primary journals), research reports, patents, official society publications, the publications of standards bodies, published theses and dissertations, diaries, memoranda, the minutes of meetings, and internal research reports. Secondary publications emerged as attempts to control the expanding primary literature and they, along with the tertiary communication system, have grown in importance as the scientific and technical literature has exploded. [39] Because the secondary system is designed primarily as a means of gaining bibliographic control of the primary literature, it consists of guides to primary research and include such bibliographic items as abstracting, [40] summary and review [41] journals, manual and electronic [42] indexing and abstracting services, references like encyclopedias, dictionaries, collections of tables and formulas, and handbooks. [43] Secondary sources can also include general and multidisciplinary periodicals meant for wider distribution as well as technical journals devoted to the specific interests of industry and whose primary function is to reformulate the primary literature in a form easily assimilated by the managers and practitioners in industry and to supply information on the state of the industry and important technological advances.

Finally there are tertiary sources. These attempt to organise both primary and secondary sources of material at the same time that they provide value added

services like identifying and locating titles in specific subject areas, or guiding researchers to speciality libraries. The most famous of the tertiary publication is *Ulrich's International Periodicals Directory.* This publication, founded in 1932, lists all regularly appearing journals throughout the world and in addition to its comprehensive listing also provides subject, cost, distribution, and contact data for each of its titles.

Here we are mostly concerned with the primary scholarly journal and its traditional functions. Primary journals perform a number of vital roles for the academy. At the most formal level, they have been the essential tools for disseminating and archiving **original** theoretical and empirical contributions. Historically they have also had a vital role to play in assisting scholars to **remain current** within their field. However the early proliferation of primary literature quickly outstripped any single individuals ability to remain current by relying on the primary literature. This restricted the primary journals ability to fulfil its original current awareness function. As noted, this difficulty prompted the development of primary and secondary sources and the eventual enlistment of the computer as an aid to scholarly research. As a result, the current awareness function of the primary journal is now widely distributed among primary, secondary, and tertiary services.

Primary journals also provide a number of additional functions besides their **formal role** in the distribution and storage of the scientific record. Probably the most important is their *gate keeping* function. According to some versions of the scientific communication process, papers in these journals are the end result of a long [44] process of evaluation and pruning that ensures that only the best papers make it to the top of the academic heap for public presentation in primary journals. Here the link with informal communication is made explicit in terms of a continuum of peer evaluation from the informal hallway exchanges which depend as much on body language and facial expression as they do on verbal exchange, to the final publication of the research result in the peer reviewed journal.

The most important aspect of the journals gate keeping is performed in the formal realm by *peer reviewers* who, by virtue of being experts in a given discipline or subdiscipline, are said to be able to objectively assess the contribution to knowledge, theoretical sophistication and empirical and methodological validity of the research work. In general, peer reviewers assess the extent to which the paper is a worthwhile contribution to the scholarly enterprise. Garvey [45] provides a succinct statement of the function of the peer reviewer: "...without rigorous scrutiny by qualified scientists a great deal of such information would be unreliable (both in terms of its replicability and relevance to science) and the foundations of scientific knowledge would become enfeebled by 'unscientific' information. Garvey further points out: [46]

> The scientific article is, and will remain for sometime, vital to the scientific community. It is the basic unit of the scientific

> journal process which provides a system for formal, public, and orderly communication among scientists. Journals are formal in the sense that article manuscripts have been reviewed, revised to near perfection, and then allowed to pass into the formal domain where they may be explicitly cited and unambiguously retrieved. They are public both in the sense that anyone can submit a manuscript for publication in them and that they are available to anyone in libraries or by subscription. The orderliness of journals is founded on their articles being selected on the basis of scientific merit, which means that (a) the research reported is flawlessly conducted and (b) its results are relevant to scientific progress in the sense that they have explicit continuity with previous work and foreshow the future course of work on the research front.

Primary journals also perform a number of social functions that have become extremely important in the highly competitive academy. Journals, for example, provide valuable (and rare) publication outlets for scholars who need to contribute regular publications in order to advance and make tenure in the academy. They also formalise and record scientific contributions for the purpose of assigning priority to discoveries and, finally, are key sources for the evaluation of a scholar's life chances (i.e., job advancement). This aspect of the primary journal has, as the competitiveness of the academy has increased, come (some would say unfortunately) to be the journals most important function. Robert K. Merton [47] has noted the extreme importance of priority and originality in the academy. He has also noted that failure to recognise the structural and institutional variables which encourage this undue emphasis on discovery and priority have led to individual attempts to adapt to the demands. This has in turn led, in many more instances than scholars would like to admit, to pathologies and questionable behaviours. Merton [48] has this to say:

> The culture of science is, in this measure, pathogenic. It can lead scientists to develop an extreme concern with recognition which is in turn the validation by peers of the worth of their work. Contentiousness, self assertive claims, secretiveness lest one be forestalled, reporting only the data that support an [sic] hypothesis, false charges of plagiarism, even the occasional theft of ideas and, in rare cases, the fabrication of data, - all these have appeared in the history of science and can be thought of as deviant behaviour in response to a discrepancy between the enormous emphasis in the culture of science upon original discovery and the actual difficulty many scientists experience in making an original discovery. In this situation of stress, all manner of adaptive behaviors are called into play, some of these being far beyond the mores of science.

Disciplinary Differences in Formal Distribution System

All formal scholarly literatures share the above basic functions. However despite the broad similarity in the communication systems of the sciences, a number of substantial differences exist between specific disciplines. At a purely quantitative level the size of the scholarly literature in various disciplines is quite different. As Table 1 indicates, some disciplines have the dubious distinction of being able to offer thousands of journals to the world.

Table 1: Serials Universe for Selected Disciplines.

	Scholarly Serials	Total Serials
Medical Sciences	3,851	13,657
Biology	2,120	6,208
History	1,659	6,997
Engineering	900	6,150
Psychology	796	2,024
Political Science	744	6,979
Physics	621	1,896
Sociology	460	1,804
Anthropology	303	486
Women's Studies	89	242

Source: Ulrich's 1995 Periodical Directory

Another difference worthy of note is the differential rejection rates of primary journals. Journals in the sciences have the lowest rejection rates ranging anywhere from 20 to 40 percent. By contrast, journals in the humanities have the highest rejection rates with journals in some disciplines, i.e. history, approaching a 90 percent manuscript rejection rate. [49] These are followed in turn by journals in the social and behavioural sciences (with rates around 80 percent), and finally by journals in the physical, chemical and biological sciences. Physics in particular has a low rejection rate (24 percent). Some commentators [50] have attributed these differences to the ontological status and epistemological security of specific disciplines by noting that there is more agreement in the natural sciences about what constitutes publishable material. [51] By contrast, as we move away from hard sciences toward the more humanistically orientated sciences, epistemological criteria become less institutionalised, less evolved, and more ambiguous. Lowell L. Hargens [52] notes: "... the greatly different prospects scholars face when submitting manuscripts to, for example, the *Physical Review* and the *American Sociological Review,* result from structural differences between the scholarly communities to which they belong."

Besides the differences in the extent of the literature and the rejection rates of

primary journals, there are also substantial differences in the patterns of information communication and use. Noteworthy are findings that indicate that the disciplines differ in their reliance on the primary journal literature. Studies have indicated that the primary journal literature is more important in the natural sciences than in the social sciences [53] and that scholarly output in the "wet" sciences is heavily waited towards other modes of primary distribution. For example, Anton J. Nederhof [54] noted that in a Norwegian psychology department, only 40% of departmental output was in the form of journal articles. The rest was made up of book chapters (27%), edited books (2%), monographs (1%), research reports (12%), and contributions to proceedings (13%). The same lack of emphasis on the primary literature is also evident in sociology. A citation analysis by Maurice B. Line [55] revealed that sociology journal articles cite other journal articles only 33% of the time.

Finally, there are differences in the speed at which scholarly writing is communicated. William D. Garvey, Nan Lin, and Carnot E. Nelson [56] have made much of what they see as the inefficiencies of the formal and informal communication system in the social sciences and the fact that it takes material in the social sciences about 4 months longer to reach the journal publication stage than material in the physical sciences. [57] Their interpretation of the differences in information flow is evident below:

> ...the communication systems associated with the physical sciences, the social sciences, and the engineering sciences differed markedly with respect to the operation and use of these elements. Of these groups, scientific communication in the social sciences appeared to be in an early stage of development. The elements of the social sciences' communication structure were relatively noncohesive; the flow of scientific information through the communication system followed less predictable sequences; and the processing of information for the archives seemed less efficient. This processing of information was more time-consuming in the sense that a considerably longer time period elapsed between initiation of work and its presentation at a meeting or its publication in a journal. It was more haphazard in the sense that the evolvement of information did not follow in an orderly manner from small restricted audiences to large public audiences; and it was more diffuse in the sense that information disseminated in a few days at a single meeting subsequently became published, after some considerable delay, in relatively large numbers of different journals.

There could, of course, be many reasons for the observed differences in information distribution patterns that have nothing to do with the purported inefficiency, epistemological fragility, or immaturity of the scholarly

communication system in the wet sciences. Take the example of differential journal rejection rates. Harriet Zuckerman and Robert K. Merton [58] offered one alternative explanation when they noted that "Journals in the sciences can apparently publish a higher proportion of manuscripts submitted to them because the available space is greater than that found in the humanities. Take the case of physics. The article in journals of physics are ordinarily short, typically running to only a few pages of print, so that the 'cost' of deciding to publish a particular article is small, and the direct costs of publication are often paid by the authors from research grants."

Other differences besides those derived from the putative hierarchy of the sciences can also play a role in differential rejections rates. For example, the norms of publication between the different disciplines can be quite different. In the sciences, for example, publication norms presuppose that submitted papers should be published whereas in the social sciences the assumption is just the opposite. Stephen Cole, Jonathan R. Cole and Gary Simon [59] suggest that editors and referees in the sciences prefer to make *Type I* errors (i.e., accepting unimportant manuscripts) whereas social sciences prefer to make *Type II* errors (i.e. rejecting potentially significant publications). These different norms, coupled with the greater space in science journals, can have a significant impact on the rejection rates of journals in the various disciplines. Cole, Cole and Simon provide a supportive example. [60]

> There are much qualitative data to support the publication-norm hypothesis. For example, the most important sociology journal in Poland, *Studia Sociologiczne,* published by the Polish Academy of Sciences...accepts a majority of papers submitted. In Poland, sociologists do not subscribe to the norm that articles should be rejected unless they are significant contributions.

The different norms can also have a significant impact on the quality of the literature. As Stephen Cole et al note, in the hard sciences there is an informal policy that encourages editors to publish articles *just in case* they turn out to be significant. This, according to the authors, [61] "often leads to the publication of trivial articles with little or no theoretical significance, a reason frequently cited by referees in social science fields in rejecting articles." Indeed, even a superficial glance at the differences of an article in the journal *Science,* and one in say, *Work, Employment, and Society,* reveals significant differences in the norms of publication. What is considered an adequate contribution in the former journal would not even be considered publishable as a research note in the latter.

Stephen Cole et al [62] also note that differences in the **diffuseness** of the journal system in the different disciplines, and differences in **graduate training** can also contribute to different rejection rates. On the former the authors note that in physics over 50 percent of all articles are published in only

two journals whereas in sociology the two leading journals publish only a fraction of the literature. On the latter, the authors note that in the harder sciences there is a more tightly articulated apprentice system. "Social science disciplines are less efficient in teaching students how to write publishable articles. Students are more likely to select their own problems, work independently of their sponsors and have relatively little guidance when preparing their first articles. As a result of these differences in training, scholars in the social sciences may be more likely to submit inadequate papers than scientists in the natural sciences." [63]

The differences in delay noted above can also be explained by different publication norms. The expectations about article length, for example, are quite different in the natural and social sciences. Articles in many social science and humanities journals are long and theoretically sophisticated running upwards of 10,000 words. By contrast, articles in science journals may be as short as 1,000 words. Obviously it takes more time to compose a 10,000 word piece that it does to toss together a 1,000 blurb on current research findings. Even commentaries on articles in social science journals are longer than many articles published in science journals. This difference in length alone could easily contribute to the 4 month differences noted by William D. Garvey, Nan Lin and Kazuo Tomita. [64]

Informal Communication

The opposite of the formal communication system of science is the informal system. The informal (or non-documentary and paperless) communication system of science consists of various elements that form a continuum that moves from the most informal oral exchanges through to the almost publishable preprint or conference presentations of scholars. On the one side of the continuum are the various oral exchanges that comprise the initial birth of an empirical or theoretical program. Here we have the informal class room debate, lunch table conversation, hallway meeting, or phone or email discussions with students and colleagues. Also on this side of the continuum are the various informal departmental meetings where faculty learn about the current work of their colleagues. Generally, this early stage of the communication process is taken up with the initial working out of theoretical or empirical research project. Potential authors may use these early opportunities for exchange as a test bed for new ideas by eliciting informal criticism and by being wary of the sometimes subtle queues that indicate to a scholarly whether an idea is worth pursuing. It is in this formative stage that the idea or project is most amenable to modification or outright destruction (i.e., it was a dumb idea) based on the input of commentators at all levels.

Somewhere in the middle of the informal continuum is the club meeting, colloquium, and regional, national, and international scholarly conference. It is somewhat of an analytical *faux pas* to toss these various informal settings into

the same pile since there is a huge difference between an out of classroom work group on a special topic, and an international conference. Yet they are similar to the extent that they all bring together practitioners for the express purpose of learning about and discussing work already in progress. These informal media serve a number of functions not the least of which is to inform a larger audience of the author's current work in the field. Such meetings can also be useful for the formation of post-meeting informal exchange networks of people working in the same field. [65]

William D. Garvey [66] has suggested that as we move along the informal communication continuum towards formal publication in a scholarly journal, the audience for the scholar's work gradually expands. This is almost certainly true since at the very early stages of the game scholars will tend to limit the distribution of their ideas for fear of disseminating poorly conceived material. However in the middle stage authors will presumably have had enough time to construct a presentable project. At this point the work will become interesting to others working in the same subject field and they will meet at conferences, colloquia, or local meetings in order to keep abreast of current work. Note that despite the fact that the author is here beginning the formal distribution of her work, the communiqués are still exploratory both for authors and those interested in the authors work.

At the far right of the informal continuum we have the various forms of written reports (i.e. technical reports, thesis or dissertations, in-house publications, preprints, and publication of proceedings) which function to pre-distribute work for the purposes of raising the general level of awareness of a forthcoming journal article or getting the work into the mainstream of scholarly communication to allow for early use and citation in other projects. These early publication efforts can also provide a preliminary draft of a more formal article for submission to a journal the purpose of which is to elicit a final informal peer commentary. [67] Prepublication reports can also function as the final publication outlet in those cases were the only requirement is to provide terminal reports to funding agencies or to the members of large research teams and the institutions within which the author of the report is employed.

Interestingly, these informal written reports can often be more substantive than the corresponding journal articles because as Garvey [68] notes, the author can "...describe his negative as well as positive results. He can include the vast amounts of data which would make his journal article too long to publish .... in the prepublication report the scientist may tell more of his personal experiences and interpretations of his work; in the journal these traditionally must be attenuated." Thus it is not uncommon to find the addition of appendices, figures, photographs, and even large representations of data in these preliminary reports. For this reason, these prepublication reports are often more useful to scholars working in the area than are the papers published in the formal journals.

Though there are differences between different aspects of the informal communication process each of the various aspects of the system share common characteristics and functions that outweigh the differences. In the first place, there is a tentative and ephemeral quality to most informal communication. "In some instances the information exchanges seem like a series of soundings - putting forth information, testing reactions to it, then withdrawing the feeler and modifying the information for a later probe". [69] Also according to Garvey, their is considerable duplication of results in the informal realm where the goal is not final publication and archival but rather the forging and tempering of scholarly ideas or pre-distribution of material for the sake of the general edification of the field. There is also an interdisciplinary element to informal communication networks that is lost when the final product of the research reaches the formal journal in its sanitised and jargonised version. [70] The strict formalisation and rigid terminology of the formal realm is watered down in the informal communications which generally tend to be accessible to a larger audience.

Another characteristic of informal communication channels are their interactivity and open-endedness. On the one hand, scholars are much more willing to speculate about their ideas and discuss past theoretical and empirical mistakes when moving in the informal realm. After all, a draft paper is a draft paper. We commit hardly anything of our reputations when distributing unfinished work. It is also during the informal communication process that the scholars interact most vigorously. Unlike publication in journals, communication in the informal realm is a give and take of scholarly insight, information, and advice. Such give and take can be obvious (you scratch my back, I'll give you my recent preprint) or quite subtle as in the almost unconscious clues about interests and personal biases that scholars give to other scholars through the content and form of their questions, or the off handed comments (or lack thereof) that scholars receive.

Finally, because of the characteristics of formal communication system, i.e., long publication delays, all articles found in journals amount to "old work" for anyone past the undergraduate stage of scholarship. Thus the informal communication system serves a vital function by keeping active researchers current in their respective fields. [71] This is, ironically, a shift in function since the early days of the scholarly communication system where the formal system was capable of keeping scholars informed. As we said however, the strain on early primary journals quickly prompted the development of secondary services designed to maintain the current awareness function in the formal realm. However, after a time even secondary and tertiary channels overloaded and became incapable of fulfilling their bibliographic control functions. Because of the structural limitations imposed on formal scholarly communication, and the rapidly increasing rate of scientific discovery, the formal literature has by and large lost the ability to provide a current awareness function. This function has partially moved, for better or worse, into the informal realm.

It would be a mistake to discount the informal communication as relatively unimportant, or as nothing more than an appendage to the *real* system of scientific communication. By some estimates the informal communication system accounts for as much as four fifths of all scientific communication. The reason for this high figure is simple. It is in the informal realm that ideas are worked out, experiments designed and refined, and colleagues hammer at each other's work. The key importance of the informal communication system was recognised over 30 years ago by Herbert Menzel [72] who noted some of the functions of the medium.

> For example, there is a certain level of know-how information about the use and setting up of scientific apparatus that seems to go by preference through word-of-mouth channels, perhaps because this kind of information is regarded as unworthy of being handled in detail in the printed word....Information that helps interpret results and information that helps a person become acquainted with a new field also seem to make their way differentially, often through personal channels.

Aside from the fact that the informal communication represents the vast majority of scholarly activity, it is important for the fact that within the informal communication networks we find what Price [73] has called the i*nvisible college.* Invisible colleges are really nothing more than a group of people who interact with each other on a regular basis either through letter, phone, email, or (less commonly) attendance at select meetings and retreats. But in the highly patriarchal and almost feudal system that is the academy, these colleges can wield a considerable amount of power over the life chances and careers of even those who are not members. For example, informal recruitment networks exist among colleges and universities [74] and every graduate student knows that being able to access these informal recruitment networks, by for example careful selection of thesis advisors, can offer significant career advantages. The benefit may be bestowed through a phone call (or perhaps an email message) to a department advocating a particular candidate over another or through the provision of key job information. Despite the simplicity of operation, the results are tangible and significant.

Informal networks also offer advantages to their members by providing more efficient access to key academic information. Informal research networks are essential for academics in that they provide easy to obtain information on the most current, important, and fruitful areas of activity in a given academic field. This latter aspect of the informal communication system is a particularly salient consideration for young scholars who may find their attempts to access invisible colleges preempted by the inherent elitism and conservatism of these institutions. It is also important given the potentially high cost of maintaining contact with invisible college structures. In the elite colleges, contact can be made to be expensive. Regular face to face meetings at various institutions

which provide 'summer' seminaries can be used to keep members of the college in touch and informed. [75] Obviously, individuals, organisations, or even countries without the requisite financial resources are severely restricted in their ability to remain at the cutting edge. Ziman [76] illustrates the consequences of this rather poignantly.

> Not to be able to attend the international conferences in one's subject, not to be able to meet one's scientific contemporaries around the world, is to be condemned to isolation, to provincialism, and eventually to the frustration of all one's efforts to keep up with the moving frontiers of research. This is the plight of so many scientists in developing countries.

Further evidence of the importance of informal communication and the associated invisible college structure is provided by considering Merton's [77] analysis of what he has called *institutionalised serendipity*. He analyses the career and intellectual paths leading up to the publication of the seminal work, *The Structure of Scientific Revolutions* by Thomas Kuhn in 1962 and finds the intellectual development and stunning career advances of the young physicist inextricably bound up in his access to key informal, and private, networks like the *Harvard Society of Fellows*. As Merton notes, early access to these networks contributed not only intellectual resources, but also helped the young Kuhn get access to, through the evaluations of eminent scientists whom he came in contact with, such prestigious awards as the Guggenheim Fellowship, and also such elite research institutions as *The Center for Advanced Study in the Behavioral Sciences.* Merton concludes that Kuhn's unique perspective on the history of science depended in no small measure on his access to the resource rich institutions through which he passed by virtue of his access to key informal networks. Merton [78] puts a class spin on this in the following quote that introduces his famous concept of the *Mathew Effect.*

> The systems of reward, allocation of resources, and social selection thus operate to create and maintain a class structure in science by providing a stratified distribution of chances among scientists for enlarging their role as investigators. Differentially accumulating advantages work in such a way that, in the words of Mathew, Mark, and Luke, unto every one that hath shall be given, and he shall have abundance: but from him that hath not shall be taken away even that which he hath.

Margaret W. Rossiter [79] has recently criticised Merton's failure to recognise the gender dimensions of institutionalised serendipity. She notes that the contributions of women are often systematically ignored and down played. Women's contributions to science are also forgotten more quickly. They are also less likely to be awarded prizes for their contributions, especially when

they have worked with a male colleague or a married partner. Rossiter [80] provides the following example.

> But perhaps the most notorious theft of Nobel credit is the case of Lise Meitner, who worked for decades with Otto Hahn in Germany and who, in 1939, realized that what they had done but could not explain was in fact nuclear fusion. She must have been stunned to learn in 1944 that he alone had been awarded the Nobel Price for one of the biggest collaborative discoveries of the century.

Rossiter suggests that the Mathew Effect should be renamed or complemented with a similar concept that reflects the unfair treatment of women of science. She suggests calling the new concept the *Matilda Effect* after the American scholar Matilda Joslyn Gage who has largely been written out of the history of science. As Rossiter tells us, Gage was an early sociologist of knowledge who recognised quite clearly the sexist dynamic and the tendency that "the more woman worked the more the men around her profited and the less credit she got." [81]

Realist Myths

As we can see, the informal communication system of science is important. It is worthwhile remembering this since the fact provides a useful context within which to view the contributions of the primary communication system. There is also a useful antidote here to the traditionalist view of the scholarly communication system which privileges the formal realm over and above the informal realm. Clearly discounting the informal system of science as a mere breeding ground of ideas contributes to a much narrower understanding of the scholarly communication system. The funnelled perception can lead in some cases to a misrepresentation of the importance and function of the informal system and to, most importantly, a theoretical gloss that misses a substantial section of analysis.

The gloss over the informal communication system has allowed the importation of a realist and empiricist mythology which in turn has contributed to an idealised description of the scholarly communication system. The idealised version essentially assumes that the primary (and only) function of the scholarly communication system is to provide a ground for the birthing and maturation of ideas. The assumption has been that the "concepts" that reference the natural world are born, pass through various stages where they are increasingly *purified* of extraneous bias or misrepresentation, to finally appear in pure form in the research paper. This substrata assumption is represented perfectly in the view that treats the scholarly communication system as a continuum were ideas are progressively purified in the analytical fires that burn in the informal and formal realm.

This rather prevalent conception of the function of the informal system has been increasingly challenged in recent years by the work of scholars in the SSK tradition who have recast the traditional conception of scientific inquiry and the traditional understanding of the informal communication process. SSKers have made a number of contributions. For example, they have convincingly demonstrated the powerful rhetorical nature of the formal communication system. [82] And, they have forced open a space for an examination of how empirical reality is constructed in the informal communication system. Early work by M. J. Mulkay, G. N. Gilbert, and S. Woolgar [83] and also H. M. Collins [84] has emphasised the importance of informal networks, tacit (craft) knowledge, and rhetorical communities for the construction or destruction of empirical reality. [85] H. M. Collins [86] has a methodological prescription for studying this hitherto dark corner of scientific inquiry.

> It is actually possible to locate this process in scientific laboratories, in letters, conferences and conversations. It is possible to perform a kind of automatic phenomenological bracketing for ideas and facts, by looking at them while they are being formed, before they have become 'set' as part of anyone's natural (scientific) world. In short, the contemporaneous study of contemporary scientific developments, I suggest can provide an entry to a sociology of knowledge which is less subject than usual to some of the philosophical and methodological problems. It should be expected that this will generate a picture of science in which the figurative 'ships' are still being built by human actors, to be subsequently erected in their bottles by a trick invented and worked by human actors also - a picture which is much more relativistic.

In the work of the SSKers, the informal communication system is recast as the location where scientific facts are constructed. The scientific communication system is no longer a continuum were a semi-valid empirical concept moves through fires of purification to emerge as a finely crafted reference to the natural or social world in the primary literature. Facts are not operated on. Instead, facts are constructed by the gradual emergence of consensus about *what counts* as a valid fact (through emerging agreement about the qualities of inscription, about how to achieve acceptable inscription or operationalisation, etc.). In this process, the goal is to convince others that what one is holding is a valid little bit of knowledge about the world. Questions about reference, while important, are secondary.

Key resources in this rhetorical battle include the material (lab equipment), institutional (prestige, funding), and persuasive (past encyclopaedic reading or the publishing record) capital which are brought to bear on the construction of fact. [87] And we shouldn't forget that it is a *rhetorical* battle. There are extremely

ridged standards of communication that must be met if a "fact" is going to be able to "speak for itself" in a primary journal The conditions are singular and simple and extend directly from the Cartesian separation of subject and object and the gradual idolisation of the "objective" observer. No trace of the investigator should remain in the final communication. This includes any political overtones that institutional affiliation might bring, and any class or economic background. In addition, any emotional overtone that might suggest that the communicator lacks objectivity, and any personal anecdotes or subjective interpretations must be rid of.

This is, in the final analysis, the essential role of the primary journal. The communication and gate keeping functions of the primary literature come together to provide a restricted outlet for only those pieces of research that meet the ridged criteria of rhetorical effectiveness. Whether or not the sanitised and purified reference to the "real" world is an adequate reference, and whether or not the standards of judgement are themselves sufficiently free of embedded class, gender, or racial overtones, are questions only examined "up to a point." Indeed, some have suggested that ideas are more likely to be rejected for their overly rhetorical content (or rather their overly obvious rhetorical content) than for their empirical correctness.

Ian Mitroff [88] in his book, *The Subjective Side of Science,* provides a fascinating look into the psychological and emotional nature of science. Unlike those who would hold to the myth that scientists are giants who dig objectively through the muck of the natural world to discover pearls of truth, Mitroff finds that we are only human and sees positive benefit in recognising the emotional and psychological side of science. [89]

> Psychological energy and commitment infuses the whole process to such a degree that it is foolish to say that scientific inquiry naturally exhibits a clear-cut dividing line, between individual scientists or between the contexts of discovery and justification ... To remove commitment and even bias from scientific inquiry may be to remove one of the strongest sustaining forces for the discovery of scientific ideas and for their subsequent testing.

It is this subjective side of science that is purified out in the formal realm. The final product in a primary journal is certainly an idea. But it is an idea constructed in the rhetorical fantasy world of pure science and then purified of all its sociological. political, and psychological content. We have seen how it is these features of the formal communication system which many who support the status quo find attractive. It is the strong rhetorical devices, strong by because its machinations are obscure, hidden, and vociferously denied and because access to the key rhetorical devices are costly. This has made science into a powerful weapon of gender, race, and classist oppression that it is. Readers, especially those who would challenge scholarly cannon, are

isolated by virtue of their inability to access the primary rhetorical devices of the scholarly article. "When reading [the scholarly article] … it is on the contrary the reader who becomes *isolated.*" [90]

The result that scholars see in the primary literature is an empirical fact that is no longer embedded in the social and political world of the scholar. Bruno Latour and Steve Woolgar explain: [91]

> The result of the *construction* of a fact is that it appears unconstructed by anyone; the result of rhetorical *persuasion* in the agnostic field is that participants are convinced that they have not been convinced; the results of *materialisation* is that people can swear that material considerations are only minor components of the "thought process"; the results of the investments of credibility, it that participants can claim that economics and beliefs are in no way related to the solidity of science; as to the *circumstances*, they simply vanish from accounts, being better left to political analysis than to an appreciation of the hard and solid world of facts!

This has obvious implications for how we view the scholarly journal and its future. Casting aside the idealised version of the scholarly communication system allows us space to insert critical sociological analysis into our analysis of the communication system and especially our proscriptions for the future of scholarly communication as it moves into the electronic realm. We find ourselves in a unique historical position - a nexus. There are two roads to follow here. On the one hand, we can accept the realist myths about the importance of the formal communication system, and its position at the top of a purifying process whereby references to reality are increasingly refined. This leads us is a certain direction when we consider the future of electronic communication. It is a direction that leads straight back into darkness with all our preconceived notions of the scholarly communication intact and, subsequently, leaves untouched the deeper functions of the system. This approach is no doubt useful and desirable to many. However, in my own opinion, which is built on such subjective and emotional criteria as justice, equality, deeper objectivity, and alternative epistemologies, this is not the way to proceed. The way forward is to question the entire communication system of science and use this historical nexus to modify the system to more adequately meet the needs of **all** scholars. Obviously, the latter task is by far the more difficult not only because we will come against strong resistance, but also because it involves deep and reflexive rethinking of how we chose to conduct our own work.

Still a case can be made for this rethinking. While analysis of the scholarly communication system are not new, the potential for a radical overhaul of the system is. Communications technologies, and in particular the WWW technologies, bring with them the potential for a true revolution in the mode of

distributing scholarly information. However before we can actualise this potential, we need to understand more about the peculiar problems in the current system of scholarly communication. This is a task we turn to now.

Chapter Two: Problems

> The production of scientific publication has long ago become so large that it is recognized that a scientific worker can only read a small fraction of the paper in what is itself a very small part of science...The result is that it has become impossible for the average scientific worker, who does not wish to devote the major part of his time to reading, to keep up with the progress in his own field, and almost impossible for anyone to follow the progress of science as a whole even in the most general way.[1]

> There are still quite a few individual scientists and engineers who say they think there is no problem. Some are foremost leaders of their fields for whom meetings, visits, and preprint exchanges have short circuited the library network (They spend so much time above the clouds they never see the flood.) Most of the others are so far removed from the main stream that they are in no danger of getting wet at all. [2]

> Scholars apparently do not fully grasp, let alone appreciate, the concept of an interdependent scholarly communication system. That concept is almost completely absent from the literature of the physical sciences, the social sciences, and the humanities. As a rule, scholars have no real interest in the organization or finance of scholarly communication beyond their own immediate needs. [3]

Introduction

As noted in Chapter One, when scholarly journals were first introduced they provided a number of useful services. They were needed as a distribution channel for current research since the old system of scholarly letter writing and book publication was no longer able to keep up with the burgeoning interest in science from scholars dispersed across several continents. Journals also provided a current awareness function that transcended the vagaries and inefficiencies of scholarly letter writing. They also helped provide organisation and control over the burgeoning literature. Also, because of the Enlightenment emphasis on the accretion of knowledge, journals were important for reasons of posterity. Finally as science grew and became institutionalised, publication in journals was used to establish priority of discovery and, latter, to provide a mechanism for evaluating and formalising scientific contributions for the purposes of tenure and advancement decisions. Here their role as adjudicators of original contribution took on extreme importance in those highly

volatile and vigorously researched areas where the timing of publication was critical and publication speed could mean the difference between an original submission and replicative research.

However, as the pace of publication has quickened and the volume of scientific discovery has grown scholarly journals have, as did books and letters before them, lost their ability to fulfil most of the key functions for which they were originally intended. Initially, the growing inability of the primary journal to fulfil its role in the system had been recognised only by those with a direct stake in the system (i.e., information specialists and scholars with a research interest in the scientific communication system). [4] However, since the late 50s and 60s the problem of information growth and the concurrent problems of organisation and dissemination have become sufficiently salient to draw increasing attention both inside and outside the academy.

The Players

Governments have been one of the more significant players in the field of scholarly communication for decades. Carrying forward their traditional role in providing the technological infrastructure for capital expansion and growth, governments have expressed a concern and desire to ensure an adequate *information* infrastructure for industry development. Responding perhaps to the prognostication of techno-gurus, [5] and goaded by the top representatives from industry, governments in developed and newly developing countries have become convinced of an economic threat posed by a poor quality and inefficient information systems. The equation is simple - so it seems. Competitive success depends on a strong information infrastructure. Developing the infrastructure is a competitive race that governments and industry, in their ongoing question to create "ease and spiritual fulfilment," cannot afford to lose. Michael Connors echoes this sentiment in a U.S. context.

> Individuals, corporations and nation states are today competing with one another for intellectual territory as they never have before. It is, however, territory which few, if any have yet reached and the competition for it is less a battle over new terrain than a race towards an ideal but as yet unrealized intelligent state. In such a state - and the word could be taken to signify a nation or a condition - the application of intelligence and a vast pool of readily accessible knowledge to the problems of life are implicitly expected to create a life-style of plenty, ease and spiritual fulfilment. [6]

In Canada, the early torch bearer of the race toward information infrastructure development was the Science Council of Canada. [7] More recently, concern

over information policy and the development of efficient scientific and technical communication systems is expressed by the Department of Industry, Science and Technology whose members are the key organisers and proselytisers of Canada's new super information highway [8] and also by The National Research Council (NRC) through its Canada Institute for Scientific and Technical Information (CISTI). [9] In the U.S. the Committee on Scientific and Technical Information [10] was established to look into the development of a co-ordinated STI system. [11] Other industrial countries have also created similar organisations and published monographs detailing the steps needed to develop efficient STI systems. In the UK the OSTI (Office for Scientific and Technical Information). In Japan the JICST (Japan Information Center for Science and Technology) and JACUDI (Japan Computer Usage Development Institute). [12] In the USSR VINITI (All Unions Institute for Scientific and Technical Information). Finally, International bodies have also expressed concern over the scientific and technical communication system and its relationship to global development. [13]

Besides government, industry has also an obvious interest. Historically, the STI system provides the information infrastructure for business development of one form or another. [14] Recently however there has been a quantitative and qualitative change in business interest in the STI system. At the end WWII, most data was produced by public agencies like U.S. Bureau of Census, research labs at many universities, and other data gathering facilities. And, more importantly, most of this information was made available publicly. Business expressed very little interest in the health of system (outside of the profiteering of a few individuals), or in the long term implications of an STI system for ongoing economic development. Herbert Schiller summarises:

> In sum, a good part of the information field a half-century ago was an orderly, routinized, and largely governmental sphere of activity. It was not particularly exciting. All the same, it constituted a vital component of the public sector. Individuals could access great masses of information if they had such an interest. Depending on the locale and character of the specific library, more or less of the information stockpile would be available. [15]

In short, following WW II, there was not much money to be made in the information infrastructure. Not that business was totally uninterested in the health of the system. Given the importance of infrastructure (roads, telecommunications, etc.) to profit generation, business is always interested in the health of societies infrastructure. But by and large the information distributed through the STI system had a non-profit, social-service character and business largely road free on the margins. This has changed however in recent years as the STI system has expanded beyond its traditional bounds, as governments pour money into R&D, and as computerisation of the system has progressed. This last point is the most important. The entrance of

computer onto the scene has expanded the horizons for making information profitable. Business has largely seen the ability of the computer to store, collate, transact and record activity as a powerful tool for the commodification of information. As Schiller notes: [16]

> In a very short time, data, if organized, accessible, and capable of being provided in manipulable and discrete units, became valuable….The commercial potential of these new information possibilities was quickly seen. It led in a few short years to the creation of an information industry whose firms produce, process, package, distribute, and retail information products and services such as legal decisions and texts, commodity and stock prices, specialized industrial statistics, government legislation, and increasingly sophisticated programs for business and individual computer use.

This growing industry interest is clearly seen in many publications which tout the benefits of expanding the technological infrastructure of society and expanding the STI system in directions friendly to commercial interests. The links are clear. The preface to Michael Connors' *Race to the Intelligent State* reads like a veritable who is who of high technology industry. Its not surprising then that many of these publications end up advocating increased information infrastructure development and an increased role for the private sector in the STI system.

This commercialisation of the STI system has largely been supported by government who see themselves, in the new neoliberal environment, as the champion of industrial interests not only in terms of paying for infrastructure but also in providing opportunities for private sector profit (witness the ongoing privatisation of public services). The State and business have developed much closer links in recent years. That is, the interests of state and business have converged. These links are implied above but also clearly indicated by the comments of government officials like Margot Montgomery of the Canada Institute for Scientific and Technical Information (CISTI). She provides a representative summary of the typical way of thinking about these issues amongst government officials when she notes, "Canada's success as a knowledge-based economy depends on a strong national information infrastructure that is responsive to the needs of the country's innovation system for industrial development." [17]

This is of course not to belittle industry's interest in the STI system or the need to take into account their needs. Industry has always depended on a publicly funded infrastructure. However it is to suggest that in the current political environment, there is a danger that the state will push the interests of business to the exclusion of other stakeholders (mostly from the public sector). This has important implications for developing solutions to the current STI crises. At the

very least it means paying very close attention to any government initiatives since they represent, almost by default, the interests of big business in society. Failure to be attentive could leave us with a system no better than our current financially bankrupt paper system. We will have more to say about this latter.

Of course, business and government are not the only stakeholders. Libraries are concerned, as we will see below, for a couple of reasons. On the one hand the proliferation of scholarly material, and the rising cost of acquisition, storing, and distributing said information, is putting incredible strain on their ability to maintain a reasonable collection of material and a reasonably close coupling with the ideals of information access and distribution. Individual scholars to have displayed sporadic interest. For them, it is the proliferation of material and their inability to sort, collate, and digest - in short to keep up with, the proliferating scholarly literature that has caused protestations about the direction of the scholarly communication system.

Representatives of scholarly societies are also interested in the health of the STI system. As Walter Ludwig notes, [18] scholarly societies have been victimised by the commercialisation of the STI system as much as individual scholars and libraries. Ludwig argues that when societies publish their own journals, these can actually be significant sources of revenue for them. However, after turning publication over to commercial presses, societies actually lose money. Ludwig gives one example of a commercial publishing house making $150,000 dollars on a society title while "the sponsoring society actually lost money on its own journal." [19] These sorts of findings are causing considerable soul searching on the part of scholarly societies.

So libraries and societies share roughly the same interests - and they are apparently opposed, in fundamental ways, to the interests of business and government. And although I am all for creating productive links between all stakeholders, still it is important to recognise that at least a part of the interests of the various stakeholders are fundamentally opposed. Its in this context we should view the upcoming discussion about the problems of the scholarly communication system and our upcoming analyses of the potential for change. In our analysis, we have to pay attention to interests of the various players and demonstrate how these interests are articulated in the ongoing debates about the scholarly communication. We'll also be concerned with analysing how these various interests are manifesting in struggles to determine the direction of the scholarly communication system as it goes electronic. Although it might sound like a cliché, it is only by exposing the various interests and examining them in detail that we will develop for ourselves the ability to resist and direct the current evolution of the scholarly communication system. The stakes are quite high here.

Finally there are the interests of individual scholars, graduate students, and those segments of the scholarly world which are undercapitalised and therefore outside of the system. In a very real sense, the interests here are

identical to the interests of libraries and even societies. Still, there are nuances in the interests of individuals which need to be explicated in order to utilise information technologies to eliminate or reduce current difficulties in the system. However, as we will see, it is in the interests of virtually all stakeholders, and even business when they think long term, to overcome current limitations in the system and to increase access and reduce cost.

Delays in Publication and Speed of Distribution

One of the problems that has been consistently emphasised over the years is publication delay. Publication delay is the delay that occurs between the initial formulation of a research program and its final publication in a primary journal. The average time between the initiation of a publishable program and its final appearance in print is 28 months for the natural sciences and 32 months for the social sciences. [20] About half of this time is taken up by the project itself, and the other half (14 months) by the time and effort needed to prepare and submit manuscripts for publication. At first glance a year between completion of work and final publication may not seem like much. However bear in mind that this is the average delay. In about 10% of the cases an article can actually take in excess of 5 years to appear in print. [21]

These delays are, however, those associated with more the more mundane publication of research and theory papers. In some disciplines, notably Archaeology, the delay can be as long as 30 years. As Paul F. Jacobs and Chris Holland note, when considering archaeology's emphasise on ancient artefacts, the uniqueness of these artefacts, the peculiar characteristics of the discipline, and the high cost of providing graphical representation of ancient artefacts, "Twenty or thirty years between discovery and publication seems more the norm than an exaggeration of fact." [22]

We are primarily interested in the delays associated with that phase of the formal communication process that occurs between completion of a project and the final appearance of the results in a primary journal. Here there are a number of factors that impact on the path of the article from author to printed page. The work of preparing a manuscript for publication is the first delay. Garvey, Lin and Nelson [23] estimate the average delay between final completion of a research project, and submission of the results to a journal to be six months for the physical sciences and nine months for the social sciences. [24]

Postal delays also add additional time and here the delay is cumulative since the post is critical at all stages of the traditional submission and refereeing process. Manuscripts are initially mailed to the editor who must then remail them to the referees assigned to review the paper. Referees in turn must mail their comments back to the editor who must then inform the author of the decision. Should the referees require revision of the paper (an extremely likely prospect in the social sciences), an additional circuit will be required. This of

course results in a complete duplication of the delays associated with the first submission round with the addition of the time it takes the author to revise the paper. Assuming that it takes a manuscript or referee reply one week to reach its destination, the time from author to editor to reviewer and back to editor and then author is approximately one month. However much will depend on the performance of regional postal systems, the geographic location of each of the parties in the review circuit, and the motivation of reviewers. Especially where articles are submitted to journals in other countries, and especially when the journal is overseas, the postal delay can be considerably longer than 1 month. [25]

Delays can also be expected to accumulate as the manuscript is processed by the editorial staff. Garvey, Lin and Nelson [26] found the average time between receipt of a manuscript and its final publication to be about 7 months for the natural sciences and 11 months for the social sciences. Half of this delay is caused by various editorial tasks and the other half by lackadaisical reviewers who, because of numerous academic commitments, tend to give low priority to submitted manuscripts. As Meadows [27] notes, "a referee may only take a short time to assess a paper; but the paper may have been resting in his in-tray for days, or weeks, beforehand, and may not be promptly returned to the editor afterwards. As a result, brown manila envelopes that contain manuscripts for review can often go ignored for weeks." [28]

Perhaps the most frustrating delays associated with academic publication are those that arise from lack of journal space. This is a problem both for prestigious journals or journals publishing in fashionable and expanding fields where many authors compete for limited space, [29] and also for more run of the mill journals. [30] Because of the high production costs of paper journals, there are absolute limits on the number of pages that can be included in each number of the journal. Because of this, it is very easy for backlogs of publishable articles to accumulate and for authors to have to wait for space before their publication can appear in print. The actual length of delays associated with publication backlogs is of great interest but is unknown at this time.

Finally the journal is published. However now the issue will have to be delivered to individuals and institutions and this adds a final post publication delay. Carson and Wyatt [31] are one of the few who have studied this post-publication delay. They found that for primary journals, average delivery delay ranged from as little as two weeks within the country of origin, to as long as eleven months for delivery to other countries. Ironically, for abstracting journals the delays were even longer. Carson and Wyatt found average delays of between 1.5 years for delivery of an abstracting journal to Australia and 2.8 years for delivery to India. The longest delay recorded was for delivery of the *Excerpta Medica* which took almost five years to reach the U.K. and just over 4 years to reach Australia. For scholars in underdeveloped countries, the problem of delay is compounded by the higher than average rate of missing

journals which Paul Nijhoff Asser has noted is as high as 30% for countries such as India. [32]

As a result of these rather extensive delays, it can no longer be said that primary scholarly journals provide a current awareness function. Nor is it possible to trust them for the purposes of assigning priority to discovery. The additive delay of manuscript preparation, postal turnaround, adjudication by referee, rejection and re-editing, and final delivery to the individual or institution is simply too long. The fact of the matter is that ideas in print are not new at all. When the various components and their associated delays are tallied, ideas can be as old as 3, 4, or even more years. This delay can make a mockery of the scientific communication process as when the general public becomes aware of key scientific findings through the general media before they reach the scholarly world through the relevant journals. [33]

Is this long delay a problem? The answer to that really depends on who you are talking to. There are those that favour an organic analogy of the communication system and they tend to either discount the delay as trivial, [34] unimportant, [35] or find greater benefit to science in the distillation process that they feel is occurring during the time that it takes to get a work published. In their own words, the delays are *functional.* Those who use this type of analysis will point out that the entire process is finely tuned to get rubbish out of the system and therefore years worth of delay are a necessary function of the filtering system of science. Furthermore, tampering with the system by, for example, introducing enhancements like the early sixties Information Exchange Groups (IEG), [36] is a definite no-no that potentially harms the quality control mechanisms of science. As William D. Garvey notes of the 1960s IEG experiments, "As so often happens in scientific communication when media are democratized to the extent that 'all scientists' can use them indiscriminately to disseminate their work, the quality of the average product suffers." [37]

Here Garvey is referring to the presumed enhancements in the form of IEGs and other mid to late sixties attempts to speed up the scholarly communication process. As he goes on to point out, IEGs and other experiments, like the attempt of the American Psychological Association (APA) to distribute preprints, met with vociferous opposition from a small *core* of members who 'were severely against the unedited manuscripts system, described by one as 'a vast sewer carrying garbage from one scientist to another.'" (p. 74). Garvey only specifies the core in an out of the way footnote. From there we learn that the core were the "10% of the membership of APA [that] maintains the core of activity in scientific communication in psychology. They furnish most of the research material that warrants information exchange, and their efforts keep psychology going as a basic science (i.e., they regularly publish journal articles and make formal presentations at meetings; they are the producers of books on psychological subjects; they are the holders of major federal research grants and often serve as monitors or advisors on other federal

grants or programmes; they are the editors and referees of journal article manuscripts; and the body governing professional psychology is generally contained within this group)." [38]

This opposition of this *elite* core of individuals immediately raises questions about the motivation for the opposition. Given the rhetorical functions of science, the explicitly stated gatekeeping role of the scholarly journal, and the historical record that indicates science has as often been about marginalising individuals and closing discourse to alternative views, it is reasonable to suggest that the opposition had more to do with an attempt to stay the development of alternative forms of recognition, reward and discourse that the established scholars had little control over. This becomes more apparent when we consider the deeper sociological problems inherent in long publication delays below.

Others recognise that there is a problem, but point to the ability of some scholars to short circuit the formal system by relying on informal contacts and invisible colleges. This is the stance taken by Price who notes that [39]

> ...one of the great consequences of the transition from Little Science to Big Science has been that after three centuries the role of the scientific paper has drastically changed. In many ways the modern ease of transportation and the affluence of the *elite* scientists have replaced what used to be effected by the publication of papers. We tend now to communicate person to person instead of paper to paper. In the most active areas we diffuse knowledge through collaboration. Through select groups we seek prestige and the recognition of ourselves by our peers as approved and worth collaborating colleagues. We publish for the small groups...

For many reasons this structural-functionalist analysis is inadequate. In the first place, using the informal realm as a bypass is problematic because the informal system itself is ill-managed and haphazard [40] and thus open to many forms of abuse. Also, discounting the delays as functional or pointing to the fact that scholars can sidestep the formal system of communication by exploiting the informal sector tends towards a highly anglocentric and elitist view of the scholarly communication system that is, curiously, clearly articulated by advocates of this method of overcoming problems caused by delay. The mostly American male scholars in elite institutions who advocate this approach fail to see the difficulty that scholars with family obligations, or those without the resources (i.e., young scholars and advanced graduates) and wherewithal to short circuit the formal system of communication by attending numerous semiprivate national and international conferences and private meetings, might face in trying to negotiate the highly competitive scholarly gauntlet with only the formal literature to rely on.

The problem may of course have been exacerbated in recent years as institutions cut back on frivolous funding areas thereby reducing access to travel grants and awards. As graduate bursaries and award opportunities are cut, it may be increasingly difficult for the young scholar to attend the requisite meetings. We shouldn't underestimate the disadvantage that might accrue. If Price and Merton are right, good connections, a bird's eye view of the discipline, and access to national and international expertise can make all the difference between pursuing a fruitful and active topic area in an informal network structure, or being isolated in a shrinking area of interest with no job connections and little future prospects in the competitive academy. Norma Vale, [41] for example, (1986: 8) quotes a Dr. Smart on informal hiring: "Networking is important...New PhDs should participate in professional organisations and conferences. Certain professional conferences are known as venues for formal and informal recruiting."

In this context, publication delay, or rather review delay, can cause serious problems for marginalised groups. For example, there is an impact on young scholars who are completely dependent on a good publication record in order to land that first faculty position. As we all known, graduate students are now "expected to have published by their third year and certainly no latter than their fourth." Clearly a publication delay of three or four years is a significant problem not so much because of the inability to get published in that time (since many students actually do get published), but in the disadvantage that some students will face because of the inability of the system to give appropriate feedback to those who do not have the benefit of a close apprenticeship with a mentor who has significant knowledge about the ins and outs of the publication game. An extremely bright student at a less prestigious university, or one unlucky enough to choose an advisor with little experience in publication or little interest in them, will be completely dependent on the feedback from peer reviewers who examine her or his first publication attempts. Not having the benefit of the wisdom and experience of a published mentor, that student will have to learn by trial and error what counts for an acceptable paper. Unlike a student who has access to expert advice, the disadvantaged student's first attempts at publication are likely to fail. This may not be any indication of the actual talent of the scholar who, given more time to develop the requisite skills, may have gone on to a brilliant career. However having to wait 1, 2 or 3 years for feedback before trying again puts the scholar at the end of the graduate program and at a point in there early career where they are expected to have published.

Being on the job market with a poor initial publication record is a death warrant. In the high stakes game of scholarship, scholars with poor initial records are not likely to get hired because they are too much of a risk. Universities and departments are dependent on the money that good research professors can bring to the academy and they are therefore careful about who they will hire. True, it is possible for graduate students to continue to develop their publication record following graduation. But there are absolute limits on

the time available to pursue this strategy since as Ralph Korteling of Simon Fraser University has noted, failure to get hired in the first few years is considered a black mark against you. Hiring committees read into this failure to get hired early "an indication they haven't met the standards elsewhere." [42] Obviously with this kind of thinking the graduate is under intense pressure to publish at least a couple of papers before graduation.

The functionalist position on publication delay also ignores the difficulties that developing countries have in keeping up with scientific progress. Even before publication delays are factored in, the ability of developing nations to keep up is problematic. [43] However, these systemic difficulties are profoundly exacerbated when delays in publication cumulate to years and even decades where resources for academic exchange and library material are lacking. [44] Of course, developed countries can also experience international delay in journal delivery. [45] However developing countries must cope with additional disadvantages that include lack of resources, isolation from informal communication networks, an imperialist global economy, and biased information flow. [46] These factors compound and interfere with a countries ability to develop a technological and knowledge infrastructure and a locally relevant scientific program. In short, the long publication and distribution delays give advanced countries a competitive advantage. Developing countries remain behind the *research front*, as Price would say, and they are therefore made dependent (if they do not choose the appropriate technology route) on the importation of cutting edge research and technology.

Perhaps scholars from developing countries could attend all the national and international meetings in order to stay informed and current. This would essentially mean entering the informal communications loop in order to stay informed. However, it is doubtful whether the resources exist for them to connect as much as they would need too to the informal networks. Long journal delays, coupled with difficulty accessing the critical informal channels has powerful implications for the science of the developing nations. Goonatilake points to the difficulties scientists in developing countries have in accessing informal networks to keep up with scientific progress. "Lack of informal channels and communications in the form of face-to-face interactions between those working at the frontier of science has therefore led to a marked degree of sterility in output. [47] The problem identified by Goonatilake is exacerbated for women. The Gender Working Group of the United Nations Commission on Science and Technology for Development notes that women, who remain underrepresented in scientific and technological careers to begin with, have "difficulty ... breaking into the formal and informal scientific networks that characterize the workings of the scientific community..." [48]

This is of course a tension here. The assumption that the knowledge produced in the centre is applicable or desirable to the periphery is far from a given. Relying on the knowledge of the centre tends to increase dependence, create conditions for hegemonic domination, carries forward an ongoing colonial style

transfer of knowledge and technology, tends to destroy indigenous knowledge systems, and creates a local market for western style goods. [49] Goonatilake recognises this tension and suggests ways to overcome scientific dependency. What's important for our purposes is that because of ongoing dependence, and because of the inability of developing countries to enter into an organic discourse with science at the research front, there ability to strategically and creatively utilise scientific research, and even their ability to set research agendas through cutting edge publication, is impeded.

Long publication delays *may* also effect the form and content of scholarly discourse though if such is the case, it is not really a significant problem in light of the concerns raised above. Steve Harnad has been the untiring advocate of this position. As he has pointed out, even when a paper finally appears in print, there is still a significant wait while practitioners track down, read, process, and incorporate the new work in their own work. This gives academic discourse a certain inorganic, choppy, and unnatural (read inhibiting) quality as it moves in fits and starts over a period of years. According to Harnad, writing already moves at an unnatural pace. The extended time period required for the maturation of an idea in the scholarly communication system compounds the problem and leaves us with a wholly inadequate system of communication. Harnad explains: [50]

> In a sense there are only three communication media as far as our brains are concerned: The nonverbal one, consisting of oral speech (and perhaps sign language), and the unnatural one, consisting of written speech. Two features conspire to make writing unnatural; one is the constraint it puts on the speed with which it allows thoughts to be expressed (and hence also on the speed with which they can be formulated), and the other is the constraint it puts on the INTERACTIVENESS of speaking thinkers -- and hence again on the tempo of their interdigitating thoughts, both collaborative and competitive. Oral speech not only matches the natural speed of thought more closely, it also conforms to the natural tempo of interpersonal discourse. In comparison, written dialogue has always been hopelessly slow: the difference between "real-time" dialogue and off-line correspondence.

In addition to the problems of an inorganic discourse, the long delay may also have an adverse impact on the continued development of fruitful ideas. As Harnad points out, because of the long delay between completion of a research project and final publication, the author may lose interest in pursuing the original line and thus the idea may become stillborn and never, sadly, achieve its desired or deserved impact.

> ...now the author must wait until his peers actually read and respond in some way to his work, incorporating it into their theory, doing further experiments, or otherwise exploring the ramifications of his [sic] contribution....[this] usually takes several years...and by that time the author, more likely than not, is thinking about something else. So a potentially vital spiral of peer interactions, had it taken place in 'real' cognitive time, never materializes, and countless ideas are instead doomed to remain stillborn. The culprit is again the factor of tempo: the fact that the written medium is hopelessly out of synch with the thinking mechanism and the organic potential it would have for rapid interaction if only there were a medium that could support the requisite rounds of feedback, in tempo giusto! [51]

Clearly, publication delay is a significant problem. If not for the elite of science, than it definitely is for the marginalised voices of the scholarly enterprise. Yet up until only very recently, publication delay has been the concern of only a few in government, industry, and the scholarly enterprise. Odylzko suggests the reasons for this is that "print journals have been an integral part of the scholarly life for so long, their inflexibility is often not appreciated." [52] This may be true. But much of the lack of concern over delay has also been linked to attempts to maintain the systems of stratification and privilege in the scholarly enterprise. As we noted above, there were early attempts to reform the system that, although very popular among some, were vociferously resisted by others. This begs the question. Why is publication delay only now becoming a significant "problem" in the eyes of many scholars?

Perhaps the reason is because it is no longer possible to resist alternative methods of distributing scholarly output. Entrance barriers are simply to low and the technology to easy to use. As a result, many from the elite who would have once resisted change are now altering their strategy to try to steer the direction of change. Certainly, this has been the approach of Stevan Harnad who has from the very start advocated increased control over the chaotic realm of electronic communication. [53] As change occurs, and as electronic journals tackle some of the intransigent problems of the primary communication system, we will need to keep in mind the various interests as we design distribution systems. Given the power of communication technology to facilitate progressive change, it makes little sense to allow the failures of the traditional system to be incorporated into new modes of distributing scholarly information.

Proliferation of Journals

> God must love the scientific journals because he made so many of them. [54]

Another problem with scientific communication that has received rather more attention over the years is the astounding proliferation of the primary, secondary, and tertiary literature. From its humble beginning towards the end of the 17th century, the scholarly journal has grown to truly gargantuan proportions. Through the years, many have called this proliferation of information a crisis and in this section we will take a detailed look at the outlines of the crisis and what it means for the two groups who it effects the most; scholars and libraries. Keep in mind as we move through the analysis that the perception of a crisis is not all together new. For example, we have already seen how abstract, review, and indexing journals were developed in the 19th century in response to a burgeoning scientific literature. A bit later, Bernal had some comments about the proliferation of scholarly literature. And then again in the mid to late 60s, scholars and governments were expressing concern. This apparent cyclical nature has prompted some writers to discount the phenomenon as irrelevant and concern about it as misguided. Price, for example, attempts to explain the problem away as an epiphenomenon of the exponential growth of the scientific establishment. [55] However others, most notably libraries, are experiencing real difficulties with the explosion of scholarly material that cannot be discounted so easily.

For scholars, the difficulty caused by journal proliferation is simple. There's too much literature to keep up with. It is rather like we were all playing the role of Sisyphus in some tragic cosmic drama acted out for the amusement of a handful of Gods. This metaphor is not to far out in left field since real desperation has at times been expressed. For example, concern over the problems of journal proliferation prompted a group of scholars in 1974 to issue a *manifesto* calling for a total boycott of new commercial journals and a move away from reliance on commercial houses, conceived of as unnecessarily contributing to the problem by twigging journals for profit rather than real need, to a system were scholars could have more control of the scholarly communication process. [56]

Those who care to comment about the crisis are of a piece about the problems that are caused by proliferation. Deana L. Astle quotes from the 1960 *UNESCO Bulletin for Libraries.*

> ...the multiplicity of journals results in a scattering of papers which makes it impossible for the scientist to keep informed of new developments, impossible for libraries to cover a field completely, and impossible for abstracting services to include all relevant articles. One writer estimates that a single article in a highly specialized periodical is of interest to only 10 percent of the workers in the area covered by the journal, that an article in a general periodical may be of interest to only 2 percent of its readers, and that an article in a local publication may interest on-quarter of 1 percent of scientists in its field. Other writers have said that intense

> specialization has made it possible for scientists to read more than a fraction of the articles in journal in their own field...A scientist subscribing to a journal is forced to pay for twenty or thirty papers which do not concern him in order to get the one paper he wants. [57]

Similar are J. C. R. Licklider's comments.

> Sixty years ago.... the 3,000-character-per-minute reader needed only 25 minutes a day to keep up with everything in his field. Eleven years hence, he will have to read continuously, every hour of every day. Of course most of us do not read so fast and so persistently. Of course most of us make do with less than total scrutiny of less than one one-thousandth of the corpus. Give or take a small factor in speed; give or take a small factor in size. The essential point is that an exponentially increasing requirement is passing a constant capability. It is our unique experience to live and work through the period in which individual mastery of a field turns from possible to impossible - in which the depth of the water exceeds the height of the banks. [58]

Clearly, journal proliferation is a problem for all but the elite of the scientific academy who can, as we noted in earlier sections, short circuit the formal system of communication. In this context, their ability to integrate into the informal networks allows them to gain insight into active and promising research areas with little effort.

What has caused this proliferation of scholarly material? King, McDonald, and Roderer identify a number of structural factors. They explain the growth in journal publication as the result of 1) the growth and maturation of science, 2) the founding of new disciplines, 3) the ongoing fragmentation within disciplines, 4) and the increasing output of other countries, in particular third world countries. [59] The last factor, according to King et. al, has been particularly significant in recent years as underdeveloped nations have made concerted efforts to overcome the knowledge/technology gap that keeps them dependent on the beneficence of the developed world. King et. al. might have added the growing importance of scientific and technological development to the economies of first world nations since this increased flow of research dollars impacts on the growth and fragmentation of the scholarly enterprise.

In addition to structural factors, various social factors, like the "publish or perish" syndrome, have also been offered as partial explanations for journal proliferation. The publish or perish syndrome is based on the well understood link between the academic job market and the publication system. Since prestige is enhanced through publication, [60] universities use an individuals publication record as a yardstick against which to evaluate scholars for hiring

or promotion. For all scholars there is thus a very real need to establish academic currency through publication. As the academy has grown increasingly competitive over the years, this has resulted in what some have called a pathogenic pursuit of publication. [61] Not only do authors want to publish, they want to publish 'fustest and mostest." [62] According to many this pressure leads to questionable practices like trying to publish the same work more than once, fragmenting material into small pieces and publishing them as "pellets of prestige" [63] or "least publishable units," [64] or engaging in a ridiculous amount of coauthorship. Deana L. Astle provides an amusing example of coauthorship abuse: "An outrageous example of this is a recent four-page article in the October 17, 1988 issue of *Physical Review Letters,* the first page of which is a list of 190 authors from 17 institutions who are given credit for the research; all of them can list the paper in their vitae." [65]

The problems that this abuse can cause are numerous. Salami publication, or publication of slices of research in order to increase one's publication record, exacerbates the problems associated with the proliferation of the literature by making it more difficult to track down and utilise key components of the scholarly record. It also contributes in a relatively straightforward way to the ongoing fragmentation of scholarly disciplines. An increased work load can also be expected because of the need to piece together a coherent picture from many papers some of which are of dubious value. Finally, there is general downgrading of the quality of the scholarly record. This is reflected, for example, in the growing concerns of many scholars about the quality of available scholarly material and its relevance to their teaching efforts. As Astle [66] notes, instructors in some disciplines are relying on earlier work from the sixties because these early papers provide a more substantive empirical and theoretical treatment of their subject matter. Apparently, current scholarship in some disciplines is too fragmented to be useful in the classroom.

One final cause of journal and information proliferation is the commercial press. Recognising the unique (read monopoly) nature of the academic market, they have exploited the increasing need for publication outlets and specialist periodicals by artificially splitting their journals to create new titles. In a process known as "twigging," commercial publishers spin off more focused specialist titles from their high prestige journals in order to exploit the peculiar dynamics of the academic marketplace. While at times there may be a real need for the additional journal titles, at others the ploy is a transparent attempt to milk the academic market. Paul Metz and Paul M. Gherman [67] note "The launching by the Hawthorne Press of twelve journals with the work *marketing* in their titles...shows that invention can have mothers other than necessity...."

While journal proliferation is a problem, it would be less significant if it were not for the impact that this proliferation is having on the economic health of the scholarly communication system. However as journals have proliferated and the costs of their purchase have skyrocketed, the system has been brought, quite literally, to the verge of collapse. In the next section we turn our attention

to the interaction of journal proliferation and rising costs, and to the dire impact this has had on the health of the academic library.

Cost

> I set up a perpetual financing machine through advance subscriptions as well as profits on the sales themselves. It is a cash generator twice over. It's no use trying to compete with me in scientific journals, because I publish the authoritative journal in each field. [68]

> Faculty need to be informed in order that they can see these publishers for what they really are, not partners in the dissemination of information but profiteers who, through their outrageously high prices, are restricting the flow of scholarly knowledge. [69]

The problems caused by the proliferation of journal titles have been compounded by the decades long rise in journal prices. The earliest statement of the problem was by Paul L. K. Gross and E. M. Gross [70] who, in their now classic citation analysis of chemistry journals, argued that in the face of growing financial restraint and the inability of small and medium libraries to maintain comprehensive collections, library acquisitions policies had to be rationalised. Ninety years latter, libraries are still unable to keep up with the exponential growth and skyrocketing cost of scholarly material.

Really intense interest in the problems of cost emerged in the sixties when writers, researchers, and government organisations began expressing concern that the weight of journal proliferation and price rises would break the back of the scholarly communication system. [71] In his comments about the crisis in scholarly publication, Richard de Gennaro [72] spins a classic tale about the declining effectiveness of the scholarly communication system, its lack of responsiveness to the real needs of those who it purports to serve, the increasing difficulty experienced by libraries in keeping up with cost increases and journal twigging, and the growing need for scholars and librarians to do something about the crisis.

All of the early examinations of the health of the scholarly publication system were equally grim about the potential long term impact. White and Fry [73] conducted a major longitudinal study (under the auspices and with the financial support of the NSF in the USA.) of journal proliferation and cost increases for the years 1969 to 1973. They concluded that the extant model of scholarly communication was "unhealthy" and that "Neither librarians nor publishers demonstrate any real ability to cope with the funding imbalance through innovative or cooperative techniques." The long term result of this, according to the authors, would be that certain disciplines like the humanities would be unable to sustain their formal communications networks without

government subsidy - an especially disconcerting conclusion given the growing unwillingness of governments to subsidise scholarly activity. They further argued, prophetically, that should government subsidy be reduced or eliminated, the results would be disastrous to the academy.

Although the crisis may not be visible to all scholars, recent years have not seen the situation get any better. [74] McCarthy [75] gives a number of anecdotal examples of staggering increases. For example, between the years of 1989 and 1992, the price of the journal *Gene* almost doubled from its 1989 price tag of $1,874 to $3,508. And the journal *Tetrahedron Letters* moved from $2,715 to $5,289. And if you think that $5,000 dollars is high for a journal, consider the *Gmelin Handbuck der Anorganishen Chemie,* published by *Springer.* Its 1994 yearly subscription price was a whopping $19, 756. At that price, a library could buy over 130 journals at the more modest price of $150.00 annually. Robert Hauptman [76] provides similar anecdotal evidence about the rising cost of publication. He notes that *Brain Research*, which had cost only $1100 a year in 1983 jumped over 600% to $8,000 in 1994.

Moving beyond anecdote, it should be noted that some useful analysis of general trends have been conducted. Paul Nijhoff Asser provided data for years 1971 through 1977. [77] He found price increases of between 14.5% and 34.2% for the years 1971 through 1974 and increases of between 18.7% and 43.5% for the years 1974 through 1977. Asser attributes the higher average increases in the latter period to the oil crisis and its impact on the costs of paper, manufacture and distribution of journals. However since that time, high annual price increases have continued. Between the years 1986 and 1994, the Association of Research Libraries [78] recorded a serial price increase for the 8 year period of 115%. [79] In some cases, especially in the sciences, the annual increases can be almost obscene. For physics and chemistry journals, the year 1989 was an extremely bad year with an average increase of 25.1%! [80]

Whatever else we might think about the crisis in scholarly communication, there can be little doubt that the cost increases and the subsequent high cost of serial purchase is putting intense pressure on academic and specialist libraries. While before 1988 there had been some discussion about the seriousness (or actual existence) of a crisis, [81] by 1988, the crisis was simply understood and attention turned to investigating retroactive coping mechanisms and proactive strategies. In 1988, Dougherty and Barr, [82] editors of the *Journal of Academic Librarianship,* conducted a survey of ARL members libraries to determine their strategies for coping with the situation. They found a number of reactive steps that libraries were taking in order to cope with rising costs while at the same time attempting to retain a comprehensive serials collection. The coping strategies included elimination of duplicate subscriptions, caution in acquiring new titles, a decline in monograph and book purchases, and even a shift of funds from salary to acquisitions!

Since these early discussion libraries have moved with vigour away from

reactive strategies towards proactive strategies that target little used or lower prestige journals, or seek to target and eliminate the publications of commercial publishers that are considered predatory. [83] In Canada, the picture has been the same and even the prestigious and well funded *Canadian Institute for Scientific and Technical Information (CISTI)* has been forced to eliminate duplicate and superfluous subscriptions, and reduce monograph purchases. [84]

Libraries have responded in other ways. Strategies, reported by Taylor, [85] have included an increase in external fund raising activity, a re-evaluation of the library as a free-for-service institution and the initiation in some instances of actual fees for borrowing, and, a move towards an extension of the interlibrary loan practice known as resource sharing whereby individual institutions co-ordinate their acquisitions policy so as to avoid duplication. Most interesting is Taylor's discussion of the introduction of a management-administrative ethos and the subsequent re-evaluation of library acquisitions and operations. The shift represented here has resulted in a reduction in personnel and the deskilling of library jobs. "Every library with which I am personally familiar has recently undergone some reduction in personnel. Tasks formerly within the domain of professional librarians are likely to have been shifted to paraprofessionals; tasks carried out by support staff are now done by student assistants in an effort to reduce costs." [86] There has also been talk of automation and the benefits of that technology can bring to managers seeking to streamline the process. "Most important, the automation of acquisition and circulation functions has provided for the first time a significant body of data for use by library managers in planning" [87]

In terms of material acquisition, the net result of the librarians push to cut corners is that holdings stay steady or decline, and that very little new acquisition occurs. [88] Metz and Gherman [89] note that the percentage of the total serial universe held by member libraries of the ARL dropped from 33% in 1973 to 26% in 1987. It perhaps goes without saying that this rising cost of journal publication, coupled with the explosive growth in research and the concomitant explosion of paper journals in the various disciplines, has made it impossible for most libraries to maintain a comprehensive selection of literature. And, as libraries reduce their acquisitions budgets, publishers feel pressure as well and drop lines that had formerly been subsidised by more profitable titles. This has resulted in the elimination of specialist lines of literature which, although useful to small academic communities, cannot regain the cost of their publication. [90]

In what can only be described as a vicious circle, everyone loses. At first, with the decline in monograph purchases, those hit hardest would have been undergraduates who rely on the book literature much more than graduate students or faculty. [91] However with the more serious retrenchment that is represented by elimination of duplicates or outright elimination of titles, scholars have begun to feel the pinch. Consider that in 1994 alone, the

University of Arizona eliminated 1,761 titles valued at $590,000. [92] There can be no doubt that such deep cuts would be felt throughout the university community. Some have argued that a serious crisis has been averted only because of co-operative arrangements, increases in interlibrary loans, and co-ordinated cutting strategies between regional libraries. However if current trends continue, it is unclear how long the earlier predictions of White and Fry about the demise of parts of the system can be avoided.

What has caused this staggering increase in cost? Part of the explanation lies in the inflationary pressures that effect all aspects of the journal production process. The overall costs of journal production can be broken down into three components - prerun costs, runoff costs, and, optional costs. [93] The initial or prerun cost includes the work of editors, peer reviewers, copy editors, compositors, proof-readers and typesetters. These prerun costs often include hidden costs such as office space or editorial time and expertise "donated" by academic organisations. The second cost category, or runoff costs, include paper, printing (presswork, binding, and wrapping) and distribution costs (mail). Finally, there are what the Task Force calls "optional" costs or "costs of operations that are not necessary to the publication of research results, but that are considered desirable adjuncts." [94] These include preparation and printing of advertisements and promotional material, production of reprints, the storing of back issues and the processing of orders for these back issues. King, McDonald & Roderer [95] outline the rises in these production costs in the period up to and including 1977. For example, they note that between 1960 and 1977, editor's salaries rose 142%, typesetting costs rose 179%, printing costs skyrocketed 175%, paper 52%, and postage and handling by 113%.

But inflationary costs are not the only reason for the increases. In his extremely caustic editorial, James Thompson [96] places much of the blame squarely on the shoulders of the commercial publishers who, according to him, have discovered the elysian fields of total monopoly production. As Thompson points out, the market for academic journals is extremely inelastic and there is **no** potential for competition between titles. If a publisher owns the prestigious or pace setting journals in a particular field, that is the end of the story since libraries and scholars must have access to it in order to remain current. Joyce and Merz, [97] noting an early and growing tendency for publishers of all varieties to engage in discriminative pricing, explain:

> The factors most heavily influencing elasticity of demand are the number of substitutes for the product and the percentage of income spent on the product. The greater the number of substitutes, the more elastic the demand. From the standpoint of substitutes an individual always has the ability to use the library's copy of a journal, whereas the reverse is hardly practical. Also, individuals can drop or switch subscriptions to journals as their professional interests change with little inconvenience. But the decision to cancel a

> particular journal or switch to another is entirely different for a library. A major objective is chronological completeness in a collection since the library cannot anticipate future faculty interest in particular journals compared with currently expressed interest. Also these cancelling or switching decisions involve the political influence of particular faculty members on the allocation of a library's serials budget. Remote acquisition of material contained in academic journals is sufficiently bothersome to make it an extremely poor substitute for the journal itself. Thus, with fewer substitutes, a library will have a more inelastic demand than an individual for academic journals. [98]

Joseph S. Esposito, president of Encyclopedia Britannica, makes the following comment about the monopoly like nature of the scholarly segment of the publication marketplace. [99] "This segment, as we know it today, was essentially invented by the late Robert Maxwell, whose entrepreneurial insight was that libraries would pay almost any price for premier publications. He was right, and he was hated for it."

This privileged position of academic journal publishers has led some of them to engage in predatory behaviour. Thompson accuses commercial publishers of price gouging and vulturistic practices designed to eliminate smaller, less fit organisations in order to leave only the big corporate publishers still in the game. Robert Maxwell of Maxwell Communication himself suggests this scenario. "I am determined that Maxwell Communications Corporation will be one of what I expect will be only ten surviving global publishing companies." [100] Thompson characterises these publishers as spoilt children who, when libraries resist and talk boycott, "use ... legal bluff and bluster to squelch it [the resistance]. Martin Gordon, of Gordon & Breach, has written irate letters to librarians who have canceled his titles, including at least one threat to sue for complaining to an editor that issues of a certain journal are now being labeled as volumes." [101]

In the early days of the cost crisis, a few authors tended to give the commercial publishers the benefit of the doubt. Michael E. Koenig [102] for example argued that the pricing policy of commercial publishers is actually beneficial to libraries and individual subscribers. In the same vein, White suggested that commercial publishers were not making inordinate profits and that when cost per page were taken into account, the sharp differentials between commercial and other types of publishers disappeared. [103] Most recently David W. Lewis [104] figured that all the fuss was the result of a misunderstanding (mostly on the part of librarians). While he seems to indicate that both librarians and publishers have gaps in their knowledge of the workings of the other, in the last analysis he places the blame squarely on the shoulders of librarians: "Librarians feel exploited, and publishers feel misunderstood. Neither side seems to be able to see the other's point of view.

This lack of comprehension occurs, at least in part, because librarians are not knowledgeable about the economics of the scholarly journal."

However now there can be little doubt that commercial publishers do make the best use of their monopoly position and authors, recognising this, are increasingly pointing out that publishers have all together too much leverage in the academic marketplace. [105] For example, there is evidence to suggest that publishers (3 or 4 of the very largest in particular) assess the market carefully while considering price raises. Consider the observation by Dougherty and Barr [106] that journals with high demand (informally operationalised as journals which are regularly duplicated in a library's acquisition strategies) tend to be those whose prices rise the highest and fastest. Then there is the study conducted by Economic Consulting Services for ARL which concluded that "'each targeted publisher has increased subscription prices for the sample of titles examined at a much faster rate than the rate at which their costs have increased.' The differentials cited for the four most intensively studied publishers (Elsevier, Pergamon, Plenum, and Springer-Verlag) indicated that prices per page had risen from between half again to more than double costs per page." [107] Kenneth E. Marks, Steven P. Nielsen, H. Craig Peterson, and Peter W. Wagner confirm these studies with their own data and conclude that "95 percent of the titles from these three [Elsevier, Springer, and Pergamon] foreign commercial publishers are in the top 40 percent of price increases." [108]

It is worth looking in detail at a study by Sandra R. Moline [109] in order to get a clearer picture of the underlying reality. While carefully controlling for the amount of material published, [110] Moline found strong evidence to suggest that commercial publishers price their periodicals not on some reasonable requirement for profit, but rather based on what the market will bear. Moline has differentiated between commercial publishers, society publishers (e.g., the APA or ASA), and "other" publishing houses which include universities, departments, university presses, research institutions and museums. She has also made a distinction between three broad categories of scholarly endeavour. Table 1 below summarises her findings.

Table 1:

Prices and Sizes of Subject/Publisher Categories

Publisher Type/ Factor	Arts/ Humanities	Social Sciences	Science	Total
Commercial				
Mean Subscription	$40.04	$83.96	$283.18	$188.69
Mean kchar/year	1681	1942	5755	4063
Mean cents/kchar	3.04	5.27	7.23	5.94

Mean pp/year	475.4	557.3	1316.3	973.6
Association/Society				
Mean Subscription	$33.11	$57.20	$129.64	$96.21
Mean kchar/year	1995	2731	6944	5103
Mean cents/kchar	2.16	2.82	2.73	2.66
Mean pp/year	529.2	633.0	1155.7	925.3
Other Scholarly				
Mean Subscription	$25.33	$46.13	$138.00	$63.11
Mean kchar/year	1489	1999	5966	2901
Mean cents/kchar	2.30	2.72	2.89	2.58
Mean pp/year	430.4	588.5	1263.9	711.0
Column Total				
Mean Subscription	$32.81	$64.66	$137.46	$127.16
Mean kchar/year	1700	2287	6327	4274
Mean cents/kchar	2.53	3.70	4.71	3.96
Mean pp/year	474.4	595.9	1236.6	904.7

Source: Sandra R. Moline (1988), *The Influence of Subject, Publisher Type, and Quantity Published on Journal Prices.*

There are a couple of things about the data that strike one immediately. First of all is the clear price differential between arts and humanities journals, social science journals, and science journals. In each category of publisher (Commercial, Society, and Other), the journals of the sciences cost more than those of the social sciences which in turn cost more than the journals of the arts and humanities. Two factors make up this difference. On the one hand, science journals publish more pages (or more characters / year) than either the social science or humanities journals. We would thus expect those categories of publication that average a greater number of pages to cost more. On the other hand, science journals publish more graphic, tabular, and mathematical information. This also effects the average price of the journal since when compared with the cost of printing straight text, graphics, mathematical equations, and tabular data are quite expensive to reproduce.[111]

Another striking feature of Moline's research is the unmistakable differential pricing policy of the commercial publishers. In addition to the fact that commercial publishers invariably charge more for the material they help produce (a fact noted again and again in the past 25 years), they also seem to be charging differentially based on the presumed status of a particular scientific field. Notice that for the categories of "Association" and "Other," the Mean Cents/Thousand Characters remains remarkably stable across

disciplinary boundaries. For example, Association and Society publishers average 2.16 cents per 1000 characters for Arts and Humanities journals, 2.82 cents per 1000 characters for Social Science journals, and 2.73 cents per 1000 characters for Science journals. Compare this with the 3.04, 5.27, and 7.23 cent cost per 1000 characters charged by commercial publishers. Surely there are no aggregate differences in the content of Commercial vs. Association journals. That is, we can reasonably expect that the ratio of graphic/tabular/mathematical data to text would be the same for each category of publisher. Were we cynical, we might think that the comments of major commercial publishers like Robert Maxwell actually reflected a broad industry policy of preying on the inelastic demand of the library market and extracting as much surplus from the system as possible.

A final interesting feature of the above data is that commercial publications cost more in all disciplines and not just scientific publication. For example, the cost per character for humanities publications is 2.3 cents for Association and 3.04 cents for commercial. The cost per character for social sciences is 2.82 cents for Association publications and 5.27 for commercial publications. And finally, the cost per character for science based publications is 2.73 cents for Association, and 7.23 cents for commercial. This means that commercial publication is 1.32 times more expensive than society publication in the humanities, 1.87 times more expensive in the social sciences, and 2.65 times more expensive in the natural sciences. And this data is 10 years old.

The important point here is that commercial publication costs more regardless of broad disciplinary categorisations. And, its probably significantly worse now since the only thing that has really changed in 10 years is government's openness to allowing commercialisation of academic services. This is an important point to make since some authors attribute the crises in publication exclusively to commercialism in the STM (Science, Technical and Medical) journal market. [112] While clearly the commercial STM market is the leader in gouging, others (especially the social sciences) are not too far behind.

Moline provides further evidence of market gouging (Table 2 below) by demonstrating that commercial publishers increased their prices in the years between 1973 and 1985 by almost twice the amount that Association publishers did. Although she enters a caveat that the data provided by Fry and White [113] on which the 1973 figures are based is not strictly comparable to her own, the data remains highly suggestive and when taken together with the comments of commercial publishers and their clear policy of differential pricing, simply adds another nail to the coffin.

Table 2:

Average Cents Per Page, by Publisher Type

Publisher Type	1973	1985	Approx. Increase
Commercial	3.7-4.0	19.3	400%
Association/Society	2.9-3.2	10.4	240%
Other Scholarly	3.0	8.9	200%

Source: Sandra R. Moline (1988), *The Influence of Subject, Publisher Type, and Quantity Published on Journal Prices.*

Other analysts have demonstrated similar patterns as those uncovered by Moline. In his study of 17 major mineralogical, geochemical, and petrological journals, Paul Ribbe [114] found that the commercial variants cost anywhere between 3 and 20 times more than their society counterparts. Commercial publishers indeed seem to get a very good deal from the current academic market. In addition to having access to a captive market, they are also able to focus only on highly profitable journals leaving the dregs (specialised literatures with small markets) for university presses, academic societies and others. This, as White [115] notes, has long term implications for the state of the scholarly disciplines.

> Of perhaps even greater concern is the uncertain support entire subject disciplines would be able to provide for journals published under a laissez-faire system. Journals published in applied science and technology disciplines are the only ones demonstrating continuing operating surpluses of profits. Pure and social science journals hover at the break-even point, while publications in the humanities consistently and increasingly report operating deficits across the disciplines which comprise them. Clearly, a system without subsidies or other buttressing devices would have devastating consequences for research and scholarship in the humanities and could even lead to the demise of all journal publication in certain humanistic specializations. It seems unthinkable that something like this should be allowed to happen.

Unthinkable, maybe, but more likely as time passes. Libraries being crunched from two directions. Journal increases and also increased pressure from business to provide services more suitable for extracting profit.

Other Problems

There are other problems with the scholarly communication system besides delay, proliferation, and rising cost. These problems primarily revolve around the ossification of the status quo in the hierarchical structure and discursive practices of the scholarly communication system. These problems, although raised at various points thus far, we will table until we discuss in more detail the deep structure of the scholarly communication system in chapters 6 and 7. At that point we'll raise them again in order to discuss the links between the deep structure of the scholarly communication system, its links to the status quo, and the potentiality for electronic scholarly communication to etch out deeper structural edifices within which to locate and defend the scholarly status quo. This discussion will involve some penetrating looks at the communication system as well as a critical discussion of the role of the university in the new world order.

Conclusion

Although we have chosen to separate the problems associated with journal proliferation and cost, in fact these two key problems have interacted with each other to create the current crisis. Perhaps the clearest indication of this interaction is provided by Brian L. Hawkins [116] when he notes that, as a result of title proliferation and rising costs, the real buying power of libraries in relation to the total output of our intellectual endeavours has declined dramatically since the early 80s. He projects current trends into the year 2001 and concludes that when the combined impact of inflation and the growth of information is considered, the end result will be that libraries will only be able to purchase two percent of the total information available. This would, says Hawkins, seriously jeopardise societies ability to capture the very essence of our civilisation - the information we produce.

The problem for Hawkins is not necessarily the predatory practice of a handful of publishers, though this is a significant factor. The problem has more to do with the fit between the current state of scientific inquiry, its high cost and high volume, and a library system based on old assumptions about archiving hard copies of material. In the midst of an information explosion brought about by the information age, current models of scientific communication don't work and must be replaced. Hawkins [117] notes that "...libraries will not scale into the twenty-first century using the current model. We cannot afford to provide new buildings to store information which is expanding at this exponential rate, much less acquire, under the current purchasing arrangements, the information itself. A new paradigm, a revolutionary paradigm, must be developed that meets the economic parameters of our institutions..."

What is this new paradigm of scholarly communication that will solve the problems of delay, cost and proliferation? Well, if we believe the agitprop, the solution lies in the realm of light speed communication of scholarly information

on our new information superhighways. With recent technological advances, it has now become possible to replace the old papryocentric system with a new and presumably better system based on the electronic journal. It is to the electronic journal and the new publication paradigms that we now turn our attention.

Chapter Three: Electronic Journals

> It keeps being said, generation after generation, that the then current system of scientific communication is in a dreadful mess, and that something ought to be done about it. Century after century, nothing much does get done about it, except that it swells to ever greater bulk. Our present discontents were originally voiced by Bernal, 30 years ago, and although many other pundits have expressed support for his diagnosis of our ills, and for his proposed remedies, nothing much has been done about these either. [1]

Introduction

If you believe the technopundits, we are standing at the edge of a bridge between universes. Behind us lies the dull, plodding, greyish galaxy of Gutenburg movable type. Before us the endlessly thrilling world of cybercommunication and scholarly skywriting. Dare we take that first step towards a new realm of *interactive* scholarship where our writing will take flight on the electronic highways? Can we overcome the resistance of the status quo? But that's a ridiculous question. We must overcome! Nothing less than the future of the academy depends on our ability to revolutionise the current means of production of scholarly information.

That revolution is currently under way. The electronic journal, that exemplar of cheap and rapid communication, is rapidly becoming a new fixture in the academy. It has almost a seemingly instantaneous transition. A handful of people talked about the possibility of an electronic journal back in the 70s, [2] and a few more actually experimented with the medium. [3] But for them the technology was simply to primitive. A bit more was done by way of experimentation towards the late eighties [4] But again, progress was relatively slow [5] largely because electronic publication has had to face and uphill technical, social and political battle which has hampered its development. [6]

This seems no longer to be the case. Now the electronic journal has moved from a position as a black sheep of the academic world to a new and more exalted position where it threatens, according to some, to topple the established interests and usher in a new infotopia for scholars and libraries. In just a few short years electronic publication of scholarly material has moved from an almost nonexistent trickle, to a veritable flood of new

journal starts. [7] Some sense of the rate of growth is given by the Association of Research Libraries (ARL). The ARL, in its formal attempt to promote alternatives to the high cost of society and commercial journal publication, keeps an up-to-date directory of electronic journals. [8] As indicated by this directory, the growth in electronic journals has been exponential. In 1991 there were 110 journals and academic newsletters listed in their directory. This grew to 133 in 1992, 240 in 1993, 400 in 1994 and 700+ in 1995. There has also been sharp growth in the number of refereed electronic journals from 74 in 1994 to 142 in 1995. [9] The pace hasn't slackened. In January of 1998, the NewJourn archive, operated by Ann Okerson and James O'Donnell, listed over 4900, journals and newsletters available on the Internet. [10]

Transformation

What has happened to the electronic realm to cause the sudden flurry of activity? There are two things. First, previous technical limitations have largely been overcome and second, the introduction of the World Wide Web has blasted through the barriers to usability characteristic of earlier Internet navigation technologies. As a result, the door has been left wide open for the development of alternative publication system on the WWW.

On technical limitations, it is safe to say that at this time, no technical obstacles remain in the way of electronic publication. Although historically hardware limitations have had a severe impact on the ability of scholars to publish electronically, now "technological progress has pushed the state of what is available with routine off-the-shelf systems far ahead of what is required for scholarly publishing." [11] For example, hard drive capability has skyrocketed while cost per megabyte of storage space has plummeted. [12] The power of central processing units has also increased dramatically. From the early 80s reliance on 8 bit technology and deathly slow (8 mhz) speeds, the technology has move to the point where now off the shelf processors operate at 64 or 128 bits and at speeds of up to 500mhz. [13] This increase in power and speed has allowed the development and migration of extremely sophisticated text processing and manipulation packages.

Data communications speed has also increased. This is so on the Internet backbone, where the bulk of data transmission occurs, and also on the users desktop. On the latter, where a few years ago the standard modem speed was 2400 bps, now it is possible to purchase modems that operate at 56,000 bps. Cable technology brings T1 speed to the desktop and business can rent ISDN lines which offer 64k and higher transmission

rates. Most universities have Ethernet networks which operate upwards of 10 Megabytes per second. As to the backbone, current internet transmission rates clock in as high as 45 Mbs. [14] A 1992 Merit Network press release veritably gushes with technological enthusiasm over the incredible speeds of the new Internet technology. [15]

> In five years, the communications capacity of NSFNET has expanded almost 700 times through the implementation of leading-edge technologies, growing from 56 Kbps to T-3. Today the network's backbone service carries data at the equivalent of 1,400 pages of single-spaced, typed text per second. This means the information in a 20-volume encyclopedia can be sent across the network in under 23 seconds!

However despite the considerable technological progress, there are still technical barriers, according to some, which prevent a more pervasive uptake of electronic publication. For example, Erwin Warkentin [16] cites the instability of electronic addressing (URLs) as a factor impacting the credibility of electronic journals. Ejournals have a recognised tendency to move around allot and their frequent address changes make scholars reluctant to assign credibility because of the difficulty of accessing and verifying content. And Warkentin is correct in noting that URLs can change frequently. This is the result of both technical changes (e.g., hardware obsolescence) and regular changes of institutional ownership of journals (thus necessitating not only a change of machines, but a change of university networks). Of course, given the way journals change institutions periodically, it is even difficult for a paper based journal to provide a permanent physical address.

Still, the solution (at least for electronic journals) is a simple and elegant one and failure to adopt it is more the result of a lack of awareness of technological possibilities than it is the result of the "operators of our networks." [17] Solving the problem requires simply registering a domain name (like www.sociology.org for the EJS or www.nissan.com for Nissan motors). The actual cost of this is trivial ($50.00 per year) and owning the domain allows the journal's addressing to remain totally machine and network independent. So for example, the EJS is addressed on the WWW as http://www.sociology.org/. This permanent address can be "attached" to a computer at the University of Alberta, or a computer at UBC. Users can thus be guaranteed that the URL will never change. Certainly this level of permanence and access rivals both institutional archiving and current office addressing schemes which make the physical address of journals dependent on institutional affiliation.

The second development allowing for the explosion of electronic publication is the World Wide Web. It is safe to say that this technology more than any other has contributed to this explosion. Prior to the development of this sophisticated and consistently evolving interface, scholars and entrepreneurs were restricted to difficult to use line mode ASCII interfaces that were ugly and counterintuitive. [18] Though there were examples of "journals" published via listserv or majordomo mailing lists, these were simply limited and primitive and didn't amount to more than extended informal colleges. Now, however, the technology has matured to the point were professional quality publications that serve the traditional dissemination and social functions of the formal journal are becoming practical. The result has been quite remarkable. Ejournals can now be said to be driving change since the elimination of the more profound obstacles has allowed them to demonstrate their efficacy in the areas of speed of distribution, low cost (in some cases), and ease of access.

All the technical virtuosity does not mean that there are not still limitations. Authors still have had to defend the electronic journals on its ability to provide an adequate aesthetic and professional standard. [19] There is very good reason to pay attention to aesthetics. Electronic journals are, after all, attempts to communicate and we must pay attention to the details which facilitate or hamper communication. For example, Martha J. Lindeman, Charles Crabb, John R. Bonneau, and Vera Fosnot Wehrli, [20] point out that with poorly designed interfaces and documents, reading speed can decrease by as much as 30%. Reading speed can be reduced by things like font, print size, kerning, [21] and also by poorly conceived document structure [22] There is also a psychology of reading. Yu Novikov [23] demonstrated that the structure of a document, its logical organisation from general to more specific, and even the presence or absence of highlighting can facilitate or impede comprehension and reading. It is interesting that Novikov situates his experiment directly in discussions about the proliferation of scholarly material and resulting need to ensure that documents are prepared with speed of browsing and comprehension in mind. Finally, as Pullinger [24] notes, there is a psychosocial link between the aesthetic quality of the journal and its substantive content such that a high quality presentation and attention to minutiae suggests to the reader that the same attention given to journal content.

Still, advances in the technology have largely overcome aesthetic limitations and any remaining concerns are well on their way to being addressed. Earlier versions of HTML, the standard text markup language used to communicate on the WWW, were quite primitive, [25] providing only basic control of document form. This was, unfortunately, an intended feature of the original HTML specification implemented by designers who

deprecated presentation concerns and emphasised the structural characteristics of documents. [26] Yet this emphasis on structure over presentation has not gone unchallenged. Philip Greenspun comments: [27]

> HTML represents the worst of two worlds. We could have taken a formatting language and added hypertext anchors so that users had beautifully designed documents on their desktops. We could have developed a powerful document structure language so that browsers could automatically do intelligent things with Web documents. What we have got with HTML is ugly documents without formatting or structural information.

This irksome lack of control over document characteristics has resulted in considerable pressure being brought to bear on the WWW community. [28] Savvy entrepreneurs have attempted to resolve the problems by taking the development of WWW standards out of the hands of the WWW governing bodies. The most infamous attempts to overcome the design limitations of HTML is provided by the machinations of *Netscape Communications Corporation.* [29] Recognising the need for control over presentation, they have built into their browser software HTML extensions which allow information providers to enhance the look of their documents and exert more control over presentation. Furthermore, the newest HTML specification, HTML3.2, has included additional presentation and structural tags. And in addition, the new specification has facilities for the presentation of tables and forms and is also incorporating a document formatting template known as Cascading Style Sheets (CSS). [30] CSSs completely overcome any remaining limitations by providing fine grained control over advanced typesetting features. [31]

In the rest of this chapter we will examine the current electronic revolution in the scholarly communication system in order to understand what all the fuss is about. Making use of my own field work in the initiation and development of an electronic journal, we will take a detailed look at some of the advantages epublication offers. However, it is probably useful to note in advance that many of the saving graces of epublication identified by various authors must operate within extant social and political structures. We can certainly expect that the operation of these stuctures will impact on the course of evolution of the electronic scholarly project. However we leave off an extended critical examination of the related issues for the following chapters.

The Benefits of Electronic Publication

Access

> More and more of us are becoming familiar with the Internet Syndrome, where one's colleague appears after an unexplained absence of several days, eyes glazed, hair unkempt, clutching an empty Pepsi can, and mumbling "I just logged on to check my Email and then it was Thursday."[32]

One of the primary benefits of scholarly electronic publication is that it offers vastly increased access to scholarly material. We can utilise the example of the *Electronic Journal of Sociology* to illustrate this. Subscribers to the EJS, if they can be called subscribers since they don't pay the journal an access fee, can read the articles from wherever there is a computer with even partial Internet access. Readers do not have to pay a subscription fee for this convenience and they do not have to make a laborious trip to the library.

This ease of access is highlighted by the hypertext capabilities of WWW publication. Not only can you access the journal, but you can also access source material and citations used in the journal articles themselves. Authors can provide hypertext links to many of the works cited in their papers. Readers are thus easily able to follow links and check on the accuracy of the citations or even make copies of the complete original texts with their local laser printer. Again, no bothersome copying of references, OPAC searches, and trips to the library to track down material or verify references. There is quick, elegant access to all the material needed to study the article.

Doing background research on topics where there is a lot of information online is a breeze. This was true, for example, in some of the background research that I conducted on electronic journals. From my perspective, it was a joy to research earlier papers on electronic journals. A fair amount of material of consequence to electronic publication is already online and freely available. All I had to do to gather the material to write this paper was use powerful WWW search engines, follow hypertext links, browse articles, and print the ones that I wanted to use. The research process was simple, easy and rapid.

To be sure the one limitation of electronic publication is that you have to have a computer. However compared to the power of today’s computer

hardware, the computer requirements for accessing the Internet are quite basic. In order to access the EJS's graphical interfaces, the user would generally need the equivalent of a 80486 microprocessor with an attached high speed modem (14,400 or better), a VGA monitor, and a SLIP or PPP connection. [33] Two years ago, these hardware requirements represented cutting edge equipment requirements. Now, the sort of computer needed to access electronic journals can be had for less than $500.

Concern about limited access has been ongoing over the years and up until very recently there was a fear that the move towards online scholarly publication would damage the ability of institutions in less developed countries to access the scholarly literature. Now however that has changed. Virtually all universities and colleges in North America [34] have been fully wired to the Internet and more and more institutions in developed and underdeveloped countries are coming on line all the time. [35] It is only a matter of time before all institutions (both K12 and University level) are wired into the global information highway. Andrew Odlyzko [36] makes the following projection

> Concern is often expressed that electronic publishing will deprive poorer institutions, especially those in the less developed countries, of access to the scholarly literature. The opposite is bound to be true. Few institutions can afford the $25 M per year that Princeton University spends on its libraries. Yet a T1 connection to the Internet (of 1.5 Mbps capacity) costs $20,000-$30,000 per year in the US, and would suffice to bring in all the scholarly information that is generated in the world, if only that information were electronic. In other countries connections are more expensive, but even so, less than 1% of what Princeton spends will pay for a satellite earth station of high capacity....Therefore electronic publication is the most promising route for scholars in less developed countries to become full participants in intellectual life.

Clearly there is considerable potential here to vastly increase world access to scholarly material. However there are additional benefits. There is, for example, considerable potential in electronic publication of all sorts to enhance the accessibility of information to people with disabilities. [37] Information that is already in electronic form (as more and more information is these days) makes the development of software add ons to accommodate the visually impaired, [38] those with hearing difficulties, [39] and those with motor disabilities fairly straightforward. [40] The structured nature of HTML, and strict adherence to standards, overcomes one of the major

difficulties normally experienced by developers seeking to enhance access to information - namely, lack of consistent electronic representation of information. Because HTML is a standard that is hardware and software independent, developers can create interfaces which, for the most part, can handle all information created for the WWW. [41]

Distribution Speed

Much more interesting than the increased access, from the individual scholar's point of view anyway, is the significant increase in the speed of academic discourse that can be achieved via electronic publication. In the electronic realm, the pace of academic discourse can be accelerated and the long and often frustrating delays associated with the paper realm completely eliminated. This is most clearly evident in the experiences of archaeologists who have successfully reduced their 30 year time lag to virtually nill by utilising powerful information technologies for enhancing the distribution of archaeological research. [42] However, in all disciplines electronic publication offers significant improvements.

There are a number of enhancements to the traditional publication process, facilitated by full electronic publication, that the EJS has been able to exploit to reduce its publication delays. Without a doubt the biggest advantage of full electronic publication is our independence of the postal system (otherwise known in Internet lingo as snail mail). At the EJS we have no postal delays. Submitted papers arrive via email to my own university account. After I give the submission a once over and decide whether it is worth sending out to the reviewers (this usually takes me anywhere from 2 days to a week), I forward the submission to the EJS reviewers. Sometimes there is a processing delay. I might need, for example, to convert the submission to ASCII or HTML format prior to shipping it off for review and this may add a day or two if I don't have the time to do the conversion immediately. But even this delay is insignificant in terms of the traditional delays associated with getting a manuscript to the reviewers.

Our board members and reviewers normally take a few days to no longer than two weeks to complete their review. This is considerably better than the response time of traditional reviewers and I believe we owe the enhancements here to the *immediacy* of the publication process. It is much easier for reviewers in the paper realm to sit on papers. They all know that reviewing papers takes time and, after all, the editor can never be sure how long the paper took to reach the author and how long the comments will take on their return trip. This gives reviewers a certain temporal leeway in

their decision to finally pick up the paper to review it. This psychological leeway does not exist in the electronic realm since it takes only minutes for a paper to reach the desktop of a reviewer. This immediacy, coupled with the fact that the paper is right there in front of the reviewer's eyes when they read their mail in the morning, virtually ensures a rapid turnaround of material submitted for review. Not, of course, that I have not had on occasion to remind reviewers of their duty. But from my understanding of the problems that editors in the paper realm face, the magnitude of this problem for the EJS is hardly worth commenting on.

Once I have received the comments of the reviewers in my email box, I have to make my final decision. Again, depending on my workload, this can take anywhere from a few minutes to two weeks. When a paper is rejected, I need to consider carefully the decisions of the reviewers. On occasion I have had to go back to the reviewers because I thought a paper that failed to get by the peer reviewers was worth publishing. Still, the entire process is greatly accelerated in the electronic realm and any concerns are usually resolved with the exchange of a few email messages. At most, resolution of the fate of ambiguous papers would take a week. Usually, however, it would take much less time.

After the decision concerning the disposition of the paper is made, I inform the author electronically. If the paper is accepted, or after the author has completed revisions, the next step is to format, copy edit, and convert the submission to the HTML and ASCII formats required for the journal. Without interruptions, this currently takes about two hours depending on the complexity of the piece.

Once the HTML markup is completed, the article can be sent to the author for a final once over and then placed on the web page. The total estimated time from submission to publication is 7 days to 2 months and in ideal cases (i.e., those cases that do not require revisions or substantial editing), two days is not unreasonable! There are of course some instances were delay enters into the process. But the delay here normally occurs only when the author has to undertake revisions to the paper suggested by myself or the peer reviewers. Since most of our submissions require some sort of revision, these delays are regular occurrences. However, perhaps as a direct result of having had their submissions handled quickly, most authors complete their revisions apace. [43] So even with this added delay, it is still possible to publish a paper in as little as a month from the time it was first submitted.

This is clearly a quantum savings in the time it takes to publish scholarly material. In the paper realm, even with the addition of electronic peer

review, the time from submission to publication can be a year or longer. Two months seems remarkable in this context. To sum up, Electronic publication is capable of completely eliminating the long standing problem of publication delay in the scholarly communication system.

The increase in the speed of scholarly discourse has some ancillary benefits. For individual authors there is the ability to receive feedback on papers while their work is still fresh in the mind. This is particularly significant for those papers that require revisions. Having to wait six months or a year for the final word on the disposition of a paper has, as noted by Harnad, a cooling off effect on the author. There is no guarantee that the author of an otherwise excellent paper requiring revisions will undertake these revision after an extended period away from the content of the paper. True, sometimes the author may remain in the content area. But in other cases the author may have moved on an the paper might simply die. It is reasonable to expect that if the author's attention can be maintained by a fast review and publication process, it would be much easier for them to undertake the required revisions.

Graduate students just beginning their career would perhaps benefit the most from the speedy turnaround time of electronic publication. No longer would they have to wait months or years for feedback on their efforts. Instead, instantaneous review would mean instantaneous feedback. Rather than taking two or three years to run through the publication and revision gauntlet, graduate students would be able to learn about the nuances of getting a paper published at a more organic and useful tempo. This faster pace would allow graduate students to experiment with their own writing style and publication strategies. The ability of electronic publication to place graduate students in a fast loop may enhance their learning by providing them with a greater opportunity to learn the craft of paper publication through more rapid and regular feedback. With enough electronic publication outlets, graduate students would be able to submit a paper to 2 or 3 journals in a single year and run through 2 or 3 (or more) different versions of the paper. An attentive graduate student could learn much from these various publication attempts. This would go a long way towards eliminating some of the difficulties and inequities in the process noted in the last chapter and place graduate students from smaller institutions, or at institutions with less faculty support for their publication efforts, on a more level playing field.

Fast electronic publication also has the potential to reduce duplicative effort by making material available shortly after a research project is finished. This potential may have more relevance in other disciplines since in sociology the breadth of content is so wide as to almost preclude scholars

duplicating someone's research down to the minutest detail. Yet the potential is still there. Unfortunately this is not an area that has received much attention and in the future it would be worthwhile to examine the area in more detail.

Developing countries can benefit greatly as well. When I put a new paper on line, everyone in the world is able to access it at the same time. Scholars in developing countries need no longer wait while the postal system and poorly operated administrative apparatus deliver them their subscriptions. The speedier distribution of a scholar's work in the electronic realm, while not eliminating the need for invisible colleges, will at least reduce their importance. This will have obvious benefits for graduate students, underdeveloped countries far outside of the research loop, and scholars attempting to switch specialities. While it may be a bit of a leap to say that the speed of electronic publication will democratise the academy, it will at least level the playing field a bit by reducing the lag between the onset of a product and its final public availability. This is especially so if the majority of the emerging electronic publications follow the new paradigm that emphasises unrestricted access and free distribution.

Cost

> Is the Net in principle different from a telephone? Does anyone charge for the CONTENT of my phone calls? Ah, but scholarly research reports are not just informal chit-chat, one might reply; a lot of work has been put into them, not only by the author, but by colleagues, referees, editors, etc. Moreover, unlike evanescent telephone conversations, the scholarly literature must be preserved and made accessible to all. All this costs money. Fine. Let the true expenses of using the medium and of producing and preserving its text be made explicitly, and then shouldered either by the "promotors" of scholarly productivity (universities, learned societies, government, society) or by the individual "consumers" of these texts (the scholars themselves). I happen to lean strongly toward the first alterative, [sic] because I think making scholarly information freely accessible to the individual scholar gratis makes for the best scholarship for all of humanity. But even if we do elect to make individual scholars pay for access to one another's work, let us make sure that we do not add on spurious surcharges

> that are merely holdovers from the obsolete papyrocentric model. [44]

The final beneficial aspect of electronic publication that we will examine in this chapter is its cost. There can be no doubt that publishing a journal with the aid of advanced information technology reduces cost. Even traditional publishers will admit that introducing information technology into the production loop results in substantial savings. Steven B. Silvern, [45] editor of the *Journal of Research in Childhood Education,* notes that the introduction of electronic page processing cuts production time and costs from between 25% and 50%. The editor of the journal *Hispania* noted savings in postage, document processing, photocopying, editorial time (including a more streamlined reviewer selection process facilitated by a key word look up of curricula vitae), costs of manuscript preparation, and space with the move to a completely paperless editorial office. [46] Jane Lago of the University of Missouri Press has also introduced IT into the editorial office. She reports savings of between $500 and $1000 per manuscript. [47] Indeed, the terrain has shifted so thoroughly that journal editors are now getting their publication points by providing tips to other traditional paper journal editors on how to use information technologies to enhance the publication process. [48]

If traditional publishers can use IT to eliminate steps and streamline the publication process, we can reasonably expect that full electronic journals can win additional savings. Estimates as to the cost savings of publishing material in electronic-only format range from a low of no change of paper based publication costs to a high of 75% [49] One explanation of the variation in emphasis focuses on the discipline specific requirements of scholarly publication. The cost of publishing humanities journals where typesetting requirements are minimal is lower than say publishing chemical journals where complex tables, math, graphics, and special characters need to be incorporated and where the labour required to incorporate these is intensive. [50] An alternative, and one pursued in Chapter Five, focuses on the need for traditional publishers to maintain their interests in the scholarly distribution system by inflating the actual costs of electronic journal production.

We can perhaps get a better handle on the situation if we consider the various processes that are involved in the construction of a paper journal. Table 1 gives a basic analytic breakdown and one estimate of the cost distribution of the various functions of journal production. [51]

Table 1: Estimated costs of Journal Production, 1975

Expense	%
Editing Labour	25
Typesetting	25
Printing Labour	25
Paper	10
Postage	10
Other	5
Total	**100**

Source: Metz and Gherman (1991) derived from Economic Consulting Services Inc., "A Study of Trends in Average Prices and Costs of Certain Serials Over Time," report to Association of Research Libraries, 1989.

Starting at the top of the table there is editing labour. This category includes things like handling the submission of manuscripts and their routing to relevant reviewers, correspondence with authors and other organisations, etc.. If the manuscript has been accepted, there is content and copy editing and general preparation of the material for formal typesetting and production. Typesetting, printing, and paper costs need no explanation. They are the actual production costs that go into the creation of a paper journal and as indicated in the table above, they make up the majority (60%) of the cost of producing a paper journal. Postal fees enter into the process at all stages. Though these are obviously more significant during the actual production and subsequent distribution of the journal, they do enter into the equation as the costs associated with the reproduction of manuscripts for file or reviewer copies, and the delivery of manuscripts to review or peer editors through the regular postal system.

Electronic publication introduces efficiencies at every stage of the production process. E-publication carries with it the potential to handle submitted texts electronically. On the one hand this eliminates the need for a number of intermediary or support positions. Rather than relying on administrative assistants for the reception and subsequent processing of the manuscript, editors who receive submissions electronically can simply forward submissions via a simple keystroke to peer reviewers who then make an optional printout of the paper and email their comments back to the editor. Most of the administrative tasks, like record keeping and creating duplicates of submitted papers for files, can be handled automatically or with a few short keystrokes. With only a moderate amount of technical savvy, the editor of an electronic journal can fairly easily handle all the administrative tasks associated with the reception and distribution of submitted manuscripts.

Papers that are finally accepted can also be handled electronically again significantly reducing handling costs and administrative overhead. Typesetting, which in the electronic world amounts to nothing more than formatting the document and converting it to ASCII, HTML, TEI, TeX, or Postscript for distribution, can be done either by the editor or by a part-time editorial assistant. Here the potential for streamlining the process is enormous. Because the texts already exist in electronic form, it is a relatively simple matter to write software or word processor macros that assist and partly automate the conversion process. In this way the editor is able to handle the typesetting and production requirements of publishing a scholarly journal. Should editors choose this route, the slot normally associated with editorial assistants can be eliminated altogether.

Publishing an electronic journal of course completely eliminates the cost of postage. All submitted papers are routed via electronic networks thereby eliminating these minor postal fees. And, the actual journal issues can either kept on a local Internet server, or distributed via electronic network to individual subscribers. In either case, the cost to the journal is negligible.

Since electronic journals eliminate postage (10%), paper (10%), and printing labour (25%), a conservative estimate of the reduction in cost of publishing an electronic journal is 45%. This figure assumes that editors rely on the traditional support mechanisms for processing, copy-editing, and typesetting manuscripts. However if editors follow a paradigm that emphasises their personal involvement in the publication process, then further savings can be introduced. It is certainly possible for editors to rely on email, programs that automate functions, and HTML or SGML macro packages to streamline the production process.

Further savings can be realised by dropping the traditional over emphasis on the costly professional services normally performed by publishing houses. Odlyzko [52] argues that the pretty page covers, aesthetically pleasing page layouts, and article and citation standardisation (which are the "value added" services provided by professional journal producers) are artefacts of a system of scholarly publication once removed from the scholars themselves. He further argues that if scholars were presented with the true cost of providing these services, which he estimates at about $4,000 per article, and in fact were forced to pay for these true costs rather than relying on library and institutional subsidising of the costs of scholarship, they may in fact choose to get by without these value added services. There of course limits to which we can take the utilitarian philosophy as we will see in Chapter Four. However Odlyzko's words are worth thinking about.

Given the possibility of editorial involvement in the production process, and the potential savings if irrelevant production costs are eliminated, it is reasonable to add an additional 25% to the reduction in cost should this scenario be adopted is not unreasonable. At this point the total reduction in the cost of producing the journal is 70%.

The only significant costs associated with publishing material electronically are those associated with the efforts of the editorial board and the peer reviewers of the journal, and those costs associated with storage and transmission of electronic texts. As to the former it is important to keep in mind that editorial functions are traditionally provided free of charge to journals. Most editors are not paid for their services and it is extremely unusual to remunerate editorial board members or peer reviewers. [53] There contributions are most often done on a volunteer basis though it could be argued that their respective institutions pick up the tab for the time they spend on the journal or reviewing submissions. Yet even if we were to factor in the cost of the volunteer editorial and review functions, the benefits of handling texts electronically would still reduce the cost in comparison to that associated with paper publication.

As for electronic storage and transmission costs, these are now quite trivial. In 1994, Paul Ginsparg [54] noted that cost for a gigabyte of storage was under 700$. This meant that the 25,000 physics papers published each year could be stored for about 3 cents apiece. Since that time the cost for a gigabyte of storage has plummeted to about $100 a gig thus further trivialising the cost of storage. However even in 1994, Odlyzko could conclude that the cost to store all current mathematical publications would be less than the subscription cost for one paper based journal! [55] As to the cost of Internet connects, these are generally shared among all members of an organisation. Odlyzko [56] noted that even with the recent withdrawal of NSF support for the Internet infrastructure and the move to commercialisation, academic storage and transmission should remain trivial because network transmission will have to remain cheap enough for commercial applications (pictures, movies, etc.). He concludes by noting that the cost of fast Internet connect will remain less expensive than the cost of a good collection of paper journals for only 1 discipline.

The EJS is a good example of the type of cost reduction that can be achieved. From the very start we have exploited the potential of the information technology to the limit. Although we have a letterhead, most of our correspondence is electronic. Papers are submitted and distributed for peer review electronically. Authors are informed of revisions and rejections electronically, and papers, once accepted, are formatted and typeset on my computer using software freely available through public domain,

shareware, or the GNU public license. [57]

Because I have the responsibility for copy editing, typesetting, production and distribution, I have been motivated to learn to program macros in various word processors and to learn the powerful programming language PERL. [58] I have thus been able to write scripts and programs that take over many of the menial tasks of publishing a journal. In addition, the power of web software continues to increase and recent additions to the HTML specification promise to make the work of publishing high quality HTML documents significantly easier than it is now. The most recent HTML specification (HTML level 3.2) has included a powerful specification for Cascading Style Sheets (CSS) that dramatically increases a publisher's ability to easily markup and control HTML documents. [59]

With style sheets, publishers will have easy and total control over all elements of their HTML pages. The power of stylesheets reside not only in their ability to more easily standardise the look and feel of web publications, but also in their ability to reduce the work required to create good looking HTML documents. In the past, creating a nicely formatted and professional looking publication required extensive knowledge of HTML tricks and continuous tweaking of HTML code. With style sheets, HTML markup can be reduced to standardized elements and special effects, like margins, can be applied across all relevant documents in an HTML tree with a single style sheet command. With these powerful developments, the work of actually producing an issue of the EJS has decreased. When I first began work on this dissertation, I estimated 8 hours of editing and typesetting to produce one volume of our journal (assuming 2 or 3 articles). I could now easily produce an issue in less than 5 hours.

The EJS is also able to rely on informal institutional support in order to reduce costs. Being located at a large university, the EJS makes use of the disk space, UNIX accounts, and Internet access routinely provided to staff and students as part of the operating costs of the university. While the university of course buys the equipment and pays the Internet access fees, these are now an essential part of the teaching and research functions of the institution and are thus provided at no charge to members of the institution. Because of this, there is no reason to believe that access to these services will ever be restricted (in fact they will probably be expanded) or that staff or students would eventually have to pay for the services. And in any case, the total cost of the required hardware support for the EJS, when calculated across the 30,000 members of the University of Alberta academic community, is trivial.

To summarise, electronic publication of the EJS eliminates the traditional

postage and production costs of the traditional journal. Editorial and administrative tasks are handled voluntarily and with the help of information technologies. Software charges are handled under the GNU public license and, the infrastructure and equipment costs are born by the University of Alberta as part of the general cost of operating a research and teaching institution. The actual yearly cost of publication, not considering the hidden costs which the university absorbs, is $0.00. This is truly revolutionary. If all journals were to follow the example of the EJS, scholarly communication would, in a few years, be not only cheap, but unrecognisable.

There are many commentators who have recognised this potential revolutionary impact of electronic journals. Revolutionary is an apt term since scholars publishing scholarly material for nothing (or close to nothing) completely changes the playing field. Some have even started to develop new models of scholarly communication designed to popularise, provide insight into future directions, and advance scholarly electronic publication. In the final section of this chapter we'll take a look at these new models of scholarly communication.

Alternative Peer Review Practices

> In addition to reducing the financial burden of scholarly communication, increasing access and improving distribution speed, ejournals also have the potential to dramatically rewrite the rules of scholarly publication. The potential here revolves around the nature of electronic communication in general, and the WWW in particular. WWW publication brings with it the ability to introduce a level of interactivity to the scholarly publication process simply unheard of (and impossible) in the traditional realm. This interactive potential can, if wisely implemented, act directly on such hallowed (and misunderstood) publication processes as the traditional peer review practices used in the primary journal.

In the world of electronic communication (as in the world of traditional paper distribution of scholarly research), the issue of peer review is an important one. Stevan Harnad [60] has written extensively about the need to implement traditional peer review practices in the world of electronic scholarly communication in order to ensure that scholarship remains an institution respected for its lofty truth seeking function. For the most part, nobody has disagreed with Harnad's prescriptions. This is for good reason I think. As far as peer review goes it provides a useful check against scholarly excess and error. Without some form of peer review, a

considerably greater part of scholarship would be worth less then the paper it is written on.

However there is a need to look in more depth at the traditional peer review process in order to improve it. While peer review in the scholarly disciplines is a bit of a sacred cow and many scholars believe that the system of peer review is an ideal gatekeeping mechanism, there remains a very real need to overhaul the system. This is particularly event when we consider the years of research in various scholarly disciplines which has clearly demonstrated that the peer review process is far from an ideal mechanism of quality control. Even a peripheral sampling of the research in peer review supports this. Early challenges to the integrity of the system came from psychologists who found evidence of systematic bias in the peer review process. [61] Others, like Gardner [62] and Grogan [63] challenge the gatekeeping process of peer review by pointing out that the scientific communication process has not always proved a stopgap against ridiculous or bogus claims or by pointing out that major contributions to disciplines can be overlooked by even the most prestigious journals.

In addition to the empirical challenges of the extant peer review system, philosophers of science have recently begun to criticise the epistemological foundations of formal scientific communication. Writers like Alan G. Gross, [64] David Locke [65] and others [66] have challenged the privileged position of science as the road to truth by arguing that there is an underlying social and rhetorical dimension that needs to be considered. These authors further argue that the essential rhetorical nature of the scientific communication process is systematically under played or ignored, or that its actualisation in the peer review process is lopsided. Yet scientists of all stripes prefer to see their disciplines as being capable of unproblematically referencing the natural or social world. Both Gross and Locke competently demolish the naïve realist assumptions of science and replace, or more appropriately embed, the processes of scientific truth finding in a context that accepts and emphasises the rhetorical nature of scientific communication.

If we accept the fact that a) peer review is far from perfect and b) peer review is intrinsically rhetorical, then we can ask an extremely important question. [67] How can we use the new technologies to improve the peer review system? A useful approach is provided by Habermas's conception of an ideal speech situation. Gross [68] paraphrases Habermas and describes the ideal speech situation in the following terms. 1) The ideal speech situation permits each interlocutor an equal opportunity to initiate speech. 2) There is mutual understanding between interlocutors. 3) There is space for clarification. 4) All interlocutors are equally free to use of any

speech act. 5) There is equal power over the exchange. As applied in the context of peer review, Gross [69] notes that ideally "scientific peer review would permit unimpeded authorial initiative, endless rounds of give and take, [and] unchecked openness among authors, editors, and referees.

Peer review as it is traditionally practised is not an ideal speech situation. That is, it does not conform to the dictates of good rhetorical practice. There is a strong element of hierarchy and authority in the process. Except for the initial act of initiating the peer review process, author's almost never initiate speech. There is no opportunity for clarification since only one round of referee critique and editorial comment is possible. Flowing from this is the inability of ensuring mutual understanding since the author can't respond directly to reviewers, nor can reviewers query authors about specific problems with the submission. That is, there is no possibility of interactive clarification. Rather, authors must "reply to every question and respond to every criticism despite the fact that, were interaction possible, some questions might not have been asked, nor some criticisms made." [70]

Gross discards the possibility of an ideal speech situation in peer review and argues against the possibility of even approximating an adequate representation of the process. He further argues that moving in the direction of the ideal speech situation in peer review via conventional means would put undue strains on the whole process and would also require more time of the peer reviewers who would have to be engage in a critical debate for longer periods. "A decided movement in the direction of the ideal speech situation would improve an author's position only at the expense of the professional time of editors and referees." [71]

However, with the advent of information technology, it has become possible to move in the direction of repairing the peer review process, at least at the rhetorical level, by introducing a form of peer review that allows online dialogue and debate between authors and reviewers. The exact form and content of this interactive peer review has yet to be investigated and I reserve a more detailed discussion for a latter date. But just in case someone else is interested in this area, suffice it to say that there is considerable potential to "fix" peer review by using information technology to move the process in the direction of a more ideal speech situation. For example, by using technology to allow authors a voice in the peer review process, it would be possible benefit to achieve a more objective and rigorous peer review process with additional and heretofore impossible checks against reviewer bias and finagling. [72] When the author is able to openly challenge certain statements of peer reviewers as inappropriate, faulty, or even in error, the reviewers are forced to reconsider their comments in light of information or clarification provided by the author.

There might also be more general benefits. I think we would all agree that the peer review process as it is currently practised in the paper realm is unwieldy and intransigent. Because of the long delays and relative powerlessness of the author, traditional peer review can discourage development of submitted paper. Authors faced with a process where they can only listen and not respond are less likely to make suggested revisions and more likely to seek alternate outlets which ask less questions but publish more papers. This is unfortunate since peer reviewers may make useful suggestions and no doubt almost all submitted papers can benefit from at least one round of peer review. However the inability of the author, who it should be remembered can also be considered an expert in the field, to respond to criticisms that may be irrelevant, misplaced, or based on a faulty understanding of key sections of the manuscripts, virtually dooms the submission to mediocrity despite the appropriate criticisms of peer reviewers. An unfortunate result is that the time, energy, and work of both authors and reviewers is lost.

We at the EJS had planned on formally investigating the potential of a transformed peer review process. However our low submission rate has forced us to table a formal investigation of the possible benefits until such time as our submission rates increase to a level that would accommodate a formal investigation. In the mean time it would be useful for someone to provide a more thorough review of the literature criticising peer review. This review would perform a number of useful functions not the least of which would be to diminish the status that the traditional peer review process holds in the eyes of many scholars and pave the way for an acceptance, or at least an openness to trying, new models of evaluating submitted manuscripts.

Realising the Benefits of Epublication: New Models of Scholarly Publication

So far in this chapter we have concerned ourselves with some of the potential benefits that publishing scholarly research in electronic journals could bring to the academy, libraries and individual scholars. However actualising these benefits will require considerable thought and concerted collective effort. Much of the effort so far has been directed towards the explication of new models of scholarly communication which attempt to harness some of the potentials of information technology to, for example, reduce delay or cost. Commentators have, in general, emphasised the key role of the central information providers (i.e., authors and societies) and distributors (i.e., libraries) in effectuating meaningful change in the scholarly communication process. Because of this, it is possible to call the new paradigms "craft models "of scholarly publication because they

emphasise eliminating middle people in order that the actual producers and consumers of scholarly information can be responsible for its distribution.

Libraries and information specialists have been at the forefront of discussions on new models of scholarly communication. Many have argued that information specialists need to actively pursue strategies to reduce the costs of acquisitions and management. Indeed libraries must rethink their position in the scholarly communication system and develop such active strategies if they are going to ensure long their term viability· Indeed, there is a certain pressure on libraries to take up new roles not only to reduce costs of STI publication, but also to preserve for themselves some useful role in the future system.

This on libraries will likely increase when, and if, more scholars begin publishing their own journals. As Dilys E. Morris [73] notes of libraries, "If we do not take an active role in shaping access to electronic information, then it is highly likely that the growth of electronic information and telecommunications networks for information transfer will diminish the role of the library and librarians in the dissemination of information." Ross Atkinson in his editorial in *College and Research Libraries* states explicitly the dynamic and the threat to libraries. [74]

> If, as now seems likely, many of the services provided by publishers and libraries in the current print environment will be done increasingly by writers and readers for themselves ... opportunities for both libraries and publishers to provide their services to users well may diminish ... the plain fact is that there may not be enough room in a primarily online environment for both academic libraries and commercial publishers of specialized scholarly information to grow and to remain key players in the academic information services arena. It is possible that libraries, if they are to continue to fulfil effectively their functions as primary service agents, will decide either to take on additional responsibilities for specialized scholarly publishing - or that publishers in order to survive and expand, will need (and will have the technical capacity) to assume many of the mediation and distribution functions previously performed by libraries.

From the perspective of the academic library, there seem to be three models for a new STI system,. All of these are mutually compatible and all should be adopted in a co-ordinated effort. Still there are weaknesses in each model and these have to be recognised and worked around. Given

these weaknesses, the argument is that only by adopting features of all these proposed models will libraries be able to effect significant change. This becomes particularly evident when we recall the current conservative ideological environment. In order to resist the neoliberal push, libraries will have to take an active role in usurping commercial prerogative.

The first option is the weakest in terms of addressing the underlying sociological and political issues. It is put forward by Dana Rooks [75] who suggests that in the new Virtual Library, librarians will have to adopt a role akin to the support personal who currently staff the support service lines of large software or hardware companies. Rooks justifies this scenario by supposing that in the new virtual library, virtual patrons will require the same sort of assistance they have always needed in negotiating the labyrinthine resources of the library. Rooks notes: [76]

> But what mechanism will supplant the reference librarian at the desk? Will libraries establish help lines or user-support 1-800 numbers? Will we staff terminals for e-mail questions? I say why not? We are librarians! We help our patrons search for, locate, and obtain documents and information....This is nothing new, it's not terrifying, it's what libraries and libraries have done for centuries.

This is a laudable and desirable position. While the extremely powerful WWW search engines now available on the net make OPAC systems look positively primitive, since they search out text in the entire document, still the explosion of material has put intense strain on the ability of the end user to acquire quality information. For example, a search of www.hotbot.com for the phrase "Karl Marx" yields close to 24,000 pages of information. Librarians obviously have a role to play here in sifting through the wealth of information available. Indeed, this role has been recognised of late. The Australian Society of Indexers has, for two years running, offered awards for the best "web indexes." [77] In addition, they are now offering online course material and class work to help individuals learn useful strategies developing web indexes. [78]

However there are some oversights in Rooks scenario. In the first place, the scenario does not address the real issues that are relevant to the survival of libraries - namely the cost of journal subscriptions. Second, Rooks is suggesting libraries adopt a completely passive and service orientated role to the changes that will be brought by electronic publication. [79] This leaves librarians and libraries in their current position as prey to the commercial publishing houses. This is perhaps an inappropriate stance given the potential of new information technologies to allow both scholars

and librarians to take an active role in the production and distribution of scholarly knowledge. Third, Rooks seems to be pinning all her hopes on the single role of reference librarians. This weakens the strategy by placing all the eggs in one basket.

The second alternative is that libraries will have to move, because of financial pressure, towards an *access*, as opposed to an *ownership*, model of information delivery. [80] The ownership model emphasises the ability of libraries to purchase the total universe of material in their areas of speciality. However as many commentators point out, with soaring cost and rapid proliferation of all types of content, that is no longer possible. [81] The access model emphasises the exploitation of network technologies and corporate licensing agreements in order to provide "timely, rapid, and electronic assess [sic] to scholarly resources held by other libraries and document suppliers world wide. [82]

There are three alternative approaches to providing "access" to scholarly materials none of which are mutually exclusive. [83] On the one hand, regional libraries can cooperate to provide document access. This can be done in one of two ways. The first way is to develop consortiums and co-operative lending arrangements between local or regional libraries. [84] Because information technologies make document retrieval and transmission to remote libraries simple, the logistical difficulties of interlibrary loans are eliminated. In the mid seventies to early eighties, this led to the creation of fifty-three regional consortia. [85] More recently, large umbrella organisations have taken a more active role in organising their member institutions in order to develop co-operative lending agreements and ILL (interlibrary loan) services. [86]

This approach to the serials crisis is not as effective as one might think. As Dennis Carrigan [87] has pointed out, it is a relatively costly solution since the average cost of an interlibrary loan (taking into account administrative and transfer costs to both libraries) is over twenty-nine dollars per article. In any case, given the ongoing concern in the literature with the crisis in scholarly communication, and the decades long experiment in co-operative lending which still hasn't significantly reduced the literature decrying the scholarly information crisis, consortia are clearly not an adequate solution to the library crisis in and of themselves.

An additional approach, still well within the access model, that is being investigated and successfully implemented is for libraries to provide access to document delivery services. [88] Although, as Carrigan points out, this approach generally tends to be cheaper than interlibrary loans "especially if delay is assigned a cost," there are still fees for the service and, unlike

interlibrary loans were the charges are absorbed as part off the libraries operating budget, document delivery charges show up as fees to be paid out of the capital pool of the library. [89]

The final alternative approach to providing increased access is provided by direct electronic access to entire journal collections. Publishers can provide an arrangement similar to how software is now provided with a site license to major institutions. Software that is licensed to an institution can be used freely by all members of that institution on an unlimited number of machines. SPSSX, the statistics software popular among social sciences, provides such site licenses to institutions. This enables specific institutions to include the software in all their computer labs, and also allows them to provide take home copies for their staff, faculty, and students at greatly reduced rates. The same model can be applied, [90] and no doubt will be applied, for accessing electronic journals. Libraries will be required to pay a fixed fee for unlimited access to a range of electronic material. Putting aside concerns about cost, the potential for better patron access is enormous. Electronic journals are never "of the shelf." A single copy of the journal can be read by numerous patrons in different locations simultaneously. Links in OPAC can be 'live' and the processing of interlibrary loans becomes trivial. [91]

While all the electronic bells and whistles will no doubt enable libraries to improve their levels of service and their ability to provide timely access to material, it is an open question whether this new publication paradigm will result in reduced costs. In the first place, a complete shift to an electronic library is unlikely in the near or even moderately distant future. At least for the next 10 years, [92] and probably for considerably longer, libraries will have to deal with a combination of traditional and electronic document systems. Libraries may then have the added burden of maintaining traditional collections while purchasing the necessary equipment infrastructure that will enable them to provide electronic access. On the other hand, the benefits of cost reduction that electronic journal publication can bring are not guaranteed. As we will see in Chapter Five, traditional publishers are, unsurprisingly, unwilling to give up their hegemonic control over the scholarly distribution system.

An additional disadvantage of the access model is that it suffers, like other options we have examined, from excessive passivity on the part of the academic library. Whether libraries approach access through consortiums or through arrangements with publishers, the publishers will continue to hold the high ground in the scholarly communication system. This will likely work to the long term disadvantage of libraries. Ultimately it is important to recognise that many publishers are commercial publishers and they will be

unlikely to offer the library substantial reductions in cost. It is perhaps more reasonable to assume that publishers will continue to see the library as a captive market and will seek ways to maximise their profits while minimising their costs.

Because of the weaknesses in these first two approaches, a much better alternative for libraries, one that is technically and financially elegant , and one that mirrors the approach taken by the craft approach advocated for individual scholars and societies, is to pursue their own publication initiatives. One early step in this direction was taken by the University of Virginia *Electronic Text Centre which* provides unprecedented levels of access to electronic books. [93] And while the University of Virginia text centre has made no moves to distribute their own electronic journals, there is no technical or social reason why libraries could not publish scholarly material.

There is pressure to move in this direction. Frank Quinn and Gail McMillan [94] outline a plan, based on the example of the University of Virginia's text centre, whereby for $200,000 a year, a single library would be able to support 200 journals. This puts the cost of each journal at $1000 per title. This doesn't sound that impressive but the authors note that if 50 libraries pursued the same goal, the consortium would be able to provide unrestricted access to all journals in the network for less than $15 per title. Now that is an impressive figure.

Libraries need not attempt to develop such a comprehensive publication strategy like the one outlined by Quinn and Gail - at least initially. Individual attempts to compete with the commercial presses can significantly shift the ground. Significantly, there have been moves in this direction quite recently. As noted in the introduction to this dissertation, the ICOLC has recently made its political position vis a vis the commercial presses clear. Following on this announcement, the American Chemical Society and a coalition of university libraries has agreed to publish a title that competes directly with the high priced commercial title Tetrahedron Letters. [95] This attempt to replace the high priced Elseviar title sets an important precedent. Not only does it unite societies with libraries in publishing efforts for perhaps the first time, but it unequivocally creates an active role for libraries in the scholarly communication system. If this initiative is successful, we can expect many more like it in the future.

Up until now, libraries have largely stood alone in their attempts to reform the system. True, a few scholars have pushed for alternative models,[96] but by and large there has been no co-ordinated effort. That is changing as both individual scholars, scholarly societies and even the editors of

traditional paper journals are recognising the unhealthy state of the system. Indeed, scholarly initiated projects that pursue independent craft based publication are starting to emerge. These projects, like the International Consortium for Alternative Academic Publication (ICAAP) follow a path much like that charted by the ICOLC. The goal of ICAAP is to reduce the cost of scholarly publication at the same time that access on a global scale is increased. [97]

The Information Glut

As you will recall from last chapter, one of the significant problems with the scholarly communication system has been the exponential growth of the primary and secondary literature. This growth, coupled with an explosive rise in journal costs, has been the prime reason why so many Information Science specialists have commented on, and desperately tried to find solutions to, the information crisis.

Electronic journals are unlikely to contribute to a solution to the problem of information growth. Why is this? The answer is simple. Primary journals are not the cause of exponential growth. The real causes of journal proliferation are to be found in underlying structural factors which influence the scholarly communication environment. These factors include: a) the expansion of the post-secondary education system; b) the growth in the output of underdeveloped countries; and c) the publish or perish imperative that encourages a pathogenic pursuit of publication. Let's look at each of these in turn.

A major factor in the proliferation of scholarly literature in North America has been the expansion of the educational system since the early 1960s. Increases in undergraduate enrolment have led to increases in graduate enrolment which have in turn led to exponential increases in the number of PhD awarded per year which means exponential increases in the number of scholars trying to make a name for themselves in the academy by publishing scholarly material. The figures for the U.S., provided by Charles A. Schwartz. [98] tell the story:

> Publication proliferation is not an individual phenomenon but rather one of sheer mass. Consider the trend in U.S. doctoral programs: in 1960, 9,829 Ph.D degrees were conferred; in 1970, 29,866; in 1986, 34,829; in 1990, 38,283.

The expansion of the output of underdeveloped countries has also contributed to an increase in the absolute number of scholars publishing

material. The phenomenon has been observable for at least two decades. Donald King, Dennis McDonald, and Nancy Roderer [99] noted back in 1981 that the *most significant* contributor to journal proliferation was the increasing output of underdeveloped countries.

At root, the problem seems to revolve around the growth in the number of people wanting to publish. If this is the case, Electronic journals can clearly not solve the problem. The number of electronic journals will simply expand to fulfil the demand for publication outlets. In fact, the exponential growth of the literature may even be exacerbated because the new technologies make it much cheaper (despite what the commercial publishers say) to publish scholarly material. Individual scholars, seeking additional publication outlets for their peers and members of their cohort, can easily and simply create their own journal to serve the needs of even very small specialities. We can also imagine a scenario were underdeveloped countries purchase high speed Internet connects and then, wisely, encourage their own scholars to utilise the technology to develop a national publication system. Of course, commercial publishers might also get into the act because they will no doubt find it easier to twig their own journals and, when considered against their resistance to cost reduction, this might have the unfortunate effect of providing more information for scholars and libraries to pay for.

I would certainly not want to advocate cutting of access to higher education or reducing the ability of other nations to participate in the scholarly enterprise. Such an approach to solving the information crisis is elitist and, in a world highly dependent on information of all sorts, ridiculously naive. But something does need to be done and fortunately, there does appear to be a solution.

Perhaps the best way to achieve a reduction in scholarly publication is to focus on the academic rewards structure that currently rewards scholars for publishing as much material as they can possibly turn out in a lifetime and that has encouraged ridiculous publication practices (e.g., salami publication, publication of identical research, co-publication, etc.). [100] In other words, the only way to reduce the flow of information is to reduce the pressure to publish.

Commentators have offered a number of approaches to reducing the pressure to publish. One area that certainly deserves close scrutiny is the current emphasis on research to the exclusion of teaching. Arguably, teaching is just as important to the long term viability of the academy. The academy cannot function without excellent teachers and arguably the student base of an institution is drawn by a consideration of the quality of

instruction. In light of this, policies could be changed to allow those academics who enjoy teaching to focus on the research and development that would enable them to pursue excellence in this area. [101] In other words, teachers could be just teachers. If administrators can change their emphasis to allow for pedagogical development, then it wouldn't be necessary to force academics to maintain a veneer of research activity even when they are not in fact interested. [102] Of course, the academy would also have to take steps to reduce the prejudice against academics who just teach. Even in my brief career I have learned, in no uncertain terms, that being "just a teacher" is an indication of lack of talent and ambition. This must change.

Another approach to reducing the pressure to publish is suggested by Marcia Angell. [103] Her approach is to place a limit or ceiling on the number of publications that would be used for promotion or funding decision. This would involve first an attempt to change the structure of the current system of reward by placing less emphasis on long publication lists and more emphasis on the quality of publications. As she explains:

> The structure of research and publications system within which scientists work and advance must be modified to favor high-quality, thoroughly researched pieces of work published, if necessary, at longer intervals. While bloated publication lists are important to advancement in contemporary science, they really should be grounds for suspicion that the research is being done in a shoddy or superficial manner. Bandwagonism in research focus and excessive preoccupation with trendy instrumentation should be viewed with appropriate levels of skepticism [104]

As a solution, Angell suggests placing absolute limits on the number of articles used in promotional and tenure decisions.

> I suggest that any institution or agency evaluating a researcher for promotion or for funding consider only, at most, the three articles the candidate considers to be his or her best in any given year, with a maximum perhaps of perhaps ten in any 5-year period. Other publications should not even be listed. Each publication would then receive commensurately more attention, both from the research and those evaluating work. After all, there may be a hundred or more papers. If the number were limited, the emphasis would shift from the quantity of the

> research to its quality, and it would become both necessary and possible to evaluate each study. [105]

This solution would certainly eliminate the intense pressure to pressure to publish. It would also eliminate the motivation for publishing fragments of research, poorly developed papers and other forms of academic fluff. Indeed, if individuals are going to be judged on only a handful of their best work, they would be much more likely to find the time and energy required to develop more substantive and sophisticated papers. In addition to reducing the flow of publication, this approach would also benefit teachers who rely on primary publications and who have, in recent years, been complaining about the declining quality of academic papers in primary journals. Placing publication limits on researchers might even reduce the intense pressure to engage in deception and fraud prevalent in the academy.

It is really quite imperative to seek a solution to the problem of literature proliferation. If it becomes easier to publish new journals and twig existing ones, then the crisis in the scholarly communication system could reach epidemic proportions especially since, as we are told by the traditional publication interests, the new electronic journals will not cost less (and may even cost more) than the traditional paper system. If we don't give attention to the rewards system, and if scholars and librarians don't develop reasonable solutions to the crisis, in twenty years or less we may see the scholarly communication system completely destroyed.

Conclusion

In this chapter we have examined the considerable potential of the WWW to solve some of the long standing problems of the scholarly communication system. We have looked at publication delay and access and, more importantly, we have examined in depth the potential of the medium to reduce cost and take a considerable bite out of the scholarly information crisis currently being experienced by libraries. There is considerable hope, I think, if scholars of all ilks can be convinced of the need to develop co-operative solutions to the information crisis.

However we have not told the entire story. We have yet to examine the obstacles that still need to be overcome in order to ensure that the potential of the WWW is fully realised. These obstacles come in two forms. There are the technological and social obstacles which prevent electronic publication from fully replacing the traditional paper based model of scholarly communication. This set of obstacles includes hardware and software limitations, limitations in the HTML standards, and certain

problems with credibility and access. These obstacles, or what remains of them, will be examined in Chapter Four.

In Chapter Five we will take a critical look at a number of additional issues. We will consider in a critical fashion the role of the commercial publication system. Commercial publishing houses are clearly threatened by the new information technology. We shouldn't expect them to just role over and die and indeed, as we will see, they are not. As they move onto the Internet they will try to protect their position. It is up to the innovative scholars who are currently advocating revolutionary change to anticipate their moves and debunk their challenges in order to resist a predatory commercial presence on the Internet. In Chapter Five we will also discuss what problems of the scholarly communication system electronic publication will not solve and, finally, take a close look at the myths of an information utopia that are currently being uncritically propagated along the worlds information highways.

We have also to deal with the dark side of information technology. In Chapters 6 and 7 we break away from a focused discussion on the scholarly communication system and take a broader look at this dark side. The material in these last two chapters is quite different from the previous five. They are also, because almost nothing has been said about the extreme potential of Information Technology to rewrite the rules of scholarship, quite speculative. In them I draw freely from related topic areas in order to piece together an initial heuristic for organising our thinking about information technology. My only goal in these last two Chapters is to force a rethink of the information technology and its potential both inside and outside the academy.

Chapter Four:
The Grand Information Future?

> I'm concerned about the way our excitement over the creation of this new information superhighway is clouding our basic common sense and our critical faculties as members of a democratic society. [1]

Introduction

Up until now we have primarily examined the positive benefits of electronic journals. We have examined the problems with the scholarly communication system and offered the electronic journal as a possible solution to the information crisis. We have recognised that there are still obstacles, but at the same time noted that many of the obstacles have simply disappearing. We have also noted that there is considerable potential for epublication to overcome the financial crisis in the scholarly communication system.

In this chapter I'd like to take a step back by offering an analysis that challenges some of the more optimistic predictions put forward thus far. In particular I wish to look more closely at the traditional scholarly presses. I'd like to follow them as they have first recognised and then responded to the challenge provided by alternative models of scholarly publication. The argument I am going to pursue here is decidedly pessimistic and based on the recognition that the traditional press, representatives of the business interests in the development of Internet technologies, has a vested interest in maintaining the scholarly communication system in forms as close to the old papyrocentric models as possible. This is especially true for commercial publishers who, as we saw in Chapter Two, have been served well by the monopoly nature of the journal system. But it is also true for the academic and society presses since scholarly journals have provided them with a consistent, if not unreasonable, revenue flow with which they have subsidised their other activities.

The Revolution that Wasn't

In Chapter Three when we examined the potential of electronic communication to solve some of the problems of the extant communication system, we also noted the gradual emergence of new models of scholarly publication. We saw how advocates of these new models emphasised replacing the traditional publishers with a scholar/librarian vanguard. Up until two years ago, it seemed likely that these new models would take hold and blossom. But then back then, the only journals on the Internet were maverick startups and random

experiments conducted by those interested in the potential of the new medium. The pioneers of these early journals all recognised the extremely low cost of producing electronic texts, the high speed at which results could be distributed, and the sophisticated access to academic material through search tools and database functions that is possible with electronic publication as benefits likely to seriously challenge traditional modes of communicating scholarly information. [2] Many were commenting on the likely demise of tradition paper based scholarly publication in the next 10 to 50 years [3] and some [4] even attempted to hasten the day when all academic publication would be done electronically and non-commercially by the scholars themselves.

This early concern to get rid of traditional paper based journals was based on a growing awareness of the limitations of paper based publication ventures. As outlined in Chapter Two, traditional scholarly communication has suffered a number problems including a stunning increase in scholarly material, [5] consistent and devastating rises in price, [6] and long publication delays. [7] The situation has been exacerbated by the greed of some commercial publishers. [8] Scholars themselves and the libraries which distribute the scholars work have recently, and after decades of not-so-quite desperation, responded to this crisis by calling for the replacement of the for-profit system by a system controlled by the libraries and scholars themselves. [9] Ann Okerson has this to say about the early dreams of scholars and librarians: [10]

> ...the real hope that many felt had much more to do with the possibility of altering the **sociology** of journal publication: ownership, control and economics. The new electronic scholarly journals were and still are local industry products. The editors were and still are more or less wholly subsidized by their academic or quasi-academic appointments, hardware, software, and network infrastructure provided at no cost to them by generous colleges and universities. In what we already call the "traditional e-journals," all the usual middlemen of publishing had been eliminated: marketing, subscription, accounting, and fulfillment functions swallowed up by the powerful listserv and distribution programs....The ethos of the new journal seemed to be the widest, freest possible distribution.

These early calls for a revolution in the way scholarly communication was to be distributed were accompanied by calls for solidarity. There seemed to be a gut sense, even before the current landscape of electronic publication emerged, that universities, scholars, and librarians would all need to come together to solve the problem. In 1989 Deana L. Astle made these comments: [11]

> They [universities] must realize the seriousness of the threat to scholarly communication raised by information overload

> and the high cost of journals. Involvement must spread to all concerned until the issue is perceived not as just a "library problem," but as a challenge facing the entire academic and research community. Faculty, especially those who sit on journal editorial boards, must be made aware of the issues and understand how they are both part of the problem and potential players in a solution.

The most forceful statement of the power of a co-ordinated effort to overcome the limitations of the current communication system is provided by James C. Thompson's. His comments are based on the recognition that the real stakeholders and the real prime movers are the scholars, libraries and academic institutions. He had this to say in his editorial in the journal *College & Research Libraries*: [12]

> In the long run, though, we hold the most important cards. The raw material of scholarly publishing, the research and writing, originates within the research community, as does the copyright to it. The commercial publishers are in the information conduit for historical and anachronistic reasons; there is no technical or economic reason why they must remain a part of it. Unthinkable as it might have seemed until very recently, the idea of the academy retaking control of the bulk of scholarly publishing is being forced into consideration by the practices of the commercial publishers themselves. Their bills simply cannot be paid indefinitely, and something must give.

It seems from our 1998 perspective that these early calls for revolution were based on an unwarranted optimism about the ability of all the stakeholders to recognize their role in an overhaul of the system. While it is perhaps to soon to be predicting the demise of the revolution, we should note that so far scholars have not (except for a handful) taken up the revolutionary banner. Further, there are also still precious few library initiatives and, sadly, it may now be too late to do any serious reconstruction of the scholarly communication system because the big guns are waking up to the threat and moving into the world of electronic publication.

The traditional publication interests began to stir about three years ago. At that time, R.A. Shoaf, President of the *Council of Elders of Learned Journals* (CELJ) worriedly made the following comments at the CEJL panel at the MLA in Toronto in 1993. [13]

> If we consider the rather remarkable fact that the era of the PC (the personal computer) is barely fifteen years old today and look, in that light, at the revolution it has effected, then I think it is easy for us to predict that within the first few

> decades of the 21st century, even more revolutionary changes will occur at every level of our profession. There is, then, a sense in which all of us are already very far behind. And although we perhaps do not want to embrace the ethos of the current joke in the marketplace, all of us in academic publishing need to wake up to he [sic] reality of these dramatic changes, or we might indeed become "roadkill on the information superhighway."

Nobody of course wants to end up as "roadkill." And all indications would suggest that isn't going to happen anyway. Since Shoaf's initial call to arms, decisions have been made, battle plans drawn up, and troops moved out into the field. Just recently, the chairman of the Association of American Publishers Enabling Technology Committee noted that "Members of the Association of American Publishers (AAP) have decided that they must become actively involved in the deployment of online information distribution systems or get left behind in the dust." [14]

The response of the commercial publishers to the threat of independent scholarly publication has been swift. A string of initiatives has placed a stunning amount of commercially viable textual material on-line for purchase or direct retrieval and more and more, this material is being provided by traditional publishers desperate to get in on the action. [15] In the U.K., the migration of commercial publishers online has been facilitated by the 1993 *SuperJournal* project. This project, funded by the British Library Research and Development Project, was specifically designed to demonstrate the potential of electronic publication to government officials, publishers and the scientific community. [16] Similar experiments have been set up in the U.S. by such big name publishers as Elsevier who have set up a program called *The University Licensing Program* (TULIP) which makes all 1000 Elsevier journals available electronically. [17] Springer-Verlag is also heavily involved on the internet. They have partnered with the University of San Francisco's health sciences division, a host of commercial and society publishers, as well as major international corporations like Bell Labs and AT&T in an experimental service designed to develop a "business model for electronic journals." [18] Smaller publishers are also placing material on line. John Wiley and Sons plans to place all of its journals (326 of them) online as does the Academic press; Taylor and France has 16 of its 125 journals online and we can assume that in the future they will place all their journals up for online access. [19]

Publishers Prerogative

The traditional publishing interests have not only responded with business plans and online initiatives. In the current environment, which some might argue is decidedly hostile to commercial initiatives, this would most certainly not be enough. Coming online after the first truly revolutionary journals had

already demonstrated the ability of scholars to publish their own material without the assistance of the traditional interests, publication houses have found themselves in the unenviable position of having to justify their existence to a world not quite prepared to accept their presence. They have approached this unthankful task in three ways none of which would win any awards for polite competitive practices or scholarly camaraderie. They have engaged in direct frontal assaults on alternative journal projects. They have tried to define alternative publishers as amateur dilettantes who are incapable of surviving in an area of endeavour much to big and complex for them to understand. And they have begun trying to argue, presumably so that they protect their current comfortable financial positions and retain their share of the financial rewards of scholarly publication, that ejournal costs are no different that the costs of regular journals.

I'd like to start the discussion here by examining an incident close to my heart and one that, in my opinion, clearly reflects the contours of the coming struggle between the independent publishers (library based or scholar initiated) and the vested interests who desire to secure a position for themselves on the new information highways. I think the incident is also useful because it reveals the defensiveness of the traditional interests and makes quite clear that they perceive the new models of electronic publications to be a threat.

The incident that I have in mind occurred recently (1996) when a new sociological journal began publication on the Internet. This new journal, *Sociological Research Online* (SRO), announced its presence with much fanfare by leafleting the globe with an announcement of its arrival late in 1995. In their leaflet, they clearly declare their affiliation with the big UK publishing houses and announce themselves as the **first** internet journal of sociology.

> Sociological Research Online, the first fully refereed sociology journal to be published on the Internet....Sociological Research Online is produced by a consortium of the British Sociological Association, the Universities of Surrey and Stirling and SAGE Publications, under the aegis of the Electronic Libraries Programme (eLib) of the UK Joint Information Systems Committee.....For 1996 the journal is free to readers, a unique feature of its electronic availability. [20]

SRO was quite incorrect to claim that it was the first sociology journal on the Internet. In fact, it was predated by at least two other journals. One was *World Systems Research* and the other the *Electronic Journal of Sociology.* This *faux pas* is relatively minor and hardly deserves comment and anyway after I politely informed the editors of their mistake, no subsequent claims about the journal being the first to publish on the Internet appeared. However mistakenly attributing to themselves status as the founding Internet sociology journal was

followed, in the very first issue of the journal, with an editorial statement that directly attacked the EJS as *less rigorous* than their own *traditional* journal. [21] SRO based their evaluation of the relative merit of the two journals on what they knew of our peer review process. They argued that because the EJS did not have a *traditional* peer review process, the journal was therefore, in their opinion, less rigorous.

Such an attack by one scholarly journal on another journal is highly irregular. As far as I or my colleagues were able to determine, no other journal in the history of the scholarly communication system has ever come online with a broadside like this one. Even conceding that the EJS might be less rigorous than the society journal because our peer review process is not quite traditional (a concession that we **do not** make), the attack is highly unusual and unprofessional. Normally, in the world of the academy, questions about rigour and scientific validity are left up to the individual scholars to decide for themselves. For a journal to take upon itself the role of adjudicator of the rest of the scholarly world is outrageous, pompous, and without precedent.

Because of the uniqueness of this event, the board of the EJS was forced to conclude that the attack was prompted by the perceived threat of the EJS to traditional interests in scholarly publication. [22] We further understood the attack to be motivated by a need to disqualify the EJS as an academic publication by appealing to some mythological and completely indefensible ideal of the rigour of peer review. There was also an implicit message contained in the attack. The editors of SRO were saying that only society and commercial publications would be able to supply the scholarly world with the requisite rigour and expertise.

The experience of the EJS is not unique nor is it the only indication that traditional publishers are going to try to construct an ideological and rhetorical landscape that privileges their contribution to the scholarly communication system. In 1995 Ronald E. LaPorte wrote an article in which he proposed the development of a *Global Health Information Server* modelled after Paul Ginsparg's High Energy Physics archive. [23] The details of the service are not relevant here. What is was the fact that LaPorte explicitly and forcefully called for the development of a system that the scholars themselves would control. The medical establishment did not respond well to his proposal. As Bernard Hibbitts notes [24]

> Laporte's proposal prompted a spirited response from the editors of the prestigious New England Journal of Medicine, who argued that the lack of preliminary peer-review in his system not only threatened to undermine "*time tested traditions*", but might potentially cost lives or cause physical harm to patients whose doctors read inadequately-reviewed literature. At the same time, the Journal moved to pre-emptively stifle any scholarly migration to the Global Health

> Information Server or other similar electronic archive by issuing an ill-disguised threat: "posting a manuscript....on a host computer to which anyone on the Internet can gain access will constitute prior publication" rendering an article ineligible for publication by the Journal itself.

Note how both of the establishment journals relied on an appeal to traditional methods of peer review to justify themselves. The difference was that the NEJM went much farther than the SRO. Not only did they argue lives and limbs would be lost if the newer system came into effect, but they also threatened to discount all publication that had ever appeared in any electronic forum in a blatant attempt to strong-arm the medical discipline into ignoring LaPorte's proposal. [25]

Traditional publishing houses have not confined themselves to direct attacks on new and independent publication projects. In the formal literature on electronic publication, traditional publishers have started to define away the scholars ability to publish their own material. Fytton Rowland, [26] for example, suggests that because of academic workloads, the size of the task, the need for quality publications, and the need to filter information for quality purposes, scholars are unfit as purveyors of scholarly information. And lest the reader of Rowland's article misunderstand his message and intent, he states it explicitly when he suggests that *all* journals need to be run by information professionals and not, in his own words, by "*academic amateurs*." [27]

The justification Rowland provides is rather thin and not credible given that scholars have already demonstrated their ability to publish material with high substantive quality. Though the standards may not be up to what the commercial and society publishers are capable of given their large organisations and considerable resource pools, this is not considered a problem by everyone. Scholars and libraries have recognised that lowering the standards is a reasonable sacrifice given the high cost of these value added services and the inability of the system to support that cost. [28] As for the size of the task, most experimental services like the EJS or Harnad's Psycholoquy have clearly demonstrated the feasibility of scholarly projects. True, there is considerable effort and time required at start-up but the time required is not inordinate and if we can revise current reward procedures to recognise editorial and technological contributions to the discipline then the time required would not even be a sacrifice of valuable advancement activity. And while it is certainly true that scholars would not be able to publish hundreds of journals, that is not the intent of the new models. They are really about decentralising the task of distributing scholarly material in order to distribute the workload over a wider area.

Rowland doesn't represent, in any obvious manner at least, traditional publication interests. He is a research fellow in the Department of Information and Library Studies at Loughborough University of Technology in Leicester,

UK. His concern and comments may be motivated more by a concern over the future role of libraries, librarians, and other similar information specialists. While his concern is understandable, it is misplaced. Nobody, as far as I know, has suggested that information specialists will not be required. And in any case, the best solutions to the scholarly information crisis have recognised the central role of libraries and information specialists by suggesting that these organisations take over a goodly chunk of the scholarly distribution system for themselves. [29] Even if all libraries do not take as central and highly active a position as publishing their own set of journals, [30] there will still be many opportunities for collaboration with university departments and individual scholars.

While Rowland doesn't represent traditional interests, Janet H. Fisher [31] of MIT press does. Ironically, she makes the same arguments as Rowland. In a nutshell, she suggests that individual scholars do not have the resources, expertise, time or inclination to successfully publish their own material. It is worth quoting at length from her passage [32]

> There are a few other problems with circumventing traditional publisher for electronic journals. First, what happens to the system of subsidiary publication of materials in other forms - University Microfilms, Information Access, CARL, Faxon Finder, and so on? The consolidation of licensing for all of these arrangements with the publisher would no longer be possible. Unless the journal editor was willing to handle these requests and get the necessary rights from authors, secondary publishers would have to go to each author for the right to produce the article in another form....The typical journal editor does not have the staff to handle this level of rights gathering. Second, what happens when a very important signal for tenure consideration of a researcher's work - the quality implied by a given publishers' name - is gone? Third, standards of reference citation and style, which are currently maintained by the publisher through the copy-editing process, and which make each discipline at least somewhat coherent, would deteriorate and eventually disintegrate. Fourth, who would do the marketing? Would the journal editor do it? Finally, what about indexing and abstracting sources? How will these services know what to cover in their publications and where to find it, given that currently the publisher is the one who contact them, sends samples, and maintains correspondence? There is no easy way out. The production, marketing, and dissemination of quality research material cost money. Publishers are essential to a coherent, efficient, quality publication process; unless funding is forthcoming from universities or the

> government, the reader - or at least a portion of the readers - must pay in order for the publisher to recover its costs.

Fisher's arguments only make sense in the context of the way the scholarly communication system operates in the paper world. Take, for example, her argument about the need to distribute material in other forms. She argues that collecting together the various article rights and contacting the tertiary distribution houses requires much to much work for individual editors to be able to handle. Certainly, if this type of redistribution of material was a requirement in the electronic world, editors of independent or library journals would not be able to provide the functions without relying on additional staff. The problem is that alternative publication outlets **are not required** when information is available on the Internet. The whole rationale for using CARL, or Faxon or any of the other tertiary services is to increase document access through the redistribution of material in separate mediums. But if the document is already freely accessible on the Internet by every scholar in the world, what on earth is the use of these tertiary services?

Fisher might be able to respond to the argument about the irrelevancy of redistribution of material by suggesting that tertiary services that collect and collate scholarly material will still be needed in order to continue to provide centralised bibliographic control and current awareness services and that editors would still be required to manage their publications interactions with these services. But again, alternate services are available that challenge the old way of doing things. Services are available on the Internet, like the Url-Minder service provided by net-Mind, or the JournalMinder provided by *The Sociology Corner,* [33] that monitor Internet documents and alert readers when changes have been made. There is no time requirement for the editor and readers all over the world are alerted in the normal course of updating the journals contents. This is a simple, elegant, and completely cost-less and time-less solution to the problem of current awareness. [34]

Fisher also makes some comments about the need to contact indexing an abstracting services to ensure that an article or book is distributed as widely as possible. But like the uselessness of alternate publication outlets, indexing and abstracting is not a requirement on the Internet. Simply dial up the Open Access search engine, enter your key words, submit and presto, you are able to locate every single document in the world that comes even close to your chosen topic area. I can't imagine that the abstracting services that exist now can even approach the speed, efficiency, and resolution of these electronic search engines. And, while it is true that these search engines often turn up much superfluous material, the same problem is experienced with CARL Uncover or any of the other available abstracting services. In either case the scholar will have to sift through material. But the benefit of the free services is that they do not require editorial efforts (beyond choosing an appropriate set of keywords) and they are not filtered by an organisation that, because of limited resources, cannot abstract all existing publication. Unlike current abstracting

services which cannot possibly abstract all material, electronic search engines can and do.

Fisher also attacks independent publishers by arguing that the name of reputable publishing houses is an extremely important added value of the current system and a key signal in employment and advancement decisions. While this is true, it is important to remember two things. One is that publishers only achieve their reputations by relying on the expertise of editors who are themselves scholars. Who is to say that an independent editor alone, or working as part of a publication team in a university or a library, or in a globally connected collection of editors and reviewers donating their time, cannot achieve the same quality and reputation as a commercial publisher? In the second place, universities are already calling for alternative methods of evaluating published contributions that offer a more direct method of assessing the impact of scholarly contributions than provided by simple publication counts or the reputations of the journals in which the piece is published. [35] Its seems most probable that universities will settle on Citation Analysis. This methodology assesses the quality (or impact) of a scholarly piece by counting how many times the article is used (i.e., cited) by other authors in the field. This method, although questionable on many grounds as we will see, does not rely on publisher's reputations. Interestingly, this method could even be used to assess the quality and impact of articles that are self-published!

As for standards of reference and citation, here Fisher has a point. The standards are likely to decline a bit, at least initially, while the new breed of publishers get their feet wet. However it is unclear whether or not these declines will be permanent. There is certainly reason to think they won't be. On the one hand, the first scholarly journals, like *The Transactions* were exceedingly primitive and difficult to read. Lack of standardisation and inexperience in scholarly publication didn't stop them from trying. And the services evolved in short order. On the other hand, there are certain technological solutions to citation difficulties in the form of professional bibliographic software like *Pro-Cite*. Authors can circumvent the difficulty of standardising reference lists simply by making use of these powerful software packages. The proliferation of this technological solution would certainly make editorial work easier.

Even if the decline in quality is permanent, it is an open question whether they will be significant enough to even be a bother. I know from my own experience reading traditional books and paper journals that references are not always accurate. I can't remember how many times I have gone to the library to track down a journal article only to find that the reference information was completely in error. If the scholarly community can suffer through the inability of the traditional publishers to ensure perfect citation, surely they can be convinced that independent scholars making the same mistakes are not a threat to the integrity of the system.

Finally Fisher points to the need to engage in professional marketing as a way of informing the scholarly world of new information. However this argument is questionable on a couple of grounds. On the one hand, it assume that scholars passively sit back and wait for someone to tell them about what new information is available in their field. This is clearly untrue given what we have learned about invisible colleges and their importance. On the other hand, the argument ignores the stunning power of information technology to automatically inform individual scholars of new development. In the electronic world, all the "marketing" that an editor will ever have to do is done simply, quickly, and efficiently by submitting the home page of the publication to a service that announces the existence of the publication to *all* available search and indexing services on the WWW. Following this, all the available search and indexing services will extract information from the publication and index and store it in their databases. Subsequently, any individual who wants to know what journals exist in a specific area, or what is contained in their pages, will only have to do a search at any one of the numerous free services available. No effort is required and the scholarly community can benefit by eliminating the completely unproductive, wasteful, and costly practice of marketing.

As we can see, the traditional publishers are trying to convince us of the impossibility of providing an alternative publication system by insisting on our inability to achieve rigorous publication, by decrying our motivation, by accusing us of sloppy writing habits, by suggesting that we cannot market our own information, and by generally painting us as amateurs and dilettantes. I have attempted, hopefully with some success, to demonstrate the weaknesses in these arguments. However in my opinion, these attempts to discredit alternative publication are not the most serious threat to new publication paradigms. Scholars are after all a critical bunch. A more serious threat to the revolution comes from the attempts of the traditional publishing houses to define electronic publication as just as expensive, or more expensive, than the traditional paper based mode of scholarly communication. If traditional publishing houses are able to convince scholars and libraries that "real" cost of electronic publication (as opposed to the "fake" costing formulas of scholars like Harnad) is equivalent to the older mode, than they will more easily be able to maintain the current costing structures and all the disadvantages that this mode has for the scholarly system of communication.

A Publisher's Victory

Would commercial publishers do this? After all, there have been thousands of words written about the cheapness of electronic publication and the benefits which it could bring to an academy cornered on all sides by funding cuts and retrenchment. And besides this, scholars in various disciplines have demonstrated quite clearly that electronic publication can offer significant benefits in terms of cost, access, and speed of distribution. Even some

traditional paper publishers (not affiliated with commercial interests!) who are publishing dual versions of their journals (one paper and one electronic) note that the "extra cost for the electronic version is rather minimal." [36] In this environment we might ask how traditional publication interests could even think about trying to justify a high cost publication system? Yet as Lubans suggested way back in 1987, traditional publishing interests are highly motivated to retain their privileged position. Lubans [37] predicted pessimistically that "... electronic publishing may enable us to make gains in space, but not in budgets; publishers will not give up earnings regardless of how many fewer 'pages' they may 'publish' in some giant computer." If Lubans is right, we shouldn't be surprised to find traditional publishers trying to pull the wool over the eyes of the scholarly world.

A few years latter, Steve Harnad [38] predicted much the same thing when he predicted the strategy that traditional publishing interests would use to prop up their claims about the high or higher cost of electronic publication. He also noted that the only publications that would report higher costs would be those advocating models of publication that tried to publish via the subscription model (and therefore required a top heavy bureaucracy to administrate the journal), those that offered all sorts of unnecessary frills (which the users would have to pay for), or those publishing in both the paper and the electronic realm.

Only a year latter we can see just how accurate Harnad was. Jack Meadows, David Pullinger and Peter Such, [39] speaking from their experiences with the UK ELVYN project, make just the claims that Harnad predicted the traditional publishers would make. In the extract below, the authors suggest two models of publication and then, for reasons not clearly articulated in their text, suggest that it is the journal with the more varied format (i.e. the model with the biggest tail fins) that should become the standard for electronic publication. The message is unmistakable. Electronic publication (in the sciences at least) offers no cost benefits.

> One publishing sector consists of individuals or specialist groups; the other of professional publishers. The first sector tends to emphasize electronic journals in the humanities or social sciences: the second is more likely to be concerned with STM (science, technology, and medicine) journals. Publications within the former sector consist primarily of text, whilst those from the latter incorporate graphics, mathematical equations, and extensive tabular material in their text. Creation of the latter type of electronic journal obviously requires more effort; its dissemination to readers, and their handling of it, is also likely to be more complicated. In terms of future electronic journals, it is this more varied format which should provide the prototype.

As noted above, the authors are not clear about the reasons why the second model should be emphasised except to vaguely suggest that it is a better format. But even if the more expensive model did offer some services that were desirable, the benefits of the services would have to be weighted against the cost to the academy. Given all we know about the scholarly communication system and the crisis in funding, scholars should, unless there is a very good reason for doing otherwise, be advocating models with less frills and add-ons in order to reduce the cost and return the system to a healthy state. It goes without saying that advocating a more complex publication model for the scholarly communication system is not in the interests of the academy. Yet here we have it, as Harnad predicted, publishers attempting to con the stakeholders into accepting a model that will continue to put strains on the financial resources of the academy.

We can give the publishers the benefit of a doubt. After all there are some disciplines that use more graphical and tabular information and this does require more labour. But even here it is too soon to tell just how much more work the more complex journal formulas will cost. Assuming that publishers utilise HTML and not some costly propriety format, we simply have to wait for HTML and the technological landscape to settle before we can start making accurate estimates of the cost of journal publication. As noted earlier, the EJS is uncertain about the future labour requirements of the journal just because the technological landscape is so volatile it is impossible to predict what future enhancements or labour saving technology might be introduced.

But besides the fact that the technology will mature and continue to advance, there are other options for *scholars* who wish to publish more elaborate journals. What if, for example, authors themselves submitted articles that already incorporated tabular and graphical data in the required format. Sure there would be still be extra formatting and copy editing work, but nothing like the effort required to create camera ready copy from tabular and graphical data for print journals. Although the EJS is only one journal among many, [40] the fact that one of our authors created a multimedia document for submission to the EJS without professional editorial assistance, and without putting an undue drain on my time and resources, would seem to suggest that it is possible for the scholars themselves to handle some of the things that libraries are now paying "professionals" to do for us.

Traditional publishers are not only justifying their higher costs by arguing that there is a need to accommodate elaborate publication formats. Jack Meadows, David Pullinger, and Peter Such [41] also point to the need to hire financial and administrative experts as one factor that will likely up the cost of electronic journals. The authors note: [42] "Establishing the electronic version of a new journal is likely to require finance and skills equivalent to those of a medium-sized publisher." However, the experience of the EJS and other independent journal projects is clearly the opposite of what Meadows, Pullinger and Such suggest. None of the independent projects have had to

hire financial or administrative expertise in order to create viable journal projects. This add on frill is only an requirement for the big traditional operations who must ensure wide distribution in order to generate profits in order to survive. Large publishers are thus completely dependent on a top heavy bureaucracy which costs money that the academy cannot afford to pay. Independent projects, whether they be conducted by scholars or libraries, have no such requirement since they can afford to simply sit tight and let the scholarly world learn about the journal in its own good time.

There are still other strategies for justifying higher costs. Some are arguing that the first copy costs (i.e., things like editing, peer review, and markup) are equivalent in the paper and electronic realm. Robert H. Marks, who is director of the publication division of the American Chemical Society, develops an elaborate model designed to convince the reader that "complete elimination of the printed journal ... will not solve the present library funding problem. It may even put increased pressure [read cost] on the library community for access to the increasing scope and quantity of scientific information that will be available on electronic networks." [43] Marks has this to say about first copy costs: [44]

> ...our studies show that elimination of the printed journal actually saves very little because the major share of high quality journal cost is still the so-called first copy costs: the expense of acquisition, peer review, and editing and converting the information into a digitized format.

It is important to understand that for scholarly journals, editing and peer review are often provided at no charge and are subsidised by the parent institutions of the scholars who serve on the editorial boards. Peer review and editing only become pay-per services for the larger publishing houses, and especially the commercial houses. And remember that the whole rationale for moving away from this model is to eliminate the need for paying for these services. As for conversion into a digitised format, this in simply not a requirement for journals produced in **HTML**. Tags can easily and almost automatically be added to word processed documents. However the scenario is quite different for journals that are published in digitised *images* of the original camera ready copy. Not only does it take a long time to scan images at high resolution, but the images require considerably more disk storage space, take longer to transfer over networks, and require camera ready copy for the input thus increasing the cost of electronic journals substantially. Again, it makes no sense to adopt this model when HTML is available and evolving.

Yet another strategy that has been used to justify high cost electronic journals is to argue that marketing an electronic journal requires more effort than similar paper journals and is thus more expensive. Janet H. Fisher compares the costs per article for the electronic journal *Chicago Journal of Theoretical Computer Science* and the traditional journals of the MIT press. She feigns surprise when she notes that the costs per article for the traditional and

electronic formats are identical. [45]

> How can that be? This print journal on our list publishes short articles and uses author-supplied disks primarily in TeX - essentially the same process we are using for CJTCS. It has a print run of about 2,000 per issue and is mailed using second-class, nonprofit rates. Printing, binding, and mailing costs account for approximately one-third of this journal's total expenses. Marketing expenses are essentially the same for the two journals. Because electronic journals are so new, much of the marketing expenses for CJTCS will come from the need to encourage contributions and reassure researchers their articles will be broadly distributed, covered by the major indexing and abstracting publications, and count for tenure consideration. This need for intense marketing may taper off as e-journals become more accepted, but marketing costs are unlikely to decrease below those for a print journal, regardless of format, based on the production work required, the number of subscribers, and the number of journals in MIT Press' entire program. Marketing and overhead expenses for CJTCS are approximately two-thirds of the total expenses, whereas for the print journal they are one-third of the total expense. Thus, the relatively heavy marketing and overhead expenses that characterize CJTCS overwhelm any savings in production costs that come from the new medium, yielding a similar overall cost picture between the CJTCS and the comparable MIT Press print journal.

To reiterate an argument that I have already made, an electronic journal published on the Internet by scholars or libraries does not require marketing - unless of course their is a need to convince the market of the need to buy the journal. But scholars surely don't need to be told about the journals in their field. They are highly motivated to track down all the relevant literature in their speciality. And even if they have no time, a simple subject search with the new Internet search engines will turn up the relevant titles. Further, scholars will not need to be told by a publishing house that an article or journal is worth reading. They are perfectly capable of ascertaining that for themselves. And if scholars don't contribute to the journal - so what. Perhaps the new electronic journal is not needed. Why get the scholarly system to pay for the effort required to justify journal twigs?

However if the goal is to market new journals to libraries, then that is a different matter. In the present environment many libraries will most certainly have to be convinced that the new electronic format is worth the money they are going to have to pay for it. They will have to be assured that the material is of the highest quality and that scholars will contribute. Libraries will not want to

buy titles that will be dead in a year and will no doubt display extreme caution in acquiring the new electronic journals. If the journals are provided free by scholars or at cost by other libraries, there is of course no risk to libraries. But as you can see, it is a different story if traditional publishers are trying to create a viable electronic journal. They will indeed have to conduct an intense marketing and image campaign.

Perhaps the most unreasonable part of the current efforts to justify continued high costs of publication come from those who would accuse scholars and libraries for not bearing their fair share of the work involved in scholarly communication. Fisher, [46] after castigating scholars for the presumptuous idea that they could publish their own scholarly material, goes on to say that if the new (commercial) system is going to offer cost benefits, then libraries and scholars will have to work harder to make it easier for publishing houses to publish scholarly material!

> ...whether there will be savings to pass on to librarians and individuals will largely depend on what librarians and individuals are willing to do for these publications. Will individuals provide well formatted, standardized files to publishers for use in production? Will the publisher's overheads be reduced because electronic publications are easier to handle internally? MIT Press has not seen any such reduction to date, but it is too early to tell.

It is true that publications like the EJS would like to see authors bear more of the burden for disseminating scholarly material. But the EJS is free and the justification for getting the authors to do more work is that the publication does not want to hire additional assistance and thus put additional financial burden on an already unstable communication system. This is certainly a different scenario than the one envisaged by Fisher where scholars are to contribute to the profitability of the enterprise by donating more of their labour. It is doubtful after all that traditional publishers who actually do see a savings in the cost of handling manuscripts would pass that along to the libraries. Chances are they would simply tell us that the cost savings are insignificant, or they are handling too many formats and that raises the cost, or that the there are hidden costs in markup, or marketing costs have gone up, etc. By accepting this argument, we allow ourselves to be sold down the river.

If talk doesn't convince libraries and scholars - a very real danger - commercial publishing houses have one more option. They can wield their not inconsiderable market clout to get their way. There seems to be two things that they can do here. First, they can use their market power to smash experiments in alternate delivery of scholarly information and second they can use their access to large volumes of scholarly information to provide value added services against which small independent operations will not be able to compete.

There is evidence these tactics have and will be pursued. In the past, some have used their market power in similarly predatory ways so we can assume that if the competition (i.e., the alternative publishers) do not just role over and die, traditional publishers will start to engage in more aggressive manoeuvres. We can see this dynamic in the following extract taken from Dennis P. Carrigan. [47] However a word of explanation is in order first. Because the first copy costs of producing paper journals are largely fixed, paper publishers prefer a subscription model where fees are paid up front. They don't like the new access model (i.e., document delivery) favoured by some libraries because it does not generate a guaranteed revenue stream. As Dennis Carrigan explains, [48] their distaste for the new model may mean its eventual elimination.

> The University of Kentucky libraries recently experienced a publisher's ability to influence the choice between the ownership and access service models. Several library clients asked the interlibrary loan office, which also handles document delivery, to obtain for them articles from the same journal, to which the library did not subscribe. When the office reached the limit of five copies permitted under the CONTU guidelines, it turned to a document supplier to meet the next request for an article from the journal. When the article copy arrived, the interlibrary loan office was shocked at the fee charged by the supplier, and when the office looked into the matter it learned that the copyright royalty fee was $10 per page. The library decided to subscribe to the journal.....Although such experiences may be infrequent at this time, they can be expected to increase, as the shift from ownership to access grows, and to exert an increasing influence on libraries' decisions. [49]

Commercial publishing houses that have been around for a long time also enjoy the competitive advantage of having a large back library of academic content to draw on in order to provide value added service. As Malcolm Getz [50] notes, this may give the large publishing houses, if they choose to use it, a considerable advantage in the online environment.

> Moreover, the present advantages enjoyed by the multititle publisher may well persist and even increase in the electronic arena. Access to targeted mailing lists, multititle advantages in advertising and distribution, and the ability to integrate new publications into the logical context of large databases may give significant advantages to the large publisher supporting titles in many related micro-disciplines. The upshot may be that, after an era of experimentation, the market for scientific publication will be no more competitive than today, and perhaps even less competitive. The gap

> between market price and incremental cost may be wider in the electronic world than in the print world.

In the present, the big publishing houses are currently positioning themselves in order that they may exploit "attractive" opportunities in the scholarly communication market. For example, Reed Elsevier plc has recently announced that it will divestment itself of IPC Magazines (a distributor of consumer magazines). This divestiture would allow Reed Elsevier's to focus on developing a strategy of increasing its ability to exploit the "high value-added areas of 'must have' information" at the same time that it reduces is "exposure to consumer markets." As the cited press release indicates, "The proceeds [of the divestiture] would be used for future development of and acquisitions within Reed Elsevier's core Scientific, Professional and Business Divisions and would provide the company with greater flexibility to respond to attractive growth opportunities as and when they arise." [51]

If this isn't worrisome enough, Reed-Elsevier has also proposed a merger with Wolters Kluwer which would, by combining the resources of two major scientific journal publishers, make publishing megalith worth 17.5 billion pounds. [52] This, coupled with its strategic partnership with Microsoft, [53] a company known for its profiteering, anti-competitive history, and arrogant desire to control the Internet, [54] has caused considerable concern. In response to the announcements of Reed-Elsevier, a coalition of 15 Dutch scientific research libraries, concerned over the anti-competitive implications, has adopted a set of principles aimed at bolstering their position in negotiations with publishers over their offerings and reducing the possible impact of skyrocketing electronic journal prices. [55]

Indeed, given the current political atmosphere, we can expect increased intrusion of commercialism in library and the academy as online services become more prevalent. Government and business have moved increasingly towards the disarticulation of the social norms that underlie free and equitable access to information towards increased legitimisation and acceptance of private sector role in information creation and distribution of information. [56] This is having important consequences even outside of our own concern over electronic journals as libraries are forced to shift resources from social use acquisitions (journals, books) towards increased reliance on IT mediated services which allow for precise and metered charges. The pressure from business to adopt new services is often couched in terms of need to increase efficiency of library distribution systems. However, these intrusions are more and more being recognised as bringing about shift of resources to for-profit, user-pay services and towards generating increased reliance on these alternative information sources. As Herbert Schiller notes: [57]

> In recent years, libraries are increasingly being put into the position of adjunct to and facilitator for the commercial information industry. Despite an initial reluctance to become

> involved in commercial practices - i.e., charging users for information, relying on private vendors for data bases, contracting out functions to private firms, etc. - libraries now almost routinely adopt such practices. Meanwhile, the distinction between a library and a commercial enterprise narrows.

With the ongoing shift to the right, and the ongoing colonisation of the academy, it is becoming an outright attack on cherished principles.

> Already some "bottom-line educators" are wondering whether there is a need for traditional library schools. Who needs librarians, educated according to a social ethic, if information can be supplied by entrepreneurs and private business unencumbered by social principles? An opaque word, "disintermediation," is coming into use to obscure a very transparent process by which librarians may lose their jobs in the future. (Schiller, 1989: 81).

The impact of these shifts is now trickling down to scholars as industry seeks out ways to make the distribution of scholarly material profitable. One of the models that publishers are currently thinking about, and the one that seems the most popular when dealing with institutions like libraries, is one based on site licenses. Site licenses for journal would essentially allow subscribing institutions and their patrons unlimited access to the complete set, or perhaps a subset, of the periodicals that a publisher distributes. Gary Taubes [58] notes:

> Once they begin charging, many of the publishers are currently planning to sell subscriptions to their on-line journals through so-called site licenses, which will allow unlimited and unrestricted access for users who log in from subscribing institutions. To set a price for these site licenses, publishers are contemplating one of two formulas: either offer them free to print subscribers or, as Bob Kelley of the American Physical Society describes it, "charge a little more for both paper and electronic, and a little less if electronic " or paper only.

This model of offering subscriptions has certain benefits. For example, journals will essentially never be off the shelf. Their contents will always be accessible by anyone who logs on with the institutions internet domain name. However it is clear that this model will not cost the libraries less and it certainly may end up costing libraries more if publishers charge additional fees for access to both print and electronic journals. It is even conceivable that the subscription rates for fully electronic journals (i.e., with no print version) will be higher since publishers will more easily be able to justify higher subscriptions based on the value added brought to the institution by unlimited access,

powerful search tools, and comprehensive journal collections. Because of the value added functions of electronic journals, it is conceivable that a journal that costs $1000 per year in the paper realm would cost an additional 5%, 10%, or more in the electronic realm.

However it is not only that publishers may be able to corner libraries with site licenses. Publishers also stand to benefit by their increasing ability, brought by advanced information technologies, to shift the burden of payment directly onto the shoulders of the users. Some commentators feel that this is an extremely likely possibility. Gerard m. Van Trier, [59] for example, fully expects publishers to exploit a direct market to consumers of information as it becomes available. Dennis P. Carrigan [60] notes that some form of direct purchase is a definite desire of many information providers because it represents a vastly expanded market for information.

> Moreover, payment for the service can be made not only from a depository account but also by VISA, MasterCharge, or American Express card, another feature that is spreading and that opens the way for individuals to deal directly with document delivery organizations. According to Martha Whittaker, general manager of the UnCover Co: 'We believe that the real growth market in article delivery is the consumer - or 'end user'. We are developing strategies to reach the individual researcher, faculty member, and ultimately, the person sitting in any office anywhere with a computer and modem.

By all indications, this direct market will be upon us in no time flat. Marvin A. Shirbu [61] reports on an experiment with the sort of technology required to institute direct user billing being conducted at Carnegie Mellon University. Called *NetBill,* the technology allows authenticated and almost transparent transactions to take place on the internet. Transactions costs are extremely low (as low as 1 cent per item) and has the capability of charging as little as 10 cents per page and maybe even less. The technology is ideally suitable for scholarly publication in as much as it will allow publishers to charge scholars for individual articles, data files, or any other subsidiary information that they feel scholars might be interested in. Netbill was designated to go into pre-commercial trials in the fall of 1995 so by now it may even be in commercial experimentation.

This technology, or some variant of it, may be a gold mine for commercial publishers. As Gary Taubes notes, online services provide a wealth of opportunities for shifting the financial burden to the user. [62] "As journals become increasingly interconnected, researchers will find themselves hot-linking from one cited or related article to the next, regardless of who the original publisher happens to have been." People will find themselves buying articles and related sources material from almost every publisher on the

internet. And what is worse, the technology is being designed to be as transparent to the user as possible. Debits are made from a central account and software will have an auto pay function that allows users to set a lower limit (say 20 cents per page) below which information items are purchased automatically.

The major disadvantage with this move is that scholars will be one of the hardest hit. This will be especially true in some disciplines since we can fully expect, given the ongoing trend of libraries to cut subscriptions, that it will become necessary for the individual scholar to support esoteric publication that might be highly relevant to a small group of researchers but that are not fortunate enough to make it into the core periodicals list of the nations libraries. Duane E. Webster and Mary E. Jackson, [63] speaking about the ongoing push for libraries to provide access to material, suggest the likelihood of this scenario.

> Recent studies suggest that institutions acting together to implement the access model may satisfy short-term needs of the faculty and administration but over the long term will damage and weaken scholarly communication. Without collective action the nation's information resources will become more and more limited. The availability of esoteric, foreign language imprints and lesser-used information will diminish and as a result the scope and richness of available collections will decline. If libraries continue to reduce collection development to focus only on local and immediate needs, then the "commons" that scholars rely on will become impoverished.

We may see a two tiered system of publication emerge. The highly popular journals in the sciences will be licensed to institutions and be freely available to faculty and students. Some journals in the social sciences, and many in the humanities, because they do not have a sufficient readership or are not used on a regular basis, will be cut from library acquisitions lists and only be accessible through services like NetBill where scholars can purchase individual articles. A worst case scenario would find those unfortunate scholars in areas that are not that popular unsuccessfully battling for increased per diems for information purchase. The chance that this could be worked into a contract is, given the current budget crisis of most universities, highly unlikely.

But the disadvantages are not just about scholars worried that their subsidy will be eliminated. Moving away from collective information services (i.e., libraries) to individually funded services will have a serious impact on the quality and cost of education. The accessibility of much information will be reduced with the new commercial models since only users who can pay will be able to access it. Universities will almost certainly not subsidise their undergraduate's access to current information in journals not locally held. And

even if universities subsidise the access of their graduate students to the information they need, the decisions are likely to be made on a per-institution basis. Wealthier institutions will be able to subsidise this access while smaller institutions will shift the burden onto the students. This will exacerbate an already existing hierarchy in the U.S. and perhaps even contribute to the creation of a similar hierarchy in countries like Canada.

Although not directly related to scholarly publication, we can see that type of balkanisation predicted for the scholarly communication system is already occurring in the public library sector as libraries focus on the popular academic pursuits at the expense of other areas. John Buschman [64] describes the loss of access at the New York Public Library caused by an emphasis on the development of Science, Industry and Business collection.

> The New York Public Library only recently has found the funds to restore staff and extend hours cut from branch libraries around the city (of primary benefit to local neighborhoods and schoolchildren). In the meantime, NYPL was able to proceed with a Science, Industry, and Business Library with an integrated technology system at a cost of $18.5 million to the public.

There seems little reason to suspect that a similar dynamic will not also occur in the academy.

Besides this balkanisation, education and intellectual development may well suffer even at those institutions that provide subsidised graduate access. This will have less to do with financial access to the material and more to do with the unbundling of the scholarly project. Currently, scholarly information is bundled in relevant packages (i.e., journals). The fact that these journals have been contained on shelves has been extremely useful both for faculty developing a new research interest and for graduate students who obtain easier access to all the relevant literature while studying for their exams. But with unbundling and sale of information in bite size pieces, graduate students will not be able to browse relevant journals in order to quickly develop a sense of the field. Nor will they be able to take home the last ten years of a journal in order to develop substantive depth in their field. Faculty will also suffer for much the same reasons. Its seems plausible to suggest that large scale unbundling of scholarly information might contribute to less depth in scholarly endeavours simply because unbundling will force scholars to focus more narrowly on their topics of interest.

It is true that the scenario predicted here, i.e., similar or rising costs for distribution of information, declining access, the development of a tiered communication system, and a decline in educational quality, is a worse case one. It might not turn out to be as bad as all that. Clifford A. Lynch notes that some universities are now turning their attention to revitalising their academic

presses. Because academic presses have been traditionally concerned with distributing material that is not profitable enough to find outlets in the commercial press, and because new technology might allow them, through reduced costs, to again offer this vital service to the academic community, the outcome of this growing concern might be the salvation of the esoteric press. As Lynch notes: [65]

> Ironically, universities, reacting the to increasingly intolerable costs of acquiring scholarly information from commercial publishers, are now asking whether their university presses can play a greater role in making scholarly information available at lower costs to the research and education communities. This is exactly what the university presses were supposed to be doing, before their parent institutions told them to act like commercial publishers.

In order to actuate this scenario, Lynch notes that a co-ordinated effort needs to be developed. University presses, scholars, societies, and libraries all have to become involved in the planning of the new scholarly communication system. And what's more, there has to be an awareness on the part of all concerned that the scholarly communication system should not be designed with profit as the primary goal.

Whether or not such action will be taken is an open question at this time. On the one hand, there is a possibility that the shift towards profitability and the ongoing fiscal pressure facing librarians will set libraries against scholars. Libraries, seeking ways to "enhance" services, may decide not resist the implementation of pay-per services and the opportunity to download costs onto certain groups of scholars. There is potential here to set these two groups off against each other despite the obvious coincidence of interests in the scholarly system. Should this divide and rule strategy not be resisted, the ability of the scholarly system to resist the commodification of the library system will be seriously curtained.

On the other hand, and despite the fact that the interest shown by scholars in the revitalisation of the communication system has been minimal so far, this may change if some key publications begin to disappear and as some scholars are forced to pay directly for there information. At that point the scholars who are dependent on esoteric titles may in fact choose to start their own electronic publications in order to ensure continued low cost distribution of their own and their peers work. This will, of course, be the most revolutionary outcome and will go along way towards creating the preconditions for the removal of commercial interests from the scholarly communication system.

Still, it is important not to forget the emerging juggernaut. While there are many scholars and information specialists who would want to see a revolution (and would even be satisfied with a partial revolution) in the scholarly

communication system, the impression that we are left with after examining even briefly the publishers response to the threat of free-for-all publication (or even some less revolutionary quick-fixes) is that there doesn't appear to be a utopian like future for scholarly communication in the works. Indeed by now it should be clear that if we are to significantly alter the sociology of the scholarly communication system, we are going to have a tough fight on our hands.

Before moving on to the conclusion I'd like to say one more thing about commercial efforts to distribute material. Whether or not the actual costs of these publication are more or less than what the scholars provide, one thing is certain. Commercial publishers will need to purchase the technological infrastructure necessary for online publication. This can be costly especially if there are numerous publishers wanting to distribute scholarly material electronically. It is a reasonable question to ask, given that universities already provide this infrastructure, why scholars and libraries have to pay many times over for technology that they already have freely available to them? Wouldn't it make more sense for scholars and libraries to exploit the technology provided by their parent institutions?

Conclusion

In Chapter Two we outlined the problems contributing to the current crisis in the scholarly communication system. We suggested in Chapter Three that electronic journals may be able to solve problems with publication delay and high cost - but not proliferation. After this Chapter, it should be clear the potential for electronic publication to contribute to a solution to the financial crisis is limited. While it is too soon to gather for a funeral of these new models, it seems increasingly likely that the traditional publishing interests will be able to use their influence and market power to undermine the truly revolutionary efforts of the early independent publishers of scholarly material. Indeed, they may even, with the help of new technologies, be able to increase their profitability by accessing the money of libraries **and** individual scholars simultaneously. Without a more sophisticated awareness of the problems, and without a coherent and concerted effort on the part of all stakeholders in the scholarly communication enterprise, it seems likely that traditional publishers will end up stealing the cake.

Chapter Five: Citation Analysis

> The invention of print, however, made it easier to manipulate public opinion, and the film and the radio carried the process further. With the development of television, and the technical advance which made is possible to receive and transmit simultaneously on the same instrument, private life came to an end. Every citizen, or at least every citizen important enough to be worth watching, could be kept for twenty-four hours a day under the eyes of the police and in the sound of official propaganda, with all other channels of communication closed. The possibility of enforcing not only complete obedience to the will of the State, but complete uniformity of opinion on all subjects, now existed for the first time. [1]
>
> The use of citations as the determining or principal basis for assessing the performance and potentialities of scientists falls into the category of `promiscuous and careless use of quantitative data for ... evaluation.' That description, in the form of a forewarning, comes, significantly enough, from...Eugene Garfield himself.
>
> Robert K. Merton [2]

Introduction

Two years ago I had a fascinating experience. As we know, the Internet has various software repositories where programmers and computer enthusiasts can glean the latest and (sometimes) greatest free and shareware software products. Last year I uploaded a set of macros that I use to assist in the production of HTML documents to one such sight. Six months later I received some material from a paper based computer programmers journal. This was highly irregular, in my opinion, since at no time in the past did I ever purchase mail order computer software, or engage in programmers workshops, or take classes on computer programming or, or in fact, do anything that would have identified me as a C++ programmer. Yet here was this journal telling me how I could improve my C++ programming techniques.

Such material is of course highly targeted and the only way that I can see that I would have been identified as a programmer was through the contribution of my macro software to one of the Internet repositories. I speculated at the time that the people at this journal must have some sort of Internet daemon that scans the header files of recent uploads to Internet repositories in order to get

the names of potential buyers of the magazine. I remember thinking at the time what a good example this was of the power of information technologies to make us visible in the marketplace.

The event prompted me to start thinking about how scholars might be made visible by the new information technologies. In the past, i.e., in Pre-Computer age, the visibility of scholars work was enhanced by a research methodology called Citation Analysis. As we will see in more detail latter, citation analysis is a way of monitoring the impact that publishing scholars have on the scholarly world for the purposes of tenure and advancement decisions. The basic method is simple enough. Collect a sample of articles in primary journals, record citation data for each citation in the text (i.e., author, name of article, journal, date) and tabulate across all works the number of times each author is cited in the works of others. [3] The resulting number gives a **quantitative** measure, so we are told, of the *impact* that a specific author has on a field. For example, if I publish an article and 4 years latter it has been cited 50 times, then we could say that that particular article had a significant impact. On the other hand, if a work of mine only gets cited 4 times (sadly a much more immediate possibility) then it can be said to only have had a minor impact.

As you can probably guess, citation analysis is a tedious process. It involves hand collating literally thousands of journal articles and their citations. But technology is the great panacea for boring and tedious tasks - eliminating routine assembly line and clerical work in the blink of an electronic gateway. Wouldn't it be possible to put information technology together with citation analysis to relieve the tedium and create a more efficient, and perhaps more intrusive, system of scholarly control?

The answer here, I believe, is yes. Technologies certainly might enable the imposition of a total panoptic system in the academy. And if this should happen, the implications are potentially profound and potentially quite negative. An increase in administrative control over scholarly discourse, a decline in intellectual autonomy, and an increased emphasis on superficial definitions of scholarly productivity. But we get ahead of ourselves here.

It is the task of this chapter and the next to examine the issues swirling around the use of Citation Analysis, and the potential impact of information technologies (in particular the emergence of the electronic journal), on the development of a system of panoptic control in the academy. In order to complete the analysis we'll have to take a wide view. We'll examine the actual CA methodology and uncover its profound limitations and profound conservative bias. And, we'll also take a look at how and why CA might become an electronic panopticon. As we will see, the current push towards making universities "accountable" may well signal the beginning of a profound attack on independent scholarly thought. An attack which may well be led by the quick and efficient implementation of a global system of citation analysis.

A Brief History of Citation Analysis

CA has a long history. Although it was first used by the legal profession in 1817, [4] its first use in the quantification of scholarly output only occurred when Paul L. K. Gross and E.M. Gross used it in 1927 to help develop a list of the most important (i.e., core) primary journals in chemistry. Their rationale for using the method in this manner was that by analysing how much journals were actually used in the formal literature, a journal hierarchy could be identified. This journal hierarchy would presumably allow small and medium sized libraries, without the financial wherewithal to purchase the entire serials universe, to target core journals and ensure a relatively comprehensive collection of the most important literature in a given field. [5] As Estelle Brodman noted in 1994, this use of CA became a quite popular method for evaluating the disciplinary impact and importance of the scholarly journals. [6]

Scientists have not been content to use CA only for the development of journal hierarchies or core holdings lists. The methodology has been extended far beyond its initial uses to include, for example, the identification of a *research front*. According to Derek de Solla Price, the research front is that portion of science that deals with cutting edge problems. [7] In other words it identifies the elite section of scientific research and discourse. We also see CA used in the evaluation of the knowledge acquisition and distribution patterns of scholarly disciplines. For example, H. G. Small and D. Crane [8] conducted a detailed study using the Social Science Citation Index (SSCI) and the Science Citation Index (SCI) in order to delineate differences between high energy physics, psychology, economics and sociology. Their findings are unsurprising in as much as their data supports the scientific orthodoxy's prioritization of scholarly inquiry with high energy physics at the top, followed by Psychology, Economics and lastly Sociology. Using data like the differential rates of book citation and age of cited items, and the lack of "knowledge clusters" in Sociology, they conclude:

> "In all three social science disciplines, cited publications were less recent than in high energy physics, a probable indication that new knowledge is being created less rapidly in the social sciences than in the natural sciences. Cited publications were most recent in psychology, followed by economics and then sociology. Another indication of rapid growth is reliance on journal articles rather than books. Here again psychology was most like the natural sciences followed by economics and sociology" [9]

CA has also be used to map the conceptual space of disciplines, [10] delineate disciplinary boundaries and [11] trace subdiscipline isolation or decline. For example, David L. Krantz [12] uses citation analysis to point out the relative isolation and insularity of the school of operant psychology from the

psychological mainstream. Comparing the self citation rates of a number of journals, he finds that the *Journal of the Experimental Analysis of Behaviour*, a leading organ for operant psychology, has an extremely high rate of self-citation. On subdiscipline decline, Bagby, Parker and Bury were able to use CA to trace the decline of dissonance theory in psychology. [13]

Another use of CA has been to uncover what we may call the psychology of science. Examples include an assessment of the impact of the passive voice on citation counts, [14] and an examination of the psychological mechanisms that lie behind some of our perceptions of scholarly eminence. For example, Richard A Wright found that simply being the editor of a prestigious journal significantly increases your citation count. Wright found that the number of personal citations "increased conspicuously" during the terms of the editors of the American Journal of Sociology, American Sociological Review, and Social Forces. Speculating about the possible causes, Wright notes: "Some authors also may have an incentive for citing journal editors in the manuscripts that they submit for publication, believing editors - either consciously or unconsciously - give preferred consideration to papers in which they personally are cited." [15]

Particularly interesting is the use of CA to evaluate broad *categories* of research in order to identify successful research strategies. Thus we have Jerry M. Newman and Elizabeth Cooper's examination of different strategies of research and their impact on subsequent citation patterns. After distinguishing three broad categories of research (refinement, extension, and exploration), they compare papers with subsequent citations and find that of the three, exploration is the most fruitful type of research in terms of subsequent citation. [16] However, as we will see below, this seems only to be the case when exploratory research represents, rather than challenges, scholarly cannon.

Finally, CA has been used, as we've already seen in relation to journals, as a tool to prioritize scientific output. This has included the identification of high quality or productive universities or department, [17] countries, [18] scholars [19] and even journal articles. A particularly amusing, if fluffy, example is provided by Holub, Tappeiner, and Eberharter [20] who provide a citation analysis of "important articles" in Economics. They use a subset of their discipline known as growth theory to carry forth the dual agenda of identifying important articles, and deriving an **Iron Law** of important articles. After carefully counting citations, they conclude that "The number of important articles in a field of economic theory increases by the square root of the total of all articles in the published field." [21] They further derive a law which might be called the *Needle in the Haystack Law* which states that probability of finding an important article decreases as a function of the total output of a given area.

We can group the uses to which citation analysis has been put into two broad categories both of which are designed to increase the *visibility* of the work of

scholars. On the one hand, CA is used in order to map the conceptual space of the different branches of the sciences, or of different disciplines or subdisciplines. Here CA is primarily a tool for uncovering the structure of scientific thought. Less often, the variables which impact on the intellectual or conceptual structure of science are also studied. Christine L. Borgman and Ronald E. Rice summarise this side of the CA spectrum: "Bibliometrics, and citation analysis, are most useful for achieving a macro perspective on scholarly communication processes." [22]

In its second manifestation, CA is used as a tool for uncovering and justifying the stratified nature of the scholarly enterprise. Here we see it used to evaluate and prioritise journals, disciplines, departments, laboratories, research programs, graduate programs, and even individual scholars. CA performs this function, as already noted, by making scholarly work visible. From our perspective, this is certainly its most interesting application. And of course, the question we must ultimately answer here is whether or not CA performs this function adequately. The answer, as we will see below, is an unequivocal no!

Criticisms of CA: A Spurious Index

It is not surprising to find that there has been considerable resistance to the ranking of scholars via CA. Indeed, shortly after it became a popular instrument for library scientists in the early part of this century, its **reliability** as an indicator of journal importance was challenged by Estelle Brodman. Brodman compared the ranking achieved via a CA of physiology journals with the rankings given by real life physiologists and surgeons at Columbia University. Participants were asked to list the periodicals which they considered most important in their work. Brodman found that the list derived from CA and the list provided by field experts correlated only moderately (between .573 and .618). And although these are certainly not weak coefficients, they are certainly not strong enough to conclude that CA is a comprehensive method capable of identifying all the really important journals in a field. [23] This finding led Brodman to discount CA as a completely valid methodology. Noting the disparity between the two measures, Brodman concluded:

> The Gross and Gross method has been extremely valuable in helping administrators to build up periodical collections in many diverse fields about which they could not themselves have expert subject knowledge. For this reason it has probably been accepted more or less uncritically, with the feeling that any method was better than no method. Yet it appears to be a somewhat unscientific and unscholarly method, as well as one that gives untrustworthy results. In spite of these extremely grave drawbacks, the method will

> probably continue to be employed by librarians until the library profession is presented with a better one. Individuals using the method, however, should be aware of the small dependence which can scientifically be placed on its results. [24]

Clearly, it has long been recognized that CA is not a comprehensive indicator of scholarly importance - at least in relation to journals. The same can be said of its use to evaluate scholars as the following statistical critique makes clear. For example, after noting that the various hypothesis for the differential rates of citation experienced by authors, i.e., differential ability or motivation, resource inequality, and cumulative advantage, are not supported by evidence, Seglen [25] points out that citation distributions are nothing more than *statistical artefacts* created by the peculiarities of the population of interest. As a result, uncitedness is to be expected and in no way should be considered an indication of author, or even journal, impact. Seglen's conclusions on the usefulness of citation analysis are worth quoting at length.

> The fact that a minority of the published articles collect the majority of citations is an inevitable statistical phenomenon that cannot be altered by intervention. A reduction in the size of the uncited fraction can only be achieved by raising the number of references per publication. If the less-cited papers or authors were to be eliminated, the result would only be a decrease in the overall volume of published science… the distribution of citedness would be just as skewed as before, and the fraction of uncited papers would remain the same…. Therefore, one should be very wary about using citedness, in absolute terms, as a guide for scientopolitical action.
>
> The fact that a large fraction of the scientific literature is uncited does not mean that it is not being read, or that it does not contribute to scientific progress, it means that the total number of citations given … is simply too small to give room for all published articles…. If nothing else, it is safe to assume that all publications exert some influence upon their authors, an influence which may become manifest in other, more heavily cited articles. [26]

Others have also pointed to the statistical difficulties of the measure. R. Plomp [27] admits that the advisability of using CA to evaluate scholarship is questionable because of an inadequate modeling of the underlying variance in citation scores. The problem is particularly salient when using a short time interval. As Plomp notes "Papers with equal citation frequencies over a long period may differe [sic] in their citation frequencies over the first few years of publication." (p. 72). Plomp attributes the difference in the short term citation

rates of **equally significant** articles to a narrow impact window which grows wider as time passes (i.e., as more people outside of a narrow field of specialists and colleagues becomes aware of the paper). Plomp further suggests that this is a type of *error variance* which must be included if hypothesis about differences in article impact are to be tested statistically. Plomp identifies a second form of variance termed *true score variance* which is simply a measure of the variance attributed to the differential quality of papers produced by the same author. Because an estimate of parameter variance is an essential prerequisite for undertaking statistical tests, Plomp goes on to develop an appropriate formula for determining statistically significant results that incorporates these two sources of variation. Not surprisingly, Plomp concludes that "due to statistical variations, we need large sets of papers, or large ratios between mean citation frequencies, in order to conclude that the mean citation frequencies of the two sets of papers are significantly different." [28] Unfortunately, neither of these conditions are regularly met in typical citation studies.

Plomp provides an interesting practical application of his formula. He examines three studies, one on the statistical output of four radio astronomy observatories, one on the trends in output of a UK university, and one that "tried to prove that the lifetime-citation rate (LCR of a woman who was not promoted nor given tenure was significantly higher than the LCRs of two men who were promoted and granted tenure." [29] In these three cases, (two of which appear significantly enough in the journal *Research Policy*), most of the findings that the author's of the original articles considered significant failed Plomp's test!

Even if we only consider the statistical problems with CA, we would seem to be drawn to the conclusion that citation analysis is an ineffective measure of scientific impact. However the problems are compounded when we delve deeper into the processes that surround citation. Its simple enough to jump into this analysis. We already know that there are differential rates of citation. Some get cited and some don't. The question before us now is who gets cited, or not, and why? Is the differential citation rates due to ability, as if often argued in the literature, or are their other, more political or psychological mechanisms at work?

The notion that there is indeed something other than strictly objective processes of evaluation and citation going on beneath the thin surface of the scientific enterprise is unequivocally supported by work attempting to develop a theory of citation behaviour. For example, Mengxiong Liu [30] conducted an analysis of the citation behaviours of 725 authors of scientific publications by mail survey. Mengxiong found that factors like the influence of those in positions of authority, the prestige of the author and journal which a work is published in, the number of citations in a cited paper, and even the desire of the author of the paper to persuade or appear knowledgeable and up to date

in the field, all had an impact on the choices authors made. Notably, none of these **motivational** and **psychological** factors have anything at all to do with the *quality of the work* contained in a cited piece. Liu concludes: "Affected by an individual's internal motivators and external factors, citations cannot simply represent an indicator of scientific influence on the authors' work, nor can they be exclusively used as an evaluation tool for science impact." [31]

If motivational and psychological confounds were not enough to discredit CA, consider its openness to manipulation. Myron Boor, for example, notes that greater citation visibility (i.e., higher citation counts) can be **engineered** through such practices as serial publication of segments of research, multiple publications of the same research either in identical or modified form, self-serving citations, and conspiratorial cross-referencing among authors in the know. [32] Like the elements of citing behaviour which have nothing to do with the quality of a cited work, these political moves in the scholarly arena only confound CA. I have my own interesting anecdote concerning these processes. I recall being instructed in the importance of "citation engineering" in my advanced graduate classes. As one very well known and established instructor put it, and I paraphrase, "nobody has ever read all the references that appear in a bibliography…. They are there because quantity equals quality and the reciprocal citation of friends and colleagues increases our visibility."

Now, if there are motivational and psychological factors that muddy the waters of citation analysis, and if citation counts can be engineered, might not there also be political battles going on? The answer to this question seems to be yes. At an international level, it perhaps shouldn't be surprising to find that Citation Analysis has a tendency towards myopia. Specifically, there appears to be an evident eurocentric bias in citation patterns. Lea Velho, who investigated the cross-citation behaviour of Brazilian and American agricultural scientists, found that things like the language barrier, the difficulty of obtaining access to foreign publications (journals especially), and the perceived low value (in the eyes of many scholars from developed nations) of contributions from other countries (regardless of their objective quality), tends to deflate the number of citations that Brazilian scientists receive. [33] That is, scientists from advanced countries make "negligible use of work emanating from Brazil or from other peripheral nations - regardless of quality." [34]

There are a couple of implications of the above analysis. On the one hand, using CA to assess the importance of broad areas of scholarly discourse, especially in an international context, will clearly privilege North American and European contributions. This of course has more to do with the biased citation behaviours of North American scientists rather than intrinsic flaws in the CA methodology. Nevertheless, using CA to evaluate international contributions as some suggest will definitely *institutionalise* the bias by *erasing* the processes behind biased citation behaviour and obscuring them with a veneer

of quantitative objectivity. As a result of these barriers, any CA analysis conducted to evaluate the "international" impact of scholarly research will systematically devalue the contributions made by peripheral nations. Ironically, this devaluation mirrors and supports an already stratified and one-way flow of information [35] and news. [36]

On the other hand, it is well known that the interests of specific groups can be imbedded in science and technology. [37] Although this is an empirical question needing investigation, it is possible that the citation practices outlined above might have a tendency to silence scientific or technological alternatives emerging from undeveloped nations. And although the bias is not intrinsic to CA, the weight generally given to measures in science that give the appearance of "objectivity" and "neutrality" (i.e., quantitative measures) would again tend to institutionalise the erasure of contributions emerging from other countries.

This hypothesised erasure of scholarly alternatives has been studied by Catherine Lutz in work that makes the gender bias of citation measures explicit. Catherine Lutz concludes that the contributions of women Anthropologists is partially erased from the citation record by the simple failure of male authors to include important contributions in key "canon setting works" (i.e., review articles in key review journals) in the Anthropological field, and also by the differential citation rates of men's and women's articles (i.e., rates of citation of women authors does not keep up with their actual production rates). [38] The implications are clear - at least in Anthropology. Using CA to measure scholarly output privileges men and institutionalises and objectifies politically informed citation choices.

This erasure occurs in sociology as well. Kathryn B. Ward and Linda Grant. [39] examined 10 major sociology journals between 1974 and 1983. Using a typology that distinguished articles on the basis of there being simple **additions** to sociology (so called sex as variable studies examining gender differences), **modifications** (conceptual, theoretical or methodological refinements), or **recasts** (attempts to recast extant theoretical frames that are clearly androcentric and deficient for understanding women's experiences), the authors found some evidence to suggest that the more radical the challenge to existing theoretical orientations, the more likely the contribution is ignored and ghettoised. The authors note:

> Recase [sic] articles were even more likely to be ghettoized and ignored in citations of later published works. In suggesting that research on gender and women move away from reliance on males as the norm, these articles raised the most fundamental challenges to traditional sociological approaches. Citations were notably absent from "state of the art" or theoretical synthesis papers published in later

> volumes of journals in which the recasts appeared. Recast papers were not criticized or refuted by authors working in substantive fields where they were relevant; they simply were ignored. [40]

Whatever the explanation for this phenomenon, the result is the same as other forms of erasure. More significant contributions, and those which challenge scholarly canon, are partially or wholly erased from the citation record.

Clearly, there are political implications to the use of CA. From its statistical failings to its tendency to erase the contributions of women and minorities, CA would seem to be a largely unreliable tool for assessing scholarly impact. You would think that any reasonable scientist, recognising that CA is statistically unreliable and that it can be used as a political weapon, will gladly put the use of the measure to one side. This, however, appears hopeless fantasy. As always, science is embedded in the social, political and psychological world of its practitioners and it is these structures which govern whether or not CA will be expanded or dropped as a useful tool for measuring scholarly impact.

In Defence of CA

Even though proponents of CA often recognize its profound limitations, they defend it. And the contortions and desperate clutches at respectability that have emerged can be both amusing and disturbing. For example, Stephen Cole and Jonathan R. Cole argue that a better indicator has not been developed and since the measure has proved useful to some, why not use it? [41] But the question remains: no better indication of what? Of scientific eminence? Of scholarly contribution? Of core journal lists? CA is worse that useless for these purposes because as numerous critics have pointed out, it does not fairly represent the entire scholarly communication system. CA tends to support a particularly one-sided, reified, and elitist view of scientific contributions that ignores (i.e., fails to make visible) certain groups of scholars and in fact justifies the highly stratified nature of the academy and, by implication, the capitalist system. If the indicator is invalid or far from ideal then that should be the end of the story. You would not find a technician charged with the calibration of a fine piece of equipment holding so lackadaisical an attitude towards the cannons of validity and reliability. Especially if an error in calibration were going to cost somebody a great deal of time and money. And should an error occur, an "its the best I could do" would not suffice to discharge the technician of responsibility.

In any case **there are** better ways to assess the communication system in science that provide a truer representation of the underlying processes. These are the historiographic methods often pointed to by opponents of CA and that, when considered with an eye not blinded by the sterile application of the cannon that suggests that quantitative methods are always superior to

qualitative methods, are clearly better suited to determining the dynamics of the scholarly enterprise.

Still, this isn't enough to put to rest the arguments for CA. Others have set up a last ditch defence by claiming validity for CA because it correlates with other measures of scientific eminence like the Nobel Prize, other large awards, e.g. the allocation of research funding, membership in major academies such as Royal Society or National Academy of Science, or the use of a scientist by Government or on advisory committees. This *criterion validity* is taken by some to be the final and irrefutable indication that the methodology is valid. Laura M. Baird and Charles Oppenheim make this argument: [42]

> All of these measures, whether objective ... or subjective ... can be measured. What is embarrassing for the critics of citation counting is this fact: whatever measure you take for the eminence of an individual scientists or of a journal or an institution, citation counts provide strong correlations with that result. This must be very frustrating for the people who criticise citation counting...So, despite the many valid criticisms of the crudity of citation counting, the fact is that they reasonably reflect the esteem that a particular author or paper enjoys.

These authors seem to be forgetting that correlation does not necessarily indicate causation and that covariance of another, invisible variable may be at the root of perceived correlations. A factor that immediately comes to mind as one explanation is the author's location in relation to the scholarly mainstream and orthodoxy. Surely it is not unreasonable to suggest that authors that receive all these wonderful awards simply tow the academic orthodoxy and are, in like fashion, rewarded for their recognition of the imperatives of the scholarly system? Might there not be an establishment science [43] supported by, and reflected in, all these various measures?

The answer is "of course." There is a science that represents the establishment and there is a science that reflects capitalist class structures. [44] And it is the ability of CA to faithfully represent this status quo with apparent objectivity that makes the measure so popular. This is, of course, a fairly broad claim and I'd like to address it in what remains of this chapter and the next. In the next section, I'd like to deal with the powerful social and political forces which are creating the pre-conditions for the expansion of CA as the methodology of choice for assessing scholarly output. Here, I'd like to bracket these concerns and suggest that the primary reason why advocates of CA are reluctant to give the measure up is because CA is one of the best methods available for those in status quo to add discursive weight to what is otherwise a questionable stratified structure in the sciences.

The Mythology of Science - Justifying Inequality in the Academy

In order to understand why CA remains such a popular tool for assessing scholarly contributions among its advocates, we have to understand the mythology or normative structure which informs our thinking about scholarly inquiry. To put it simply, the structure of science is hierarchical and class, gender and racially stratified. A system such as this must of course be justified and such justifications have been typical of reflexive scientific thought since Galileo first wrote that science was "for the minds of the wise" and should be kept from "the shallow minds of the common people" lest they "become obstinate, and contumacious in yielding assent.…" [45]

There is nothing new about this insight. Bukharin noted that a class system of science existed despite the protestations of its practitioners. And others have noted the "male-operated exclusion mechanisms," [46] the infiltration of male intellectual and cultural values (absolutism, authoritarianism, determinist thinking, cause-effect simplifications, androcentrism, ethnocentrism, and pretensions to objectivity and neutrality), [47] and the hegemonic service science provides for class, gender, [48] and colonial structures of domination. [49] Bukharin notes:

> Bourgeois scholars always maintain that they are the representatives of so called "pure science", that all earthly sufferings, all conflicting interests, all the ups and downs of life, the hunt for profit, and other earthly vulgar things have no relation whatever with their science. Their conception of the matter is approximately the following: the scholar is god seated on a sublime eminence, observing dispassionately the life of society in all its varying forms; they think (and yet more loudly proclaim) that vile "practice" has no relation whatever with pure "theory"…. This being the case, it is perfectly clear that the social sciences have a *class* character. Each class has its own practice, its special tasks, its interests and therefor its view of things. The bourgeoisie is concerned chiefly with safeguarding, perpetuating, solidifying, extending the rule of capital…. [50]

Part of the safeguarding and extending of the rule of capital will be through providing systems that justify inequality. Science's exemplary record of twisting biological reality to support and naturalise systems of class and gender hierarchy is well known. [51] But what is less well known and understood, apparently, is the scientific and discursive work that scientists themselves engage in to support the stratified nature of their own discipline.

Before going on to discuss the role that CA analysis plays in the discursive justification of scientific hierarchy, it would be worth reviewing the evidence

against rigidly hierarchical views of the scientific enterprise. As some have recognized, there isn't really much evidence to support naturalistic interpretations of stratification in science. We've already seen some of the counter evidence in our earlier analysis of CA where we demonstrated that CA erases certain forms of scholarly contribution. This erasure of course objectively devalues contributions and muddies interpretation of scientific eminence.

However there is better evidence. The most explicit and sophisticated attack on the notion that there is a naturally based hierarchy of ability, devoid of political or social-structural infiltration, was made by Dean Keith Simonton [52] He recently published an extensive and detailed evaluation of the social context of the career of scholars. Simonton's findings are worth quoting at length:

> …the most significant consequence of the present study is the demonstration that even the highest grades of genius are closely integrated into a larger social world. Within the scientific community...exceptional accomplishments are part of a more comprehensive set of influences and interactions, some cooperative and others competitive, some intimate and others remote. Particularly remarkable are the numerous correlations that ultimate eminence and lifetime productivity both exhibit with the quantity of representatives in diverse social relationships. Even after introducing a respectable number of statistical controls for such contaminants as data quality and richness, disciplinary domain, and year of birth, the more famous a scientist eventually becomes, the greater the expected frequency of influencers, professional contacts, associates, collaborators, rivals, intimates, and influencees. Furthermore, even after inserting additional control for eminence, individual differences in the total output of contributions are positively correlated with having numerous influencers, contacts, associates, collaborators, rivals, intimates, siblings, pupils, successors, and influencers who have also made names for themselves in the annals of science. Hence, no matter whether we examine eminence or lifetime productivity, outstanding contributors are more likely to form part of a complex network of relations both contemporaneous and cross-generational, both personal and impersonal, both symmetric and asymmetric. [53]

Simonton's finding is echoed by Robert Merton's analysis of *institutionalised serendipity* and the *Mathew Effect* [54] and also by Margaret W. Rossiter's *Matilda Effect.* [55] What we are seeing clearly here is evidence for the importance of cultural (or rather scholarly) capital [56] in the creation of the great

men [sic] of science. Perhaps a better way to phrase this would be to suggest that class background is a key determinant of subsequent scholarly eminence. This finds support even when applied to women who have succeeded in the predominantly male scientific system. As Hilary Rose notes of the autobiographical accounts of women who have made it, "reading across these autobiographical accounts typically shows a highly privileged class origin and the unusual support and encouragement of a scientist father or husband." [57]

Its not just that class plays a role in determining scientific advance. It also plays a determining role in the expensive lab where vast arrays of fixed and variable capital (instrumentation and human labour) are arrayed into a system of production that benefits those with privileged access. Those with such access are able to use the laboratory to produce research articles which can then be used in a recursive fashion to justify the hierarchy that went into creating them. The individual who is considered the senior author is generally seen as "responsible for the inspiration and intellectual content of the research." [58] In this way, individuals are able to exploit access to the scientific means of production and thereby advance their own programs without necessarily extending credit to all involved. The prestige and eminence that accrues to the "head" of a research program is justified by citation analysis and the support labour which went into the final product is erased from the record. As A. J. Meadows notes, studies

> of informal communication in groups reveals the major part played by transient members, not simply in research, but in communication. Many heads of groups find it difficult to keep up with formal channels of communication, so use the junior members of their groups to filter relevant information from the literature and pass it on to them orally. Though research students and other junior members may only figure individually to a small extent in formal communication, they can thus play an important part in informal communication. [59]

So class, rather than innate ability, plays a major role in determining scientific advance. For practitioners of science the problem becomes justifying this hierarchy. The is done in two part swing of the pitard. In the **first part**, individuals are decontextualised and stripped of their historicity and embeddedness in a class, gender, and racially stratified system of rewards. And individuals and by creating a mythology of scientific greatness. In the **second part**, the decontextualised individual is recreated as a "great man" or mediocre peon. When the pitard is sheathed all we see is a naturalised and justified hierarchy.

First, CA decontextualises. As a methodology, it naturally extracts those individuals most embedded in the scholarly status quo. Because CA is just a number, all the historical and social complexity that went into creating this

eminent individual is lost. We don't have to worry to much about that though because what CA tells us can be taken as a given. This is because CA participates in the commonly held bias that science progresses through the objective application of neutral methodologies. Traditionally, the most "neutral" and "objective" methodologies have been the quantitative ones. These are the one most resistant to claims of bias. Of course, science is not neutral. And neither are quantitative measures. But most everyone likes to believe science is - and numbers are - objective. And this belief has profound consequences. Sandra Harding notes that seeing science as fostering belief that science is neutral depoliticizes science and obscures the ways in which knowledge is generated. Buying into the simple objectivity most often articulated by practitioners of science....

> certifies as value-neutral, normal, natural, and therefore not political at all the existing scientific policies and practices through which powerful groups can gain the information and explanations that they need to advance their priorities. It functions more through what its normalizing procedures and concepts implicity prioritize than through explicit directives."[60]

It is the same way with quantitative measures, and especially with CA. Buying into the belief that they are objective depoliticizes them. Once this has been done, the information embedded in the numbers (i.e., scholarly eminence, intelligence quotients) are not seen as participating in a political or social [re]ordering. They become "objective." Never mind that the numbers might represent a certain view of reality by ignoring certain aspects of reality. That possibility is no longer entertained because investigators have bought the myth. Once this construction of "facts" has been accomplished, the facts can perform a duty as arbitors of ultimate reality without being subject to constant criticism as, for example, more "qualitative" methodologies might be.

Once we have a decontexualised number, we have to interpret it. Numbers are not simply explanations. Theory enters both into the creation of the number and into its subsequent use in a theoretical explanation. It follows that an additional part of the process of justifying inequality will be the creation of a theoretical and discursive frame on the other side of the number. And this has been the case. Various authors have developed suitable discursive frames and explanations for the numbers given by citation analysis. This discursive frame is articulated in a "great 'man'" myth of science. Sometimes this mythology has found expression in an evaluation of the journal literature as in Derek de Sola Price's classic citation analysis which distinguishes between "classic" and "ephemeral" publications outlets based on the citation patterns and half-life of particular articles. Based on his findings, he offers the following conclusion:

> From a preliminary and very rough analysis of these data I am tempted to conclude that a very large fraction of the alleged 35,000 journals now current must be reckoned as merely a distant background noise, and as a very far from central or strategic in any of the knitted strips from which the cloth of science is woven. [61]

More often, the mythology is directed explicitly at individuals. There is Ziman for example who says:

> Not only is there too much scientific work being published; there is much too much of it ... the need to get recognition by publication forced each of us to shout a little longer and louder so as to be noticed at all in the gathering, swelling, crowd of voices .... The result has been a proliferation of semi-literate, semi-scientific, half-baked and trivial material which threatens to swamp the whole system. [62]

It is here, as a justification for scientific inequality, where CA functions most clearly in support of a "great man" myth of science. CA provides an objective indicator of eminence - one that cannot be easily argued with - because CA is an "objective" indicator. But it obviously cannot do this without the prior elaboration of the "great man" discursive frame in order to certify and situate CA as a suitable methodology for supporting the hypothesis. Advocates of CA have created this discursive framing in their elaboration of a "hypothesis" which they then claim CA discredits with the full weight of scientific rigor. The hypothesis, simply stated, as that *all* individuals involved in science contribute to the scientific enterprise. The goal is to reject this naive hypothesis through the acid test provided by CA. It is in the development of this frame, and especially in the oversights which led to the creation of this mythology, were the ideological underpinnings of CA become most apparent.

All this, the creation of the mythology and the certification of CA, is powerfully reflected in the work of advocates of CA, Jonathan R. Cole and Stephen Cole. These authors are quite straightforward about the philosophical and normative underpinnings of their use of CA. In a seminal article, they draw on the work of Jose Ortega y Gasset in order to frame their position. They argue that Ortega believed that science advanced via the contribution of a great mass of men [sic] of only mediocre talent. Cole and Cole call this the *Ortega Hypothesis* and set forth to deny this hypothesis with, you guessed it, citation data. They conclude, not surprisingly, that because CA clearly indicates that only a few get cited while the vast majority go uncited (the skewness of the CA index), science advances only through the contribution of great "men." In a complete rejection of their interpretation of the Ortega hypothesis the Coles suggest that:

> ...even the scientists who make these 'smaller' discoveries come principally from the top strata of the scientific community. In the proper perspective of the history of science, 'normal science' as Kuhn defines it, is not done by the average scientist, but by the elite scientists. Indeed, in the longer perspective, the work of many of today's outstanding scientists, such as Nobel Laureates and members of the National Academy of Sciences, may turn out to be minor footnotes in the history of science. [63]

This sort of thinking has held on for decades. Most of the time it has been barely held beneath the surface. Recently though it has been pushed with renewed vigour in response to critical questioning of the mythology. A criticism of CA and its use to reject the Ortega hypothesis by M. H. MacRoberts and Barbara R. MacRoberts [64] appeared in volume 12, number 5 issue of the journal *Scientometrics*. The appearance of the article prompted a vociferous defence of CA and the original rejection of the Ortega hypothesis. Further evidence of a renewed popularity is provided by David P. Hamilton and recent controversial findings from the Institute for Scientific Information (ISI). [65]

Recent work has pointed directly to the processes whereby this mythology of science was created - that is, through creative oversight and omission of key passages in Ortega's original work which *changed* the meaning of what Ortega had said. Heidi Lee Hoerman and Carole Elizabeth Nowicke [66] provide a brilliant analysis of the critical error on which CA has been based. After noting that Ortega, contrary to the characterization of the Coles, had a singularly negative view of the modern specialist scientist, and in fact lumped all modern scientists into the category of "learned ignoramus" because of the overly specialised, one-sided, and thin view of the world they possessed, Hoerman and Nowicke go on to demonstrate how Cole and Cole in their 1972 article [67] misunderstand, misrepresented, and misquoted Ortega in their conceptualization of scientific eminence and how this misquotation subsequently became the unfortunate shibboleth of numerous authors who drew not on original sources but on the faulty interpretation of the Coles'.

In the extract below, Hoerman and Nowicke [68] first present the quote as used by Cole and Cole after which they point out the missing sentences that indicate Ortega's true thinking about scientific advance. Curiously, the omissions of the critical passages was not even signalled by Cole and Cole with an ellipse. A transcript of the analysis of Hoerman and Nowicke is provided in the following three paragraphs. First follows the original quotation as used by Cole and Cole. Note the spaces where discrepancies are noted but not acknowledged in the original citation

> For it is necessary to insist upon this extraordinary but undeniable fact: experimental science has progressed thanks

> in great part to the work of men astoundingly mediocre, and even less than mediocre. That is to say, modern science, the root and symbol of our actual civilization, [discrepancy 1] finds a place for the intellectually commonplace man and allows him to work therein with success. [discrepancy 2] In this way the majority of scientists help the general advance of science while shut up in the narrow cell of their laboratory, like the bee in the cell of its hive, or the turnspit of [discrepancy 3] its wheel.

Hoerman and Nowicke draw our attention to the missing sentences in discrepancy 2. Interestingly, in those sentances, Ortega y Gasset damns **all** scientists for their one-dimensional and superficial view of the world, and characterizes them as the epitome of the mass-man.

> The reason of this lies in what is at the same time the great advantage and the gravest peril of the new science, and of the civilisation directed and represented by it, namely, mechanisation. A fair amount of the things that have to be done in physics or in biology is mechanical work of the mind which can be done by anyone, or almost anyone. For the purpose of innumerable investigations it is possible to divide science into small sections, to enclose oneself in one of these, and to leave out of consideration all the rest. The solidity and exactitude of the methods allows of this temporary but quite real disarticulation of knowledge. The work is done under one of these methods as with a machine, and on order to obtain quite abundant results it is not even necessary to have rigorous notions of their meaning and foundations.

Clearly, Ortega is not providing support for anything like the "great man" hypothesis as conceived by the Coles. He is suggesting that science has become characterized by reified knowledge and has become over specialisation to such a point that any one can contribute regardless of ability. He futher slams the compartmentalised mind and points to superficiality of knowledge as the ultimate consequence of the direction of scientific advance. That the Coles would twist this passage is key evidence that they are constructing a frame and supporting it by creating a positive modality with links to older literature. The presence of this error points directly to their discursive work. It also points, in as direct a fashion as is possible, to the underlying motivation of advocates of CA. We can reasonably ask why they have distorted the views of Gasset. The answer is simple. This myth, supported by this "hypothesis" provides the critical entry point for CA. Once situated in this position as arbiter of scientific eminence, CA can then justify the hierarchical nature of science. If nothing else its an interesting example of how scientists

manipulate modalities in order to creation rhetorical weight for their statements. [69] However as I have tried to argue, it is much more than that.

Conclusion

In this chapter we've examined CA analysis and found it an unreliable and mischievous measure. We've noted its bias and that it represents that status quo in science via a powerful rhetorical frame that decontexualises the CA number at the same time that it weaves a "theory" to explain what has been uncovered. In this regard, CA functions rather like an IQ test. Both are presented as an objective "best guess" about underlying intellectual potential. Elimination of both is resisted because there is "no better measure." Both reify intellectual processes and ignore subtle social, psychological, and cultural variables (they decontextualise). Both ignore cumulative advantage. Both marginalise and further disadvantage groups that have been traditionally disadvantaged. Both justify inequality and stratification. Both are overly simplistic measures and finally, both can be used for social control. [70] Everything that IQ testing does, CA does - only applied to the academy. The similarity of the two measures has prompted some authors to make a direct comparison. "Bibliometric measures are like IQ tests, " [71] we are told flatly and without ceremony.

We also noted in this chapter that CA continues to be a popular and much advocated measure. Is this continued popularity a problem? Maybe. In the next section we'll take a speculative look at how CA might interact with new communication technologies to produce a much more ridged, hierarchical, and authoritative academy than currently exists. After first exploring in more detail the political reasons for CA's continued and recently revived popularity, we go on to examine why we might expect CA to flourish, and how CA might fit into the development and spread of a total system of control made possible for the first time by electronic communication systems.

Chapter Six:
The Cybernation of the Academy

> The figures are far from useless. But their value comes into their own for outsiders. They constitute science... as an object ready for discipline. It is grouped, ordered, ranked, graphed and compared. Complex fields are broken down into lists of numbers; sophisticated theories are present only as titles and numerical codings. CA provides a world where some can be rewarded and others punished; a world where funds can be targeted and weak spots identified. In fact, it is a world ready for the workings of the science barons - the Research Councils, the Committees of Vice-Chancellors, the Universities Finance Council, the Department of Education and Science. [1]
>
> The games that could be played with citation indices are a computer programmer's dream come true! [2]
>
> The panoptic sort victimizes because it decontextualizes. Status is divorced from circumstance. The circumstance cannot be recaptured; an assessment will always be incomplete. However, the ways in which context is misrepresented are not randomly distributed but reflect an institutionalized bias; a bias established by race, gender, age, class, culture, and consciousness. [3]

Introduction

In the last section we spent some time discussing citation analysis. After a brief historical overview and epistemological critique, we went on to suggest that one of the primary reasons why CA remains such a popular and proselytised tool is because of its discursive function in naturalising and objectifying a scientific hierarchy. In this section I'd like to extend the discussion of CA into the political and electronic realms. After discussing the administrative uses of CA, another of the reasons for its ongoing popularity, I'd like to suggest that we are rapidly moving towards a future academy characterized by panoptic (or cybernetic) system of control over scholarly discourse. This future, first envisaged by Lyotard, is being foisted on

> academics by ongoing global restructuring and the colonization of the academy with a neo-right discourse of efficiency and control.

It is the argument of this section that CA may eventually become recognized as the most suitable measure for controlling scholarly discourse. This is not only because developing citation indexes will be made easier as more and more journals move online and their citation lists become accessible by automated search engines, but also because, with the move towards destructuring scholarly discourse (i.e., scholars publishing their own works), CA will become one of the only (if not the only) method for adjudicating the quality of publication. This potential for the development of this cybernetic panopticon has profound implications for scholarly discourse.

As we move through our analysis of CA in the electronic realm, we will also attempt to develop an alternate way of conceptualising information technology in general, and scholarly communication in particular. There have been a number of calls and suggestions for developing an alternative discourse for understanding and thinking about technology and the new cyber realities. For example, Dennis Dion [4] notes of the new cyber realities...

> ...we do not yet have an adequate language for conceptualizing the profoundly reconfigured political and social worlds we now inhabit. So many folks "wear" a discourse on technology, ideology, symbolic meaning, economic activity and politics that is a bad fit for understanding contemporary arrangements.

In this chapter I would like to offer cybernetics and cybernetic systems of control as one possible avenue for understanding the profoundly reconfigured academy that may emerge as the result of the penetration of the new information technologies.

Quantitative Indicators and the Administration of the Academy

In last chapter I suggested that CA remains a popular measure because it faithfully represents the status quo in science. As a result, for many, it is an extremely useful way of reifying the social order in science. Indeed, it is very good at this not only because it sits inside a discursive frame which provides theoretical support for this use of CA, but also because CA is a quantitative and, by association, and objective measure. The quantitative nature of the measure gives it considerable weight over and above fallible human judgement and it is this aspect of the measure which makes it

potentially so attractive to academic administrators.

As many now understand, there is a certain mythology about scientific methodology in general, and quantitative indicators in science in particular. [5] Quantitative measures are the *hard* measures of an *objective* science. They are, according to this mythology, objective, rigorous, and closed to the incursion of personal bias. By contrast, qualitative methods are seen as soft, less rigorous, and open to the incursion of personal bias. [6] Because of the propensity of some in the academy to see numbers in this semi-mythological light, quantitative methods become difficult to argue with. Numbers give us "just the facts," nothing more nor less. In this context, numbers come to carry a discursive weight that far outweigh any reasonable levels of confidence we should attribute to them. This is so not only because it has become obvious that all observations are theory laden, but also because over 20 years of research has repeatedly pointed to the ways in which conservative, sexist, and androcentric biases infiltrate even the most objective measures of science. [7]

Yet despite the well established problems with an uncritical empiricism and uncritical acceptance of the methodological canon of science, many continue to accept and propagate the myths. We can see this dynamic clearly in the case of CA. The method is seen by many as useful not so much because it can provide a representative view of the scholarly communication system, but because its status gives CA a certain credibility that makes the indicator difficult to question. Consider the following comments by W.E. Snizek [8] as he responds to a suggestion by M.H. MacRoberts and Barbara R. MacRoberts [9] that more qualitative indicators of scholarly contribution would provide a more **valid** representation of the system. Note how he disparages qualitative methods and argues that CA as a quantitative method is vastly superior.

> I consider as unrealistic the extremes to which MacRoberts and MacRoberts would take these methods. From a metasociological perspective it would appear that such a technique all but precludes objective nomothetic investigation in favor of a more subjectivist idiographic description. In the final analysis, various cannons of science concerning the reliability and validity of measures, as well as the reproducibility of results must be upheld. In this regard I find citation analysis to hold imminently more promise of success in testing the Ortega hypothesis when compared to the blatantly unscientific nihilistic agenda advocated by MacRoberts and MacRoberts."

We know from the last chapter that the only blatantly unscientific and nihilistic research agenda surrounding the scholarly communication system is being pursued by advocates of CA. Given this, the claims of the MacRoberts that softer approaches to measuring scientific contribution might provide **more valid** (and hence more objective and rigorous) measures should at least be considered. Yet Snizek clearly and inappropriately generalises from the myths of quantitative indicators and discounts the words of the MacRoberts by appealing to scholarly cannon concerning the relative validity of quantitative vs. qualitative measures. Why, despite the fact that even advocates of CA acknowledge its limitations, do they continue to pursue it so vigilantly?

Essentially this is because CA shares in the mythology of quantitative methods. As such, it becomes an extremely useful weapon to be used in various positioning struggles inside the academy. Its status as an objective indicator obviates the need for political or social justification for decision making. Indeed, the very notion that CA might be an inherently political measure is neatly obscured by the canon of objectivity. CA becomes, in short, a political tool that deftly makes invisible underlying politics or bias. And this is key. After all, the "hard facts" cannot easily be trifled with. This dynamic of CA, i.e., its ability to give rhetorical weight to overtly political decision making, at the same time that it obscures the politics of the decision, is explicitly recognized by advocates. Consider that Waldo C. Klein and Martin Bloom like CA because it allows administrators to ease the overtly political process of hiring strangers and evaluating faculty. They say of their own studies of CA: [10]

> This article presents forms of citation analysis at national, local, and personal levels to provide a perspective on scholarly productivity that is free of the biases associated with traditional approaches of evaluating candidates for jobs or tenure and promotion.

I. Crewe also argues that objective methods of evaluation can be important for overcoming political difficulties. Crew [11] details the growing UK interest in the "quality of universities and their departments" and notes that attempts to assess graduate programs have been fraught with controversy because of the apparently subjective basis of previous methods of evaluation:

> The public ranking of universities inevitably provokes controversy, particularly when the ranking criteria depend primarily on personal private judgements ... professors and heads of politics departments signed a letter of

> protest to the UGC about "the lack of public information about the criteria used, the data gathered and the priorities employed" in its assessment. Maybe subjective and vague criteria are unavoidable. The 1985 Green Paper. The Development of Higher Education into the 1990s, acknowledges that "there are significant difficulties in measuring performance in higher education", that "some benefits may not be quantified readily at all", and that no significant progress has been made in developing performance measures for research.

Crewe's use of the 1985 Green Paper quote that suggested that "some benefits may not be quantified readily" might lead us to think that Crew was aware of the limitations of quantitative methods. However that is not the case, Crew goes on to describe the validation of quantitative indicators that would presumably provide a more widely acceptable, though no more adequate, measure of departmental performance.

Of course, as we pointed out in the last chapter, CA does not adequately represent the entire range of scholarly output. It has a traditionalist and conservative bias built in. Any use of CA to "depoliticize" university decision making quickly and deftly institutionalises and obscures this bias at the same time that it strengthens the political hand of certain segments of the academy. However, eliminating the political processes involved in hiring, faculty evaluation, or even institutional evaluation are not the only, and probably not the most sinister, uses to which CA can be put. Elkana et al. [12] explicitly point to CA as a method for controlling scholarly discourse. The authors suggest that quantitative indicators of science are useful for "Providing a means of discrimination within, and intellectual control of, the burgeoning information flows of 'applied social science' (while creating cognitive forms appropriate to the social discourse of an expanded policy-forming apparatus)."

Seeing CA as a administrative weapon helps explain why CA remains popular despite its obvious inadequacy. When we consider the CA may be less about an accurate representation of reality and more about political influence and control, then the anomalies in the discourse go away and the curious ambivalence of proponents becomes understandable. CA is indeed useful for certain purposes and for these purposes, the validity of the methodology is less of an issue that the perception that, because its a quantitative method, it provides an objective snapshot of scholarly contribution.

The Future of Administration

> By the latter part of the twentieth century, the early promise of practical usefulness had been realized to the fullest possible extent. Many scientists trained in an earlier era thought that it had gone much too far. For all knowledge production was by then linked directly to practical application, either by means of centrally implemented national science policies or by market mechanisms. The universities and colleges of higher learning had been replaced by what came to be called "knowledge factories." And the nation-states ... vigorously pursued policies of maximum economic growth, the success of which depended almost entirely on the regular production of new, science-based technologies intended to increase humankind's control over, and exploitation of, the natural world. [13]

So, advocates of CA see it as an administrative tool useful for lubricating university decisions and controlling scholarly discourse. However we still have to question the possibility of CA moving from the realm of possibility into an arena where the potentials of CA for increased administrative control become an actuality. It could easily be argued that this is not likely given the ongoing political struggles and positioning that occurs in all university departments. After all, the academy has always prided itself on its history of free and independent inquiry and development. And there have always been checks that prevent scholarly discourse from being shaped by anything other than the requirements of the critical thought (tenure for example). A few people might like to see CA expanded beyond its currently limited purview. But perhaps they are only engaging in self-interested promotion of their chosen area of speciality. We might have good reason to believe that CA will never be allowed to penetrate into the academy.

Yet there are a number of forces that are combining to push the academy towards an increased reliance on quantitative indicators. What Fairclough [14] calls the commodification of the academy is one such factor. A combination of funding cuts, declining government expenditure on basic research, increasing competition from community and four year colleges causing declining enrolments, and increased emphasis on securing research dollars through private sector partnerships, [15] has put tremendous pressure on universities and have pushed them towards the adoption of strategies of operation and forms of discourse normally found only in the private sector. Fairclough calls the general process whereby the

conceptual framework and day to day operations of an institution are replaced with forms of discourse and strategies peculiar to another realm as colonization. When the previous discourse is being replaced by ones found in the commodity sectors, the process is called commodification. Fairclough notes: [16]

> Commodification is the process whereby social domains and institutions, whose concern is not producing commodities in the narrower economic sense of goods for sale, come nevertheless to be organized and conceptualized in terms of commodity production, distribution and consumption....In terms of orders of discourse, we can conceive of commodification as the colonization of institutional orders of discourse, and more broadly of the societal order of discourse, by discourse types associated with commodity production.

As academic institutions become more like business organisations (they increasingly seek to make "cents"), they are adopting similar priorities. One such priority is an overarching concern with productivity. As the pressure to ensure market viability builds, organisations are searching around for adequate performance measures and in the process they are settling more and more on quantitative and "objective" measures that, in the eyes of many, unequivocally demonstrate performance or lack thereof. In the quote below, A.J Meadows [17] speaks about this increasing pressure and the push for the use of quantitative indicators. Though his comments do not specifically apply to academic settings, he nevertheless captures the current pressures faced by all organizations.

> In recent years, the pressure to justify library activities has grown. It has been particularly noticeable in the U.K. where it has been part of a wider drive for accountability in public enterprise, but the trend is worldwide. Justification for new ventures in times past has often been provided by the gut feelings of experienced librarians. Such feelings may actually be quite good indicators. Unfortunately, they are becoming increasingly unacceptable to committees. Justification, not only for new developments, but for maintenance of the status quo, is now seen as requiring systematically presented evidence preferably based on quantitative data.

Productivity measures are of course not new and therefore in and of itself, the commodification of the academy is not a sufficient condition for arguing

that CA will grow in importance. In the past universities have got by with simple publication counts. Unfortunately, publication counts are likely to decline in importance and be eclipsed by *post-publication* measures of scholarly impact as we move more towards alternative delivery of scholarly output in the electronic realm. James S. Gardner Vice-President Academic and Provost of the University of Manitoba speaks directly about the impact of electronic publication on simple publication counts. He notes that because of extremely low entry cost and the relative ease of publishing electronic material, it is much easier for individual scholars or groups of scholars to publish, without the assistance or imprimatur of traditional presses, their own material. This ease of publication may encourage the proliferation of publication outlets outside the mainstream.

According to Gardner, new publication technologies like the WWW allow authors to circumvent the traditional methods of quality control (i.e., peer review) thereby undermining the validity of traditional publication counts. If authors can publish their own material without the assistance of traditional journal publishers, how can we assign publication credits based on the prestige of selected outlets? Gardner suggests [18] that as this shift occurs, measures based on the post-publication impact of a scholar's work will become much more important in the evaluation process. Post-publication measures (of which CA is the quintessential example), Gardner believes, are much more sensitive to the *quality* of a publication. Poor quality papers will largely be ignored in subsequent scholarly work whereas higher quality papers will be used (i.e., cited) in the work of others. Therefore, these post-publication measures provide a quick and objective assessment of the impact of a scholarly work regardless of its method of distribution.

When universities start searching for a way to evaluate the post-publication impact of articles, they will inevitably fall upon CA as the ideal method. What better way to assess the impact of a scholarly paper than by counting how many other people have used the article in their own work. And even if some argue that the method is far from ideal, it is after all the only one available (always the last line of defence of advocates of CA). And what's even better, it has the additional benefit of being easy and inexpensive. Garfield's annual citation indexes are the only research tools that are currently required to determine the Citation IQ of individual scholars.

While CA is easy enough to do with present tools, the future holds the possibility of automated citation indexes and perhaps even daily citation update services delivered directly to the desktops of administrators and scholars. This indeed will be the final value added function of CA likely to bring a gleam to the eye of embattled administrators and ensure its widespread adoption. As scholarly communication moves online, it will

become increasingly easy to develop powerful citation engines that index all the relevant publications on the WWW (i.e., those that are the core journals in a field). Like the WWW search engines that now automatically traverse the web gathering documents for indexing in massive databases, Citation Worms will nightly span the internet gathering citation data. Analysis engines will collate the data and Mail Daemons will send out summary packages allowing subscribers to track the ongoing impact of their or their colleague's work.

The development of CA on the Internet is not as far off as we might think. There are already calls for the development of online citation databases capable of indexing all publication. [19] So two or three years seems a reasonable prediction. The first requirement for automated citation analysis, that all relevant journals be online, is already well on its way to realisation. The second requirement, that appropriate citation engines be developed, is discussed by Andrew Treloar. [20]

> It is possible to imagine some sort of automatic Web-traversing robot that builds up a picture of which links pointed to which documents for the purposes of citation analysis. Whether anyone will undertake this task, and what the bandwidth implications would be, are another matter all together.

Treloar expresses doubt about the technical feasibility of Citation Robots. But his doubts are now misplaced. The bandwidth certainly exists. Current internet capacity already allows for 2 or 3 dozen different WWW robots to continually traverse the web gathering and collating information. Robots performing citation counting will require much less bandwidth because they will be targeted at only two or three thousand scholarly journals, and they will only be dealing with the reference section of the document rather than the entire text. As for the time required to develop a citation robot, this is unlikely to be a difficult or time consuming task. The easiest approach would be to modify currently existing WWW robots. However even if this approach is not taken, there is now sufficient programming expertise to enable an entrepreneur to easily hire the requisite programming skills.

These factors, i.e., the push for administrative measures of productivity, the growing need for post-publication measures of scholarly impact, and the growing ease and power of information technology to automate citation indexing, all point in the direction of a growing utilisation of citation analysis in the academy. This is highly relevant to our discussion of electronic scholarly communication since as we have seen, electronic journals will facilitate a global use of automated citation indexes. In the next section I

would like to examine some of the implications of a wider use of CA in the academy. Keep in mind as you read the next section the material contained in Chapter Five since the profoundly negative implications that will be discussed in the next section depend on an understanding of the limitations and biases inherent the methodology of CA.

Implications

There are those that might argue that the colonization of the academy with the language of the market and the resulting emphasis on productivity and quality is not necessarily a bad thing. In popular discourse, the term often used to point to the benefits of increased awareness of the importance of the consumer is "accountability." Perhaps the trend is towards more "accountability" in the profession and therefore not a bad thing. Take teaching for example. One possible scenario of making instructors more "accountable" to the people they teach might be a more dialectic and less hierarchical relationship between knowledge producers and knowledge consumers. Should this occur it would be a great step forward since it would result in a less hierarchical, more nuanced, and multivocal approach to teaching (i.e., anti-racist, anti-sexist).

However such a postmodernist spin on teaching will not emerge - despite the agitprop used to soften resistance to the incursion of commodity discourse - simply because the resources needed to actuate a truly consumer orientated teaching curriculum are not available. Funding cuts and restructuring mean dwindling resources which in turn leads to increased class size, increased work load, and less workday porosity within which to design and implement truly progressive teaching strategies. In this context, satisfying the consumer (student) requires increasing the structures within which information is provided and enhancing the unidirectional flow of information rather than eliminating structure and moving towards more dialogic intercourse. [21] It is the only way to cope. If resources don't allow greater student participation, then they can only be satisfied (i.e., convinced they are getting good value for their money) by reducing ambiguity and specifying exactly what is required of them. We all understand this. Our own ambivalence towards productivity measures in this area, as well as the contradictions, are manifest in our discourse. We speak of quality education and in the same breath struggle like Sisyphus to increase our student output. More product, less funding. More science, less time. There can be only one result.

So too the measurement of research productivity is not necessarily a bad thing if enough thought is given to the fact that scholars can contribute to the scientific enterprise without ever publishing in the formal literature. But

because CA is completely insensitive to all but the most formal contributions to the academy, and even then it tends toward the conservative, it is biased and inadequate. CA does not fairly represent the entire range of contributions to the academy and hence using it as a way of measuring contribution will exacerbate already existing hierarchies and ensure that the vast pool of scientific workers never gets credit for their contribution. As we have seen, CA is a perfect prop for supporting the status quo.

Another potentially aversive impact of CA derives from its status as a high level scholarly IQ test. Blaise Cronin and Kara Overfelt note that citation counts have *diagnostic value*: [22]

> From an administrators perspective, the kind of data presented in this study could become the basis, or at least a component part, of a performance indicators database....there are obvious benefits. For example, the data in Table 13 provide an at-a-glance sense of how citation performance relates to, inter alia, faculty status, time-in-field, gender, and remuneration levels. This table has diagnostic value, particularly if one looks at variability across status lines....That kind of information has value in that it raises questions relating to the bases on which tenure is awarded, and may be useful in developing predictors of citation performance for tenure-track faculty. Given that recent research has shown that "there appears to be moderate to high correlations between citations to earlier work and citations to later work" ... it should be possible, with a dynamically updated database, to predict the future citation performance of junior faculty and/or to use such data to facilitate the development of customized research and publication strategies in order to improve an individual's citation profile over time. In that sense, citation-based auditing could be a useful self management tool for academic administrators at both the unit and institutional levels, particularly if the data are used to complement other information, both quantitative and qualitative, on faculty performance and productivity.

Here citation analysis could be used to assign scholars *Citation IQs*. Much like students are now streamed using SAT or GRE scores, scholars could streamed into careers most appropriate given their level of ability. Those that had lower citation IQs could be directed into streams that focus on instruction and be encouraged to take their first job in a community college.

Alternatively, those who have the highest citation IQs could be directed into research streams. No doubt prestigious research institutions will feel most comfortable hiring someone who has demonstrated research potential. Finally, older scholars who show creeping senility (i.e., declining CA scores) can be offered early retirement packages or other options that take them out of the scholarly mainstream.

Any form of early streaming is ridiculous because it accentuates over the long term any educational disadvantages that might exist for scholars in the early stages of their career. For example, if early citation data from the initial publication efforts of young scholars is used to predict the future performance of the up and coming next generation, then we might reasonably expect that those scholars coming from smaller, less well funded institutions with poorer overall publication records would experience a disadvantage. The unfortunate outcome of tagging scholars with a Citation IQ early in the game is that it sets up the conditions for a self fulfilling prophecy. Scholars may well accept their lower status and not even try to overcome any deficiencies (however minor) and, even if they don't accept the validity of the measure, universities will. In the financially strapped environment that is the academy these days it will make little sense for universities (especially the prestigious ones) to risk hiring someone with a low Citation IQ.

While the potential use of CA in hiring, promotion, and even retirement decisions is certainly something to be struggled against, a much darker implication of CA derives from its potential to direct and control scholarly discourse through a form of self policing. As you will recall, CA has a tendency to make visible only certain types of scholarship. It favours that status quo. It is biased against gender, race, and country. And, most important here, it tends to ignore substantive contributions that challenge scholarly cannon and favours run of the mill scholarship (what Kuhn might call Normal Science). Because of this, it isn't hard to imagine that scholars hoping to increase their Citation Quotient would consciously (and perhaps unconsciously) direct their research and teaching interests towards those disciplines and subdisciplines that have clearly demonstrated their greater ability to garner citations. By doing so, they would steer clear of the more controversial areas. The result would be a conservative shift in scholarly discourse.

The insidious part of all this would be that the reasons for the shift away from critical scholarship would be completely invisible to most scholars. Because the evaluative mechanism is embedded in "objective" technologies, the tendency would be towards becoming disciplinary subjects of a new and invisible discursive order. Individuals would

internalise the disciplinary mechanisms and the locus of administrative control would disappear in a puff of electronic smoke. This would fit nicely with technologies already in place for disciplining professionals and creating disciplinary subjects. Grey describes the transformation of overt administrative control towards the internalisation of discipline: [23]

> The rating procedure is thus transformed. Instead of being constituted as an irksome, intrusive and threatening technique of management control, it becomes a benevolent process for their realisation of this perfection, a technique to assist individuals to become their true selves and to realise their aspirations. Even the act of sacking is reconstituted through the personnel department as 'counselling' out'. A supposedly mutual career decision for the employee to leave the firm.

This phenomenon of self-policing might have the greatest impact on graduate students who, understandably nervous about having the best record possible when they enter the job market, would choose their substantive interests with the intention of increasing their post-publication impact. Graduate supervisors might also be expected to get into the act. The sage advice of a student's master may eventually be concerned less about just getting published and more about choosing the *correct* areas to publish in order to ensure a good Impact IQ. We can easily imagine a fatherly supervisor imparting the following pearl of wisdom. "The game has changed, publications are no longer good enough. You have to have citations." It is even conceivable that the prestigious departments, as part of their professional training seminars, would supply lists of highly cited areas in order to assist their graduate students, who ultimately contribute to the reputation of the department, to choose their intellectual interests wisely.

As noted, the beauty of the system is that it is invisible. And the potential for self-management and self-control will be even greater when and if CA is utilized with electronic journals on the Internet. At that point, it will become possible to plug individual scholars directly into a citation loop and have them monitor themselves on an ongoing basis. Each university could develop or purchase its own web roaming robots that go out into the net and retrieve data on its own members. Universities would easily be able to provide monthly or even daily citation updates. The scholar, worrying about tenure, advancement, or job openings will perform all the behavioural modification necessary to ensure the continued structural integrity and functioning of the scholarly system.

Having faculty evaluate themselves is not as far fetched a notion as it might appear to some. A number of departments at the University of Pittsburgh School of Medicine are experimenting with a faculty performance appraisal system. Faculty are given an appraisal form on computer disk. They are then required to update their CV any time they advance their research, teaching, administrative, or patient care activities. [24] In this way much of the work of the administrator can be shifted onto the scholar. Of course, there is the added benefit of shifting responsibility as well. This immunises the administrators against charges of bias or unfair evaluation since it looks like it is the scholars themselves who are performing the control function.

Cybernetics

So far in this chapter I have tried to argue that there is sufficient reason to expect that CA will become a widely accepted way of measuring scholarly output. I have also tried to suggest that if this is allowed to happen, then there will be a conservative impact on the hiring practices and discourse in the academy. I have also drawn links between CA and the new information highways and suggested that when CA is combined with electronic journals and other internet technologies, there will be a synergistic effect that will create a citation analysis far more pervasive and powerful than is currently possible. In this section I'd like to engage in a bit of creative speculation. Specifically I'd like to discuss cybernetics. And, I'd like to offer cybernetics (or at least my version of it) as one possible way to conceptualise new information technologies.

The question before us now is what is Cybernetics. Popular discourse on cybernetics characterises it as a collection of information gathering, processing, and transmitting tools that empower the user of technology and make the individual at the centre of the cybernetic system more powerful. [25] This way of thinking is of course an extension of the all too typical utopianism characteristic of much popular and even some academic discourses on technological change. For example, Dilys E. Morris [26] follows this line when he suggests that information technology is about democratisation, enhanced participation, and freedom.

> As the amount of information has grown, more and more people have come to work with it ... In most areas decision making has become considerably more participatory because the volume of information available on almost any subject can no longer be digested and understood by the limited number of individuals who once made decisions.

Extending this belief in the benign and empowering impact of technology into the realm of cybernetics, we are offered a vision of a machine enhanced human being - a cyborg [27] - that has capabilities far beyond the limits placed on our species by physical limitations. And individual power and control are only the first offerings of the new cybernetic technologies. There have even been claims that human/machine integration is the next step in human evolution. [28]

Like much of the current eulogising about the benefits of information technology, these popular myths about the beneficent have it about half right. Cybernetics *is* about enhanced power and control over the environment. But it is certainly not about individual democratic control of the environment or an enhanced evolution. Rather it is about centralised control or, in the words of Norbert Weiner who coined the term, command and control.

Cybernetics, according to Weiner, is about control through communication. Weiner had originally been concerned with the development of automated weapons systems. He had hoped that with developing computer technology it would be possible to increase the efficiency and accuracy of military technology through the application of cybernetic principles. The principles are easy enough to understand. Weiner felt that effective control (be that of technology or even humans) required a constant flow of *information* not only about, as in the case of weapons systems, the target, but also about the current state of the weapon itself. Weiner felt that this information could best be provided by designing mechanisms that would sense both environmental conditions and the state of the weapon. This information would then be collated and transmitted to either a automatic device or a human being charged with destroying enemy personnel.

Weiner's conception is really quite revolutionary when you think about it. Normally information is associated with greater democracy. But here Weiner is suggesting an association between information and centralized power. Weiner himself says it best. [29]

> In giving the definition of Cybernetics...I classed communication and control together. Why did I do this? When I communicate with another person, I impart a message to him, and when he communicates back with me he returns a related message which contains information primarily accessible to him and not to me. When I control the actions of another person, I communicate a message to him, and although this message is in the imperative mood, the technique of

> communication does not differ from that of a message of fact. Furthermore, if my control is to be effective I must take cognizance of any messages from him which may indicate that the order is understood and has been obeyed.

There are essentially four parts to a cybernetic system. There must be **something to be controlled**. This could be a machine, a weapon, a house, or even another human being. The nature of the thing being controlled is unimportant because theoretically cybernetic systems can be incorporated into anything that operates on the environment if information exchange mechanisms can be attached. There must also be a **central control unit** responsible for directing the actions of the system. There also needs to be **sensing devices** that gather the requisite information and finally, there needs to be a **technology capable of transmitting information** back and forth.

Weiner [30] was fairly unspecific about the types of mechanisms that would be suitable as sensors. According to him, pretty much anything that could provide useful information on a continuous basis to a central control unit would be acceptable. As to the command unit of the cybernetic system, Weiner was much more specific about its nature. According to Weiner, "the modern ultra-rapid computing machine was in principle an ideal central nervous system to an apparatus for automatic control." [31] But according to Weiner not just any computer would do. He compares the digital computer against early analog computing devices (never developed) and concludes that for the purposes of command and control, digital computers are much better suited to the task. Weiner [32] explains:

> This all-or-nothing machine is called a digital machine. It has great advantages for the most varied problems of communication and control. In particular, the sharpness of the decision between " yes" and "no" permits it to accumulate information in such a way as to allow us to discriminate very small differences in very large numbers.

The final aspect of cybernetic systems, the transfer technology, is not discussed by Weiner. However it is implied in the description of the system and based on the requirements of the other components of the system, we can construct what the transfer technology would be like. These transfer mechanisms are the hard wired connections between the components of the cybernetic system. In a system that controls the climate in a house, for example, the transfer technology consists of the wires, computer interface cards, and connections that link the system together and provide

information flow between the CPU, the sensors, and the actuating units (i.e. the air conditioner and the furnace).

It may seem a bit trivial to discuss the transfer mechanisms when the more important considerations are clearly the sensors and CPUs of the cybernetic system. However a closer look reveals that most people who discuss information technology focus almost exclusively on transfer mechanisms. That is, it is a consideration of the transfer mechanisms, and especially their speed and power, which populate the current utopian discourse of information technology. It is an appreciation of the increase of transfer power, the move from 56kb lines to the high speed Internet connections, and the potential for rapid exchange of information that have people singing the glories of the new information revolution. People love talk about the way high speed technology or high speed information exchange is changing the face of the world.

What is generally lacking in these discussions is a more systematic analysis of all aspects of the cybernetic system. As we will see below, when you conceive of information technologies like the Internet as part of a cybernetic system, it is much easier to develop a deeper analysis of the implications of developing information technology. Using the analogy of cybernetic systems forces a consideration of the control implications of internet technologies. It is thus a useful metaphor to approach an analysis of information technologies in general, and information technologies applied to academic discourse in particular.

Social Cybernetics

> We do acknowledge the concerns of those who view citations as "brightly colored glass beads" which can be "exchanged for promotions and salary increases and intellectual reputations in the academic world." However, we also believe that citations can be used, along with other performance indicators and measures of esteem...to help shape individual faculty productivity profiles. [33]

Having outlined the components of a cybernetic systems, we are now ready to extend the metaphor to a consideration of what we might want to call *social cybernetic systems.* I'd like to define Social Cybernetics as a system of hard and soft (i.e., software) technology, including the sensing devices, transfer mechanisms, and central processing units, that is designed for the purpose of enhancing control over human beings. The ultimate purpose of increasing control lies outside the cybernetic system, is

likely to vary situationally, and needs to be inferred from specific applications of cybernetic technologies.

Considering our discussion of the elements of cybernetic systems above, social cybernetic systems follow the general outline provided by Weiner. That is, cybernetic technology, as discussed by Weiner, is made up of four basic components. There is the thing being controlled, the transfer technology, the sensing technology, and the controlling unit or CPU. However there is an additional component of social cybernetic systems not required of exclusively technological systems. In social cybernetics the devices which operate on the environment are human and given to having ideas of their own. Therefore, in order to increase the probability of getting humans to respond to the command and control dictates of the system, the social cybernetic system must also be linked to a system of reward and punishment.

This, in a nutshell, are the components of the social cybernetic system. Thinking of Information technology and its interaction with human systems in terms of cybernetics has considerable potential in helping us theorise information technology because it allows us to focus on aspects of IT that are normally excluded from the analysis. For example, the current popularity of computer aided manufacture (CIM), Just in Time (JIT) systems of production control, flexible manufacturing systems, quality control circles, and the like, can all be fruitfully analysed with the cybernetic metaphor. Consider the comments of one management theorist. Pay particular attention to the requirements of effective employee control outlined below.

> What has not received enough attention is the equally dramatic but much less visible impact of the new technology on management control systems...The controlling function consists of actions and decisions managers undertake to ensure that actual results are consistent with desired results. Effective control requires three basic conditions: (1) standards that reflect the ideal outcomes, (2) information that indicated deviation between actual and standard results, and (3) corrective actions for any deviations between actual and standard results. The logic is evident that information technology facilitates effective control. In any organization, managers are concerned that resources are productively deployed, job responsibilities properly stated, and various assignments adequately coordinated. To ensure that resources are used appropriately, managers develop

> structures and use processes, such as planning, monitoring, and reporting, to maintain control. With the advances in information technology, comprehensive control systems based on a global or corporate view are applied today. [34]

Although the existence of a cybernetic system is only referred to in the above quote tangentially as a "comprehensive control system," the actual details of such a system can be extracted from the short account. The goal of the management control system outlined above is to enhance the efficiency and performance of employees in the organisation through total control of the operational environment. The goals of the system are facilitated through the gathering of information at a central location for the purposes of planning, co-ordinating, and adjusting (disciplining) the employees of the organisation. Information, which is key to cybernetic systems, is recognized as an essential requirement of the system. Some form of transfer mechanism is implied since the information will have to find its way to top management. Also implied are sensing devices which monitor the actual performance levels of employees. These sensing devices can be anything from cameras that overlook the workplace to detailed sales and product reports provided through software analysis packages. There is also the recognition of the role of a central processing unit which, in the above case, is upper management. Finally, there is the system of reward and punishment which in the case of management control systems will be the threat of demotion or expulsion from the organisation.

The author characterises the new managerial systems as enabling "comprehensive control." This is a rather broad claim. However there is considerable potential in cybernetic systems for this type of total control. Consider the ongoing elimination of middle management levels in major corporations. One of the biggest mistakes that we can make is to assume that just because organisational hierarchies are being eliminated, decision making power is being devolved to the lower employee levels. [35] With the implementation of cybernetic systems, middle management is simply no longer needed in their traditional role as gatherers and digesters of organisational information for upper management. Management theorists explicitly recognize this development and advocate it as a preferred way of doing business because it offers a *purer* form of control. Karake [36] notes of the total control made possible by these new management IT systems:

> The result is a wider span of control, fewer levels in the hierarchy, and lower complexity. Information technology may also lead to less formalization in organizations....

> Since computer technology can warn top management of the effects of any decision... however, it enables them to take corrective action if the decision is not to their liking....we can conclude that even though information technology helps in the decentralization of the decision making process, it does so with no commensurate loss of control by top management. [37]

The cybernetic metaphor can also help us unpack the current discussion of electronic scholarly communication. At the very least, it allows us to identify and locate various components of the current debate about Internet technologies and electronic journals. As we learned earlier, electronic scholarly communication offers faster peer review, faster distribution, and global access. [38] However when we bring into the analysis an understanding of cybernetic systems, we can see that the current emphasis on speed is really only about the transfer technology of cybernetic systems. Harnad's conceptualisation of a post-gutenburgian revolution and the emergence of scholarly skywriting deals with this side of IT.

> Suffice it to say here that the tempo of a spoken conversation is in the same neighborhood as the speed of thought, whereas weeks, months, or years of lag between messages are not. Whatever ideas could have been generated by minds interacting at biological tempos are forever lost at paper-production tempos. Scholarly Skywriting promises life for more of those potential brainchildren, those ideas born out of scholarly intercourse at skyborne speeds, progeny that would be doomed to still-birth at the earthbound speeds of the paper communication. [39]

What's left out of the current discourse is an explicit analysis of the command and control functions of information technology. However advocates do seem peripherally aware. Harnad, for example, recognises that there is a greater potential for controlling the scholar and scholarly discourse. [40] He is an untiring advocate of bringing traditional methods of ensuring quality in scholarship, i.e., peer review, into the electronic world. But when he discusses peer review, he notes that the new electronic systems could offer better control of peer review. [41] At some levels, Harnad seems to see a much bigger picture. According to Harnad, in additional to enhancements in the traditional methods of peer review, electronic scholarly communication will allow for a more total control over the scholarly communication system. As Harnad explains: [42]

> Let me close by returning to the question of quality control. I have argued that peer review can and should be implemented on the Net, and hierarchically, much as it was in paper, generating a pyramid of periodicals, with the highest quality ones at the top and the unrefereed vanity press at the bottom. ....But now let me say a few words in praise of the chaotic regions of such a partially constrained system: Sometimes the brakes applied by referees are "unbiological" too: If all of our ideas and findings had to pass through narrow peer scrutiny before they could elicit wider peer feedback, perhaps certain ones of them would still remain stillborn. Within the many possible structures and nonstructures one can implement on a Net, unrefereed discussion, perhaps among a closed group of specialists with read/write privileges (while others have read-only privileges) would be a useful complement to conventional peer review or even to electronic adaptations of BBS-style editor-filtered peer commentary in the form of editor-filtered "skywriting" of the kind BBS's electronic counterpart, PSYCOLOQUY specializes in.

Harnad appears to be suggesting here that even the *informal scholarly communication system* (his chaotic unrefereed discussion) can be incorporated into a system suitable for monitoring and controlling scholarly discourse. Much of the detail of the system is left up to the reader to imagine, but Harnad's suggestion that access to the system be controlled by differentially handing out "read/write" privileges to the discussion forum suggests that his view of the system is quite autocratic and authoritarian. Harnad's justification of the increased span of control is that it will help bring quality onto the Internet. But that argument is specious. Quality control has only ever been needed in the formal communication system. It makes little sense to start formalising the informal realm of communication and bringing it under greater administrative control. However the point here is that the potential for a greater span of control is there and it is recognized explicitly.

So where does Citation Analysis fit into this? Well, if what I have suggested is true, as scholarly move towards individualised publication, as more and more journals move onto the Internet, and as the academy moves further towards business models which emphasise productivity, CA may very well come to be the core of a social cybernetic system of control. The implications are profound and the theory and potential applications of these new systems to the development of panoptic systems of control of the

labour process have not been overlooked by management theorists [43] or critics of information technology. [44] It seems naive to discount the potential application of panoptic/cybernetic systems to the academy and academic discourse. They are already established in the labour process of other disciplines, they are making inroads as systems useful for controlling pedagogical content, [45] and, given the increasing emphasis on performativity in the academy and the increased need for administrative control, it seems only a matter of time until these new technologies are applied to academic discourse. Of course, the intention may not be to institute Orwellian forms of control over scholarly discourse, but given the unintended effects likely to accrue because of the conservative bias of CA, there seems to be sufficient reason for concern.

Conclusion

> There are a few *brave new* electronic journals... [46]
>
> Let us be imaginative in exploring the remarkable possibilities of this *brave new* medium....[47]

In this chapter I have attempted to demonstrate the there is a potential inherent in the new information technologies for the development of panoptic and cybernetic systems of control over scholarly discourse. This system may be implemented through a modified for of CA suitable for automatic monitoring of scholarly output on the Internet. As I have suggested in this and the last chapter, these systems of supervision and control will not, and are not neutral and objective measures. Because of their conservative bias, they tend to make visible only certain parts of the academic corpus. Mostly, we find CA representing the status quo in science.

Will this potential be realised? The answer to that question remains open. However, given some of the trends visible in the academy, i.e., the ongoing commodification of the academy and the increased emphasis on performativity and administrative control, given the salience of panoptic and cybernetic technologies to management theorists, and given the ease with which CA might be implemented in the new medium, it is at least reasonable to suggest that this is something to be watchful of. What we should not do is discount the threat. We have to recognise that information has a political dimension and that there are those who will use the power information gives for political purposes. Jean-Francois Lyotard [48] recognised this back in 1979 when he wrote that information technologies would change the power dynamics in society.

> For brevity's sake, suffice it to say that functions of regulation, and therefore of reproduction, are being and will be further withdrawn from administrators and entrusted to machines. Increasingly, the central question is becoming who will have access to the information these machines must have in storage to guarantee that the right decisions are made. Access to data is, and will continue to be, the prerogative of experts of all stripes. The ruling class is and will continue to be the class of decision makers. Even now it is no longer composed of the traditional political class, but of a composite layer of corporate leaders, high-level administrators, and the heads of the major professional, labor, political, and religious organizations.

We might add to Lyotard's concerns an additional apprehension about the advisability of collecting certain types of information. If the case can be made that information of a particular type is inherently biased and inherently political, why gather the information at all?

Conclusion

> I think it's safe to say that everyone in this room is aware that a communications revolution is under way that is as profound as the introduction of the printing press. This information revolution promises the creation of a worldwide resource with social and economic implications that have the capacity to alter dramatically the course of history and to change the way we live. [1]
>
> Transforming information into a salable good, available only to those with the ability to pay for it, changes the goal of information access from an egalitarian to a privileged condition. The consequences of this is that the essential underpinning of a democratic order is seriously, if not fatally, damaged. This is the ultimate outcome of commercializing information throughout the social sphere. [2]

Like most who have chosen to examine the scholarly communication system and the primary journal at the end of the millennium, I would like to believe that the revolutionary potential of new information technologies will solve many of the current difficulties of the paper based system like high cost, slow speed, etc. I would also like to believe that these potentials will also lead towards more equitable and egalitarian forms of scholarly communication that overcome some of the structured inequalities in the global scientific enterprise. However, as we approach the millennium, and as the friendly face of welfare capitalism gives way to the jeering mask of neoliberalism, it seems that we fight an increasingly uphill battle for reform or revolution of that system. Indeed, simply standing still seems likely to only guarantee that our control over the system will slowly slip out of our hands.

Of course, in some ways the struggle we fight is not new. As we noted in chapter one, the history of the scholarly journal is the history of bourgeoisie politics, colonial domination and scientific justification for stratification. This "agenda" has been carried forward by the creation of a scientific communication system that isolates and marginalises the voices of the colonised by giving undue weight and epistemic privilege to scientific discourse. This epistemic privilege feeds forward into other elements of the scholarly journal we discussed in chapter one. For example, giving epistemic privilege to scholarly discourse allows the gatekeeping role of scholarly journals to be perceived exclusively as a quality control method and not as a potential system of exclusion supporting class, gender, or racial hierarchies. Mechanisms need not be Machiavellian and may simply rely on the transference of cultural capital. Yet the potential for systemic abuse is certainly there.

The transference of cultural and scholarly capital is supported not only by the formal system, but also by the informal communication system which much more transparently acts as an arena for the socialisation of scholars. Indeed, the characteristics of the informal system support structured inequality in a number of ways through the invisible college structure, as a venue for the distribution of tacit and technical knowledge (which are indispensable for successfully "doing" science), and as the arena were scholars find jobs. The importance of the informal network for assisting in the transference of cultural capital is powerfully represented by the successful career paths of those tied into the networks at an early point in their careers. These features of the informal system intersect and provide essential support for the primary system and cannot be ignored.

As long as we believe in the epistemic primacy of scholarly discourse, criticism of the structures of that discourse have been difficult since it was always possible to fall back on the presumed superiority of scientific methods. The SSK critique of science has performed an important role in weakening the referential links between scientific research and social or natural reality. This has been critical because it allows us to easily insert sociologically (and even psychologically) informed critique of the scholarly communication system. This moves us easily and naturally towards the more radical critiques (found in the feminist critiques of science and the postmodern critiques of the enlightenment project) which note how the system has functioned in intersection with class, race, and gender to support traditional systems of hierarchy, colonial domination, and restriction of access. For example, the importance of tacit knowledge for the doing of science, problems with journal delay, and the nature of informal colleges all come together to create a system that disadvantages whole groups of individuals and even societies.

Uncovering these hidden dynamics of the scholarly communication system has been useful as a way of developing a strategy for movement out of a system with clear disadvantages for most and moving towards a system that more clearly represents the interests of a more diverse group of scholars. Chapter two carries this task forward by identifying more mundane problems with the extant system of journal communication. Here we examine problems like delay, journal proliferation and high cost. Again, we pay attention to the intersection of class, race, and gender. For example, whether you think of delay as important or not may reflect your subordinate or superordinate position. Delay may be functional for those who can circumvent the primary system of communication via access to informal colleges and conferences. But delay may be a serious hindrance for those without the necessary resources (financial or otherwise) to access the hidden system of communication. Of course, the problems of proliferation and cost are less ambiguously problematic. Here we simply note that the way the system is currently organised (with commercial houses retaining an unacceptable level of monopoly control) is putting strains on the system which must be dealt with to avoid crises.

Chapter three and four look at electronic journal and its potential to overcome the current limitations of the system. Here we suggest that given the nature of the technology, problems associated with delay, cost, and differential access may be overcome. The potential is certainly there for technology to truly revolutionise the scholarly communication system by, for example, lowering the cost of entry to the system and allowing entrance to other stakeholders. It is particularly exciting because it could support a true internationalisation of the scholarly communication system and open up access to the means of scholarly production for formerly marginalised groups. This can help overcome, among other things, what Cornel West calls the problematic of invisibility and namelessness where lack of ability to represent oneself means lack of ability to "contest the bombardment of negative, degrading stereotypes…." [3] It might also go a long way towards revitalising the scientific traditions of non-western countries (which have suffered from the effects of colonial domination and parasitism).

Still, this potential will not be realised without a struggle. This is certainly the lesson we draw from examining how the commercial presses are struggling to maintain their vested interests in the system of scholarly communication. These houses, and there representatives, are engaged in a struggle of position that is attempting not only to delegitimate the activity of individual scholars, but also, by arguing for various forms of value added production, attempting to raise the entry barriers to electronic publication. Of course, raising the entry barriers will at once reduce the possibility of any sort of radical reform in the system. There certainly seems to be a choice in the offing here. We can choose to add all manner of complication to the electronic journal production process (advertising, marketing, the use of propriety technologies, etc.) or we can choose a simpler system which fulfils the needs of scholars at the same time that it avoids the excesses which privilege private handling of the scholarly communication system. Recalling the systemic biases in the system, choosing the high cost alternative will move towards a reproduction of system bias.

As noted above, in some ways there is nothing significantly new in our analysis of the problems of the scholarly system. Mundane treatments of the difficulties with the paper communication system, postmodern criticisms of enlightenment conceits, SSK deconstructions of positivist presumptions, etc., are all well rehearsed. What is new and significant is not only the unique potentials inherent in new information technologies to overcome past restrictions and limitations on the system, as suggested above, t also the new political structures within which the system expresses itself. This is perhaps the most significant contribution of this work. As others have attempted to unpack the implications of neoliberalism for the human condition, social reform, ideological resistance, and (de)colonisation of developing nations, so this dissertation has attempted to unpack the implications of neoliberalism for the academy.

The neoliberal assault on the academy shares many features with neoliberalism's incursions more generally. We have, as in other areas, an attempt to turn all public space into an arena for the generation of private profit. In the case of scholarly journals, this expresses itself as a desire to expand the purview of commercial publication into the new electronic spaces, but also as an attempt to design systems that enable a more refined "metering" of the flow of scholarly information. That is, the desiderata of commercial production would be to be able to sell information on a per page (or even better, a per-byte basis). This would ensure that information attains its true value (as commodity) in the new system. This desire to eliminate unprofitable (read free) exchanges expresses itself in many other areas were public space and public service use to follow a less vacuous morality.

With neoliberalism's assault we also have an attempt, or at least a potentiality, for increasing hegemonic control over intellectual discourse. In one sense, this is not new. Theorists have commented on the creation of hegemonic structures and endowment of one-dimensionality for some time now. However, by and large hegemonic control has been seen as applying only to the "masses." Intellectuals, at least those with the appropriate "critical credentials," have been seen as largely immune. Of course, perceiving a certain degree of immunity is not a failure but rather a result of an actual independence allowed by the system. Perhaps this independence was, like the post World War II labour "pax" which enabled production to continue without undue interruption, necessary to enable the intellectual class to engage in long term research (i.e., basic research) which the capitalist class could never, because of the competitive dynamics of the system, bring themselves to support. As Flamm [4] pointed out a decade ago, independent scholarly research is critical simply because business, with its emphases on short term gain, cannot see through to funding the development of many of the most significant advances in human society. Without independent research and thought, technologies like the digital computer, which required 30 years of funding before it became a profitable technology, would never have been developed. The implications of a business led academy and scholarly communication system are as clear as the text on the screen of electronic journals world wide.

However, with the ascendance of neoliberalism and the push to close all public spaces, we should now be worried about the extension of one-dimensionality and control into the academies. The peculiar relationship with capital which academics have enjoyed is currently undergoing significant transformation. We might say that the ambiguity of our relationship with capital is being replaced with a no nonsense system which explicitly locates us as servants of the system - with no pretensions to independence. This no nonsense approach includes, among other things, a great desire to ensure we are "accountable" in our work. Here accountability means, of course, that our intellectual output serves more directly the needs for capitalist growth and accumulation. Why our class relations should be undergoing such change at this historical juncture is way beyond the scope of this work - though we may

offer globalisation and a declining rate of profit (essentially an accumulation crises) as reasons for capitals sudden interest in what we do. This is a significant turn of events since it signals the potential demise of the independence intellectual and his or her replacement with a capitalist lackey whose only raison d'être is in the service of the system of accumulation.

The contours of the neoliberal assault on the academy have been outlined in more detail elsewhere. [5] For the purposes of our examination of the scholarly communication system, the focus was on how new technologies would lubricate the penetration of neoliberalism and enable the restriction of intellectual discourse. As the dissertation attempted to argue, new technologies bring with them the potential for increased surveillance and hegemonic control through cybernetic feedback and anonymous regulation. This was the argument of chapters six and seven where we noted that the control of scholarly discourse might be enabled through techniques of surveillance which not only decrease our ability to engage in critical pedagogy, [6] but also reduce our ability to sustain critical inquiry in the academy - over the long term. Panoptic and cybernetic control over scholarly discourse through the mobilisation of scholarly desire (e.g., the desire for prestige) threatens to erase intellectual opposition.

It might be argued that the potential for such a neoliberal hegemonic assault on the academy is unlikely. However as this dissertation has attempted to show, the social structures are currently being built that would essentially force us towards adoption of various forms of disabling disciplinary mechanisms. Our ultimate direction becomes part of the logic of accumulation and efficiency being foisted on the academy by a government bent on realising its role as facilitator of capital accumulation and a public bent on seeing universities remain "accountable" to their interest (a deep irony to be sure). Performance (or performativity) become the yardstick against which accountability is measured. And the methods we choose to assess our own performance tend, because of inbuilt biases, to reproduce and enhance conservatism in the system. The end result is the erasure of opposition and the endowment of mediocrity.

When this occurs, it will very likely signal a full assault on critical discourse in the academy. Since CA is a measure that is inherently conservative, using CA (whether web based or traditional) will push academic discourse away from the avant-garde, the risqué, and the radical, and towards the mundane, the dull, safe "normal science" that most often gets cited by other authors. True, critical discourse will likely not disappear from the academy overnight. But we can expect the vitality of alternative scientific traditions to be slowly sapped away as graduate students, dutifully attentive to their publication records and desperate to impress their masters, slip away from an engagement with critical discourses. This loss will no doubt be facilitated by the decline of critical pedagogy and its replacement with consumerist style education that encourages the "customers" to set the pedagogical standards. This will no

doubt distort outcomes and favour the priorities of some forms of knowledge and some producers. [end here, or can I include the Marcusian nightmare scenario below??]

The implications are profound and disturbing. Total one-dimensionality amongst the privileged of the world allowing for the total domination and exploitation of the underprivileged. The triumph of technocratic discourse over critical understanding means that intellectuals (whatever discipline they will be in) will be able to myopically apply technique to adjust and control. The attenuated voices of critics will not penetrate this one-dimensionality. Technologists will apply the latest technological fix for our environmental degradation until the earth fails systemically. Behaviourists will apply the required behavioural fixes until chemical straightjackets are the only solution. Sociologists (or rather criminologists) will provide the infrastructure for an expanded police and surveillance state. And while capital may continue its accumulation for some time to come, the possibility that resistance will be eliminated raises the spectre of accelerated global and environmental destruction with the end result only imagined in the bleakest dystopian fantasies.

Still, there may be something in the elimination of critical discourse and resistance. It is widely recognised that capitalism has survived so long because its has been able to adapt and avoid the crises which would finally signal the end of this mode of production. Arguably, part of the ability of the system to adapt has been provided by the intellectual classes who analyse, systemise and provide the capitalist class with the collective knowledge it needs to accommodate and/or control the population. This adaptive ability has been achieved, no doubt grudgingly, by ensuring a certain degree of intellectual independence in the university system. Assuming that the ruling classes have come to the conclusion that the freedom of the intellectual classes are no longer a sustainable luxury, and assuming the almost total elimination of critical discourse, we can expect the bonds, which have restrained the realisation of pure capitalism, to be loosed. Perhaps at that point we will finally reach the "end of history" where the emaciation of the workers of the world reaches such an extent that there really will be nothing left to loose but the chains. [melodrama courtesy of Grad students Inc.]

Policy Coda

So, what conclusions can we draw from this study? We have seen that technology will not lead inevitably towards information utopias of free and rapid scholarly communication. In fact, inactivity is likely to lead in exactly the opposite direction as neoliberalism pushes the envelope of scholarly communication. While this might seem an uninviting place to end up, still we are in a better position than when we began this examination - especially if our intent is reform. Our trip through the scholarly communication system has

perhaps given us tools to develop a critical praxis capable of least resisting the incursions of neoliberalism at the level of the primary journal. In plainer language, the hope is that our new found knowledge has given us the ability to construct an alternative system of scholarly communication free of some of the worst excesses of previous systems and resistant to the incursions of neoliberal restrictions on intellectual discourse. Obviously, the task before us is not an easy one.

Still, it is perhaps a fitting conclusion to this dissertation to attempt an outline of what a critical praxis of a new scholarly communication system would look like. Obviously, the first step is to develop a system capable of resisting the incursions of neoliberal commercialisation into the academy. This can perhaps most easily be done by ensuring that scholars and libraries themselves retain control over the system of scholarly communication. Given the fact that we do most of the work, and given the potential inherent in new technologies to lower entrance barriers and reduce production work, there is no real justification for commercial control of this vital communication system. Academics already do most of the critical work and, arguably, the prestige of titles is tied up not so much with a publisher's name but with the name of the editor and the quality of editorial work. So there seems little reason not to at least think about shifting the system away from the commercial presses.

The benefits to a system of scholarly communication system firmly in the hands of scholars are easy to enumerate. Keeping the system in our hands provides us with the means to resist hegemonic control of scholarly discourse. We can do that either totally by rejecting the performativity=accountability equation (the preferred approach) or by simply developing our own measures which enable a more comprehensive assessment of scholarly contribution. This of course means rejecting narrowly empiricist readings of acceptable measurement and expanding this to include more comprehensive, and perhaps qualitative (but obviously more expensive) methods. Either way, keeping the system and the technology in our hands helps ensure that we will not become the victims of cybernetic control over scholarly discourse.

Another benefit of keeping the system in our hands is ensuring that cost benefits of electronic scholarly communication flow to the academy and stakeholders rather than to private industry. The benefits here include relief for strained library budgets, maintenance of a low entrance barrier to ensure equitable access and internationalisation of the scholarly communication system, and greater economic health to the commercial system of monograph production which is also strained (in a contradictory fashion) by its own penetration of the primary journal market. And here I think it is worth pointing out that solutions equitable to even the commercial presses can, I think, be found. However equitable solutions will depend on a more equitable distribution of market power. The current system, which clearly favours the larger commercial providers, can only lead toward further monopoly and further strain. They will also depend on the commercial presses realising that

the unrestrained pursuit of opportunity ultimately leads towards long-term failure of the system. Of course, we should probably not hold our breath while hoping for this realisation.

Despite the useful system which might emerge as a result of scholarly initiative, still not many academics have taken the plunge into independent scholarly communication. And for those who have moved into the field, their endeavours face serious limitations because of lack of resources, lack of technical expertise, lack of institutional supports, etc. This lack of movement into the field, and the relatively unorganised and diffuse activity of those who have moved into the field, is a failure because it leads, among other things, to duplication of effort, lack of standards which might enhance the prestige of scholarly communication, and general inability to face head on the challenges posed by commercial publishers. Perhaps it is this failure to organise which has encouraged commercial houses to grow bolder in their attempt to extract hard cash from the scholarly communication system.

Unless it is our intention to allow commercialism to take over every aspect of the academy, it is time we addressed the weaknesses which have so far prevented us from initiating a project capable of competing against the big commercial publishing houses. If we can systematically address our failures and oversights, perhaps we can more effectively resist the ongoing commercialisation of higher learning. The questions before us now are, what are the barriers which have slowed the revolution in scholarly publication and what can we do to overcome these limitations?

As we have already seen, the two fundamental barriers to increased local initiatives are a) lack of organisation and b) lack of interest. Of the two, probably the more significant revolves around the lack of interest and therefore lack of initiative. So why this lack of interest? A number of barriers account for the scholar's reluctance to move into the field. In the first place is simply the amount of work involved. There can be no doubt that editing journals takes allot of work. And it takes even more work when you add typesetting (i.e., HTML markup), production duties, technical tasks, indexing, and all the other duties which go into the successful production of journal to the editorial job. Add to this the necessity of developing a high degree of computer literacy and you have a daunting barrier to participation in the electronic revolution - one that will not be so easily overcome as long as there is no attempt to develop an infrastructure for alternative communication.

A second barrier, closely related to the first, is simply that editorial duties are not as highly regarded as publishing scholarly articles or books when it comes to tenure and advancement decisions. As a result, even those individuals who may be willing to take on some of the work of publishing a journal in their field may be reluctant to do so because of the time it takes away from other, more important, activities. In a decision between putting time into an activity that moves you towards tenure, and editing an electronic journal, the current

reward structure virtually guarantees that scholars will pass up opportunities to start publication projects.

Third, access to the system of scholarly communication, especially the STM system, is not as open as it might be. Legitimate restrictions on access and a high entrance barrier, while ensuring quality, do little to encourage creative innovation in all but a few fields (like Postmodern social theory). Yet certainly given the need to reform the system, there should be more effort at encouraging innovative projects. This failure of the scholarly establishment to move quickly with experimentation of new technologies, and their failure to develop systems of passing on the craft based knowledge of publication, surely has contributed in part to the failed communication revolution, especially in the STM journal markets.

Fourth, independent scholarly publication has been seen as largely an anomaly and there are still questions about the quality of electronic publication on the net. These concerns, still expressed periodically, seem largely undeserved. Publications like the Electronic Journal of Sociology get tens of thousands of hits a month (approaching 300,000 hits a year), draw papers from all levels of the academy (including established authors), and have achieved international recognition. And the EJS is not alone in this. So, quality publication and electronic communication are not incompatible. The problem here seems to be one of successfully proselytising the success of established journals to those who remain unaware of the potential. In other words, established journals perhaps need to find ways to market themselves better.

Fifth, and hinted at above, is the lack of infrastructure for independent scholarly communication. By infrastructure we mean not only the physical internet, software and hardware (which already exist and are available to scholars) but also the technological enhancements and services (for lack of a better term) which would facilitate independent scholarly communication. We noted above that one of the obstacles was the amount of work involved in producing an electronic journal. The work involved extends far beyond editorial and production duties to include attention paid to archiving and ensuring permanent access to journal titles, indexing, basic internet management (including link checking and verification), and a host of other duties. While it is unreasonable to ever think that individual scholars could accomplish these tasks, it is possible, given the potential of information technology to enhance and automate many tasks, to centralise many of these services. The development of such a technological infrastructure would go along way towards facilitating independent and even international uptake of the potentials of electronic communication.

Sixth, while individual electronic journal projects have abounded, there have been few attempts to develop the level of organisation required to create a broad, multi-disciplinary coalition of educators, researchers and administrators dedicated to revolutionising the scholarly communication system. Such an

organisation is sorely required. Not only because a coalition of journal editors and institutions will raise the standards and acceptability of craft based journal publication (a problem pointed to above), but also because such a coalition will be able to exert considerable market power in the highly monopolistic world of academic journals.

Take the following example as an illustration of the potential of a broad based coalition. Imagine the effect on the commercial publishing industry if such a coalition strategically targeted journals for cancellation, at the same time that the coalition developed free or lower cost alternative titles. Such a future is within reach despite the protestations of the commercial industry that only they have the necessary expertise and ability (Sosteric, 1996). We should not be fooled.

This is a call for participation in such a coalition. This coalition, already formed under the name International Consortium for Alternative Academic Publication (ICAAP), proposes to remedy the deficiencies of independent scholarly publication by bringing together scholars and institutions from all countries and all disciplines who are interested in bringing economic health back to the scholarly communication system. ICAAP resolves to work towards overcoming blocks which have prevented a shift away from commercial dependence.

ICAAP resolves to:

Provide Editorial Assistance

In order to increase the chance that individual scholars will publish their own journals, ICAAP will devote the bulk of its revenue to hosting journals and providing final production assistance (HTML markup, copy-editing, etc). ICAAP will also develop online resources and tutorials in order to help facilitate the development of alternative outlets for scholarly work.

Enhance the Prestige of Editorial Work

In order to encourage scholars to develop their own journal projects, editorial work will have to be sufficiently rewarded. We believe it is possible to raise the status of editorial work in the academy simply by exposing the difficulties, challenges, rewards and contributions which editors make to the progress of science. ICAAP will tirelessly proselytize this cause to the scholarly world.

Enhance the Prestige of Independent Electronic Publication

There should be no reason that commercial publishers are **seen** as any more prestigious than independent publication efforts. After all, it is we who provide the critical editorial and review work. Commercial houses simply provide production services and editorial standards. To remedy this imbalance, ICAAP will develop cross-disciplinary publication standards. In developing these

standards, ICAAP will focus on developing high-quality publication without the expensive frills that commercial publications most often use to justify higher prices.

Provide a Route For Apprenticing Young Scholars in the Craft of Scholarly Communication.

Recognizing that part of the problem is a lack of appropriate training, ICAAP will, through its official organ *The Craft*, provide a venue for apprenticing young scholars in the art and science of scholarly communication. It is hoped that eventually these scholars will go on to initiate their own independent journal projects outside of the commercial mainstream.

Provide Technical Expertise and Standards to Move Independent Efforts Towards Greater Standardization.

It is imperative, if independent scholarly communication is to advance beyond its current uncoordinated state, that technical standards are developed to ensure efforts are not duplicated or wasted and that the cost to the system is not raised through this duplication of effort. ICAAP proposes to develop, in consultation with stakeholders, technical standards for the production and distribution of scholarly information. One such effort already underway is our web indexing robot. This robot is unique in the realm of web robots not only because it targets only scholarly resources (thus eliminating the problems associated with the many less discerning robots on the web), but also because it is capable of structuring the indexing of journals on familiar search fields. The capacity of this robot to structure document indexing (with very little additional editorial work) allows highly structured document queries and in the future will facilitate automated extraction of information for submission to the major search services.

ICAAP already has affiliations in Australia, Britain, Canada, Denmark, Mexico, and Russia. We are currently seeking to expand our board of directors with interested scholars from all disciplines, and we are seeking affiliations with stakeholder organisations with an international scope. We at ICAAP hope that you and your organisation will join us in our mission to revolutionise the scholarly communication system.

This call for participation in ICAAP is not altogether original. The government of Canada, recognising the many difficulties with the journal system as it stand now has, under the rubric of Industry Canada, initiated a project called the Virtuoso Group. [7] This group, which has representation from major university presses, a research library, and a scholarly journal association, has stated objectives, which are totally in line with the principles outlined in this policy coda. These objectives are as follows:

1. To develop non-profit, sustainable, peer-reviewed electronic

> scholarly publishing models controlled by the academic and research communities;
>
> 2. To foster electronic publishing expertise through joint projects, and the coordinated sharing of knowledge and techniques; and
>
> 3. To create electronic dissemination options for Canadian and international scholarly information.[8]

As laudable as this project sounds, and despite its adequate assessment of the problems facing the journal system, it has failed to generate significant innovation. [9] We can only speculate about the slow progress of the virtuoso group. However I'd like to suggest that the main difficult with this group, and indeed with any government led initiative including "Royal Commissions" and international review panels, is that government led initiatives, at this point in time anyway, are led by neoliberal priorities. This problem with this is that this underlying neoliberal trend contradicts in fundamental ways, the both the stated objectives of the Virtuoso group, and also the objectives of its primary stakeholders. It is possible that the slow progress of the Virtuoso group is, to put it crudely, simply the result of an inability to find ways of subsuming public sector ideals within private sector prerogatives and opportunities for private profit.

This would certainly explain the way Industry Canada has handled the project. Instead of developing a broad range of publication choices, something that is absolutely required given the range of publication interests (i.e., STM, humanities, social sciences, etc.), Industry Canada chose to devote all available funds to Les Presses de l'Universite de Montreal. This angered a number of organisations and journals in Canada because funding only one publisher with limited publication choices was seen as inappropriate. [10] We might speculate that the reason for the tight channelling of resources was to ensure "profitable" outcomes rather than collective solutions. This might seem a bit unfair. However, the thought that government might be representing the interests of private capital and be using a project like Virtuoso to steer the upcoming revolution is at least, given the current neoliberal political environment, a possibility that needs to be considered.

The lesson we draw from this is simple. We will not be able to rely on governmental solutions to our difficulties. As individualistic and voluntaristic (and naïve?) as it sounds, allowing the government to steer projects might lead us directly back to the place we started from. An overly commercialised system benefiting private profiteers rather than the public good.

Ultimately it is important that scholars, libraries, and academic institutions lead the way. Until we have developed our own system of scholarly communication (as we did at the dawn of the scientific age) we will remain totally at the mercy of commercial interests. And while it is naïve to thing that any of us can remain

totally independent of the capitalist system, it is within our right (and duty) to claim degrees of independence.

Endnotes

Introduction

[1] See for example Denis Grogan (1976), or A. J. Meadows (1974: 1979). Mike Sosteric (1996), Rowland Lorimer (1997).

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[8] Reed-Elsevier. Possible Divestment of IPC Magazines. October 27, 1997b. Press release available at http://www.reed-elsevier.com/Reed-Elsevier/newsreleases/nr32.asp

[9] Reed-Elsevier. Proposed Merger of Reed Elsevier and Wolters Kluwer. October 13, 1997a. http://www.reed-elsevier.com/Reed-Elsevier/newsreleases/nr31.asp

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[13] Reed-Elsevier. Reed Elsevier and Microsoft Announce Important Strategic Relationship. May 6, 1997. http://www.reed-elsevier.com/Reed-Elsevier/newsreleases/nr25.asp.

[14] Jerrold L Kachur (1995).

[15] Gary Teeple (1995: 1).

[16] Gary Teeple (1995: 1).

[17] Herbert Schiller (1989).

[18] Pat Armstrong et. al (1997). Tony Clarke (1997). Stephen McBride and John Shields (1997). Janine Brodie (1995). Linda McQuaig (1995).

[19] Mike Sosteric, Gina Ratkovic, and Mike Gismondi (1998).

[20] (Sinclair, Ironside, and Seifert, 1996; Firestone, 1994; Ball, 1993)

[21] Mike Sosteric, Gina Ratkovic, and Mike Gismondi (1998).

[22] Jerrold L. Kachur (1995: 127).

[23] (Goonatilake, 1982; Haraway, 1986; Shields, 1987; Jacob, 1988; Harding, 1993).

[24] Jerrold L. Kachur (1995).

[25] Richard Rorty (1993).

[26] Jerrold L. Kachur (1995: 35).

[27] Jerrold L. Kachur (1995: 35).

[1] David Locke (1992).

[2] While it would certainly be possible to extend the history of scholarly communication back to the Ancient Chinese, South Asian, Greek, Roman and Byzantine civilisations (Goonatilake, 1984; Alioto, 1987; Eamon, 1994), such a detailed treatment is a) beyond the scope of this work and b) not really necessary since arguably the system as we now know it began its development in the 17th century with the birth of the scholarly journal and the subsequent evolution of the formal scholarly communication system. Accounts of the communication system, then, can reasonably leave of the prehistory of scholarship and scholarly

communication without sacrificing necessary substantive depth or breadth of coverage.

[3] Henry Olenburg, (1665). "Introduction" to Volume One, *Philosophical Transactions,* Monday, March 6, 1665.

[4] Margaret C. Jacob (1988).

[5] See David A. Kronick (1976), J.R. Porter (1964), Houghton (1975), McKie (1979), and Barnes (1934).

[6] Bernard Houghton (1975).

[7] Douglas McKie (1979).

[8] Derek de Sola Price (1963).

[9] Figures are from Ulrich's 1995 periodical directory.

[10] Jean Gimpel (1977).

[11] M. T. Clanchy (1979).

[12] David C. Lindberg (1992).

[13] Francis Bacon (1929).

[14] Henry Oldenburg, (1671). "A preface to this Seventh Years of these Transactions." *Philosophical Transactions,* 6 (March 25): 2088.

[15] Henry Oldenburg (1761) "A Preface to this Seventh Years of these Transactions. *Philosophical Transactions,* 6, March 25: 2088.

[16] Henry Oldenburg (1666), "A Preface to the Third Year of these Tracts." *Philosophical Transactions*, 2 (March 11): 412.

[17] Henry Oldenburg (1666), "A Preface to the Third Year of these Tracts." *Philosophical Transactions*, 2 (March 11): 412.

[18] J. R. Porter (1964).

[19] David A. Kronick (1976).

[20] David A. Kronick (1976).

[21] David A. Kronick (1976).

[22] Sherman B. Barnes (1934).

[23] Margaret C. Jacob (1988)

[24] Margaret C. Jacob (1988: 156)

[25] All from the *Transactions*, March 6, 1665. Volume 1.

[26] Henry Oldenburg (1647). "An Advertisement." *Philosophical Transactions,* 8 (February 9): 7002

[27] Margaret C. Jacob (1988).

[28] Susantha Goonatilake (1986); Stephen Jay Gould (1993).

[29] Thomas Campanella quoted in Margaret C. Jacob (1988: 29).

[30] Margaret C. Jacob (1988: 30).

[31] Susan Bordo (1987).

[32] Nancy Leys Stepan and Sander L. Gilman (1993: 174-175).

[33] Margaret C. Jacob (1988).

[34] Jerrold L. Kachur (1995: 253).

[35] Bruce M. Manzer (1977).

[36] Bruce M. Manzer (1977).

[37] William D. Garvey (1979).

[38] Saul Herner (1969). William D. Garvey and Belver C. Griffith (1979). Denis Grogan (1979).

[39] As the scientific and technical literature has exploded, abstracting and indexing services have grown in importance not only for individual users who utilize them for retrospective and current awareness searching of the voluminous and scattered literature, but also, and perhaps more importantly, for libraries who can no longer maintain comprehensive collections. The services provided by the various secondary publications makes it easier for libraries and users to access material not currently held by their local libraries via interlibrary loan and document delivery services.

[40] Abstracting journal function just as their name implies. They supply abstracts of journals, conferences, meetings, and even the output of entire countries as with the *Abstracts of Bulgarian Scientific Literature.*

The abstracts themselves contain summaries of the contents of a document and citation pointers to the location of the full text, the author, and possible institutional affiliation. They can either be *indicative, informative* or *slanted* (Houghton, 1975). The *indicative* abstract, otherwise knows as the *descriptive* abstract is used to indicate the "scope and content" of the original document. It generally contains only descriptive statements about the original article. The *informative abstract* summarizes the main data and arguments only, contextualized the article, and provides a basic level of analysis. It treats the article in more detail and can often function as a replacement for the original. The *slanted abstract* goes a step further than the informative abstract by emphasizing information relevant to a particular speciality or discipline. Journals that provide slanted abstracts are most common in the industrial and technical literature.

41 Review journals offer a quick but substantive glance at the scholarly literature by providing a critical summary and evaluation of the material found in primary journals (Lambert, 1985). There are hundreds of review journals. Review journals can be distinguished from abstracting journals that supply informative or slanted abstracts by their emphasis on substantive evaluation of the literature by acknowledged experts in the field of interest.

42 Computers were first used in abstracting services in 1961 by the *Chemical Abstracts Service* which introduced a system called KWIC (Keyword-in-Context). This initial foray into the computer world was highly successful and spawned a host of similar machine readable indexing services like the extremely popular and still influential MEDLARS (Medical Literature Analysis and Retrieval System) in 1964. By the end of the decade, almost all abstracting services had shifted to computer based format (Houghton, 1975) and were being used for current awareness and retrospective searching. Other electronic value added services were also experimented with. There was, for example, an SDI service (Selective Dissemination of Information) which distributed information to users based on a user profile that consisted of keywords that were matched each week against new publications. The first SDI service was the Chemical-Biological Activities (CBAC) serviced introduce din 1966 by Nottingham University.

With recent technological advances (i.e., developments in computer (the PC) and storage technology (the CDROM)) these services have become quite popular and are now fixtures at most research libraries. Accessible online, these services include indexes to scientific (Cambridge Scientific Abstracts), social science (PsychINFO, ERIC, Current Contents), medical (Medline, Cancerlit), biomedical and pharmaceutical (Excerpta Medica (EMBASE)), and business (ABI

Inform) literature. All services provide author, subject and keyword searches. Some however only provide index and tables of contents (OCLC is one such service) and others go a step beyond by providing full text in CDROM libraries (ABI Inform) or through various forms of electronic delivery of documents (normally fax).

The breadth of coverage of these services can be quite impressive. The Colorado Alliance of Research Libraries (CARL), for example, provides access to 14,000 multidisciplinary journals and various other commercial databases. CARL has a user profile search and document delivery system which has recently migrated to the WWW at http://uncweb.carl.org/ and which contains citations and abstract information from 17,000 journals representing some 7,000,000 articles. Canada has a similar service which provides access to databases like MEDLARS, user profile services, and document delivery, and automated ILL services. An overview of the services can be found at http://www.nrc.ca/cisti/cisti.html.

[43] Denis Grogan (1979).

[44] The entire process of scholarly communication, from the time a scholar gets her first idea to the time the work is disseminated and integrated, is extremely lengthy. Garvey (1979) has estimated that for psychology the average time span is about 13 years. Garvey suggests that this lengthy delay is essential in order for the system as a whole to be able to weed out questionable and irrelevant material. The average time from initiation of a research project to its final publication is shorter than the 13 year distillation period (see Chapter Two).

[45] William D. Garvey (1979: 28)

[46] William D. Garvey (1979: 69)

[47] Robert K. Merton (1973a).

[48] Robert K. Merton (1973a: 323).

[49] Harriet Zuckerman and Robert K. Merton (1971).

[50] William D. Garvey, Nan Lin, Carnot E. Nelson (1970) and William D. Garvey, Nan Lin, Carnot E. Nelson, and Kazuo Tomita (1979).

[51] This comparison of the status of the hard and soft sciences is repeated constantly in the literature in one form or another. For example Derek de Sola Price (1970) developed something he called the "Price Index," which is a measure based on the age of citations in the reference sections of an article. The measure presumably gives an indication of

the *immediacy* of a body of scholarly literature (the extent to which it draws on recent research). According to Price the hierarchy of science is reflected in the *Price Indices* of the various disciplines. Note that devaluation of the soft sciences contained in Prices' (1970: 13-15. Italics added) explanation below.

> Perhaps the most important finding I have to offer is that the hierarchy of Price's Index seems to correspond very well with what we intuit as hard science, soft science, and nonsciences as we *descend the scale*...With a low index one has a humanistic type of metabolism in which the scholar has to digest all that has gone before, let it mature gently in the cellar of his wisdom, and then distill forth new words of wisdom about the *same sorts of questions.* In hard science the positiveness of the knowledge and its short permanence enable one to move through the packed down past while still a student and then to emerge at the research front where interaction with one's peers is as important as the storehouse of conventional wisdom.

[52] Lowell L. Hargens (1988: 149)

[53] A. J. Nederhof, R. A. Zwaan, R.E. De Bruin, and P. J. Decker (1989). Anton J. Nederhof (1989). Maurice B. Line (1979). David J. Hanson (1990; 1975).

[54] Anton J. Nederhof (1989).

[55] Maurice B. Line (1979).

[56] William D. Garvey, Nan Lin, and Carnot E. Nelson (1970).

[57] William D. Garvey, Nan Lin and Kazuo Tomita (1979).

[58] Harriet Zuckerman and Robert K. Merton (1971: 474).

[59] Stephen Cole, Jonathan R. Cole and Gary Simon (1988).

[60] Stephen Cole, Jonathan R. Cole and Gary Simon (1988: 153).

[61] Stephen Cole, Jonathan R. Cole and Gary Simon (1988: 153).

[62] Stephen Cole, Jonathan R. Cole and Gary Simon (1988).

[63] Stephen Cole, Jonathan R. Cole and Gary Simon (1988: 153-4).

[64] William D. Garvey, Nan Lin and Kazuo Tomita (1979).

[65] William D. Garvey, Nan Lin, Carnot E. Nelson, and Kazuo Tomita

(1979).

66 William D. Garvey (1979).

67 William D. Garvey (1979).

68 William D. Garvey (1979: 58).

69 William D. Garvey (1979: 23).

70 William D. Garvey and Belver C. Griffith (1979).

71 William D. Garvey, Nan Lin, and Carnot E. Nelson (1979).

72 Herbert Menzel (1966: 1001).

73 Derek J. de Solla Price and Donald Beaver (1966).

74 F. Reif (1961)

75 Derek J. de Solla Price and Donald Beaver (1966).

76 Ziman (1977: 111-2).

77 Robert K. Merton (1977).

78 Robert K. Merton (1977: 89)

79 Margaret W. Rossiter (1993).

80 Margaret W. Rossiter (1993: 29).

81 Margaret W. Rossiter (1993: 33-7).

82 Bruno Latour (1987).

83 M. J. Mulkay, G. N. Gilbert, and S. Woolgar (1975).

84 H. M. Collins (1975).

85 For an explication of these processes, and of the SSK program in general, see Barry Barnes and Steven Shapin (1977), Bruno Latour and Steve Woolgar (1986), and Karin D. Knorr-Cetina (1981). For a more politically orientated deconstruction of science see the excellent collection edited by Hilary Rose and Steven Rose (1980) and also Bruno Latour (1988).

86 H. M. Collins (1975: 206-6).

[87] Bruno Latour (1987).

[88] Ian Mitroff (1974).

[89] Ian Mitroff (1974: 73).

[90] Bruno Latour (1987: 33).

[91] Bruno Latour and Steve Woolgar (1986: 240).

Chapter Two

[1] J. D. Bernal (1939: 118).

[2] J. C. R. Licklider (1965: 1046).

[3] Charles A. Schwartz (1994).

[4] J. D. Bernal (1939).

[5] Daniel Bell (1973), Alvin Toffler (1980), Marshal McLuhan (1989), Robert Reich (1991).

[6] Michael Connors (1993: 5-6). See S. Nora and A. Minc (1980) for France. Yoneji Masuda (1981) in Japan.

A three decade old belief (still current) is in something called *Development Communications.* Developmental communications is a model whose proponents suggest that information technology is an agent of industrialization. Its an agent not only because of it enables information flow, but also because a country with a well developed communication system can use this system to root out primitive forms of consciousness and primitive ways of being in the world which hamper industrialization and development.

> One of the main impediments to the developing world's increasing its stock of goods and service is the consciousness of its people. Locked within their own experiences they have no way of knowing what is available to them. If they could be made aware of the gains made in other parts of the world, they would have a different perception of what they could become. They would abandon their traditional ways and habits and refuse to be satisfied with producing only for their immediate needs and would produce a surplus that would bring them and their society additional income, which would then enable them to

> generate greater earnings, more production more saving, more investments, more jobs, more demand, and so on (Dordick and Wang, 1993: 22).

[7] Science Council of Canada (1969).

[8] Jocelyn Ghent Mallett (1993). As an aside, Canada is strongly encouraging the partnership" of education, industry, and government in the development of an information infrastructure. Canada's information highway is called CaNARIE (Canadian Network for the Advancement of Research, Industry and Education). Although ostensibly a co-operative network, it would appear that the priorities are primarily commercial as Mallet (1993: 5) notes: "Although CaNARIE undoubtedly will benefit all Canadians by offering improved access to education, its primary purpose is to serve industry by providing an effective means for research and development (R&D) and information sharing. Therefore, only industry could accurately define the requirement for CaNARIE and then fulfil it."

[9] CISTI is at http://www.nrc.ca/cisti/

[10] Committee on Scientific and Technical Information (1969).

[11] Over the years, the U.S. government has had an ongoing concern with the state of their STI systems as evinced by the almost continuous flow of funds into research designed to investigate and enhance the system. See for example the National Academy of Sciences (1969), Ackoff et al, (1976), King, McDonald, Roderer, and Wood (1976), Garvey, (1979), King, McDonald, and Roderer (1981). By

[12] The JACUDI plan was formalized and outlined in

[13] Ralph H. Phelps and John P. Herlin (1969). United Nations Educational, Scientific and Cultural Organization (UNESCO) (1979).

[14] Roberta Lamb (1997).

[15] Herbert I. Schiller (1989: 68).

[16] Herbert I. Schiller (1989: 71).

[17] Margot Montgomery (1997).

[18] Walter Ludwig (1997).

[19] Walter Ludwig (1997: 121).

[20] William D. Garvey, Nan Lin and Kazuo Tomita (1979).

[21] William D. Garvey, Nan Lin and Kazuo Tomita (1979).

[22] Paul F. Jacobs and Chris Holland (1997).

[23] William D. Garvey, Nan Lin and Carnot E. Nelson (1979).

[24] For a more detailed description of the differences between the social and natural sciences see William D. Garvey, Nan Lin, and Carnot E. Nelson (1979). However don't expect much in the way of a satisfactory explanation from the authors. They can do not better than offer up the credulous notion that the scientific communication process in the social sciences is less evolved, more eclectic, more haphazard, more diffuse and less predictable than that of the natural sciences.

[25] In my experience it usually takes about 6 months from the time a manuscript is sent to an oversees periodical before I even get an acknowledgement. If we give the publisher about 1 month for administrative turnaround, the postal delay is 5 months.

[26] William D. Garvey, Nan Lin and Carnot E. Nelson (1979).

[27] A. J. Meadows (1979: 105).

[28] The delay associated with the refereeing process has caused concern and prompted various attempts to reform the process. Meadows points to the efforts of the American Institute for Physics to take three months of the reviewing process. Similarly, a social science journal attempted to elicit comments from its reviewers within two weeks. Following their efforts, however, the range of delays remained between less than a week for 8% of contributions, to more than six weeks for 19%. This prompted the author of an article in *American Sociology* to conclude that procrastinating referees are a major bottleneck in the editorial process (Rodman, 1970: cited in Meadows, 1979).

[29] Jill Lambert (1985).

[30] One Canadian journal receives only about 100 manuscripts a year. Yet the editor (in an informal conversation with the author) noted that they had publication backlogs.

[31] J. Carson and H.V. Wyatt (1983).

[32] Paul Nijhoff Asser (1979). A number of reasons were given for missing journals including problems with postal service, misdirection in departments of large users, inadequate addressing, address change,

theft, and inadequate wrapping. The primary factors identified in their multiple response questionnaire were faults in the postal system (90%) and misdirection (76).

[33] David A. Shepard (1979). Shepard describes the fate of an article on poisoning due to the *Amanita Verna* mushroom. The article in question described a little known (in America) treatment using thioctic acid. The article was sent to four major American medical journals which all rejected the article. The article was subsequently published, in its original form in the state medical journal and went on to receive national and international attention. However by that time news of the treatment had received wide distribution in the non-academic press.

[34] J. M. Ziman (1969: 319-20) argued, for example, that when considered in the context of the entire process from initiation to final publication, the "4 months between the receipt of a typescript and its publication in a reputable journal is not a significant portion of the time required to 'make a discovery'".

[35] Thomas P. Stossel (1985) argued that "There is little evidence that the rate of publication today has a limiting effect on scientific and medical progress or on the publication welfare." Stossel points to an over concern with establishing priority as a major concern for publishing results in journal quickly.

[36] Eugene A. Confrey (1966). In the early sixties the National Institute of Health experimented with a centralized and computerized system of information exchange known as an Information Exchange Group. The IEG was designed to overcome long publication delays. According to Confrey the experiment was highly successful and quickly grew beyond the NIH's ability to handle. Yet the experiment was terminated and decades have gone by before similar attempts to set up informal information exchanges.

[37] William D. Garvey (1979: 73).

[38] William D. Garvey (1979: 89).

[39] Derek J. De Solla Price (1963: 90-1; emphasis added).

[40] Don R. Swanson (1966).

[41] Norma Vale (1986: 8).

[42] Ralph Korteling quoted in Norma Vale (1996: 8).

[43] Susantha Goonatilake (1984).

[44] Susantha Goonatilake (1993).

[45] J. Carson and Wyatt (1983) note, for example, that a paper published in the *Israel Journal of Medical Sciences* reached the U.S. two years after publication.

[46] Susantha Goonatilake (1984).

[47] Susantha Goonatilake (1984: 102)

[48] Gender Working Group (1995: 8).

[49] Susantha Goonatilake (1984).

[50] Steve Harnad (1991).

[51] Steve Harnad (1991: 44).

[52] Odylzko (1994l).

[53] Stevan Harnad (1995a)

[54] Harold Wooster.

[55] Derek de Sola Price (1963) notes that because of the exponential growth of the academy and scholarly literature, at any given time, the majority of all scientists who ever lived are alive an publishing. This gives the "impression" that information is running away from us when in fact it isn't.

Donald W. King, Dennis McDonald, and Nancy Roderer (1981: 61) in their analysis of the U. S. scholarly communication system provide some empirical evidence that at first glance might cause us to side with Price. They noted that the proportion of publications to number of authors did not change in the period of their survey. "For the nine fields of science combined, the average number of articles per scientists or engineer changed very little between 1965 and 1977." This is fine as far as it goes. But in this case, a proportion hides the magnitude of journal proliferation behind a relativized figure. A simply count of the 25,000 + scholarly journals now in existence should be enough to indicate the that proportions are not the best indicator to use in this circumstance.

[56] C.J. Ballhausen, F.A. Cotton, A. Eschenmoser, E. Havinga, R. Hoffman, R. Huisgen, H.G. Khorana, J.M. Lehn, L. Salem and G. Wilkinson (1974).

[57] Phelps and Herling (1960: 62) quoted in Astle (1989).

[58] J. C. R. Licklider (1966).

[59] Donald W. King, Dennis McDonald and Nancy Roderer (1981).

[60] Robert K. Merton (1973a).

[61] See for example Mary Frank Fox (1994), Stephen Lock (1994), T.P. Stossel (1985) and William J. Broad (1982).

[62] F. Reif (1961).

[63] According to Broad (1982), the average length of life science papers is about 7 pages!

[64] W. J. Broad (1981).

[65] Deana L. Astle (1989: 152). William J. Broad (1981: 645-6) gives the following example of abuse of the system.

> A different and much more serious type of coauthor abuse is seen in the large lab where a senior scientist provides little work or inspiration but manages, nonetheless, to walk away with a large measure of the credit for the efforts of his underlings. Today it is not uncommon for the name of a prominent biomedical lab chief to appear on 500 or 600 papers produced in large measure by his juniors. An example comes from the case of immunologist Robert A. Good, who worked at the Sloan-Kettering Institute for Cancer Research and who, in a 5-year period, coauthored almost 700 scientific reports, a feat achieved in part by establishing a large empire of research workers under his personal banner.

[66] Deana L. Astle (1989).

[67] Paul Metz and Paul M. Gherman (1991: 317).

[68] Robert Maxwell, Pergamon Journals Commercial Publishing House. Quoted in William Kay, in the journal Global Business, Spring 1988: 42).

[69] Anon in Dougherty and Barr (1988: 5).

[70] Paul L. K. Gross and E. M. Gross (1927).

[71] Richard de Gennaro (1977). Herbert S. White and Bernard M. Fry (1979).

[72] Richard de Gennaro (1977).

[73] Herbert S. White and Bernard M. Fry (1979: 54),

[74] Scott Bennett (1992).

[75] Paul McCarthy (1994).

[76] Robert Hauptman (1995).

[77] Asser's (1979) data is based on the results of two survey's conducted by the Journals Committee of the International Group of Scientific, Technical and Medical Publishers. The samples are quite small comprising only 43 responses from an initial sample set of 158 questionnaires sent to interested publishing houses. All together, about 1,417 journals in the life, physical,, medical, and engineering sciences from various countries were represented in their sample.

[78] The Association of Research Libraries is an organization of 58 of the largest North American research university libraries (Okerson, 1995). The ARL home page is located at http://arl.cni.org. For a description of the purpose of ARL and a list of member libraries see http://www.lib.washington.edu/~tdowling/arl.html.

[79] Data provided by ARL in the graph, *Monograph and Serial Costs.* It is available at: http://viva.lib.virginia.edu/socsci/arl/1994/arl952.gif

[80] Complete data on Periodicals is available in Alexander and Hammell (1995). Also see Chaffin (1995) for an analysis of serial publications.

[81] For example, Herbert S. White (1976: 361) discounts concern over a journal crisis by arguing that "during the years 1969-1973, the growth of publication of new American scholarly and research journals was not as rapid as many librarians supposed."

[82] Richard M. Dougherty and Nancy E. Barr (1994).

[83] Paul McCarthy (1994).

[84] Canadian Institute for Scientific and Technical Information (1994).

[85] Alan R. Taylor (1978).

[86] Ibid (1978: 48).

[87] Ibid (1978: 48).

[88] A graphical representation of this point is provided by data collected by ARL. For the ARL member library universe, serials acquisitions have declined by 4% (while expenditures on serials increased 93%) in the period 1986 to 1994. Monographs took an even harder hit declining 22% in the same period.

The ARL statistics page is available at [http://viva.lib.Virginia.EDU/arlstats/1994/graphs.html]. The graph of serials expenditures, price increases, and acquisition data is available at [http://viva.lib.Virginia.EDU/arlstats/1994/arl952.gif.].

[89] Paul Metz and Paul M. Gherman (1991: 24).

[90] Eugene Vance (1994).

[91] Richard M. Dougherty and Nancy E. Barr (1988).

[92] Steven Bosch, Doug Jones, and Nancy Simons (1994).

[93] Task Force on the Economics of Scholarly Publication (1979).

[94] Task Force on the Economics of Scholarly Publication (1979: 25).

[95] King, McDonald & Roderer (1981).

[96] James C. Thompson (1988).

[97] Patrick Joyce and Thomas Merz (1985).

[98] Ibid (1985: 274-5).

[99] Joseph J. Esposito. Document available at gopher://arl.cni.org:70/00/scomm/newsltr/esposito

[100] Quoted in Thompson (1988: 481).

[101] James C. Thompson (1988: 482).

[102] Michael E. Koenig (1984).

[103] Herbert S. White (1976). White pegged the annual rate of profit for commercial publishers in 1973 at 14.1%. However besides this figure being based on the self-reporting of commercial publishers, it is also *highly* suspect because it is based on a response rate of only 14% of the commercial publishers! This is obviously not an adequate response rate since it is highly likely that there is serious response bias in the sample (i.e., those with high profit margins chose not to respond to the

survey!). Because of this, it is impossible to draw meaningful conclusions from this data. Also as White notes, the figures are only relevant for the years 1969 to 1973 which are the years just before massive inflation and budgetary cutbacks began to seriously threaten the journal enterprises.

To further complicate this matter, White notes that the figures for the rate in price increases provided by the commercial publishers and the libraries which participated in the study do not coincide. Commercial publishers noted they had price increases of 9.89 percent per year while libraries (drawing on their accounting deparmnents) reported an annual price increase of 11.2% for academic libraries and 12.4% for special libraries. White attributes the differences to the role of subscription agents who inject various service charges into the equation though there could be other reasons for the differential.

104 David W. Lewis (1989: 674).

105 Robert Hauptman (1995).

106 Richard M. Dougherty and Nancy E. Barr (1988).

107 Economic Consulting Services Inc., quoted in Metz and Gherman (1991: 317).

108 Kenneth E. Marx, Steven P. Nielson, H. Craig Peterson, and Peter E. Wagner (1991: 136).

109 Sandra Moline (1989).

110 Ribbe (1988: 460) notes that "In order to meaningfully compare the prices of journals, it is necessary to somehow normalize the database. To consider price per page would be misleading, because formats vary widely. For example, word density in *Mineralogy and Petrology* is ~ 500 per page, but in *Contributions to Mineralogy and Petrology,* it is > 1000." An oversimplified analysis based on price per page was what led White (1976) to his mislead support of commercial publishing houses.

Various analysts have approached this problem in different ways. Ribbe (1988) for example uses the cost per source item (article) and Moline uses cost per character.

111 These price differentials are duplicated in the most recent data from the U.S. Periodical Price Index (Alexander and Carpenter, 1995).

112 Rowland Lorimer (1997).

[113] Bernard Fry and Herbert White (1976).

[114] Paul Ribbe (1988).

[115] Herbert S. While (1975: 372).

[116] Brian L. Hawkins (1994).

[117] Brian L. Hawkins (1994: 24)

Chapter Three

[1] J. M. Ziman (1969: 318).

[2] John Senders (1977) spoke about electronic journals and their **inevitability**. His account is interesting not so much for its prognostication but for the fact that the current situation was predicted even before the PC hit the stage in the early eighties.

[3] Jeanne Guillaume (1980) reports on an early experiment investigating the feasibility and operational characteristics of electronic journals. This experiment, funded by the U.S. NSF, failed to find much support for an electronic journal. Guillaume accounts for the failure by pointing to group dynamics. However the failure of the project probably has as much to do with the primitive and unappealing user interfaces available in the early 1980s (Guillaume, 1980: 27). For example, see Cliff McKnight (1993) for an overview and examination of some of the limitations of some of the early experiments with electronic journals. See also Murray Turoff and Starr R. Hiltz (1982).

[4] Ann L. Okerson (1993) notes the Ejournal became a more serious possibility with the initiation by Willard McCarty in 1987 of the Humanist discussion list. Following this, in the same year, graduate students at Syracuse University started *New Horizons in Adult Education.*

[5] Anne B. Piternick (1989) provides a good overview of earlier experiments with Synopsis Journals, Selective Dissemination (SDI) services, and Miniprint and microfiche experiments. As Piternick notes, by and large these alternatives, some of which make use of information technology, have failed in their bid to replace the traditional primary journal. Her diagnosis is that the early projects failed not only because of technical difficulties and reluctance of authors to submit articles to unappealing distribution formats, but also because they were not aimed at finding true alternatives to primary journal publication. Rather they were attempts to find "additional ways of disseminating articles" (Piternick, 1989: 265).

[6] Cliff McKnight (1993).

[7] The WWW Virtual library maintains one list of sociology journals. It is located at http://www.shu.edu/~brownsam/vl/resource.html#4. A similar list can also be found at http://www.yahoo.com/Social_Science/Sociology/Journals.

To get to the Yahoo index home page, leave of the last portion of the URL and enter [http://www.yahoo.com]

[8] The ARL list of electronic publications is located at gopher://arl.cni.org/11/scomm/edir. Another similar list available in HTML is published at the World Wide Web Virtual library. It is available at http://www.w3.org/hypertext/DataSources/bySubject/. A list of sociology journals is at http://www.shu.edu/~brownsam/vl/resource.html#4

[9] Ann Okerson (1994; 1995).

[10] The NewJourn list is located at http://gort.ucsd.edu/newjour/

[11] Andrew Odlyzko (1994: 14)

[12] Andrew Odlyzko (1994: 2-3)

[13] Andrew Odlyzko (1994)

[14] Susan R. Harris and Elise Gerich (1996). The U.S. NSFNet upgraded its older and slower T1 communications technology to the faster 45Mps 1 Technology in April of 1995.

[15] Merit Network (1992).

[16] Erwin Warkentin (1997).

[17] Erwin Wrkentin (1997: 45).

[18] Cliff McKnight (1993).

[19] Ann Okerson (1994:11).

[20] Martha J. Lindeman, Charles Crabb, John R. Bonneau, and Vera Fosnot Wehrli (1992).

[21] Steven Silvern (1987:5).

[22] Andrew Dillon (1991).

[23] Yu Novikov (1979) notes that the structure of a document can either facilitate or retard the reading process. When faced with the decision of whether or not to read a specific journal article, readers invariably utilize a browsing strategy which includes scanning the table of contents and abstract, examining the heading and sectioning of the journal, and reading the introduction and conclusion.

[24] David Pullinger (1994).

[25] J. Price-Wilkin (1994).

[26] See the original HTML specification by Tim Berners-Lee and Danial Connolly (1993). It is available at http://www.w3.org/pub/WWW/MarkUp.archive/html-spec.txt. For reference, Tim Beners-Lee is the inventor of the WWW.

[27] Philip Greenspun (1996).

[28] See the document *Life on the Bleeding Edge* at http://www.stratcom.com/edge.html.

[29] Netscape is online at http://www.netscape.com/

[30] Information about stylesheets can be found at http://www.w3.org/pub/WWW/Style/. The currently accepted specification is for Cascading Style Sheets. It is available at http://www.w3.org/pub/WWW/TR/WD-css1.html.

[31] The WWW is also becoming an increasingly programmable interface. The Common Gateway Inteface (CGI) and the development by Sun Microsystems web programming technologies like the JAVA language have pushed the envelope beyond what is required for basic electronic publication. It is now possible to develop sophisticated JAVA interfaces and applications that allow, for example, three dimension interactive rendering of molecules.

[32] Bill Readings (1994).

[33] SLIP (Serial Line Internet Protocol) and PPP (Point to Point Protocol) are the two high technology alternatives to the traditional VT100 and Kermit interfaces. These protocol allow personal computers to interface directly to the Internet. Host computers using one or the other of these interfaces have their own domain names and can transfer material directly from their home (or office) computer to any location on the Internet. Older interfaces of course required one to use Kermit or some other form of serial modem protocol to upload/download material to a

local institutions mainframe before transferring material down to the desktop computer.

[34] A Merit Network press release of dated December 1992 had this to say about the connectivity of U.S. institutions.

> Today every major research, graduate, and four-year university is tied together through NSFNET, along with private and federal research institutions and industries. Over 700 colleges and universities are connected representing 80 percent of the nation's student population and 90 percent of the nation's federally sponsored research. Further, NSFNET provides access to hundreds of high schools, libraries, community colleges, and smaller educational institutions. With over 1,000 public and private research and education institutions, NSFNET links an estimated 10 million users. As the commercial Internet has grown, links are expanding between education and business communities which are promoted through expanding connectivity.

[35] The most recent statistics available on the number of wired countries are from May 1995. At that time, 93 countries had purchased the equipment and infrastructure to connect to the Internet. The countries which have most recently come online are Algeria, Armenia, Belarus, Burkina Faso, China, Columbia, Dominican Republic, French Polynesia, Jamaica, Lebanon, Lithuania, Macau, Morocco, Mozambique, New Caledonia, Nicaragua, Niger, Panama, Philippines, Senegal, Swaziland, Uruguay, Uzbekistan, and Vietnam. The most recent estimates (i.e., April 1997) of the total number of people wired to the net puts the figure at over 20 million worldwide.

These statistics are available from the Merit Network FTP server at ftp://nic.merit.edu/nsfnet/statistics/history.hosts and ftp://nic.merit.edu/nsfnet/statistics/nets.by.country

[36] Andrew Odlylzko (1994: 18).

[37] Paul Fontaine (1995).

[38] See the short text by Mike Paciello at http://www.webable.com/mp-blnax.html.

[39] Terry Winograd (1995). See http://www-pcd.stanford.edu/pcd-archives/pcd-seminar/1994-1995/0034.html

[40] Computer in general increase accessibility. Just one example of current developments that are aiding the impaired is provided by T.V. Raman's

page on the EMACS general purpose UNIX tool at http://www.research.digital.com/CRL/personal/raman/emacspeak/emacspeak.html. His EMACS implementation provides a complete voice enabled interface to the UNIX operating system allowing the visually impaired total access and control over the computers functions.

41 While the WWW offers vastly increased potential for those with disabilities, there are difficulties. In particular web designers have to pay careful attention to design conformance (Venderheiden, 1995). Developing nonstandard documents is a concern because there are a two companies in particular which are playing a game to increase their market share by introducing enhancements to standard HTML without first going through the standards body responsible for the WWW. We will have more to say about this difficulty in the next chapter.

42 Paul F. Jacobs and Chris Holland (1997).

43 One author took almost two years to complete suggested revisions. However this delay was more about securing copyright permissions to use a number of images than it was about changes to the text. The author finally resubmitted the paper with revisions, but without images since he was unable to secure the requisite permissions.

44 Steve Harnad (1992).

45 Steven B. Silvern (1987).

46 Estelle Irizarry (1993). Irizarry also notes some additional benefits of the move to an electronic editorial office. Probably the most interesting is the increased international representation of the editorial board since submissions are reviewed electronically and transmitting electronic documents internationally is much easier with email than snailmail. She also noted a decreased document turnaround time citing a lower limit of three months from submission to publication. Supplying the manuscript on disk also reduced the introduction of errors at this stage of the process since rekeying by the typesetter was made unnecessary. Finally, the digitizing of the journal database has made it easier to track journal functioning.

Irizarry also noted some difficulties. Because the journal is a foreign language outlet, they experienced difficulties with the inability of ASCII to handle diacritics. This has necessitated the use of a marking system that uses semicolons to indicate accents and tildes.

47 Jane Lago (1993). Lago also notes that style and copy editing is much easier when utilizing the cut and paste and spell check functions of

wordprocessors. "Surprisingly enough, I have found that I can read a manuscript much more closely on the screen than on paper, and that I miss far fewer details." (p. 108).

48 Andrew W. Appel (1996) provides such a how-to manual on the use of email to referee manuscripts.

49 Ellen Finnie Duranceau (1995). See also Lorrin Garson, Paul Ginsparg and Steve Harnad (1994).

50 For an in-depth discussion of these issues, see the email interchange between Lorrin Garson, Steve Harnad and Paul Ginsparg (Garson, 1994).

51 Malcolm Getz (1992) provides a useful overview of the cost savings in editing, production, and distribution wrought by a shift to electronic systems of journal production.

52 Andrew Odlyzko (1994).

53 A number of commentators have called for empirical research into the remuneration practices of the scholarly press in order to substantiate their claims that editors and editorial boards are not paid for their services. It seems a useful and timely project.

54 Paul Ginsparg (1994).

55 The significant reduction in the cost of storage has one ancillary benefit. It eliminates concern over page length. Traditionally, paper based journals have placed strict limits on the length of articles they would publish. This of course has everything to do with the cost per page of publication and nothing to do with the requirements of scholarly communication. This restriction may have had an inordinate influence on the style of cutting edge scholarly discourse which, because of the need to pack as much information into 10,000 words as possible, is often thick and difficult to wade through, obtuse, and even occasionally poorly written. This has resulted in some cases in a discourse that, though not intentionally so, is fundamentally exclusionary. With the advent of electronic publication this straight jacket is removed since it costs fractions of a penny more to publish a 60 page document than a 30 page document. Of course, whether or not this will have a significant impact on scholarly discourse is an empirical question.

56 Andrew Odlyzko (1994).

57 The GNU (Gnus Not Unix) public license is an extremely innovative way of developing and distributing software. Developers contribute their time

and knowledge to the development of free software that duplicates the functions of commercially available packages. GNU software currently includes a BSD compatible UNIX operating system complete with X-Windows, a sophisticated suite of compilers (including, C, C++, Objective C, Lisp, and numerous others), text formatting tools like the commercially used LaTeX typesetting package, Networking utilities (HTTPDs, FTP deamons, etc.), and every other utility, application, or package currently available as a commercial product. With GNU software, it is possible to install, configure, and operate a complete Internet site without ever having to buy any software.

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>
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> We protect your rights with two steps: (1) copyright the software, and (2) offer you this license which gives you legal permission to copy, distribute and/or modify the software.
>
> Also, for each author's protection and ours, we want to make certain that everyone understands that there is no warranty for this free software. If the software is modified by someone else and passed on, we want its recipients to know that what they have is not the original, so that any problems introduced by others will not reflect on the original authors' reputations.
>
> Finally, any free program is threatened constantly by software patents. We wish to avoid the danger that redistributors of a free program will individually obtain patent licenses, in effect making the program proprietary. To prevent this, we have made it clear that any patent must be licensed for everyone's free use or not licensed at all.

[58] PERL stands for Practical Extraction and Resource Language. Information about the language is available from the WWW page *Perl Resources* at [http://www.ee.pdx.edu/~rseymour/perl/] or from the page *Perl Information and Pointers* at [http://ajs.com/perl/]. The book *Learning Perl* by Randal L. Schwartz (1993) is an excellent and easy to understand introduction to the language.

[59] Information on the CSS standard can be found at http://www.w3.org/Style/

[60] Stevan Harnad (1995).

[61] M.J. Mahony (1976). James G. Simmons (1978).

[62] James S. Gardner (1981).

[63] Denis Grogan (1976).

[64] Alan G. Gross (1990).

[65] David Locke (1992).

[66] Most notably those working in the Sociology of Scientific Knowledge.

[67] If we cling to the myths of peer review then this question is logically impossible. In this circumstance the only reasonable question concerning peer review, when discussing electronic journals, is how can we do traditional peer review electronically.

[68] Alan G. Gross (1990).

[69] Alan G. Gross (1990: 137).

[70] Alan G. Gross (1990: 131).

[71] Alan G. Gross (1990: 138).

[72] Alan G. Gross (1960: 130).

[73] Dilys E. Morris (1990: 63).

[74] Ross Atkinson (1993).

[75] Dana Rooks (1993).

[76] Dana Rooks (1993: 24).

[77] The Australian Society of Indexers is located at http://www.zeta.org.au/~aussi/.

[78] The WWWalker Web Indexing course can be found at http://www.wwwalker.com.au/webcourse.html.

[79] The passivity of the library response is common enough to have prompted Charles A. Schwartz (1994: 101) to comment that

> the literature in our field stresses that libraries, as captive markets, are in a dependent position in the system, with little potential for collective action.

[80] Some authors (i.e., Carrigan, 1995) draw on managerial discourse and refer to a move from a *just-in-case* to a *just-in-time* model or resource delivery. In the former model, libraries own as much material as they can afford just in case someone needs it. In the latter model, libraries arrange to provide access so that the material is available if it is needed.

[81] See for example Bart Harloe and John M. Budd (1994). Also Paul M. Gherman (1991).

[82] Duane E. Webster and Mary E. Jackson (1994).

[83] Beth Brin and Elissa Cochran's (1994) report on the initiatives of the University of Arizona library makes clear that libraries can pursue, and probably should pursue, a number of different approaches to providing document access.

[84] Calls for cooperative collection development are at least forty years old (Downs, 1945). More recent statements are provided by Richard M. Dougherty and Nancy Barr (1988) and Tine E. Chrzastowski and Karen A. Schmidt (1993).

[85] Charles A. Schwartz (1994).

[86] The ARL in the U.S. and CARL (Canadian ARL) in Western Canada have developed cooperative strategies amongst their member organizations. Libraries in the consortium hold shared subscriptions to journals. When the new issue of a periodical arrives, the table of contents of that issue is faxed to other members of the consortium (Piternick, 1989). A similar strategy was implemented in 1984 by the Network of Alabama Libraries (NAAL) (Medina, 1992: 7).

[87] Dennis P. Carrigan (1995).

[88] Dennis P. Carrigan (1995).

[89] Dennis P. Carrigan (1995: 178).

[90] Michael Lesk (1992). Back in 1992 there was some debate about the Economic Models that would be most viable and useful to libraries, publishers, and end users in the electronic marketplace (Czeslaw Jan Grycz, 1992). Now however publishers seem to be pursuing the cite license model in temp guisto so the debate has been rendered academic.

[91] Gail McMillan (1992).

[92] Joseph Branin (1991).

[93] The University of Virginia's Electronic Text Centre: An Interview with David Seaman. [http://etext.lib.virginia.edu/articles/VirgLib/virglib.html]

[94] Frank Quinn and Gail McMillan (1995).

[95] Chronicle of Higher Education (1998: July 10). http://chronicle.com/

[96] The EJS is one such example. Our mission statement reads as follows.

> Because the EJS enjoys low overhead, and is published with the goodwill and hard work of its board members and peer reviewers, the EJS is free of charge to individuals, libraries, academic and commercial organizations. It is part of, and a model for, a new publishing paradigm whereby the

scholars themselves retain control over all aspects of the scholarly communication process.

97 ICAAP can be found at http://www.sociology.org/ICAAP/

98 Charles A. Schwartz (1994: 108).

99 Donald King, Dennis McDonald, and Nancy Roderer (1981).

100 E. J. Huth (1986).

101 R. G. Petersdorf (1983).

102 R. G. Petersdorf (1983).

103 Marcia Angell (1986).

104 Marion Namenwirth (1986: 36-7).

105 Marcia Angell (1986).

Chapter Four

1 Sonia Jarvis (1993).

2 Andrew Odlyzko (1994). Bill Readings (1994).

3 Steve Harnad (1991: 1995). Bernard Naylor and Steve Harnad (1994).

4 Steve Harnad (1994).

5 Astle (1989). J. C. R. Licklider (1966). Donald W. King, Dennis McDonald and Nancy Roderer (1981).

6 Richard de Gennaro (1977). Metz and Gherman (1991).

7 William D. Garvey (1979).

8 Richard M. Dougherty and Nancy E. Barr (1988). Sandra Moline (1989). Kenneth E. Marx, Steven P. Nielson, H. Craig Peterson, and Peter E. Wagner (1991).

9 Richard M. Dougherty and Brenda L. Johnson (1988). Andrew Odlyzko (1994). Ann L. Okerson (1993).

10 Ann L. Okerson (1993: 1.2).

11 Deana L. Astle (1989:155).

[12] James C. Thompson (1988: 482).

[13] R. A. Shoaf (1994).

[14] Quoted in Vivienne Monty (1996: 59).

[15] Nancy Duxbury (1994). Gary Taubes (1996) describes the wave of publication starts as a *tidal wave.*

Two WWW pages give a good overview of what is now available from traditional publishers. One is provided by a service called E-doc and is available at http://www.edoc.com/ejournal/publishers.html. The other is provided by the British library and is available at http://www.comlab.ox.ac.uk/archive/publishers.html. Nancy Duxbury (1994) also provides a list of university presses now on line.

An exhaustive compilation of UUAP presses is available at http://gopher.pupress.princeton.edu. Another list of traditional journal publishers, is provided by *Project Muse* at http://muse.jhu.edu. For examples of electronic texts on the internet see *The Catalog of Electronic Texts on the Internet* [http://www.lib.ncsu.edu/stacks/alex-index.html] or *The Online Books Page* [http://www.cs.cmu.edu/Web/books.html].

[16] David J. Pullinger (1994). The Superjournal home page is at http://www.dlib.org/dlib/january96/briefings/01super.html

[17] Ellen Messmer (1994). Also Gary Taubes (1996).

[18] RedSage Home Page [http://www.cnri.reston.va.us/home/dlib/august95/lucier/08lucier.html]

[19] Gary Taubes (1996).

[20] Pamphlet Distributed by SRO, December 1995

[21] The editorial by Martin Bulmer and L. Stanley (1996) is available at http://kennedy.soc.surrey.ac.uk/socresonline/1/1/editors.html.

[22] See the response of the EJS at http://www.sociology.org/vol002.001/

[23] The server is located at http://xxx.lanl.gov/.

[24] Bernard Hibbitts (1996; emphasis added).

[25] This appeal to traditional publication practices will, given its early use in the struggle between independents and traditional publishing houses,

likely become a key strategy for traditional publishing houses in the coming years and is therefore something to be watched for. A key counter strategy will be to confront the argument head on my systematically dismantling the myths of peer review.

[26] Fytton Rowland (1995).

[27] Fytton Rowland (1995: 85; italics added).

[28] Andrew Odlyzko (1994). Ann Okerson (1994)

[29] Frank Quinn and Gail McMillan (1995).

[30] Frank Quinn and Gail McMillan (1995).

[31] Janet H. Fisher (90).

[32] Janet H. Fisher (1995: 90).

[33] Available at http://www.sociology.net/socinfo/journalminder.html.

[34] A similar service called *ContentsDirect* has recently been announced by Elsevier Publishers and is, according to the publishers, "the fastest and most direct alerting service for Elsevier Science Journals." The service is operated via traditional Bitnet Listserver and provides table of contents pages 2 or 3 weeks prior to the official release of the publication thereby obviating the need for other current awareness services. More information on the service can be found at http://www.elsevier.com/homepage/about/caware/condir/

[35] James S. Gardner (1993).

[36] Hans-Christoph Hobohm (1997).

[37] John Lubans Jr (1987: 181).

[38] Steve Harnad (1994).

[39] Jack Meadows, David Pullinger and Peter Such (1995).

[40] Gary Taubes (1996) identifies the projects of both Steve Harnad and Paul Ginsparg as publications that are also seeking to shift some of the responsibility for publication onto the scholars themselves.

[41] Jack Meadows, David Pullinger, and Peter Such (1995: 231).

[42] Jack Meadows, David Pullinger, and Peter Such (1995: 231).

[43] Robert H. Marks (1995: 86).

[44] Robert H. Marks (1995: 85).

[45] Janet H. Fisher (1995: 89).

[46] Janet H. Fisher (1995: 90).

[47] Dennis P. Carrigan (1995).

[48] Dennis P. Carrigan (1995: 100).

[49] Dennis P. Carrigan (1995).

[50] Malcolm Getz (1992: 29).

[51] Reed-Elsevier (1997b).

[52] Reed-Elsevier (1997a).

[53] Reed-Elsevier (1997).

[54] Timothy Egan (1998).

[55] These guidelines state that libraries that subscribe to a print version of a journal should not have to pay more than an additional 7.5% for electronic access to that same journal, and that libraries should not pay more than 80% of the print rate to subscribe exclusively to the electronic version. "We've been talking about a 'journal crisis' for years," says one of the Dutch librarians. "It looks like it's finally arrived. We're fed up." Quoted in International Federation of Library Associations mailing list IFLA-L@INFOSERV.NLC-BNC.CA From: Terry Kuny Terry.Kuny@xist.com. Article in (Science 28 Nov 97).

[56] Herbert Schiller (1989)

[57] Herbert Schiller (1989: 80).

[58] Gary Taubes (1996).

[59] Gerard M. van Trier (1992)

[60] Dennis P. Carrigan (1994).

[61] Marvin A. Sirbu (1995).

[62] Gary Taubes (1996).

[63] Duane E. Webster and Mary E. Jackson (1994: 262).

[64] John Buschman (1994: 222-3).

[65] Clifford A. Lynch (1994: 27).

[65] Andrew Odlyzko (1994). Bill Readings (1994).

[65] Steve Harnad (1991: 1995). Bernard Naylor and Steve Harnad (1994).

[65] Steve Harnad (1994).

[65] Astle (1989). J. C. R. Licklider (1966). Donald W. King, Dennis McDonald and Nancy Roderer (1981).

[65] Richard de Gennaro (1977). Metz and Gherman (1991).

[65] William D. Garvey (1979).

[65] Richard M. Dougherty and Nancy E. Barr (1988). Sandra Moline (1989). Kenneth E. Marx, Steven P. Nielson, H. Craig Peterson, and Peter E. Wagner (1991).

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[65] Ann L. Okerson (1993: 1.2).

[65] Deana L. Astle (1989:155).

[65] James C. Thompson (1988: 482).

[65] R. A. Shoaf (1994).

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65 David J. Pullinger (1994). The Superjournal home page is at http://www.dlib.org/dlib/january96/briefings/01super.html

65 Ellen Messmer (1994). Also Gary Taubes (1996).

65 RedSage Home Page [http://www.cnri.reston.va.us/home/dlib/august95/lucier/08lucier.html]

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65 Pamphlet Distributed by SRO, December 1995

65 The editorial by Martin Bulmer and L. Stanley (1996) is available at http://kennedy.soc.surrey.ac.uk/socresonline/1/1/editors.html.

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65 Fytton Rowland (1995).

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[65] Gerard M. van Trier (1992)

[65] Dennis P. Carrigan (1994).

[65] Marvin A. Sirbu (1995).

[65] Gary Taubes (1996).

[65] Duane E. Webster and Mary E. Jackson (1994: 262).

[65] John Buschman (1994: 222-3).

[65] Clifford A. Lynch (1994: 27).

Chapter Five

[1] George Orwell, 1984.

[2] Robert K. Merton (1977: 53)

[3] As Garry Stevens (1990: 349) notes, there is a distinction between a citation and a reference. "If an author cites a particular work five times in the course of the text, this counts as five citations but only one reference. Virtually all so-called citation studies ...are actually reference studies...."

[4] Fred R. Shapiro (1992).

[5] Paul L. K. Gross and E. M. Gross's (1927).

[6] Estelle Brodman (1944) provides a useful list of these types of analysis up to 1944. Disciplines represented in her analysis include mathematics, education, electrical engineering, agriculture, geology, child guidance clinics, biochemistry, and psychology.

CA is still used for this purpose even today. Early studies have been followed by examinations of the sociological (Lin and Carnot, 1969; Glenn, 1971; Roche and Smith, 1978) psychological (White and White, 1971; Rushton and Roediger, 1978; Buffardi and Nichols, 1981; Everett and Pecotich, 1993), Social Work (Furr, 1995), Economics (McCain, 1991), Marine Science (McCain, 1992), Women's Studies (Mack, 1991) , and now electronic literature (Butler, 1995) .

[7] Derek J. de Solla Price (1965).

[8] H. G. Small and D. Crane (1979)

[9] H. G. Small and D. Crane (1979: 460). This conclusion of course does not follow. Alternative explanations of the differences noted might focus on the overall complexity of knowledge in the disciplines, or on

Sociology's resistance to fragmentation, or even on different norms for article publication. Unfortunately, none of these alternate explanations are considered.,

[10] Louis V. Xhignesse and Charles E. Osgood (1960). In addition to assessing the prestige of applied and clinical psychology journals, Everett and Pecotich (1993) provide a conceptual map of the disciplines as measured by the cross-citation of journals included in their sample. Similar is Hoffman and Holbrook's (1993) and Zinkhan, Martin and Saxton's (1992) analysis of the Journal of Consumer Research.

Meyer and Spencer (1996) use CA to determine the amount of interdisciplinary influence of Library and Information Science journals. By analyzing citations they are able to indicate the extent to which fields like Sociology, Psychology, Education, Medicine, Chemistry, etc. draw on (i.e., cite) research in Library Science Journals.

[11] P.C. Friman, K.D. Allen, and M.L. Kerwin (1993).

[12] David L. Krantz (1971).

A similar analysis was conducted by Cohn and Farrington (1990). They analysed the differing citation patterns of British and American journals of criminology and concluded that American criminologists "rarely read or cited any non-American research" (Cohn and Farrington, 1990: 467). They attribute the insularity to the greater predilection for quantification in American criminology by noting that "the rate of citation of BJC articles in Criminology and in the SSCI increased with their degree of quantitativeness" (Cohn and Farrington, 1990: 481). They end by noting that "if British criminologists wish to influence their American counterparts, they should carry out high-quality quantitative research using the most sophisticated and up-to-date statistical techniques." (Cohn and Farrington, 1990: 467)!

[13] Bagby, Parker and Bury (1990).

[14] Arthur M. Diamond Jr. and David M. Levy (1994) provide evidence that use of the passive voice in the presidential addresses of the American Economic Association is associated with fewer subsequent citations thereby proving what anyone who has ever taken an English composition class already knows - that using the passive voice makes reading and comprehension difficult.

[15] Richard A. Wright's (1994: 40).

[16] Jerry M. Newman and Elizabeth Cooper (1993)

[17] John Hattie, Murray Print and Krzysztof Krakowski (1994) rank Australian Universities. See also W. Miles Cox and Viola Catt (1977). Rushton and Endler (1977). G.S. Howard, D.A. Cole and S.E. Maxwell (1987)

[18] Eugene Garfield (1993).

[19] Johnny L. Matsion, William Drew Gouvier and Ramasamy Manikam (1989), Howard, Cole and Maxwell (1989) and, more recently, Waldo C. Klein and Martin Bloom (1992) who rank individuals, deans and directors, and entire institutions in their analysis.

[20] Holub, Tappeiner, and Eberharter (1991).

[21] Holub, Tappeiner, and Eberharter (1991: 317).

[22] Christine L. Borman and Ronald E. Rice (1992). I will not be pursuing a critique of this use of citation analysis. However see David Edge (1979).

[23] Estelle Brodman (1944).

[24] Estelle Brodman (1944: 482). William Dosite Postell (1946) modifies Brodman's methodology somewhat. He uses circulation data rather than individual testimony to rank journals but nevertheless arrives at the same conclusions as Brodman.

[25] Per O. Seglen.(1992).

[26] Per O. Seglen.(1992: 635).

[27] R. Plomp (1989: 71).

[28] Ibid (p. 77).

[29] R. Plomp (1989: 78).

[30] Mengxiong Liu (1993).

[31] Ibid (p. 22).

[32] Myron Boor (1982).

[33] Lea Velho (1986).

[34] Ibid.

[35] William Wresch (1996).

[36] Robert A. Hackett (1991).

[37] David Noble (1979).

[38] Catherine Lutz (1990).

[39] Kathryn B. Ward and Linda Grant (1985).

[40] Kathryn B. Ward and Linda Grant (1985: 152).

[41] Stephen Cole and Jonathan R. Cole (1987: 345)

[42] Laura M. Baird and Charles Oppenheim (1994: 8).

[43] Nikolai Bukharin (1925). Margaret C. Jacob (1988). Donna Haraway (1986).

[44] Ruth Bleier (1986).

[45] Galileo quoted in Margaret Jacob (1988: 17-23).

[46] Hilary Rose (1986: 60).

[47] Ruth Bleier (1986).

[48] Donna Haraway (1986).

[49] Michael Adas (1989).

[50] Nikolai Bukharin (1925: xi).

[51] Steven Rose (1980). Steven Rose, Leon Kamin and R. C. Lewontin (1984).

[52] Dean Keith Simonton (1991).

[53] Dean Keith Simonton (1991:461).

[54] Robert K. Merton (1977).

[55] Margaret W. Rossiter (1993).

[56] Pierre Bourdieu (1984).

[57] Hilary Rose (1986: 61).

[58] Marion Namenwirth (1986: 22).

[59] A. J. Meadows (1987: 316).

[60] Sandra Harding (1994: 83)

[61] Derek de Sola Price (1965: 15. emphasis added).

[62] J.M. Ziman quoted in Anon (1970: ????).

[63] Jonathan R. Cole and Stephen Cole (1972: 369).

[64] M. H. MacRoberts and Barbara R. MacRoberts (1987).

[65] David P. Hamilton (1990;1991). David Pendlebury of the institute provided the figures for Hamilton's reports. Of particular interest was the breakdown of uncited papers by academic discipline. Pendlebury found that the Arts and Humanities had an uncited rate of 98%, the social sciences of 74.7%, engineering of 72.3%. The high average figure, and the disciplinary breakdown, prompted Richard Young of MIT to note that if the bottom 80% of the literature "just vanished," the scientific enterprise would not suffer in the least. This rather stunning comment echoes an earlier statement made by Jonathan R. Cole and Stephen Cole (1972: 372).

> Consider only one problem emerging out of the findings that needs a great deal of further research: the size of the research establishment of modern science. If future research on other fields of science corroborates our results, we may inquire what it implies about the relationship between the number of scientists and the rate of advance in science, and whether it is possible that the number of scientists could be reduced without affecting the rate of advance .... Clearly most of the published work in even such an outstanding journal makes little impact on the development of science. Thus the basic question emerges, whether the same rate of advance in physics could be maintained if the number of active research physicists were to be sharply reduced.

David M. Bott and Lowell L. Hargens (1991) reject the institutes findings. They find, for example, that sociologies low ranking on citation measures is the result of limitations in computer matching algorithms and a failure to understand the uses to which sociology puts its literature. The end result is an artificially low citation count. Contrary to the high rates reported by the institute, they find an uncited rate of 9.2%.

[66] Heidi Lee Hoerman and Carole Elizabeth Nowicke (1995).

[67] Cole and Cole (1972).

[68] Hoerman and Nowicke (1995: 424).

[69] Bruno Latour (1987).

[70] Steve Rose (1979). See also Jonathan Harwood (1979).

[71] A. L. Porter, D. E. Chubin, Xiao-Yin Jin (1988).

Chapter Six

[1] Diana Hicks and Jonathan Potter (1991; 485).

[2] Myron Boor (1982).

[3] Oscar Gandy (1993: 18).

[4] Dennis Dion (1996).

[5] Malcolm Ashore (1993).

[6] Sandra G. Harding (1986).

[7] Sandra Harding (1995).

[8] W.E. Snizek (1987: 313).

[9] M.H. MacRoberts and Barbara R. MacRoberts (1987).

[10] Waldo C. Klein and Martin Bloom (1992: 292). Similar comments are made by Roger C. Myers (1970: 1041) as he laments the political indecision fostered by traditional methods of recruitment

> Recently, a selection committee at the University of Toronto was charged with the responsibility of finding a scientifically eminent psychologist for appointment to an important position. The committee secured the advice of a large number of consultants in the discipline and, in time, collected a list of 45 potential candidates. The names of the candidates were then sent individually to each of the professors in the Department of Psychology, who were asked to give independent ratings of the quality of the research of each person on the list as either "good," "fair," or "poor." Ratings were received from 23 professor-judges.

> There was not a single case in which all of the judges agreed in their ratings of the quality of a candidate's research. In 18 cases (40%), two of the three ratings were used. In 27 cases (60%), all three of the ratings were used. The extent to which "experts" disagree when asked to make this kind of judgment is impressive.

Myers goes onto say the he prefers more quantitative indicators free of the intellectual and political biases that emerged as part of the qualitative survey conducted by the U. of T. psychology department.

[11] I. Crewe (1988: 235-6).

[12] Yehuda Elkana, Joshua Lederberg, Robert K. Merton, Arnold Thackray, and Harriet Zuckerman (1978:2).

[13] Michael Mulkay (1989).

[14] Norman Fairclough (1992):

[15] The Canadian government has been highly active in encouraging increased ties between industry and the academy. The Canadian approach has been to fund and develop "centers of excellence" with funding going to those academic institutions who have successfully demonstrated their ability to link academic research with commercially viable products. I have been part of two such attempts two secure government funds and both times there was tremendous pressure to partner with commercial organizations. Of course, commercial organizations are enticed with the promise of potentially viable products.

[16] Norman Fairclough (1992: 207).

[17] A. J. Meadows (1990: 1)

[18] James S. Gardner (1993).

[19] Robert D. Cameron (1997).

[20] Andrew Treloar (1996: 147).

[21] For more on this see Peter Roberts (1998) and Mike Sosteric, Mike Gismondi and Gina Ratkovic (1988)

[22] Blaise Cronin and Kara Overfelt (1994:70). Emphasis added.

[23] Christopher Grey (1994: 489).

[24] Janet Bickel (1991).

[25] Found in Heylighen Cybernetic Technology. [http://pespmc1.vub.ac.be/CYBTECH.html]

[26] Dilys E. Morris (1995: 56).

[27] Derrick de Kerckhove (1995).

[28] Found in Heylighen Cybernetic Technology. [http://pespmc1.vub.ac.be/CYBTECH.html]

[29] Norbert Weiner (1954:16).

[30] Norbert Weiner (1961: 26).

[31] Norbert Weiner (1961: 26-7).

[32] Norbert Weiner (1954: 64).

[33] Blaise Cronin and Kara Overfelt (1994: 70-1). Emphasis added.

[34] Zeinab A. Karake (1992: 10).

[35] Mike Sosteric (1996).

[36] Zeinab A. Karake (1992: p. 18).

[37] As Karake notes, many companies, including such giants and innovators like Hewelett Packard and General Motors, are moving back towards centralized control structures as IT enables them to introduce (in my words) cybernetic systems.

[38] Mike Sosteric (1996).

[39] Stevan Harnad (1991).

[40] Stevan Harnad (1995).

[41] Stevan Harnad (1995).

[42] Stevan Harnad (1995).

[43] Gibson Burrell (1988), Barbara Townley (1993), Paul du Gay and Graeme Salaman (1992).

[44] Linda Fuller and Vicki Smith (1991), Graham Sewell and Barry Wilkinson (1992).

[45] Mike Sosteric, Mike Gismondi, and Gina Ratkovic (1998).

[46] Stevan Harnad (1995). Emphasis added.

[47] Stevan Harnad (1995). Emphasis added.

[48] Jean-Francois Lyotard (1979: 14).

Conclusion

[1] Susan Nutter (1993: 3).

[2] Herbert Schiller (1989: 75).

[3] Cornel West (1994: 71).

[4] Kenneth Flamm (1987).

[5] See the Electronic Journal of Sociology's special issue on the commodification of the academy for an extended treatment of these issues http://www.sociology.org/vol003.003/.

[6] Mike Sosteric, Gina Ratkovic and Mike Gismondi (1998).

[7] The Project web site is at http://www.hil.unb.ca/Texts/burk/in_can/

[8] Taken verbatim from the Virtuoso Statement of Purpose online at http://www.hil.unb.ca/Texts/burk/in_can/

[9] Email conversation with Lesley Strutt, Executive Director of the Canadian Association of Learned Journals.

[10] Email conversation with Lesley Strutt, Executive Director of the Canadian Association of Learned Journals.

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