Scholarly Communication and Epistemic Cultures

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Communication in science

The ways in which scientists communicate have long attracted scholars' attention (e.g., Meadows, 1998). Structural-functionalist accounts of how science works as a social system (Merton, 1976) coexist (and sometimes compete) with thickly descriptive laboratory accounts of science-in-action (e.g., Latour & Woolgar, 1979), discipline-specific mappings of communicative processes (e.g., Garvey & Griffith, 1971) and painstaking socio-historical accounts of scientific practice (e.g., Shapin, 1994). Science, sometimes seen as "the premier knowledge institution throughout the world " (Knorr Cetina, 1999, p. 1), has been studied intensively, and scientists themselves are a much-observed species, both from afar and also up close in their natural habitats. Knowing how scientists work, how they interact with their peers and publics, is not just intrinsically interesting to ethnographers, sociologists of science, and sundry others, but has a bearing on the development of effective academic information resources and information support systems. Why that is the case will shortly become clear.

A great deal of scientific communication, indeed, scholarly communication in general, is informal in nature. Information diffusion depends on the conductivity of socio-cognitive networks, sometimes referred to as "invisible colleges" (Crane, 1972). Nevertheless, the ultimate goal of science, to appropriate the language of Bruno Latour and Steve Woolgar (1979), is to produce inscriptions—to publish. Scientists, of course, are motivated by curiosity, intellectual excitement and discovery, but publication is the means whereby the results of their cogitations and endeavors move into the public sphere. The formal publication practices and preferences of scientists are also well documented (e.g., Meadows, 1974; Friedlander & Bessette, 2003). Indeed, the quantitative analysis of the dynamics of scholarly publishing, most commonly and traditionally known as bibliometrics, has become an academic sub-field in its own right with a robust research literature (see, for instance, the journal *Scientometrics*). But the pioneering work of scientometric grandees such as Derek de Solla Price (1961, 1965) is not just of interest to a select band of researchers in information science and cognate domains. There is an applied dimension: bibliometric tools and techniques can be used to inform collection development and collection management policies in academic libraries. More generally, bibliometrics provides insights into the communicative dynamics of science and, thus, can help librarians better understand the evolution, structural characteristics and interactions of the disciplinary corpuses they manage. With the advent of the Web, classical and novel bibliometric measures—webometrics, netometrics, and sitations are just some of the associated neologisms—are being developed

and used to quantify cyber communication phenomena and behaviors (e.g., Cronin, 2001a; Thelwall, Vaughan, & Björneborn, 2004).

Crisis statistics

But you don't have to be a born-again bibliometrician to generate useful trend data. The ARL (Association of Research Libraries) has produced a set of indicators on monograph and serial costs in North American universities that has achieved near iconic status both within and well beyond the academic library community (see: http://www.arl.org/stats/arlstats/graphs/2002/20002t2.html). These statistics and related extrapolations show a seemingly ineluctable trend dating from the mid-nineteen eighties: journal prices (in science, technology and medicine, especially) are outpacing the CPI (Consumer Price Index) to such a degree that library budgets cannot absorb the strain (see also Cummings, et al. [1992] for an earlier assessment of the phenomenon). Between 1986 and 2002, serial unit costs rose by 227%, while the number of serials purchased by academic libraries grew a mere 7%. During the same period, monograph expenditures increased 62%, while the number of books acquired by libraries decreased 5%. The ARL has also produced a set of supplementary statistics covering electronic resources (see http://www.arl.org/stats/sup/index.html). The most recent data show that expenditures for electronic resources account for 16% of ARL library materials budgets. Expenditures for electronic serials have increased by 75% in the past two years, and almost nine-fold since the mid-1990s. Such concerns have been voiced before. White's (1977) survey of over 400 libraries showed that serials subscription costs rose on average 13% per annum for the years 1969-73.

The ARL statistics provide powerful ammunition for those who maintain that there is a "crisis in scholarly publishing" and that "publishers have been able to get away with economic murder" (Guédon, 2003, p. 130). Judging by the frequency with which the word "crisis" appears in the titles of conference papers, journal articles, and technical reports, and also the regularity with which the ARL data are cited by librarians and proponents of the open access movement (for instance, Stevan Harnad's July 13th, 2003 posting to the SEPTEMBER98-FORUM speaks of "a genuine serials pricing crisis"), this is anything but a minority view. Dissenting voices, it seems, are relatively rare, but they do nevertheless exist.

According to Paul Ayris (see http://www.ucl.ac.uk/scholarly-communication/ADVOC.PPT.), "Sir Brian Follett says the escalating costs in periodicals and subscriptions are marginal to a university." In fact, as I discovered, Sir Brian (Chairperson of the Follett Report and Implementation Groups on Information Technology in the U.K.) said something slightly different. His actual words were: "I'm not optimistic about influencing journal prices. They are a marginal cost to research" (see http://www.sconul.ac.uk/event_conf/interconsortia2002/summary.html). Presumably, if they are marginal to the cost of research, they are even more marginal to a university's general operating fund. [Scientists and humanists would react differently to the use of the word "marginal" in this context, I imagine.] A quick back-of-the-envelope calculation illustrates the essential point. Indiana University's Bloomington campus had an expenditure budget of $900 million for FY02-03. The university library's slice of the pie was $30

million, of which $10 million was allocated to materials. In all, $5 million were spent on serials, roughly 0.5% of total campus outlay. Understandably, university presidents and vice chancellors may not be inclined to invoke the rhetoric of organizational crisis for oscillations in what constitutes a relatively modest base-budget line item.

Volatile ecosystem

Whether the ARL data describe a credible crisis, or whether it is the case that librarians are parochially over-sensitized, can be debated ad nauseam. The fact of the matter is that the ARL numbers do not tell the whole story; rather they are a surface manifestation of deep changes taking place in the scholarly communication ecosystem. Nor has this development occurred overnight. Many of us have had a vague sense that the Old World Order was set to change, without necessarily being clear as to the contours of the emerging New World Order. Fifteen years ago I wrote as follows (Cronin, 1989, p. 24): "Academic researchers give the fruits of their intellectual labour (conveniently packaged in article form) to publishers who sell them back (bound between covers) to the same population of individual scholars/authors. A bizarre system, in which the resultant profits are in no way commensurate with the value added, or the business risk taken, by the middlemen. This historical dependency can be broken, but it will require a sustained and unusually high degree of co-operation between authors, universities and learned/professional bodies. The solution is forward integration, with learned societies and/or universities becoming mainstream publishers…An alternative would be the foundation of a number of university publishing consortia, or joint ventures involving universities and learned societies." In the eighties, the lexicon of open access and institutional repositories hadn't been created.

In the late eighties and early nineties, path-breaking initiatives such as SPARC (Scholarly Publishing and Academic Resources Coalition, see: http://www.arl.org/sparc/home/index.asp?page=0) were still not even a twinkle in the profession's eye, but we were at least taking the first tentative conceptual steps towards the kinds of radical, collectivist solutions (e.g., PubMed Central, Public Library of Science [PLoS]) which today seem set to shake up, if not in fact alter irrevocably, stakeholder relationships in the scholarly publishing marketplace. Most revealing, though, was my use of the reductionist phrase "[t]he solution is forward integration." There is a crisis (escalating periodical prices), for which there is but *one* solution (vertical integration). I seemed to feel then that there was a single model of scholarly publishing and if that model was broken it was a straightforward matter of slotting another into its place. With hindsight, this kind of one-size-fits-all thinking seems wholly inappropriate given the multidimensionality of the wider scholarly communication system. A decade or so on and I am espousing a more nuanced view (Cronin, 1999a, p. A25): "Disciplines differ significantly in terms of their sociocognitive structures, degrees of paradigmatic consensus, funding mechanisms, collaborative intensity, and institutionalized quality assurance mechanisms…Given the diversity of stakeholders and heterogeneity of culturally validated practices, it would be foolish to imagine that a monolithic e-publishing model or

system could emerge. Pluralism, plasticity, and adaptivity will be hallmarks of the new world order." How one's perspective changes over time!

Tribal customs

A number of factors have contributed to the sometimes simplistic conceptions of scholarly communication and publishing that prevail. First, the communication ecosystem is highly diverse, consisting as it does of many species (academic tribes, to use Tony Becher's [1989] term) characterized by often markedly different behaviors and drawing upon a bewildering array of (information) resources—and, in turn, a rich array of institutional arrangements for the management of those resources.

Second, the velocity and variety of experimentation in electronic and online publishing (think of TULIP, Project Muse, PEAK, HighWire Press, ELectronic Society for Social Scientists, PubMed Central and numerous other more or less persistent and influential initiatives) make it extremely hard to see the wood for the trees. There is continuous experimentation and many plausible development trajectories suggest themselves: in short, a patchwork of possibilities—of electronic publishing futures— exists (e.g., Wellcome Trust, 2003). Innovations may be ten a penny, but consolidation and coherence are still notable by their absence.

Third, there is a lack of discursive consistency and semantic stability. What exactly does it mean to publish in the digital age? What is an electronic journal? What do we mean by the term "eprint"? For many these are unproblematic constructs, but Kling and colleagues (e.g., Kling & Callahan, 2002, p. 134) have demonstrated that there is a degree of sloppiness in our use of many of the key terms routinely deployed in discussions of electronic publishing and scholarly communication. By way of illustration, they distinguish between four kinds of electronic journals: pure e-journals (distributed only in electronic form), E-p-journals (primarily distributed in electronic form but with limited distribution in paper form); P-e-journals (primarily distributed in paper form); P+e journals (initiated with parallel paper and electronic editions). This is not an exercise in pedantry but an attempt to unbundle the idea of the electronic journal and to better understand the interactions between publishing technologies on the one hand and the behaviors and value choices of scholars/users on the other. In a recent *ARIST* (*Annual Review of Information Science and Technology*) chapter, Kling (2004) disambiguates foundational terms such as "manuscript," "publication," "preprint," "article," and "working paper," analyzes a variety of architectures used for communicating "unrefereed e-scripts," and considers why such practices are, for now, confined to a minority of academic disciplines. Kling (Kling & Swygart-Hobaugh, 2002, online) has also examined the effect of the Internet on the velocity of scholarly journal publishing, concluding thus: "[O]ur results challenge the belief that the Internet has speed (*sic*) up scholarly communication through journals across all disciplines. While the Internet may have possibilities for increasing the velocity of the publication process, several other factors, such as publication volume, differential acceptance and/or exploitation of Internet capabilities, disciplinary differences in demand for rapid publication, peer-review processes, submission rates, etc., also play a significant role in this process." In these and related studies (e.g., Kling & McKim,

2000), Kling has demonstrated how careful empirical analysis can expose fallacies in thinking related to scholars' electronic publishing and digital communication practices.

Fourth, there is a tendency to talk deterministically with regard to the effects and impacts of ICTs (information and communication technologies) on scholarly publication practices, as if options and outcomes were shaped exclusively by the features, affordances, and functionalities of available toolsets, or, alternatively, to assume that the aggregate institutional apparatus of science and scholarship exhibits a path-dependent pattern of development (see North, 1991 on institutional change and path dependency). Rob Kling and Geoff McKim (2000, p. 6) refer to this general tendency as "information processing theorizing," an approach that "elides and homogenizes field differences." Although other scholars have applied socio-technical systems approaches to developments in ICTs (e.g., Bishop & Star, 1996), Kling has sharpened our intuitive sense of how the technologies of electronic publishing and disciplinary publishing regimes are co-constitutive.

A fifth factor that encourages over-simplified understandings of electronic publication is the progressive privatization of higher education, which has been accompanied by the unseemly expansion of "the bailiwick of the apparatchiks" (Cronin, 2001, p. 133). In the U.K., the new managerialism has its own lexicon: the world of learning is now referred to without irony as "The Sector." The primary allegiance of career administrators is to corporate accounting, not disciplines. In such an environment, it is not hard to see why across-the-board/off-the-shelf/one-size-fits-all solutions are most likely to find favor.

Epistemic cultures

How do scientists create new knowledge, and why should we care? In her ethnographic study of high-energy physics and molecular biology, Karin Knorr Cetina (1999) uses the notion of epistemic cultures to explain and contrast domain differences in knowledge-making processes. She defines epistemic cultures (p. 1, emphasis in original) as "those amalgams of arrangements and mechanisms…which, in a given field, make up *how we know what we know*. Epistemic cultures are cultures that create and warrant knowledge…" This dovetails neatly with Kling's (e.g., 2004, in press) accounts of publishing practices within high-energy physics. He has described how membership of, and authorship responsibilities within, large collaborations (such as DZero and BTeV at Fermilab) are tightly controlled by the collective. These localized norms and practices are part of what is denoted by the phrase "epistemic culture"; what Nancy Van House (2004, in press) terms "the complex texture of knowledge as practiced." Many physicists work in huge, distributed collaborations comprising hundreds of individuals. These largely self-regulating collectivities have local rules and procedures governing participation, internal review, and publication practices. High-energy (and other) physicists routinely post their papers on e-preprint archives/institutional repositories, and co-authorship is the norm—often involving scores or hundreds of individuals.

The epistemic culture of high-energy physicists (as far as media use is concerned) is substantively different from other scientific disciplines, such as chemistry, where online posting/archiving of pre-prints is largely deemed unacceptable and adherence to the Ingelfinger rule —which prohibits the publication of

previously posted material (see http://www.nejm.org/hfa/ingelfinger.asp for a discussion in respect of the *New England Journal of Medicine [NEJM]*)—remains widespread. [As it happens, the *Journal of the American Medical Association (JAMA)* is presently rethinking its policy in this regard.]. Since many of the premier chemical journals refuse to publish articles that have been posted on the Web, Elsevier, the leading commercial publisher in the discipline, recently launched an electronic preprint server, ChemWeb (http://www.chemweb.com/0), in an effort to stimulate the kinds of practices pioneered by the physicists. Judging by Warr's (2003) interim evaluation of the Elsevier service, there is still considerable resistance to the idea of preprint servers within the ranks of chemists (but not quantitative biologists who have just launched the q-bio archive). In part, this may be a function of differences in the biorhythms of the two disciplines. Physicists came up with *Physics Letters* and then e-preprint servers as ways of getting their results to the peer community as quickly as possible: rapid publication matters more in physics than chemistry.

At the risk of stating the obvious, the epistemic culture of the high-energy physics community is far removed from the world of humanities scholarship (e.g., literature and literary criticism) where the scholarly monograph is revered and the phenomena of fine-grained division of "cognitive labor" (Kitcher, 1993, p. 303), "collective cognition" (Giere, 2002), and "hyperauthorship" (Cronin, 2001b) are virtually unknown. In a brace of longitudinal studies, we (Cronin, Shaw, & La Barre, 2003, 2004) have recently shown how the intensity of collaboration (revealed, for instance, in the number of trusted assessors mentioned in acknowledgments) and degree of co-authorship differ significantly across disciplines (chemistry, psychology and philosophy were three exemplary literatures surveyed) and also how these measures have changed over time (see also Cronin, 1995). In extreme cases such as high-energy physics and large-scale clinical trials studies, massive collaborations can result in "the erasure of the individual as an epistemic subject" (Knorr Cetina, 1999, p. 166), which is periphrastic way of saying that it is well nigh impossible to determine exactly who did what (see also Davenport & Cronin, 2001 on credit allocation in group work). These are not typically issues that occupy the minds of humanities scholars—an admittedly coarse-grained if convenient portmanteau term—for whom sole authorship is still overwhelmingly the norm. The problems of attribution and accountability that have been extensively documented in the biomedical sciences (Cronin, 2001) do not occur in the humanities, where text and author are tightly coupled; where the process of inscription implies intimacy with one's materials.

Disciplinary discourses

Disciplines and epistemic cultures differ in a variety of ways, at once obvious and highly subtle. Media use is a case in point; academic discourse another. There are many genres and sub-genres of academic writing reflecting the different ways scholars produce, evaluate, and present evidence. As has been noted, "if one wishes to produce discourse successfully within a particular field, one must observe the forms and formalities of that field" (Thompson, 1991, p. 20). The ways in which arguments are constructed, and the rhetorical tropes favored by different academic tribes, have been sedulously documented (e.g., Bazerman,

1988), and John Swales (1998) has produced a "textography' of a university building, *Other Floors, Other Voices*, capturing the discursive lives of its different occupants. Academic writing is anything but standardized: just pick up any two issues of, say, *JAMA* (*Journal of the American Medical Association*) and the *PMLA* (*Proceedings of the Modern Language Association*) and the structural (the chunking and sequencing of the text) and stylistic differences (e.g., discursiveness, disputatiousness, subjectivity) will be nothing less than striking. Nor is writing something that one does as an afterthought to the serious business of thinking and experimentation: as Hyland (2000, p. 3) argues, "discourse is socially constitutive rather than simply socially shaped; writing is not just another aspect of what goes on in the disciplines, it is seen as *producing* them." And, of course, the forms of writing and communicative conventions favored by different epistemic cultures will have a bearing on how ICTs, specifically electronic publishing technologies, are adopted and co-opted.

The reward system

Discursive and publication practices are shaped by the way the academic reward system is operationalized from one discipline to another. For example, an assistant professor of French will typically be expected to have published at least one sole-authored scholarly monograph with a reputable university or commercial press when coming up for tenure, while his opposite number in high-energy physics might achieve promotion/tenure for contributions to a number of multi-authored, online papers. In the former case, the candidate will likely have written every word of the book himself; in the latter, the nominal "author" may have written very little, if any, of the e-preprints/journal articles that constitute his promotion dossier. This illustrates the fundamental difference between "authorship" and "contributorship," a distinction that reflects not just variations in the stylistics of academic writing, but deep differences in the culturally validated knowledge-creation processes sanctioned by different tribal communities. Our physicist may actually have "written" little of the papers to which he has contributed, but the collaborative, transparent, and inherently trusting nature of the high-energy physics research world ensures that he will have made a material contribution to the collective effort.

There is, of course, no in-principle reason why our stereotypical humanist scholar could not break out of the monograph mold and submit a series of thematically linked journal articles, online postings, and/or presentations as his dossier. As John Unsworth (2003, online), a professor of English, library school dean and founding editor of the electronic-only journal, *Postmodern Culture*, has remarked, the solution to the "crisis in scholarly publishing in the humanities"—a crisis foregrounded by Stephen Greenblatt in his year as MLA president (see http://chronicle.com/jobs/2002/07/2002070202c.htm for his open letter to the MLA membership)—is "to accept several scholarly articles in place of a book." Unsworth practices, or exemplifies, what he preaches: "I…got tenure in a top-ranked English department without a book—tenure was based, instead, on article-length pieces, many of which were published electronically, and on applied research (in electronic publishing)." He is not a lone pioneer; Kate Wittenberg (1998, online) has described a discipline-based Web site created by Columbia University Press, a "venue in which international-affairs

scholars can disseminate their best work at different stages and in different forms…making available creative, cutting-edge scholarship quickly and widely to a large community of users." Many other possibilities exist.

But things are not altogether straightforward. Within parts of academia there is considerable inertia and old practices die hard. There is no insurmountable technological reason why humanists should not produce article-length output for publication in electronic journals or deposit in institutional archives, or experiment with novel technologies. For instance, in assessing historian's use of e-books, Ron Musto has been quoted as follows (see: *Library Journal Academic Newswire* (TM): The Publishing Report for July 31, 2003, online): "In e-publishing the classic image of the monograph falls apart." The article continues: "The idea of the monograph, a single copy of an expensive, static book, is slowly being replaced…by the notion of a "database" of cross-searchable, highly enhanced works that offer clear advantages, such as links to source documents and multiple, remote user access…[R]esearchers and students, who generally use portions of monographs for their work, are finding that e-books better support much of their current behavior and offer much promise for expanding the way scholarship is communicated."

Humanists are no less competent than physicists, mathematicians and other scholars when it comes to posting their papers on preprint servers. Rather, the publication practices of most humanists are shaped by a set of institutionally embedded norms and material practices that are not found in other epistemic cultures. For many humanists, at least those who make up the domain's promotion and tenure committees, the monograph remains the most revered mode of publication, and the integrity of a scholar's work is guaranteed by the reputation of the sponsoring press and the associated peer review procedures. This publication model is inscribed in many of the promotion and tenure guidelines used in North American universities, and is very far removed from the "post-traditional communitarian structures" (Knorr Cetina, 1999, p. 165) embraced enthusiastically by high-energy physicists. The dogged focus on publication form (scholarly monograph, etc.) that defines the status quo is unlikely to persist, as Andy Clark (2003, p. 156) has observed: "bundling information into preset, pretagged physical packages…may thus become less and less crucial, as users learn to soft assemble resources pretty much at will, tailored to their own specific needs. One result of this may be the gradual erosion of the firewalls that currently separate various documents and sources of information."

The university press

But there is another wrinkle. For many scholars in the humanities, securing a contract with a university press may be critical to launching their careers. However, as Willis Regier (2003) has shown, the university press, itself, is something of an endangered species. From 1980-2000, the output of university presses grew faster than the market, and they began to target the trade book sector as a source of revenue. During the same period, faculty in U.S. universities grew only 65%. In 2000, university presses produced 31 million books, of which libraries bought 5 million. In recent years, sales of the average scholarly monograph produced by university presses have fallen, roughly speaking, from 1,500 to 200 units. Which brings us

back to the ARL statistics. Library budgets for monographs are declining and fewer scholarly monographs are being purchased. And this is happening at a time when, according to a Modern Language Association report, *The Future of Scholarly Publishing* (Ryan et al., 2002), competition for faculty positions is intensifying and the emphasis on monographic publishing is increasing. Proposals for reform of the status quo have been put forward recently by Davidson (2003).

Additionally, there are fewer publishing outlets for scholarly (as opposed to trade) books than previously. Junior faculty in some disciplines now find themselves between a rock and a hard place. Given the present situation, there is, arguably, a need for (a) alternative forms of scholarly expression and (b) greater emphasis on the quality rather than quantity of an individual's work. In analyzing the predicament facing many humanities scholars, the MLA report (Ryan et al., 2002, p. 180) wonders if "our local and professional practices [are] encouraging the best work of individuals?" Note that the report did not comment on the capabilities of electronic publishing technologies, but on "local and professional practices," precisely the point made repeatedly by Kling and his colleagues.

Material practices

The diversity of epistemic cultures within the academy translates into a wide range of distinctive practices. Chemists, as already noted, are disinclined to submit their papers to preprint services, while many physicists consider it perfectly normal practice to post their finding on the Web. In fact, the use of e-print archives varies strikingly by discipline, according to a study by Ibironke Lawal (2002). He sent a Web questionnaire to a random sample of 473 scholars and researchers in North American colleges and universities. I should note that the sample does not appear to have been weighted by size of discipline or professional status. Further, we are not told what the response rate was, nor are we provided with the raw data from which the percentages are derived. Of those responding, 18% used e-prints; 82% did not. According to Lawal, the use of e-print archives ranged as follows: physics/astronomy 52%; mathematics/computer science 29%; engineering 7%; cognitive science/psychology 7%; biological science 4%; chemistry 0. Even allowing for concerns about reliability, these figures reinforce the anecdotal evidence that adoption rates for e-print archiving vary appreciably from discipline to discipline.

Different modalities of publication hold primacy in different disciplines. Refereed conference presentations in computer science are as, if not more, valued than peer-reviewed journal articles. Economics has been described as "probably the most rigid discipline in terms of the hierarchy of journals that count" (La Manna, 2002, online), a discipline, moreover, for which self-archiving "as a strategy to free access to peer-reviewed research…is, and can easily be proved to be, a total non-starter." In linguistics, journal article publication is the norm. Historians, to take another example, rely heavily on monographs (Collier, 1999); writing and referencing scholarly tomes are central to historiography. However, there are signs that this may be beginning to change. Classicist Gregory Crane (2003, p. A37) believes that the growth in small-scale digitization projects means that "[h]istorians won't be building their work around the assumption that paper based projects are the be-all and end-all." And historians will typically fly solo: the

kinds of co-authorship practices that dominate scientific publication are unheard of, if not unimaginable, in history. The lone scholar may be an anachronism in most scientific and many social scientific disciplines, but that is not the case in the humanities. Collaboration—for which co-authorship is the most visible and compelling indicator—is established practice in both the life and physical sciences, reflecting the industrial scale, capital-intensiveness and complexity of much contemporary scientific research. But the "standard model of scholarly publishing," one that "assumes *a* work written by *an* author" (Cronin, 2001b, p. 559, italics added), continues to hold sway in the humanities. As already noted, the behaviors of high-energy physicists, a specialty group studied closely by, amongst others, Kling (e.g., Kling, McKim, & King, 2003), Knorr Cetina (1999), and Sharon Traweek (1992), are very different from those of the average professor of history or French. In the humanities, career advancement depends on conformance with an essentially individualistic model of scholarly production. Relatively few primary electronic sources will be referenced because of lack of perceived respect for online sources and also because much potentially relevant materials is simply not available in electronic form (medieval manuscripts housed in a monastery; uncatalogued third world government archives) (see Graham [2002] on historians' use of electronic resources). This, of course, is a simplification. There are many electronic publishing pioneers and enthusiasts to be found in the humanities, Gregory Crane, Jean-Claude Guédon and John Unsworth being three notable examples. But broad-brush differences in scholars' material practices do, nonetheless, exist.

Trusting relations

By way of contrast, physicists do not mandate sole-authored output. Nor does the research monograph have a place in their world. Co-authorship is necessarily the norm. Posting, reading and referencing e-preprints is established practice. Theirs, too, is a much more trusting system, and it works partly because of what Traweek (1992, p. 117) describes as physicists' "agnostic evaluation of other physicists," that is to say, the high levels of self-scrutiny and internal vetting that characterize this particular epistemic culture. The social bases of trust in science have been explored by two of the authors cited in the opening paragraph of this paper, Robert Merton and Steven Shapin. Merton (1976) was instrumental in positing a set of governing norms for scientific conduct (communism, organized skepticism, etc.), and Shapin (1994) has detailed how in the seventeenth century the trustworthiness of scientific claims was explicitly linked to the trustworthiness (i.e., social standing/gentlemanliness) of those making the judgments. The conventions for evaluating research may have changed in the last few centuries, but trust, a manifestation of the "normative ghost in the scientific machine" (Cronin, 2004), and peer review, the instrument for ensuring trustworthiness, remain central to the conduct of science in general (Davenport & Cronin, 2001) and, specifically, to the smooth functioning of the primary communication process: "If it [an article] is cited many times as the basis for other research, then it gains trustworthiness. The laboratories and scientists who have been cited successfully in turn become validating voices for other articles when they cite them" (Schlossberg, 1999, p. 69). The ways in which trust is constituted and sustained vary across epistemic cultures, and these variations will influence developments in digital media and publication technologies.

In some domains, notably biomedical research, the issues of fraud and honorific authorship have been widely discussed in recent years. Richard Horton (1998, p. 688) in a *Lancet* editorial, has spoken of "the shattered system of academic reward and its symptom, broken rules of authorship." Such has been the level of concern about excessive (and unwarranted) co-authorship practices that there has been a move to retire the term "author" and replace it with "contributor." By listing all the contributors to a study/paper and specifying their particular inputs (e.g., experimental design, statistical analysis), so it is argued, confusion about who did what will be removed and credit can be allocated equitably (Rennie, Yank, & Emanuel, 1997). The author may be dead in biomedicine, but it is unlikely that the radical contributorship model will find easy favor in other epistemic cultures, particularly those where the act and craft of writing are so intimately associated with the scholarly end product, where "thinking *via* the act of writing" (Clark, 2003, p. 5) is a fact of life.

As might be expected from the argument thusfar, the way in which trust is instantiated in institutional arrangements for peer review varies across epistemic cultures. The physicists who read and cite papers posted on Paul Ginsparg's emblematic e-print archive, arXive.org, are not rejecting conventional peer review (e-preprints, in any event, migrate in time to refereed journals), but their confidence in the robustness of the collective arrangements for validating new knowledge claims in their domain is such that they can reliably draw upon the work of their peers *before* it comes to rest in the relevant journal of record. The mechanisms that facilitate social trust building in their professional culture entail relatively little risk. [Parenthetically, they instantiate Merton's classical norms.] Things are otherwise in the world of chemistry, where researchers' material practices and also modes (and scale) of collaboration are quite different from those in high-energy physics. In addition, the voice and views of the leading professional body, the American Chemical Society (also the leading publisher), have a bearing on scholars' writing and publication behaviors (see, for instance, the ACS style guide: http://www.oup-usa.org/sc/0841234620/0841234620_1.html). As Eberhard Hilf (2003), a physicist, noted in relation to the growth in the number of papers being posted on arXive.org. there will never be "saturation such that all papers will go this way, since in different fields culture and habits and requirements are different."

When it comes to the humanities, the inter-cultural differences are even more pronounced. Here trust is less social than institutional in character. The community of scholars is more dispersed and less self-knowing. Humanities scholars typically work alone rather than as members of a large collaboration. It is the reputation of academic publishers and journals, and the presumed rigor of the associated peer review procedures, that provide the foundations of the system's trustworthiness rather than the intensity of personal/socio-cognitive ties between individuals or distributed workgroups/research teams.

The limitations of the peer review system [at least in the field of cultural studies] were famously exposed by the so-called Sokal hoax (Sokal & Bricmont, 1999). The publication of Sokal's parodic article ("Transgressing the boundaries: Toward a transformative hermeneutics of quantum gravity") in the journal, *Social Text*, shows just how important mastery of discursive moves and tactics is in certain academic contexts. That a member of another tribe (a physicist) could so deftly ape the convoluted writing style

favored by some postmodernists, and also persuade the journal's referees of the merits of his intended nonsense, is most revealing. In their critical review of *soi-disant* postmodern scholarship, Alan Sokal and Jean Bricmont (1999, p. 6) make an important observation: "There are many different degrees of abuse. At one end, one finds extrapolations of scientific concepts, beyond their domain of validity, that are erroneous but for subtle reasons. At the other end, one finds numerous texts that are full of scientific words but entirely devoid of meaning." Paradoxical though it sounds, literal meaninglessness seems to matter in some epistemic cultures or sub-cultures—even classics (see Hanson, Heath, & Thornton, 2001)

Peer, career and fear review

Resistance to "scholarly skywriting" (Harnad, 1991) stems from concerns about bypassing of the long established, albeit flawed, peer review process—the lynchpin of the quality assurance system. New modes of posting and self-publishing are seen as potentially corrosive to the bases of trust and integrity traditionally associated with scholarly publishing. It is, of course, mistaken to equate open access initiatives (e.g., Public Library of Science, Open Society Institute) with subversion of established peer review practices. However, the new forms of Web-based and electronic publishing make it possible to bring one's work directly and swiftly to a much wider audience by circumventing the normal editorial filters and controls. Liberationists argue that open (or deep) peer review has much to commend it, as shoddy work and dubious findings will be subjected to the full glare of public scrutiny an, thus, will revealed for what they are. Stevan Harnad (1979) has long been vocal in the vanguard of the drive for open peer commentary and the promotion of "creative disagreement" in scientific communication and debate. Proponents of open access argue that the Web invites public inspection and fosters transparency. Andrew Odlyzko (2002, p. 9) believes that pointers to Web-based resources (implicit recommendations, in effect) constitute "a form of peer review." Establishmentarians, on the other hand, feel that the untrammeled flood of papers and postings on the Web will make it well nigh impossible to sort the scientific wheat from the proliferating chaff. Battle has been joined.

But it is not a strict dichotomy. It may be more helpful to think of peer review as a spectrum, moving from heavy (anonymized, double-blind) peer review through peer review 'lite' to open peer review (I label the latter "fear review" because one's work is exposed to 360 degree scrutiny, which may well be more nerve-racking than having it read discretely by only two or three peers). In addition, Kling (2004) has coined the term "career review" to describe the kind of internal screening that takes place in the context of guild publishing enterprises, such as an academic department's occasional, working or technical paper series. Materials added to such a series (whether print-based or electronic) are not peer reviewed as such; admission instead depends on one's institutional affiliation and acceptance by the local authorial/scholarly community. In exceptional cases peer review is anything but peer-based. In law, it is a case of the lunatics running the asylum. Law reviews are, in fact, edited by law school students not by the field's leading scholars. [The rankings of law reviews/law journals are largely a reflection of law school rankings.] Here

the apprentices critique the masters' submissions. This idiosyncratic model seems to work despite its inverting of established practice (Thorin, 2003).

Plurality and plasticity

The world of research and scholarship comprises many disciplines and a mélange of epistemic cultures. This heterogeneity of behaviors and practices means that ICTs are deployed differentially, hence the title of Kling and McKim's (1992) paper, "More than a matter of time." Of course, it is not just a matter of time before all disciplines reach the "tipping point" (Gladwell, 2002); in other words, before they adopt the arXiv.org (or some other more or less standardized) approach to electronic publishing/archiving. The picture is much more complicated, and the reasons why, and ways in which, researchers commit to particular e-publishing regimes will be a function of a field's socio-cognitive structure, history, normative character and institutional circuitry—not just its metabolic rate.

Even if we accept that there are significant differences in the ways in which scholars create and disseminate new knowledge, and even if we allow that scholars' material practices are shaped by the prevailing epistemic culture, is it not the case, you might ask, that the resistors—the e-publishing laggards—will eventually come on board as Web-based publishing technologies become ubiquitous and even easier to use? Will conservative humanists be able to resist the lure of electronic publishing and digital archiving, given the nature of the job market, the demands of the academic reward system and the grim economic realities of monograph publishing? Will the principled resistance to piecemeal publishing crumble in a generation or so, as the ancien régime fades and a new breed of hybrid scholar moves through the ranks, populating promotion and tenure committees and progressively rewriting/reinterpreting the guidelines? If high-energy physicists are the early adopters of Web-based self-archiving, why not simply see the majority of historians, philosophers, and French professors as the late adopter population? Perhaps it is, in fact, just a matter of time. But this is to confuse infrastructural with cultural change. Much (not all) scholarly publication will migrate to the Web—the default platform of choice—but the ways in which ICTs are used to communicate with one's peers and disseminate one's ideas will still mirror underlying differences in epistemic cultures and value systems.

Disruptive technologies

This is a good point to step back from the minutiae of tribal behaviors and look at the bigger picture—at the structural dynamics of the scholarly communication ecosystem. Clayton Christiansen (2000) has described with well-documented examples from a range of industrial and service sectors how disruptive technologies can lead to the undermining and eventual demise of a product or service that dominates the market. He chronicles how many companies have engaged in "sustaining innovation," adding, in other words, more and more layers of sophistication or feature-richness to a product for an important and established customer segment: he terms this "performance oversupply." One consequence is that those who lack the financial means or skills to benefit from a particular product or service are effectively locked out of the market. In

Type I disruptions, a competitor launches a product which is comparatively simple and/or cheaper to use and thus of potentially broad appeal. The innovator in this case is providing the market with a new product; examples he cites include PCs, the telephone, and personal financial management software. Each of these technological innovations created a new market: for those who could not access mainframes; for those who could not use/get to the telegraph office; and those who couldn't afford tax consultants. Type II disruptions, on the other hand, occur when a supplier or manufacturer targets the soft underbelly, or low end, of the existing market with an appealing alternative to the prevailing model. Examples of this are discount retailing, steel mini-mills, and no-frills airlines. Over time the once-dominant players begin to see their market share diminish and their customers migrate to the products of interlopers.

Andrew Odlyzko (2002, p. 10) has applied Christiansen's displacement model to scholarly communication, arguing that electronic publishing exhibits three important characteristics of disruptive technologies (underperforming existing products; enabling new applications; rapid performance improvement). He contrasts macro-level trends, and concludes that the writing is all but on the wall for the Old World Order. The first is annual attrition in print journal subscriptions rates (in the 3-5% range); the second is a continuous growth in online access (he provides multi-year visitor/host data on a variety of Web sites to support his contention along with download statistics on arXiv articles). The Old World Order is waning and the New World Order is waxing. We may be some way off the tipping point, but, as he goes on to say (p. 8), "the attraction of a much greater audience on the web, and the danger that anything not on the web will be neglected, are likely to become major spurs to scholars' migration of their work online." Supporting evidence for his thesis comes from the two sets of ARL statistics mentioned earlier.

The appeal of the Web, for both producers and consumers of texts, is undeniable, if sometimes exaggerated: access to wider readership; potentially higher citation rates; shorter time-to-market; bypassing of filters and barriers; dynamic updating of content; local authorial control; self-branding opportunities; informal/open peer review; ease of use; linking to multimedia resources; control over presentation. As more and more publication and dissemination activity moves to the Web, the resistors will be forced to reconsider some of their engrained practices. For example, it is hard to imagine that the world of monograph publishing in the humanities will not change in the next decade, that early experimentation will not stimulate further exploration of new modalities of scholarly communication by the various stakeholders—individual scholars, universities, consortia, university presses, institutional and commercial publishers. The attractions of online publishing and posting, combined with the inefficiencies of the traditional scholarly publishing model, make trans-disciplinary innovation irresistible, as the MLA and other organizations are increasingly aware. In a recent editorial in the *PMLA*, Carlos Alosnso (2003, p. 222) suggests that "the association could become the electronic repository of manuscripts recommended by divisional executive committees, thereby contributing to the dissemination of research judged significant by some of the best scholars in the field."

Advances in electronic publishing will certainly not erase the cultural differences between disciplines. Instead, deep-rooted normative and behavioral differences will stimulate the creation and

adoption of yet new ways of doing business. Odlyzko is right to focus on the high-level trends. The large-scale shift from print to Web-based publishing and the gradual rise of the open access movement are two trends that will change fundamentally the balance of power in the scholarly communication marketplace, but it is still not clear what form self-archiving will take. As Harnad (2003) put it recently: "The reason institutional self-archiving is more likely to speed up self-archiving and to generalize it across disciplines is that researchers and their institutions both share the benefits of the impact of their research output, whereas researchers and their disciplines do not. It is not the discipline that exercises the incentive of the "publish-or-perish" carrot-and-stick on researchers, it is their research institutions." It is unwise, however, to assume that to a single model will emerge and dominate the landscape. Disciplines will continue to view and co-opt ICTs in different ways, and the upshot will be a kaleidoscope of initiatives and local adaptations. Indeed, the long-term effect of widespread disciplinary differences coupled with a rich array of technological possibilities will be the formation of a much more heterogeneous and dynamic publishing ecosystem than before, one that supports a multiplicity of epistemic cultures; "an opportunity environment," in which various species "co-evolve" (Moore, 1996, pp. 11, 16) resulting in new hybrids with new tolerances.

Looking ahead

There is a voluminous and many-sided literature—speculative, advocacy, and research-based—on developments relating to scholarly communication and academic publishing. Statistical data and insightful analysis on the economics of scholarly and commercial publishing are abundant (e.g., Noll & Steinmuller, 1992; McCabe, 2002), and the professional literature is replete with descriptions and evaluations of experimental online services and products. In addition, there is a growing body of evidence to suggest that being online increases the visibility of one's work (e.g., Lawrence, 2001). But back to Merton once again, and this time the norm of communism, the idea that scientists are impelled to share and make freely available the fruits of their intellectual labor. What better way to achieve optimal exposure than by posting one's work on the Web, and in so doing making access to new knowledge as simple as possible for the wider, international community? The converse of this is that greater exposure ultimately leads to greater recognition (all things being equal).

As online and Web-based publishing become increasingly common, altruism and self-interest coincide: "Whether we like it or not, scholars are engaged in a 'war for the eyeballs'" (Odlyzko, 2002, p. 18)—a kind of "vanity fair" (Franck, 1999), if you will. One result is that new measures of "scholarly salience" and "presence density" are likely to emerge in the very near future (Cronin, 1999b, p. 953), complementing traditional bibliometric impact measures. Branding, competition, and vanity will help propel the open access publication movement, championed by organizations such as PLoS and buttressed by the introduction in June 2003 of the *Public Access to Science Act*, a bill that would "exclude from copyright protection works resulting from scientific research substantially funded by the Federal Government" (see: http://www.theorator.com/bills108/hr2613.html). In concrete terms, we can expect to see more initiatives such as MIT's DSpace—a digital repository to capture, distribute and preserve the

institution's intellectual output and make it accessible via the Web to the general public (see: http://www.dspace.org/)—and University of California Press eScholarship Editions, which makes hundreds of its titles available to the public at no cost (see: http://escholarship.cdlib.org/ucpress/).

The picture that emerges is of a very complicated ecosystem, the contours of which are still blurred. Perhaps it would be more helpful to think in terms of an ethological approach focusing on the behavioral characteristics of the primary stakeholder groups. What is missing is a compelling analysis of the structural dynamics of the scholarly communication marketplace, one that focuses upon the array of stakeholder relations, technological drivers, and competitive forces (and their interactions) that are reconfiguring the ecosystem. One looks in vain for the kind of high-level, strategic analysis a Michael Porter might provide (e.g., Porter, 1998; Porter, 2001), particularly one informed by the results of socio-technical systems studies and meticulous ethnomethodological investigation.

For most of the twentieth century there was one medium of scholarly publishing: print. At the same time, there were a few well-established genres of academic writing, the monograph and journal article being the dominant textual forms across disciplines. In such an environment it made sense to talk about "*the* primary communication system" or "*the* scholarly publication system." Monothetic thinking was not out of place. Today there is a plurality of media and genres; scholars can publish, distribute, post, and archive their research in a variety of ways. New publishing modalities are emerging, new forms of collaboration are establishing themselves, and new approaches to peer review being trialed. Disciplines have different biorhythms; the pace of new knowledge creation is faster in some disciplines than others, the need for interaction and feedback more pressing. Different approaches and solutions will be adopted at the local level. The present environment allows communication channels and information resources to be matched more effectively with the cultural characteristics and needs of epistemic communities. In short, the big picture has to be considered along with a collection of miniatures, if the dynamics of scholarly communication and publication are to be properly grasped.

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