



ASSOCIATION OF RESEARCH LIBRARIES

**IN OLDENBURG'S  
LONG SHADOW  
LIBRARIANS, RESEARCH  
SCIENTISTS, PUBLISHERS,  
AND THE CONTROL  
OF SCIENTIFIC PUBLISHING**



**Jean-Claude Guédon**  
Université de Montréal

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# **In Oldenburg's Long Shadow**

**Librarians, Research Scientists, Publishers, and the  
Control of Scientific Publishing**

**by Jean-Claude Guédon**

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## **About the Author**

Jean-Claude Guédon holds his doctorate in the History of Science from the University of Wisconsin, Madison, and is currently Professor of Comparative Literature at the Université de Montréal. Dr. Guédon is founder of *Surfaces*, the first Canadian electronic scholarly journal still in existence, begun in 1991. His interest in both theoretical and practical issues regarding electronic publishing has led him to produce numerous papers on the subject as well as to present at conferences worldwide. Dr. Guédon is not new to the research library community. In 1994, he presented a paper at ARL's Fourth Symposium on Scholarly Publishing on the Electronic Networks and he has been actively engaged in projects pushing the boundaries of scholarly communication, including serving on the steering committees of the Canadian National Site Licensing Project and the Digital Library of Electronic Theses and Dissertations.

## **About the Paper**

Dr. Guédon made a presentation on these ideas in May 2001 at the 138<sup>th</sup> Membership Meeting of the Association of Research Libraries (ARL), a meeting held in conjunction with the Canadian Association of Research Libraries in Toronto. The presentation was received very positively and, as a result, Dr. Guédon agreed to write a paper to encourage further discussion. The result is the paper in hand. *In Oldenburg's Long Shadow* is published by ARL with permission of the author in order to stimulate further discussion and new thinking on the important issues that he raises. It is also available on the ARL website <<http://www.arl.org/arl/proceedings/138/guedon.html>>.

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# 1 Introduction

The so-called “serial pricing crisis” has been with us for a long time. Documented by librarians, denied by commercial publishers, its reality has finally been established as common knowledge and the behavior of commercial publishers and a few learned societies has been singled out as its major cause. Various spurious causes have also been disqualified, for example, the notorious currency fluctuations: reconciling a fluctuating phenomenon with monotonous growth is, to say the least, difficult.... Cost of living does not work either: journal prices have far outstripped this variable; they have even outstripped other sectors of publishing, thus demonstrating that the phenomenon, far from affecting the whole industry, touches only one very specific sector of it. The responsibilities are now clearly identified: they rest squarely on the shoulders of commercial publishers. How this situation emerged and works is also understood in broad terms, but many important details remain to be filled in.

The last decade or so has allowed pointing fingers at commercial publishers, particularly a cohort of large international enterprises located mainly in Europe and secondarily in the United States. Many have publicly deplored the commercial publishers’ attitude, and a few have even vented some healthy anger, but little else has been achieved until recently. In the last few years, however, and perhaps because of the added challenges presented by digitization, attempts to slow down, stop and even reverse subscription price increases have emerged. Alternative forms of publishing are being explored and a petition—that of the Public Library of Science—is fueling several discussion lists. Gradually, the issue is coming to the surface and is beginning to reach even the general media. However, despite these positive developments, no really viable and efficient counterstrategies have been designed. My belief is that mapping effective counterattacks will require a fuller understanding of the situation and its roots. This presentation’s ambition is to contribute a little to this understanding.

In the last 50 years, publishers have managed to transform scholarly journals—traditionally, a secondary, unpromising publishing venture at best—into big business. How they have managed to create extremely high profit rates is a story that has not yet been clearly told. What is the real basis behind this astounding capability? What is the source of their power? How can it be subverted? This presentation will address these questions, but more research is clearly needed, and it is of such scope as to require a concerted, sustained effort; but, in my opinion, it would be

energy well spent.

Recently, because of the advent of digitization and the Internet, the technical system of scientific communication has undergone a profound change that is still unfolding. The imposition of site licenses and the corresponding development of library consortia signal changes so deep that the very status of the "document" and the ways in which individuals may interact with it appear quite incommensurable with the past. The role of libraries is also deeply subverted, as we shall see. In Thomas Kuhn's vocabulary, we are witnessing a paradigmatic shift. The consequences stemming from these developments are difficult to ascertain, but we can be sure that scientific communication is morphing. Into what? To whose benefit? What transition phases can we expect? This presentation cannot hope to give final answers to such complex questions; more modestly, it will endeavor to sketch out two scenarios that are presently unfolding on courses that, although relatively separate for the moment, will eventually collide. Each one of these scenarios corresponds to a different take on the paradigmatic shift. Which one will win is unclear; it may even be that these two scenarios will compete for quite some time. In any case, we need to acquire an image of the territory we are entering, however grained, and of the forces that are shaping its contours, if mapping out strategies is of the essence. And I believe it is of the essence for librarians at this time in history.

I will start by moving the analysis back to the point when the system of scientific communication began to emerge, thanks to the novel way in which a few creative individuals managed to harness printing. In this manner, we will be able to retrieve some of the original meanings and intentions of the system itself, as well as the intentions that presided over its inception. Both meanings and intentions have remained remarkably constant over time; the only difference between then and now is that some people have found a way to graft a new and efficient money-making device on the communication system of science. The only difference between the present and the future is that some feedback mechanism appears to be setting in between the communication system and the money-making device, leading to a gradual shift in the very scope and meaning of the ways in which fundamental research results are broadcast and made accessible. In effect, this presentation asks whether the results of fundamental research in science, technology, and medicine—results that clearly stand at a pre-competitive stage if viewed in commercial terms, results that may even, in some cases, save lives—will remain part of humanity's knowledge commons, or whether they will be gradually confiscated for the benefit of smaller and smaller scientific and business elites.



# **PART I**

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## **How and Why Were Scientific Journals Created?**



## 2 A Social Registry of Scientific Innovations

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Henry Oldenburg created the *Philosophical Transactions of the Royal Society of London* in early 1665, and while his reputation has largely faded from our collective memory, his brain child has not. The London periodical, sometimes affectionately referred to as *Phil Trans*, has become a venerable institution. The motives for its founding are not so clear as one example will show: it is regularly compared with the *Journal des sçavans*, as if the two publications were twin brothers—I made that mistake myself a few years ago. While it is obvious that the two publications are periodicals, deal with natural philosophy, and began appearing within a couple of months of each other, it is not so clear that they really pursued similar aims. The French publication actually reflected the somewhat gossipy, news-oriented patterns of manuscript epistolary exchanges that were so typical of the Republic of Letters; as such, it stands closer to something like *Scientific American* than to a modern scholarly journal, and thus appears firmly rooted in the emergent art of scientific journalism. Although the *Journal* did occasionally publish original papers, they appeared as a particular expression of news among other types of news. By contrast, *Phil Trans*, although it also dealt with new information, really aimed at creating a **public record** of original contributions to knowledge. In other words, the Parisian publication followed novelty while the London journal was helping to validate originality. Therein lies the significant (and profound) difference between the two periodicals.

*Phil Trans* was also conceived at a time when the question of intellectual property occupied center stage and many of its characteristics can be seen as addressing this broad area of concern. In particular, it introduced clarity and transparency in the process of establishing innovative claims in natural philosophy, and, as a result, it began to play a role not unlike that of a patent office for scientific ideas. The purpose was to tame and police “scientific paternity” and priority controversies and intellectual polemics so as to make this potentially unpleasant spectacle disappear from the public eye. If scientific disputes could be handled in a quiet, orderly, and civil manner, Oldenburg and others calculated, natural philosophers would stand to gain a better, more dignified, public image. At the same time, the presence of a public registry of scientific innovations would help create internal rules of behavior leading to a well structured, hierarchical society.

The need for a public registry of intellectual property had long been felt; its absence had sometimes led natural philosophers to resort to strange tactics to ensure their intellectual paternity rights. For example, Galileo had sent an anagram of the phrase describing his discovery of Jupiter's satellites—the so-called Medicean planets<sup>1</sup>—to Kepler (and many others) in order to establish his priority. The idea was to place a potential rival in the uncomfortable position of reluctant witness. Galileo's move was somewhat awkward, relatively idiosyncratic, certainly brilliant; it also shows how difficult it was to assert, let alone prove, something like ownership of ideas or "intellectual property" in the early part of the 17<sup>th</sup> century. However, Galileo's move also incorporated a strong potential for divisiveness that could weaken the whole house of science. A public registry of discoveries could help steer away from such dangerous shoals and sport a more respectable façade to a world that was then anything but spontaneously sympathetic to the cause of scientific knowledge.

Thus gradually emerged a co-opting system that bestowed various degrees of worth upon natural philosophers. It amounted in effect to a kind of "intellectual nobility", but a nobility granted by peers, and not from above. It worked as if Parliament could confer "nobility" upon commoners—an innovation that may have carried a good deal of political overtones in a period dominated by a fierce power struggle between King and Parliament.

The social forms adopted by "intellectual nobility" were just as eponymous as that of feudal titles, but they worked backwards, so to speak. Whereas a commoner could add the name of a domain to his own, thanks to royal prerogative, a scientific author could assign his/her name to some natural "law" or "property" through a collective, peer-reviewed fiat. The disseminating powers of print then ensured a degree of universal applicability to the local decision: enough copies could be distributed to enough relevant and significant institutions to make the naming decision stick. In short, the Republic of Science claimed the right to grant intellectual property to scientific "authors" and *Phil Trans* was its instrument of choice.

The new scientific republic was never egalitarian, nor did it ever strive to approximate this ideal. Like Solomon's House in Francis Bacon's famous *New Atlantis*, science was conceived as a hierarchic activity. Unlike the Baconian utopia, the hierarchy was not based on a division of knowledge or of labor that could be justified by some inductive epistemology; it was simply an intellectual hierarchy based on excellence. It was justified by the unfortunate, yet unavoidable, uneven distribution of intelligence among humans. Concretely, the more intelligent you were,

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<sup>1</sup> And not Galilean planets: Galileo could not put his symbolic capital into a market; all he could do was to look for a patron and the way to get one was to play on the eponymous dimension of scientific discoveries. The invention of *Phil Trans* also allows scientists gradually to move beyond the patronage system and to begin behaving more like modern "authors".

the greater your chances to acquire intellectual property as measured by the ownership of valuable scientific results. But scientific results became valuable only if they were made public; thus was the game of science set around publication rules that allowed claiming and proving property titles over some of nature's ... properties. All this, of course, was taking place in the same historical period that saw John Locke write long, fundamental pages on the very concept of property—a period that has aptly been described by C. B. McPherson, the famous University of Toronto Professor, as that of possessive individualism....

As secretary of the Royal Society of London, Oldenburg understood all this when he promoted the creation of *Phil Trans*. In fact, he entertained even greater ambitions for his new journal—an important point Adrian Johns develops in his fundamental and monumental study, *The Nature of the Book*.<sup>2</sup> Oldenburg also understood that if only he could attract the majority of Europe's significant scientific authors to register their discoveries in the *Phil Trans*, his innovative use of print technology would become a defining moment of the European scientific movement. As a result, London could do for science what Paris was striving to do for taste: it would become the universal (this is the way Europeans often refer to themselves) arbiter of natural knowledge—an enviable position indeed among the great cities of Europe. Had he completely succeeded, the notion of “core journals” would have remained in the singular and the librarians' job would have been much simpler indeed.

Oldenburg's ambitions have cast a long shadow that reaches to the present. In fact, it appears ever more clearly, even as the digital age begins to deploy itself fully. In Oldenburg's time, print had not yet settled on stable economic bases. This is also presently the case for digitized materials. The roles of writers, printers, and bookstore owners, as well as their boundaries, were still contentious topics. Likewise, the present relationship between Internet Service Providers, networks, so-called “content providers”, and users are also in question, and in a state of constant flux. In the 17<sup>th</sup> century, solutions so successful as to appear now absolutely natural to us were still meandering toward existence in a slow, often painful, sleepwalking manner. Presently, many of the legal and political skirmishes that we observe are probably little more than stages toward the slow, painful, meandering, and sleepwalking invention of new legal categories and institutional settings related to digital publishing.

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<sup>2</sup> Chicago: Chicago University Press, 1998. Many ideas in the previous pages have either silently rested on Adrian Johns' magnificent book, or have been inspired by some of his remarks.

### 3 The Question of Intellectual Property

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The question of “intellectual property”, so central to our present discussion, appears relatively natural and clear to us; yet, its history reveals a daring and paradoxical extension of the concept of landed property. This bit of legal creativity was actually motivated by the stationers who needed to establish legally viable claims over the texts they printed, if only to protect their trade from imitation and piracy. To them, this meant exclusive and perpetual ownership, as is the case for land property. But they were not the only players and, as a result of various court actions, the definition of what they actually claimed to own remained murky for several decades, almost a century, actually.

When writers—remember in passing that they were not yet “authors” in the modern sense of the word—transferred a manuscript to a printer, the latter acquired nothing more than sheets of paper soiled with ink stains purporting to carry some meaning; nothing legal could really prevent the writer from sending a second copy to another printer. Understandably, the first printer wanted a firmer leg than personal trust to stand on; one solution was to “own” the text, and not just the paper covered with ink spots. The printer wanted the ability to claim full ownership rights over the text; and he wanted to gain access to the full repressive force of law to prevent the repeated sale of the same text to different people. In short, he wanted to own the text as one owns material goods.

The printers’ concern was legitimate, but a series of thorny questions had to be resolved before it could be satisfactorily addressed. What is a text without its material substratum? Is it words? Plots? Ideas? Mode of expression? Besides, how can something immaterial be owned, let alone transferred or transacted? Interestingly, the solution lay in inventing what amounted to an oxymoron: the concept of “intellectual property”. Then, through the notion of work, it could be related to someone in particular—namely, an “author”. Once printers could face an author-as-owner, they could proceed with a transaction that, although highly “philosophical” in nature, would nevertheless grant them perpetual and exclusive rights over something that could be transmuted into print and sold as any other object. Intellectual property, although philosophically problematic, was indeed marvelously suited to shore up “philosophical transactions”. Indeed, the Royal Society could not have chosen a better title for its new journal!

The story becomes even more surprising when we start examining how modern copyright was saddled with an unexpected characteristic—namely, a time limit on its effectiveness. This brings it closer to a royal privilege or a modern patent than to a traditional form of property. In the anti-royalist rhetoric that was commonly used in the young United States and, a little later, in revolutionary France, such time limitation was justified in lofty democratic terms. In the U.S., it was portrayed as a way to facilitate innovations and, therefore, the public good; in France, it was viewed as reward and temporary protection granted by the Republic to its citizens. The rhetoric notwithstanding, however, the time limit as it was initially imposed on intellectual property actually reflected autocratic will: it was imposed by English lords, not so much to support the Public Good as to reassert the principle of Royal Prerogative at a time when it needed support (I have already alluded to the tug of war between absolute and constitutional monarchy in 17<sup>th</sup> and 18<sup>th</sup> century Britain). Because it was new and somewhat paradoxical, the concept of intellectual property appeared vulnerable and, therefore, seemed to offer a promising angle with which to attack the more general question of property as sacrosanct principle—a clear limitation on the King’s claim to absolute power. Besides, the stationers were clearly accumulating too much power; it was, therefore, time to signal the need for some moderation: it took the form of a time limit on intellectual property!<sup>3</sup>

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<sup>3</sup> Mark Rose, *Authors and Owners: The Invention of Copyright* (Cambridge, Mass.: Belknap Press of Harvard University Press, 1995). *passim*. I want to thank Kathryn Hayles for having brought this important book to my attention.

## 4 **Introductory Conclusions** **(Another Oxymoron)**

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We are now contemplating a number of somewhat startling results:

The democratic, egalitarian ethos of scientists that the great sociologist, Robert K. Merton, has done so much to reveal to us, now appears as the visible face of a hierarchical system echoing the structure of feudal nobility, but doing so in terms—e.g., peer review—that reveal a parliamentary flavor.

The design of a scientific periodical, far from primarily aiming at disseminating knowledge, really seeks to reinforce property rights over ideas; intellectual property and authors were not legal concepts designed to protect writers—they were invented for the printers' or stationers' benefits.

If the latter saw finally their property rights limited, this decision did not stem from some desire to protect the Public Good, but rather from the will to reassert absolute royal authority and its essential right to arbitrariness in principle, if not, of course, in practice (the so-called "royal prerogative" that had been so eroded around the Cromwell episode).

In short, a good deal of irony presides over the emergence of scholarly publishing: all the democratic justifications that generally accompany our contemporary discussions of copyright seem to have been the result of reasons best forgotten, almost unmentionable. The history of scientific publishing either displays Hegel's cunning of history at its best, or it reveals how good institutions are at covering their own tracks with lofty pronouncements!

This quick "surfing" over some historical quirks of scientific publishing allows us to introduce two important points that should be kept in mind in the rest of this presentation:

1. Many of the fundamental categories that presently structure the legal underpinnings of our various countries and that inform the work of international institutions, such as WIPO, were not part and parcel of the eighth day of creation. On the contrary, they are very much the product, the construct in fact, of a particular history. They reflect an equilibrium between conflicting interests



that was attained with considerable difficulty and that never stood completely still; indeed, it has kept on drifting, but sufficiently slowly and gradually to maintain the appearance of continuity and thus remain manageable through various amendments added to existing intellectual property laws. We are still in this game, as the DMCA shows in the United States. This is slow history, to be sure, especially when compared with the pace of human lives and the even more frantic pace of technology. But being conscious of slow and deep historical movements is what thinking historically is all about, and we need the scale of this historical thinking to make sense of the serial pricing crisis.

What the evolution of print teaches us in this regard is quite instructive. It unveils flat plateaus of stability of great proportions; it teaches us that what is fundamentally at stake changes very little over time. Essentially, and beyond all the madcap events that Mark Rose gleefully recounts in his remarkable *Authors and Owners*, the essential issues remain few: there is control, there is property extended to new objects, and there is lusting for profit. Once this is clearly set and observed, printing history becomes much easier to unravel. The same remains true of the digital era; the objectives of control do not change, only the tools do, but we need to identify the real nature of these tools.

1. With the advent of digital technologies, equilibrium points that have proved essential for the management of copyright issues have been deeply disturbed. Technology no longer works in sync with the law, and no one knows for sure whether the disturbances are still reversible. The possibility of radical discontinuities increases. We have clearly entered a revolutionary phase in the proper sense of revolutionary; real changes in power structures and social relations are in the offing. Thinking no more than ten seconds about the Napster phenomenon, actually a minor epiphenomenon already deserted by all but mesmerized lawyers and a few bedazzled journalists, is enough to begin understanding what “radical” can mean in this context. Stripped of some of its technological props, intellectual property appears much more difficult to protect in practice.

Not surprisingly, this situation generates panic and cataclysmic predictions while armies of lawyers and engineers search for suitable substitutes. But no one knows whether such substitutes exist and various clues actually point in the opposite direction. The DVD protection case is quite telling in this regard. This means that the present concept of intellectual property may have to be scuttled in the end, and something else invented, probably just as oxymoronic. Moreover, the issue will not be resolved next month or next year. After Gutenberg, it took about two and a half centuries to devise a relatively stable copyright law; correspondingly, we may expect at least several decades of legal wrangling to reshape or perhaps even dismantle copyright laws as we presently know them. Should the latter hypothesis prove right, it remains

difficult to see what can replace them. At the same time, the incapacity to imagine something radically different does not point to its impossibility. Observers of the 17<sup>th</sup> century would have found it just as difficult to anticipate a solution based on such problematic categories as “authors” and time-limited “intellectual property”.

Finally, and despite appearances, copyright laws have already changed a great deal. If we stop and reflect about the way copyright issues are handled nowadays, in particular in the context of site licenses, one may legitimately wonder why the corresponding laws remain so central beyond what they have to say about the fundamental legitimacy of intellectual property. Commercial publishers, when they devised their licensing schemes and shifted over to a purely contractual context, essentially shed the established copyright regulations because they appeared as basically incompatible with profitable digital transactions. We can be sure they did so without any exaggerated sense of nostalgia for the destruction of the old playground they had done so much to design in the first place. Librarians and scholars must cast a similarly hard and lucid glance at the emerging world and thus avoid cutting fog with scissors, as the Japanese used to say in the years following capitulation.

## **PART II**

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### **In the Gutenberg Era: The Functions of Scientific Journals and Scientific Articles**



## 5 The Perspective of Scientists and Scholars

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As a scholar myself, I hope you will permit me to claim that scholars and scientists are essentially schizophrenic beings: as authors, let me call them Dr. Jekyll; as readers, they become Mr. Hyde. Sad as it may be, librarians generally meet Mr. Hyde rather than Dr. Jekyll, especially when he<sup>4</sup> comes into the librarian's office complaining about recent subscription cancellations. On the whole, Dr. Jekyll is a good deal nicer. However, he seems a little bemused by the economic dimensions of his published lofty ideas, or he treats such mundane matters with benign neglect. When he publishes an article in a journal, Dr. Jekyll is very attentive to its visibility, its authority, its prestige, as well as to a quantity known as the "impact factor",<sup>5</sup> as we shall see below; on the other hand, Dr. Jekyll generally turns a blind eye to the costs of the journal where he publishes, even though his reader side suffers from that very price. Somehow, the two sides of his personality do not seem to connect—hence the schizophrenic qualifications that I ascribe to most of my scholarly colleagues.

Scientists-as-authors need archived articles mainly at the footnoting stage, i.e., when property rights have to be recognized—a fitting tribute to Oldenburg's insight. These articles are retrieved within journals, reprints or off-prints, so long as they have been duly certified by peer review. As a rule, preprints will simply not do; not all journals will do either: scientists want the best citations from the most authoritative sources possible and this shows that scientific publishing actually rests on the perception of a pecking order among journals. Finally, scientists also monitor five to ten "essential" titles they deem fundamental for their specialty. In this manner, they check the progress of colleagues and potential competitors.

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<sup>4</sup> It is difficult to remain politically correct when you find yourself straddled with a novel character with a defined gender. However, let the reader be assured that women, alas, do not escape the fate of schizophrenic behavior when they become researchers....

<sup>5</sup> The impact factor is a standardized form of measure introduced by the Institute of Scientific Information (ISI). It is calculated by dividing the number of citations a journal receives a given year to articles published in the two previous years by the number of articles published in those same years. Thus the 2001 impact factor of a journal is the number of citations received in 2001 to articles published in 1999 and 2000 divided by the number of articles published in 1999 and 2000. See M. Amin & M. Mabe, "Impact Factors: Use and Abuse", *Perspectives in Publishing* 1 (October 2000), 1-6. Available online at <<http://www.elsevier.com/homepage/about/ita/editors/perspectives1.pdf>>.

Of course, they also monitor other news of a more professional nature (conferences, new research centers, new programs, etc.), but this activity has little to do with the direct transmission of scientific knowledge

While footnoting, the scientist-as-author is actually dealing with intellectual property issues, whether he/she realizes it or not. In this capacity, he/she acts very much like a prosecution lawyer building his/her case in court. By contrast, the information-gathering method appears much less systematic, much more improvised; it is adapted to peculiar situations, and it involves a fair amount of varied investigative skills. In his/her lab, the same scientist acts like a detective carrying out an investigation. Conferences, seminars, telephone calls, and, of course, e-mail are part of his/her investigative arsenal, as has been shown by various participatory observations of laboratories, for example by Michael Lynch.<sup>6</sup> With the generalized use of e-mail, what could be termed the social knitting of scientific texts has never been so closely intertwined with human contacts—a situation which, incidentally, beautifully vindicates the etymology of the word “text”.<sup>7</sup>

The lesson to be drawn from all this is obvious: research scientists treat articles and published journals exactly as Oldenburg had anticipated, i.e., as registers of intellectual property whose functions are close to that of a land registry. In effect, journals record the ownership titles (articles) and they define limits and boundaries. Ultimately, scientists are more interested in articles than journal titles, exactly as anyone would be more interested in locating a particular land title than a title office. Yet, knowing where the title office lies is obviously very important as well.

A scientific journal does not act only as a public register; it also labels, or, even better, it brands. Colleagues note whether your latest article appeared in a journal like *Cell* or *Nature*, or whether it appeared in a less prestigious journal. The reason is simple: being published in a well-known journal is a bit like appearing on prime time television. It delivers audiences; it creates visibility. For example, if you are a faculty member in a second-rank university and you publish in *Nature*, you will probably be celebrated in the local campus rag. In other words, the transmission of scientific information is not the primary concern of journals; branding is. And the consequences of this state of affairs are many. Of course, transmission does take place, but journals are generally too slow to respond to the investigative demands and needs of research scientists.

This said, the context of being published in a well-known journal is somewhat more complex than first meets the eye. It also means being accepted (sometimes grudgingly) into some sort of restricted intellectual

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<sup>6</sup> See his *Scientific Practice and Ordinary Action: Ethnomethodology and Social Studies of Science* (New York: Cambridge University Press, 1997).

<sup>7</sup> From the Latin “texere”, to weave.

space through a review process that guarantees as much one's belonging to a certain kind of club as it guarantees the quality of one's work. An editor receiving a manuscript—assuming it is a manuscript obeying to the general standards of its discipline—can make it sail through the review process with varying degrees of ease or difficulty simply by choosing some reviewers rather than others. In this particular phase of science publishing, we encounter one constitutive element of what could be termed the micro-physics of paradigm stability.<sup>8</sup>

A dense web of institutional and individual hierarchic relationships thus structures the scientific system and in order to appear in the best publication spot, it is important to avoid “wrong” steps. Wrong here does not so much mean “false” as tactical or strategic bad judgment: for example, throwing a challenging gauntlet without ensuring a sufficient stock of symbolic and institutional resources. In other words, simple caution dictates that brilliance—assuming it is present—must be exercised within well-established boundaries rather than outside!

Among scientists, those who manage to play an active editorial role in the publication process enjoy a special and rather powerful role, that of “gatekeeper”. As mediators, they are supposed to extract the wheat from the chaff. Of course, this judgmental role can only be justified if it is cloaked with the integrity (and authority) of the scientific institution. Any hint of systematic arbitrariness or bias would threaten the whole edifice of scientific communication. In this regard, a scientific editor acts a bit like the Keeper of the Seal without which royal prerogative cannot be exerted in the physical absence of the King. The difference lies in one important detail: in science, there is no King: only truth and reality are supposed to prevail. Silently, the journal's editor, therefore, has come to occupy the role of guardian of truth and reality or, in other words, the role of a high priest.

The scientific editor also fulfills Oldenburg's desire to create a universal science registry. To the extent that the publication under his/her responsibility is sufficiently accessible and sufficiently referenced in bibliographies and other similar tools, it contributes to the general and distributed effort to keep track of who did what in science. However, the ability to select and the capacity to register also amount to concentrating a certain amount of institutional and individual power into the editor's hand. For these reasons, his/her role is highly coveted and any opportunity to play that role will be examined with great interest. We

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<sup>8</sup> Kuhn envisioned paradigm stability only through the learning experience, including the tools and methods that dominate a field in a certain phase of its history. However, other power relations contribute to maintaining paradigmatic stability and science publishing should not be ignored in this context.

shall see that publishers understand that point very well; in fact, it provides the basis for a very fruitful alliance with a certain category of elite scientists: the so-called "gatekeepers".



## 6 *The Science Citation Index* and Some of Its Consequences

To understand how the scientific journals market underwent a deep metamorphosis after the Second World War, I will make a detour through work undertaken among librarians before the war. Those efforts were part and parcel of another transition that transformed library art briefly into library science before it changed into information science.

Bradford's law, first published in 1934,<sup>9</sup> essentially tried to respond to a question that financially-strapped librarians—this was the Great Depression—were trying to solve: namely, lowering the cost of each document use. The impossible dream was to find a way to buy only what users actually needed. Already, the goal to dispense with buying the costly “just in case” materials is visible in this quest for it amounted to saying, in effect: how much can we dispense with buying without really endangering the “just in case” situation in practical terms?

Everyone in the library profession knows Bradford's distribution law. It posits a multiplier,  $b_m$ , actually derived from a ratio: if you need 5 journals to survey the essential parts of your specialty and these 5 journals yield, say, 12 interesting articles, and if to find another 12 articles, you need 10 journals, then  $b_m$  will be 2 (10 divided by 5). And if you want to find another collection of 12 articles, you will multiply the 10 journals by the multiplier and will find 20. Obviously, returns diminish rapidly, as the multiplier grows exponentially, and it explains why scientists have long learned to moderate their urge for exhaustive searches. This bit of pragmatic wisdom makes all the more sense that, as we have seen earlier, the purpose of reading articles in journals generally corresponds to bounding one's own intellectual property while giving back to Caesar (or whomever) what belongs to him. Common wisdom makes it imperative not to forget the important characters playing within one's research area; on the other hand, missing a reference in a second or third tier layer of journals is forgivable and, in any case, can generally be corrected in a subsequent paper, should the author(s) complain.

Bradford's law was eminently useful to the library community: with it, one could indeed move to decrease the cost per consultation of library journals by identifying the “core publications” for each specialty. Thus,

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<sup>9</sup> Samuel C. Bradford, “Sources of Information on Specific Subjects,” *Engineering* 137 (January 26, 1934), 85-6.

librarians could offer a better fit between the needs of the local research scientists and their library holdings. Note that, at this stage of the story, i.e., until the early sixties at least, dozens of separate little cores coexisted, each one corresponding to a particular specialty.

The Second World War taxed the information systems of all the warring nations in totally unprecedented ways. In the United States, it led to Vannevar Bush's celebrated meditation on a possible device—the Memex—that would allow building richer, more powerful, and more flexible, information systems. This widely known article, which appeared in 1948 in *Atlantic Monthly*, inspired the development of hypertext, in particular in the work of Ted Nelson and Doug Englebart; but it also stimulated the thinking of Eugene Garfield and the program of the Institute of Scientific Information (ISI). The latter saw in the citation system of scientific articles the basis for a gigantic web of knowledge and he proceeded to explore how best to realize his dream.

Linking all the articles of the world's scientific literature could never have been implemented, had it not been for the existence of such hyperbolic distribution laws as Bradford's law or Lotka's law of scientific productivity.<sup>10</sup> Together, they helped bring the citation-tracking problem down to manageable proportions. In effect, what Garfield did was to collapse the entire set of little specialty "cores" into one big "scientific core" and he used this set of journal titles as the basis of ISI's emerging *Science Citation Index* (SCI). The number of core journals, although it gradually grew, has been confined to a few thousand titles, a small fraction of all scientific journals published in the world. All this took place in the early sixties.

Garfield's pragmatic solution to a thorny problem—namely finding ways to manage the tracing of thousands upon thousands of citations—carried with it a very large theoretical consequence. In merging all sorts of little specialty cores that had been culled from the coverage of leading bibliographies, and from interviews of many key scientists, Garfield, in effect, gave substance and reality to a new notion, that of "core journals" for "core science". What used to be a useful tool to assist in making difficult choices had become a generic concept with universal claims. "Core science" suddenly existed and it could be displayed by pointing to a specific list of publications. Disagreements were voiced, of course, but they had to do with this or that lacuna, rather than with the very concept of "core journals". In effect, these discussions strengthened, rather than weakened, the "core journals" concept as concept. Garfield had won!

Garfield's basic intentions were essentially bibliographic, but he has commented that no one could have anticipated all the uses that have emerged from the development of the SCI. In any case, the possibilities

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<sup>10</sup> If  $N$  scientists produce 1 paper/year, the number of scientists producing  $n$  papers/year will approximately be  $N/n$ .

for evaluating the impact of a given article on subsequent publications surfaced rather rapidly. In the late sixties and early seventies, many a university or research center mulled over the best way to build an objective scale with which to “grade” the performance of research scientists. The enhanced bibliographic tool Garfield conceived in the wake of Bush’s 1948 meditation suddenly appeared promising in this regard. After a while, ISI began to publish the impact factor of the journals it used in SCI, in effect rating these journals against one another, as if all disciplines and specialties within disciplines, harbored common citation practices, as if all of science were but one great unified culture. With this move, SCI was ready to drift into a whole new business area, that of career management tool.

Research centers and universities commonly use journal impact factors. Although pertaining to periodicals, this indicator finds itself applied to the case of individual scientists’ performance, simply because the figures are published and, therefore, readily available.<sup>11</sup> However, this lazy approximation undermines the very meaning of the exercise. The quantitative side of impact factors connotes objectivity, of course. To some people, particularly science administrators, this connotation seems to be more important than the appropriateness of the method because it allows them to generate powerful forms of judgmental rhetoric. It also keeps everyone mesmerized on journal titles and relegates articles into the background. As we shall see, the interest of commercial publishers is to keep pushing journal titles, and not individual articles, as they are the foundation for their financially lucrative technique of branding individual scientists.

With core journals identified and rated according to their measured impact factor, scientists, particularly the better ones, have little choice but to try and publish in the top publications. In effect, placing core journals into the spotlight lead productive researchers to seek visibility, prestige, authority (and improved institutional ranking) in these publications. In effect, the winners of Lotka’s law are racing to publish in the interesting journals as identified by a combination of Bradford’s law and impact factors. And the relative importance of journal titles, as compared to articles, grows even further.

In mapping the citation patterns among the articles of these journals, ISI had purported to create a new bibliographic tool; however, in pragmatically limiting its citation analysis device to a selected number of “core” journals, ISI had really constructed a knowledge or scientific space located somewhere between excellence and elitism. Excellence has to do with quality; elitism with value. In introducing elitist components into the scientific quest for excellence, SCI partially subverted the meaning of the science game.

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<sup>11</sup> I owe this detail to Michel Bosc, a scientist at the National Institute for Agronomic Research (INRA) in France, with considerable experience in scientific publishing.

By placing a core set of journals into sharp relief, ISI (and a few other prestigious bibliographies) managed to skew a quest for excellence into a race for elitist status. In itself, such a trend might not have been a matter of grave concern if the control of scientific publishing had solidly remained in the hands of the scientific communities, and if the elitist impulse had been kept within reasonable bounds. Hierarchy, as we have already seen, is the stuff of science, and science can even flirt with elitism without seriously harming its basic structure. Also, improving the tools to refine the competitive rules of science should certainly be applauded. However, the transformation of a quest for excellence into a race for elitist status bore important implications for any research library claiming to be up to snuff: once highlighted, a publication becomes indispensable, unavoidable. The race demands it. It must be acquired at all costs. There lies a crucial phase in the transformation of scientific publishing and it also lies at the heart of the serial pricing crisis. It amounted to a first revolution in the economics of knowledge and it was felt as a serial pricing crisis by librarians.

## 7 Scholarly Journals as a New Publishers' Eldorado

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For a long time, scientific publishing remained largely in the hands of learned societies and similar, scientist-driven institutions. However, as Jack Meadows<sup>12</sup> has shown, problems of delays and recurring tendencies to censor some areas of research (such as phrenology or, more surprisingly, statistics), gave commercial publishers an opportunity to enter this particular sector of activity. At first, their presence was probably beneficial as it helped keep the associations and scientific societies on their toes. At the same time, the peculiar nature of scientific publishing presented publishers with a number of problems that severely hampered their activities: reconciling the highly targeted demands of scientific publishing with mass production tools and keeping the whole enterprise profitable proved a daunting task. In those days, scientific periodicals, unlike treatises and manuals, rarely turned a profit, but printing a few of them could bring some prestige; more importantly, it secured contacts with authors who, one day, might want to write profitable manuals or treatises. Until well after World War II, commercial publishers remained a fragmented and marginal lot in this secondary business field centered around scholarly journals.

With the advent of SCI, the situation changed rapidly. The economic possibilities attached to the “core journals” became all the more obvious when, at about the same time, the size and number of libraries also grew a great deal due to the post-war explosion of university studies, first in the United States, and later everywhere in the industrialized world. Printed scholarly “core journals” suddenly enjoyed a sizable and secure market.

Commercial publishers did not take long to realize that a new, potentially lucrative, situation had just emerged. There was gold in those there stacks after all! What librarians viewed as crucial core journals, publishers translated as the constitutive elements of an “inelastic market”, i.e., a market where demand was little affected by pricing (and vice versa).

Capitalist ideologues and neo-liberal types often recite stirring

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<sup>12</sup> A. J. Meadows, “Access to the Results of Scientific Research: Developments in Victorian Britain,” in *Development of Science Publishing in Europe* (Amsterdam: Elsevier Science Publishers, 1980), A. J. Meadows, ed., pp. 43-62.

mantras about the market as universal solution to most, if not all human, problems; but they forget that deep in the heart of real capitalistic practice lies the quest for monopoly situations. Bill Gates has never been interested in a perfect market, i.e., a competitive one, a point the U.S. Department of Justice has taken some care to document and prosecute. Exploiting inelastic markets wherever they exist (or may be created) is the real name of the capitalist game. Commercial publishers, predictably enough, were not about to ignore such appealingly lucrative entreaties emerging from the unlikely quarter of scientific journals.

The serial pricing crisis began to surface in the early seventies, a few years after the beginning of the SCI. This delay can be interpreted as the time it took to digest the economic implications of the emergence of the "core journals" and to implement the new commercial strategies. Core journals rapidly found themselves targeted by corporate interests from the late sixties on.<sup>13</sup> Various publishers have untiringly tried to lay their hands on those titles through a variety of means: direct acquisition, sale of publishing services and, of course, a number of spectacular mergers or acquisitions. Science journal publishing is concentrating at an accelerating pace. Elsevier, now Reed-Elsevier, has acquired the dimensions of a behemoth. It now controls around 20 percent of the core publications. Its latest acquisitive success is Academic Press (as part of the Harcourt Brace deal).

By the end of the eighties, the new publishing system was firmly in place and its financial consequences had become hurtful enough to elicit some serious "ouches" on the part of librarians. It even attracted the attention of some scientists, such as Henry Barschall, the University of Wisconsin physicist who pioneered some very interesting statistics showing that, between various journals, the cost/1,000 characters could vary by two orders of magnitude; if weighted by the impact factor, the variations could reach three orders of magnitude.

Simply pointing to this 1 to 1,000 range in weighted prices is enough to demonstrate the total arbitrariness of the pricing of scientific journals, i.e., its complete disconnection from actual production costs. It should also suffice to respond to Ann Okerson's (hopefully rhetorical) question: Do we really pay too much for our scientific information?<sup>14</sup> Of course, we pay way too much! Otherwise, prices could not vary as wildly as they do.

But more fundamentally, we are not so much paying for information as we are paying for evaluation. Placing one's article in *Cell* or *Nature*

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<sup>13</sup> Here again, it would be nice to have a precise history of the financial or commercial careers of the journals appearing in SCI since its inception. Another team study, no doubt....

<sup>14</sup> See Ann Okerson, "What Price Free?" <<http://www.nature.com/nature/debates/e-access/Articles/okerson.html>>. My attempt to refute Ann Okerson's arguments can be found at: <<http://www.nature.com/nature/debates/e-access/webdebdata/webdebate.taf?format=moderatorlist>>.

brands! Now, if we correlate the branding process with the extravagant pricing of scientific journals, we can readily see that scientific excellence, already somewhat skewed into scientific elitism, has by now neatly dovetailed with financial elitism. Only the rich (and presumably the smart) can read up-to-date scientific information. For their part, poorer institutions in some rich countries and all institutions in poor countries have suffered enormously from the financial bonanza made possible by the revolutionary invention of the “core journals”.<sup>15</sup>

That Henry Barschall had hit a sensitive nerve with his careful comparative statistics became obvious from the public reaction of at least one publisher, Gordon and Breach. This publisher sued Henry Barschall, the American Institute of Physics and the American Physical Society in four countries: the U.S., Germany, Switzerland and France. Cautiously, not to say cowardly, Gordon and Breach did not sue in its home country, Britain, where its assets lay.... Gordon and Breach ultimately lost everywhere, the last time in France in June 2000, and the firm was still appealing in August 2000.<sup>16</sup> The point, of course, was not to win—a patently impossible task—but merely to intimidate a courageous individual and to gain as much time as possible during which no one would dare work on similar statistics and publish them.

Alas, Henry Barschall did not live to see this final victory. Although exceptionally brutal, Gordon and Breach’s attitude must be viewed as a frank expression of the commercial publishers’ true feelings toward markets. In the economic sense of the word, a market requires fully informed consumers who are supposed to make rational choices. To claim as Gordon and Breach did that information about the scholarly journal prices amounted to distorting and manipulating the market shows the company’s utter contempt for the economic concept.

Let us now turn our attention toward a different kind of question: how are new journals created? It will allow showing how another alliance—that between gatekeeping scientists and publishers—is established and works. How does one go about creating a commercially interesting new journal that is also attracting worthy scientists? How does one go about recruiting a new editor-in-chief? The competition and fragmentation characterizing the workings of invisible colleges<sup>17</sup> go a

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<sup>15</sup> Since the ARL meeting in Toronto last May, the World Health Organization and the Soros Foundation have announced they had managed to negotiate some access to medical journals for poor countries. In effect, they pay a reduced rate for these journals and they will deliver them free or at a very low cost to countries according to some GDP/capita formula. This is certainly a welcome piece of news, although it must be understood that this agreement also opens up unexpected new markets for the commercial publishers. See <<http://www.who.int/inf-pr-2001/en/pr2001-32.html>>.

<sup>16</sup> <[http://www.gbhap.com/about/French\\_Decision/top.htm](http://www.gbhap.com/about/French_Decision/top.htm)>. Thankfully, the firm was bought by Taylor and Francis in February 2001 and it now looks as if the French case has definitely closed in favor of Henry Barschall and his supporters.

<sup>17</sup> See Diana Crane, *Invisible Colleges: Diffusion of Knowledge in Scientific Communities*

long way toward explaining how this can be achieved relatively easily.

Scientists cannot hope to launch a new journal corresponding to some new sub-specialty without serious backing, both intellectual and financial. While the scientific component is certainly fundamental, money cannot be neglected; without it, the best of projects will not reach fruition. Moreover, financial support must be ensured for a sufficient while, if the goal is ultimately to bring the new journal up to "core level status". Of course, with success the journal becomes not only financially secure, but quite profitable. In this fashion, a publisher can imagine investing in a new venture, help bring it to core status and then pocket the profits. Because journals brand their authors, launching a new journal bears some resemblance with launching a new brand.

As noted above, scientists acting as gatekeepers enjoy an enhanced status within the scientific community. If a scientist of some repute is offered the chance to head a new journal, the response will generally be positive, perhaps even enthusiastic. The ability of offering this status-enhancing role to various scientists lies, I believe, at the foundation of a *de facto* and largely unexamined alliance between a number of key research scientists and commercial publishers. Thanks to abundant capital, commercial publishers can easily and quickly strike deals with scientists working either in new areas or in areas that a particular publisher does not yet influence very much. In this fashion, a publisher can even try to prop up a journal aiming directly at another, similar one owned by a different publisher.

University presses and learned societies that behave like real learned societies<sup>18</sup> obviously do not command similar financial resources; consequently, their ability to move into new specialties or to create new journals is very limited. They hang on to what they have for as long as they can, until, tired, discouraged, the editors accept the seductive proposals of some large publisher. After all, they retain editorial control; as for the cost of the revamped journal, well... the old schizophrenic syndrome... you know?

The alliance between gatekeeper scientists and commercial publishers obviously generates a win-win situation for the two partners. What is often forgotten, however, is that win-win situations, so often used rhetorically to justify some desired course of action, often keep silent about a possibly losing third estate. In the case of excessively priced

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(Chicago: University of Chicago Press, 1972), *passim*.

<sup>18</sup> It is important here to check what these categories can cover. As noted earlier, some learned societies, although non-profit in theory, use their accumulated funds to carry on lobbying activities or develop ever more diversified activity which extends their institutional power. The American Chemical Society is an old hand at this. Some university presses work as truly academic presses while other adopt more commercial outlooks. Again, general categories fail to capture the bewildering variety of behavior patterns we encounter and a case-by-case analysis ought to be done, and it should be based on clearly established criteria, so as to permit various kinds of comparisons.



journals, I am talking not only about libraries, but also their covering institutions, such as universities or research centers, and even the governments that finance them in whole or in part. All these players see their budgets flowing inexorably into the pockets of the large publishers. They, too, should be very concerned.

## 8 The Libraries' Perspective

Libraries were the first to feel the financial pinch of the new business plan applied to scholarly journals; they were also the first to document its existence with care. However, "core journals" did more than financially hurt libraries. I will use the Canadian example as it is much simpler than the huge and complex American scene. According to figures gathered by the Canadian Association of Research Libraries, the University of Toronto spends about three times more on periodicals than the next Canadian research library in size (University of Alberta), and this must translate into something like three times more titles. However, to know how many more interesting articles a scientist will find in Toronto compared to Alberta, we must divide this figure of three by the Bradford multiplier. If this multiplier is about two, the tripling in the number of titles brings about only 1.5 times the number of interesting articles. The added investment rapidly becomes less productive, which explains why only one library in Canada manages to soar above the others and keep up with the very best American libraries; it also suggests why Canadian research libraries that spend around 6-7 million Canadian dollars on journals (4 to 4.5 million U.S. dollars at the present exchange rate) are overrepresented: this sum roughly corresponds to what is needed to acquire the core set, with minor local variations.<sup>19</sup> In short, research libraries seem to use the core target as basic benchmark; it has become a common target. Some manage to do a little better, others try not to fall too far behind, at least in the areas where research is locally active, but all act with their eyes trained on that threshold and they buy as much as they can from that core set. In effect, it has become the "must" set.

Faced with this situation, librarians reacted patiently, even stoically. However, in being so reasonable, they inadvertently contributed to treating a new and intolerable business invention as if it were a mere dimension of unavoidable reality. In other words, librarians strove to act "realistically". However, the staggering escalation of costs and the slowly growing realization that most explanations put forth by publishers simply amounted to obfuscation finally led some of them to adopt a more militant attitude. Frustration, then anger finally surfaced. I am tempted to add: it was high time!

The new combative spirit of librarians is apparent in the SPARC

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<sup>19</sup> I have not done the work for the U.S., but it is interesting to note that, for periodicals, the average expense of ARL libraries was US \$4,431,593. I am quite aware that, in statistics, an average is not a mode, but this is a start....

initiative and the “Create Change” movement. Both demonstrate the librarians’ desire not only to innovate, but also to confront the large commercial publishers. The goal is to reinstate competition where, for all intents and purposes, it has disappeared. The question one must immediately raise, however, is whether the SPARC strategies are really effective. My answer will be a cautious “yes”, as the evidence is only beginning to accumulate. Let us say that the jury is still out but some early signs are encouraging.

The SPARC initiative is based on a good argument: if the excessive cost increases of journals are made possible by a quasi-monopolistic situation, then, SPARC argues, competition must be reintroduced and reinforced. The solution, therefore, requires creating or supporting journals that will compete head-to-head with the big, expensive journals of the big commercial publishers. However, the point is not to take on every expensive journal and reduce it to intellectual rubble; ultimately demonstrating that the present political economy of scholarly publishing is neither normal nor fatal will be more important than beating a few commercial journals at their own game. If enough scientists come to realize that big publishers are not invincible and that, furthermore, their influence on science is not universally positive, then a very powerful movement may begin to gather momentum, thanks to SPARC.

Several tangible examples now exist, thus demonstrating that the time of implementation of the strategy has begun in earnest. Several fields and specialties are already covered, in particular in mathematics: *Topology and Its Applications* (Elsevier, \$2,509<sup>20</sup>) faces *Algebraic and Geometric Topology*, a SPARC alternative that is published for free by the Mathematics Institute of Warwick University in the UK;<sup>21</sup> *Chemical Physics Letters* (Elsevier, \$9,637) is confronted by *PhysChemComm* (\$100), a publication of the Royal Society of Chemistry, again in the UK; *Tetrahedron Letters* (Elsevier, \$9,036) must now share the organic chemistry field with *Organic Letters* (\$2,438—by far the most expensive of SPARC journals, but still much cheaper than the Elsevier publication).<sup>22</sup>

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<sup>20</sup> The figures are taken from a presentation by ARL’s President, Shirley K. Baker. See <<http://www.arl.org/sparc/resources/07-01LIBER/sld001.htm>>. I would like to thank Alison Buckholtz and Richard K. Johnson, both from SPARC, for having kindly provided me with data and advice on this section of my presentation.

<sup>21</sup> Warwick also publishes *Geometry & Topology* (free) against *Topology*, another Elsevier publication. The second most expensive journal published under SPARC auspices is *Crystal Growth and Design* (\$1,600), also published by ACS. Actually, of the ten SPARC journals listed by Shirley Baker, three are free, the two RCS publications cost \$1000 apiece. The ten SPARC journals cost \$5,238, of which over \$4,000 correspond to the two ACS journals. The ten equivalent commercial titles cost \$40,677.

<sup>22</sup> The latter is actually a publication of the American Chemical Society which does not exactly practice low prices either. SPARC, to its credit, apparently manages to play one big player against another in order to keep all prices down. Incidentally, *Organic Letters* is beating *Tetrahedron Letters* at the impact factor game. Could this account for the cautionary tone of the Amin and Mabe article mentioned above in note five?

Interestingly, several learned societies are involved in deals with SPARC: beyond the American Chemical Society and the Royal Society of Chemistry, already mentioned, we meet IEEE (*IEEE Sensors Journal*). But BioOne may offer the most extensive example of this kind of collaboration: it involves nearly 50 journals that belong to smaller scientific societies. Finally, university presses are also collaborating with SPARC (Cambridge University Press, MIT Press), thus completing an impressive array of partners.

The price differences between titles are also very encouraging, of course, even though such comparisons are not as convincing as costs/1,000 characters or, more simply, costs/page or costs/article, and the latter have sometimes revealed far less impressive results.<sup>23</sup> However, the most encouraging result of the SPARC publications lies with their apparent ability to rein in competitors' prices, at least to some extent. The price of *Tetrahedron Letters*, already at \$5,200 in 1995 appeared poised to reach \$12,000 in 2001, had the trends maintained themselves; actually, it has shown some signs of leveling off, reaching a price of about—I almost wrote “only” but recoiled at the last minute—\$9,000 in 2001. The flattening of the price curve of *Tetrahedron Letters*, interestingly enough, coincides with the launching of SPARC and it has accentuated since *Organic Letters* started to compete (3% increase from 1999 to 2000; 2% the following year). The following episode may even be more interesting: thanks to SPARC's support, at least one editor has been able to convince his publisher to lower subscription prices: The American Association of Physical Anthropology has convinced to lower the institutional price to \$1,390 from \$2,085!<sup>24</sup> The possibility of launching an alternative journal must have played some role in the background of these discussions.

In effect, SPARC is using a number of tactics to multiply contacts and forms of collaboration with learned societies, individual journals, and even individual scientists as authors. The goal is to compete successfully for the best articles from the best scientists and provide them at the best possible price. Undoubtedly, much thinking has gone into the design of this strategy, as well as a good deal of real gallantry, for such battles are not easy to wage. Will it work over the long range? Personally, I hope so, but it will not be easy. In fairness to SPARC, no solution is likely to be easy, if only because the commercial publishers are entrenched within very powerful positions. To many, this situation is daunting to the point of paralysis; to SPARC's credit, the association has managed to restore a sense of initiative among many librarians and to bring back a sense of

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<sup>23</sup> There was a bit of controversy in 2000 in Marcia Tuttle's *Newsletter on Serials Pricing Issues*; see particularly the criticism of *PhysChemComm* pricing in No. 250 (June 16, 2000) with a reply in issue 252. Further discussion on the pricing of chemistry journals appeared in issue 253, under the pen of Dana Roth, from the California Institute of Technology. Far from necessarily revealing inherent flaws in SPARC's strategy, these discussions may simply reflect the fact that initial launching conditions are always a bit difficult.

<sup>24</sup> See <<http://www.physanth.org/newsletter/spring00.pdf>>. The announcement with the four main points are detailed on page four of the newsletter.

hope in the form of tangible possibilities.

Journals, as we have already seen, really work like a triple-function device: branding tool, key to property title, and evaluation instrument of individual scientists' performance. With regard to these functions, there is no doubt that well-known, core journals offer good value, unlike more obscure journals. For example, being "branded" by a leading journal always provides valuable and exciting visibility, and it is a flawless prop at tenure time. Moreover, core journals are well known and they can rely on the lazy familiarity of many scientists who often retain some of them in their rapid application of Bradford's law.

As a result, and against a well-established journal, a new SPARC publication must expect to fight an uphill battle: it must establish credibility and it may have real difficulties to make it to the core, even with good support from influential scientists. If the considerable financial resources of large commercial publishers are factored in, as well as their impressive technical expertise, and if the economies of scale that accrue from possessing hundreds of titles are also added to the picture, it amounts to a kind of David *vs.* Goliath situation. This is already bad enough, but we must also be sure that David has not chosen the wrong battlefield. In short, I fear that a frontal attack against a large commercial publisher such as Elsevier may be premature, despite encouraging first results. These are difficult words to say because my heart lies with SPARC, and I deeply believe in its motives and aims. I even want to support their efforts. However and alas, the purity of motives has never guaranteed efficiency or success.

Of course, SPARC does not limit itself to creating new journals; in fact, it uses a whole array of actions to network research scientists, university presses, learned societies, and libraries into a coherent whole that may have the clout to slow down and even reverse the incredible lust for profit that motivates commercial publishers. By exploring the possibility of helping learned societies to retain control over their journal whenever they are facing financial difficulties, by helping editors to negotiate better deals with their publishers when they are in the hands of a commercial publisher, by educating research colleagues about the economic realities of scientific publishing, SPARC works toward creating stronger alliances between libraries and at least that category of gatekeepers which refuses to give in too willingly to the big publishers.

We must remember that if something useful is to be achieved in the area of scientific publishing, it will have to rest on a renewed alliance with scientific institutions: associations, learned societies, invisible colleges (i.e., networks of scientists doing research in the same specialties) appear crucial, if only to strengthen the ties between library associations and gatekeeping scientists. It may be that kind of linkage that led the whole editorial board of the *Journal of Logic Programming* to resign *en masse* from this Elsevier publication and found a new journal, *Theory & Practice of Logic Programming* (Cambridge University Press). Professor Maurice Bruynooghe even won a prize for this courageous move. In

February 2001, Elsevier signaled its desire to regain lost ground by launching a periodical with a slightly different title, the *Journal of Logic & Algebraic Programming*,<sup>25</sup> with an editorial board led by J. A. Bergstra (Amsterdam & Utrecht) and J. V. Tucker (Swansea). I certainly will not wish good luck to this new editorial board but I must admit that, alas, scientists often place the enhancement of their personal career ahead of the collective good. For one Professor Bruynooghe, there may be ten others eager to enjoy the opportunity to act as gatekeepers. It is this kind of narrowly selfish mentality that SPARC must try to overcome, and that will simply not be easy. However, administrators and people in charge of evaluation can help by reforming the criteria used to evaluate their research personnel. But will they?

In conclusion, SPARC appears as a versatile tool designed to strike pragmatically defined agreements with various partners while keeping everybody's eyes trained on the need to reopen the scholarly communication "market". Its main strength probably rests with its flexibility and its realism; far from rigidly sticking to admirable principles, it carries on a series of practical moves destined both to weaken the big publishers' grip over the scientific system of communication and provide an alternative vision of scientific exchange capable of mobilizing researchers as well as administrators of research. Its best future, in my opinion, is to pursue what it is already doing and, in particular, to restore a degree of elasticity into a market that big publishers try to monopolize. Its major challenge is to find ways to coordinate its actions with the Open Archives Initiative (see below) so as to create a synergy that will bring increased efficiency to these parallel developments. SPARC will need this alliance if it wants to break the barrier that keeps its action at a level that must appear quite puny from the perspective of the big commercial publishers. A dozen SPARC-labeled journals is wonderful, but, let us face it, it may appear to many as more symbolic than anything else, especially when compared to the hundreds of titles in commercial baskets. The situation appears more promising, however, if the idea is to catalyze change rather than creating all of it *ex nihilo*: if indeed SPARC acts like a good catalyst, a small initial effort may well turn into a self-sustaining and ever-expanding movement that will soon be unstoppable. Skeptics would do well to think about the Linux movement that just celebrated its 10<sup>th</sup> anniversary this summer to realize that SPARC may be far ahead of the present situation occupied by the "free source code movement" when it too reaches ten years. The challenge is to preserve the symbolic value while drastically improving the fire power.

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<sup>25</sup><<http://www.cis.upenn.edu/~bcpierce/types/archives/current/msg00540.html>>

## **PART III**

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# **The Advent and Implications of Networks and Digitization**





## 9 Researchers' Mixed Responses: From Indifference to Active Involvement

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About when Henry Barschall began battling the rather vindictive publisher that we encountered earlier, i.e., the late '80s, digitized content and the Internet became visible in publishing circles. Immediately, the development of electronic publishing generated a number of interrogations that led to revisiting the basic functions of scientific publishing. Essentially, the question everyone asked was: What is my real business in this new context? Publishers, in particular, wondered, probably more extensively than any other group, what omen these new technologies carried: their sources of profit and even their survival were at stake. Likewise, librarians got rapidly interested in electronic publishing, if only because they were looking for various ways to escape the stranglehold of the publishers. In fact librarians, and in particular ARL, were among the first to monitor the rise of electronic journals. On the other hand, the majority of research scientists remained largely indifferent, although a small minority played influential roles in the concrete development of electronic publications. Each group had its own agenda, of course, and each form of revisiting scientific publishing yielded different perspectives.

As authors, scientists quickly realized that digital publishing did not change their situation very much. Of course, they enjoyed submitting their articles electronically because it saves time, postage, and a manila envelope. But, fundamentally, scientists-as-authors still had to deal with journals, editors, and the peer-review process. Delays were not shortened in any significant way. In most cases, the paper version was still available—a most tangible, almost reassuring, reminder that the earth kept circling the sun.

The scientists' indifference to the digital medium can be explained in part by the poor exploitation of the new media's possibilities. Scientific articles remain exactly as they have been for several centuries: a paper-based assembly of text, diagrams, and fixed illustrations (or, more recently, photographs). Digital or not, their ultimate fate remains constant, so it seems: print. It matters little to scientists whether they do their own printing with a laser printer, or rely on a print shop. In a sense, photocopying had already made them familiar with individualized printing. Oldenburg himself would have had no difficulty in recognizing

a digital scientific article, once printed. Only photography and color would have surprised him, but these are hardly new technologies.

As readers, scientists have been much quicker to grasp the advantages provided by digital documents: most immediately obvious was the easy retrieval of information. New strategies based on full-text searching appeared. Copying and transmitting a digitized document turned out to be easy, rather cheap, and very fast. With the generalization of online services, bibliographic searches also became easier and efficient, to the point that reference librarians especially trained in database searching techniques were no longer indispensable. With access to online journals from the office or home, facts or techniques could be checked in stride, right in the midst of complex experiments, right when and where they were needed, "just in time". All these advantages have been perceived as real progress, of course, and scientists, understandably, clamor for these services as soon as they get a taste of it. Publishers know this and that is why they periodically offer free temporary trial access to their online journals to libraries. They know that a brief exposure will lead to addiction and pressure to keep the service open will be so great as to make any return to the *status quo ante* very difficult. As a librarian, try and shut the faucet once it has been opened, and see if you keep your job!

The first experiments with electronic journals actually came from a few exceptional scientists and scholars (e.g., Jim O'Donnell at Penn, Stevan Harnad then at Princeton, etc.) who, obviously, were sporting their reading, investigative side in so doing. They had quickly grasped the improved possibilities of electronic publishing for the scientific communication system. At the same time, they were approaching the whole question of scholarly journals strictly as scientists and scholars, which meant that they treated the serial pricing crisis and the librarians' worries as relatively secondary matters. Their motives, although diverse, and sometimes even divergent, generally included the following points:

1. Electronic publishing could help reduce publishing delays and thus better synchronize the pace of scientific publishing with that of results pouring out of the labs, and with discussions carried out within invisible colleges.
1. The publishing costs could be significantly decreased. "How much?" has been the object of hot debates and answers have oscillated between 0 and 90%, with a minimal and fragile consensus on the "at least 30%" figure.<sup>26</sup> The answer largely depended on who was talking, but in the end, the existence of some savings could not be denied: the cost of printing, mailing,

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<sup>26</sup> The percentage figure must refer to production (fixed and variable) costs, and not to revenues as Christine Borgman, for example, does. Linking printing costs and revenues is meaningless. Revenues include profit and it stands to reason that, the higher the profit is, the lower the printing cost will appear.... See Christine L. Borgman, *From Gutenberg to the Global Information Infrastructure: Access to Information in the Networked World* (Cambridge, Mass.: MIT Press, 2000), 86.

and much of the storage expenses disappear on the supply side as soon as a periodical is digitized.

1. The possibility (and viability) of free journals was also frequently mentioned (and just as frequently attacked), but the presence of a significant (and growing) set of free electronic journals gradually demonstrated the reality of the thesis.<sup>27</sup>
1. The startup costs of journals were much lowered, allowing for the launching of many new journals. Depending on the viewpoint, this was viewed either as a good or a bad thing. Gatekeepers already in power, or close to it, generally did not appreciate seeing their role possibly diluted or relativized; those who were not gatekeepers, on the contrary, perceived electronic publishing as opening opportunities to correct and even contest some hierarchical elements in the structure of science.

These pioneer efforts did not go unnoticed. In fact, representatives of commercial publishing houses regularly attended the early conferences on scholarly electronic publishing, often intervening vigorously to contest the possibility of much cheaper publication costs or, worse, the suggestion that e-journals might become freely accessible. Smaller university presses often sided with big publishers, not because they wanted to maintain a high profit return on investments, but simply because their financial situation was (and still is) fragile. The financial crisis that affected the universities in the early '90s generally resulted in lower or even no institutional support. Decreases in revenue could then mean death and electronic publishing spelled menacing uncertainty at best!

In short, early electronic publishing initiatives gave rise to a variety of tensions. In particular, they were quickly perceived as potentially threatening to the recent, but highly profitable, business plan revolution based on "core journals". The large commercial publishers realized that they had better study the situation closely, and be ready to revise their business strategies accordingly. Benign neglect could mean losing a lucrative business.

Retrospectively, the year 1991 appears emblematic of the new era: it witnessed the emergence of two electronic publishing scenarios that are still with us. It is in that year that Elsevier launched the TULIP project; the same year, Paul Ginsparg began his physics preprint server at the Los Alamos National Laboratory. In the rest of this presentation, I would like to study these two lines of developments in order to try and clarify what

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<sup>27</sup> Stating that an electronic scholarly journal should be free does not amount to claiming zero costs to produce that journal; it only amounts to saying that the costs can be so reduced that they can perhaps be picked up by institutions interested in expanding the public space of science. To the extent that no cost enhances access greatly, this works in the direction that both librarians and scientists support. All the costs related to subscription management also disappear.

libraries can do and what roles they can play. This will lead to examining the kinds of realignment that might take place among libraries and a large fraction of scientists. It will also require educating the higher administration of our universities: most university administrators understand library problems only in the vaguest and most general of terms, and some may even be lulled into thinking that it is but a minor cost irritant, compared to the cost of research in general—a point of view that, surprisingly enough, I have even heard expressed by at least one prominent librarian....

## 10 **How Commercial Publishers Managed to Harness the Digital Revolution into a Counterrevolution**

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Concerned by the rise of electronic publishing, commercial publishers of scientific journals reacted rapidly, as demonstrated by the early launching of the TULIP project by Elsevier in March 1991. The TULIP project sheds a most interesting light on the main worries of the publishing industry. Exactly as was the case in the early history of print, questions of profitability quickly linked up with questions of control, and the technology was shaped to try and respond to these needs.

A number of prominent U.S. libraries took part in the TULIP experiment. In fact, I am sure that some people in the audience know far more about the project, its inception, conclusion, and ultimate demise, than I ever will.<sup>28</sup> In the end, TULIP did not quite manage to build a consensus among libraries: at least one—Princeton—seems to have abandoned the project relatively quickly; but it certainly provided a rich harvest of important results which, although largely negative, nonetheless proved useful to commercial publishers.

With hindsight, the TULIP experiment appears notable for the following reasons:

It was conceived as a licensing system—the “LI” of TULIP. The move to licensing seems to have been inspired by the software industry: let us remember that the code of software is generally covered by copyright laws and not patents (even though strong forces in the U.S. and in Europe are presently pushing to change this situation). In order to avoid some of the dispositions of the copyright law, such as first sale provisions, software was licensed rather than sold. Elsevier extended this notion of license to scientific documents, thus setting a revolution into motion, or rather a counterrevolution, as we shall see.

TULIP was based on the distribution of physical digital media on

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<sup>28</sup> See <<http://www.elsevier.nl/homepage/about/resproj/tulip.htm?mode=tulip>>. Princeton University is mentioned in the first Newsletter. TULIP—a perfect acronym for a Dutch company—stands for: The University Licensing Program. Elsevier could not have been clearer, but apparently objections to moving to a licensing scheme were few, if any.

each participating site. These "ROMs" were mounted on local servers.

The 42 journals that were included in the pilot collection were delivered as page images in the TIFF format. The use of page images solved several problems: by acting as electronic paper, they protected the integrity of the documents, very much as PDF files do nowadays; because of their size, these files could not be comfortably transmitted over regular modems and were limited to ethernet LANs—in practice this meant a campus or part of it. Full-text searching was also offered, but the user could not gain direct access to the text file. All these precautions point to the vendor's almost obsessive preoccupation with control. It is sometimes said that digitized birds know no cage; in effect, the Elsevier people had chosen to ignore this bit of wisdom and were desperately trying to design a new, suitable, textual prison.

Printing the page images was slow, given the size of the files and the memory of printers at the time. Securing a paper copy of a given article—a legal possibility within the "fair use" provisions of the U.S. copyright law, for example—became much more cumbersome than old-style photocopying. Whether this goal was consciously sought or not remains moot, but it certainly did not contradict other embedded elements of the publisher's agenda.

From the TULIP experiment, Elsevier eventually retained little more than the licensing scheme; it abandoned the idea of locating a physical copy of the database on each campus; instead, it resolved to tighten its control over the data bank of articles by setting up a central server. A suitable, but limited, number of mirrors were added to improve response time and make better use of the bandwidth available. Those too remain under the tight control of the publisher.

Elsevier's new design incorporated a series of important consequences. For one, it relegated libraries to the passive role of "knowledge pumps";<sup>29</sup> worse, it managed to invert the library's function in a radical way: instead of defending a public space of access to information by buying copies of books and then taking advantage of the first sale provision of the copyright law, librarians were suddenly placed in the position of restricting access to a privatized space. They no longer owned anything; they had bought only temporary access under certain conditions and for a limited number of specified patrons. Moreover, in order to gain access to a collection of journals, libraries had to accept (and learn) to negotiate licensing contracts. Not only were the required skills rare within libraries, but all the traditional safeguards slowly conquered and defended within the limits of traditional copyright laws were

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<sup>29</sup> See my Leiter lecture of 1998, "The Virtual Library: An Oxymoron?" in the *Bulletin of the Medical Library Association*, available online at <<http://www.mlanet.org/publications/bmla/leiter98.html>>.

suddenly laid aside and everything had to be discussed anew—in short, a lawyer’s paradise and a librarian’s hell. At the same time, the organizing and cataloguing roles traditionally exercised by librarians—what I like to call applied epistemology or epistemological engineering to underscore the importance and dignity of these tasks—were also forfeited.

Ironically, the publishers’ desire for control met with almost too much success as they found themselves saddled with functions they apparently did not care to maintain—in particular, the long-time preservation of archival resources. As a result, the emerging system is being rapidly reformed: recent negotiations between Elsevier and Yale University (and Harvard is doing the same with other publishers) seem to indicate that publishers do not really want to take on this role, that they would rather unload that task onto librarians. And librarians may even look kindly upon this development as it gives them back what they should never have lost in the first place. But at what price?

*In toto*, we can say that the division of labor between publishers and librarians is being negotiated anew and that the power relationship does not appear to favor librarians. It is not inconceivable that librarians could end up simply holding the local nozzle of a universal knowledge pump when they deal with current materials, while sitting on top of largely obsolete or secondary information. Worse, even the pump-holding function is not assured; it really depends on the publishers’ decision; so long as they think they can decrease their operating costs by dealing with libraries, they will preserve the nozzle-holding function. However, if we imagine a situation where e-commerce has really taken off on a large scale, with secured transactions and easy, well-established forms of micro-payments,<sup>30</sup> then the need for libraries as intermediaries might not appear as obvious.<sup>31</sup> Should this dire prediction come to pass, libraries would end up as little more than dusty museums where old books would be stored and old digital files would be periodically refreshed to remain compatible with a fast-evolving technology. What a stimulating and vibrant future that is!

All these points explain why I refer to the use of a licensing framework as nothing less than a counter-revolution in the political economy of documents: it allows bringing back every debatable (from the publishers’ perspective) point of copyright laws to the negotiating table; for example fair use or open access to the library space can be questioned anew. In effect, licensing contracts subvert copyright legislation on all but one basic point: they do not question the fundamental legitimacy of intellectual property because the whole licensing structure ultimately

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<sup>30</sup> Microsoft is working on such a system. Rumors of a convergence between Microsoft and Reed-Elsevier also regularly recur.

<sup>31</sup> Deans, in several universities, have been rumored to mumble that it would be more cost efficient if the publishing scholars and scientists were given the means to buy the accesses they need as individuals, and if libraries were confined to undergraduate teaching support.... Let us hope it is only a rumor.

depends on it. In short, it permits setting the clock back several decades.

This wave of ominous changes has been camouflaged by a vocabulary designed to connote modernism and progress: it has variously been described as the decisive move from a "just in case" situation to a "just in time" setting, or as the mutation from old-fashioned ownership to modernistic access. Brave words, these, but when duly examined, they lead straight to a librarians' Waterloo (as seen from a French perspective, of course!).

The next stage in this scenario is easy to guess: taken by surprise by this unexpected onslaught on their traditional positions and roles, librarians bent, groaned and finally managed to regroup. The result has been the formation of consortia. The question is: How effective are they?

A defensive move at best, consortia have led to complex and somewhat contradictory results. They have allowed for some degree of sharing, particularly of legal experience, and, by virtue of increased collective spending, they have managed to reopen a little room for negotiations on the price of journals. But when all this is said, the most positive side benefit of consortia is that it has stimulated dialog and collaboration between libraries. Traditionally, these institutions have acted in a rather autarkic manner, better suited to the competing demeanor of collection fortresses than to the behavior of nodes working within a "space of knowledge flows".<sup>32</sup> Suddenly, with the advent of consortia, libraries had to recognize that collaborations had to go beyond a bit of interlibrary loans, that networks were rapidly becoming the key to understanding the emerging paradigm; they began to examine themselves as open elements within a distributed intelligence system. But in the meanwhile, consortial activities often amounted to dubious battles.

Why dubious battles? To be sure, consortia have gradually learned how to fight better, but with ambiguous results, as the example of the Canadian project, CNSLP,<sup>33</sup> demonstrates. Let us see exactly what was accomplished and how.

Armed with 50 million Canadian dollars, CNSLP designed a strategy based on premises that, retrospectively, look about as smart as could be imagined. If the CNSLP experiment is so interesting, it is also because it shows that with even the best people and the best ideas, site licenses lead to a mixed bag of results.

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<sup>32</sup> On the concept of "space of flows", see Manuel M. Castells' *The Information Age: Economy, Society and Culture*. Vol. 1 *The Rise of the Network Society* (Oxford: Blackwell Publishers, Ltd, 1996), chap. 6.

<sup>33</sup> Canadian National Site Licensing Project. It is a pilot project based on the coalition of 64 institutions supported by money from the Canadian Foundation for Innovation (40%) and by other sources of money, the recipe changing with each province (60%). See <<http://www.uottawa.ca/library/cnslp/>>.



The strategies deployed by CNSLP include the following points:

The Consortium only bargains for full collections. This is meant to create a financial bottleneck effect that contributes to putting pressure on vendors: given the price of a global deal, only a few can be bought. Which ones? Each vendor knows it is an all-or-nothing situation.

The Consortium creates a ranking of vendors that remains confidential. This aims at creating a degree of uncertainty and anxiety on the side of the vendor.

The Consortium lets it be known that it will negotiate with vendors according to the order of the confidential ranking; any unsuccessful negotiation will simply lead to moving down the (confidential) list to the next candidate.<sup>34</sup>

This hard-nosed attitude did pressure vendors: they realized that they had only one pass at getting a share of a sizable sum of money. Significant savings resulted from this approach, as well as capping on price increases.<sup>35</sup> It is interesting to note that Elsevier's "Science Direct" offer exceeded the 50 million dollars at the disposal of the Consortium. As a result, it was left aside. And this remark also leads to a possible downside in the CNSLP approach. If, for whatever reason, a negotiation goes astray, CNSLP's strategy leads to forfeiting access to the whole corresponding collection. It will also be hard to repeat: once a deal has been struck with a publisher, it will be difficult not to renew it, thus reducing the possibility to negotiate a second time from a position of relative strength.

When it started negotiating, CNSLP was in a rare state of preparedness despite very short timelines.<sup>36</sup> Its intense professionalism took a number of publishers by surprise. This, too, helped achieve really

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<sup>34</sup> There are other dimensions in the CNSLP strategy that should not be forgotten, such as: letting publishers present themselves to a procurement call (and treating licenses as a form of procurement), negotiating subscriptions only in local currency, imposing a model contract that comes from the consortium and not from the publisher, etc. However, these points, although crucial, remain tangential to the main thrust of my argument here.

<sup>35</sup> How significant would require comparing the results achieved by CNSLP with those of other consortia. This may be made a bit difficult because of clauses calling for a degree of discretion on results obtained. All consortia should actually reserve up front the right to exchange all information they want, in an open and free manner, with anyone they choose. Accepting the discretion clause effectively brings us back to pre-Barschall days, when comparative figures were not available, or to the days before the Gordon and Breach trials were settled. With discretion clauses, vendors enjoy a panoptic vision of the market, while buyers remain atomized. I will develop this notion of "panoptic vision" a little later.

<sup>36</sup> I would like publicly to commend its executive director, Deb de Bruijn here for the extraordinary level (and intensity) of her work. The negotiating team also did a wonderful job.

interesting results. But, again, what will happen when CNSLP engages, as we all hope, in a second round of negotiations? Publishers can certainly draw the necessary conclusions from this unexpectedly difficult skirmish, and they too know how to regroup. More fundamentally, have consortia—CNSLP or others—changed anything to the counter-revolution of licensing? My answer is that they have not, beyond buying a little time, a couple of years at best.

Consortia, in my opinion, are little more than a transitory defensive adaptation to the counter-revolutionary assault of publishers; it provides few opportunities for counter-attacks. In some circumstance, as in the case of the Canadian consortium, subtle use of (relative) poverty and a resolutely hard-nosed approach that eschewed niceties and cocktail talks accompanied by PowerPoint-ed trivia, has allowed saving some money. This is a lot of fun and it has worked this time; but it bears some of the trappings of a Pyrrhic victory: more like that one and we surely lose the war! We also know that the element of surprise will not be so easily repeated. Publishers will be ready for the next few negotiation rounds.

Is there life beyond licenses? This is an important question. To be credible, a positive answer will ultimately require moving well beyond licenses.

In reacting to consortial strategies, Elsevier is showing the way again. Unable to increase its sale revenues in Canada via the consortium—for I believe this was their primary objective—they are now trying to do it piece by piece, knowing that a number of large research universities simply must access a significant proportion of their holdings. Individual picking of libraries has, therefore, begun. The negotiation roughly takes the following form: Ladies and gentlemen, begins the Elsevier rep, you already have a license with Science Direct that gives you access to some of our journals, say around 300. Your licensing costs hover around \$5-600,000 US.<sup>37</sup> The Elsevier representative then explains how much it would cost to add another 100, 200, 300 titles; and suddenly he drops a bomb: how would you like to have it all, say for \$900,000 US. In other words, Elsevier is saying: if you give us the chance to increase our revenues by 50%, we multiply your access by four. Again, bear in mind that what imports here is the disconnection between the price increase and the increase in the number of titles. Again, these are not exact figures, but many of you here could easily fill in real number by looking at your institutional situation. This is now referred to as the "Big Deal" and Ken Frazier of the University of Wisconsin has strongly warned us against these offers.<sup>38</sup> Let us see why I believe he is fundamentally right.

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<sup>37</sup> I have heard figures of that magnitude here and there, in various corridors. It obviously varies with conditions and size of institutions. I submit this rough approximation without any guarantee, but I do so all the more comfortably that it does not affect the logic of my reasoning.

<sup>38</sup> Kenneth Frazier, "The Librarians' Dilemma: Contemplating the Costs of the 'Big Deal'" *D-Lib Magazine* 7.3 (March 2001) <<http://www.dlib.org/dlib/march01/frazier/03frazier.html>>.

Why should Elsevier float this kind of proposal? Clearly, as I have already indicated, to increase revenue. Elsevier, fundamentally, does not care about the number of accesses it sells; it cares mainly about revenues and bottom line. Remember that connecting the nozzle to the full tank does not cost more than picking, selecting and choosing—actually it may even cost a little less to manage. Consequently, the increase in revenue immediately translates into an equivalent increase in profits!

But this is not the only reason that brings Elsevier to offer a “Big Deal”. Imagine that our earlier university library has decided to get all of Science Direct, and suppose Elsevier, after three years, says: “Well, now, you know, with currency fluctuations”—they love currency fluctuations at Elsevier—“production costs increases and other factors, we can still offer you the ‘Big Deal’, but it is going to cost you \$1.5 million US. Of course, if you do not want to pay as much as this, we can examine the cost of partial deals such as the one you enjoyed before the ‘Big Deal’, i.e., 300 titles.”

Obviously, a librarian confronted by this kind of bargain has but little choice. Moving back to 300 titles would create an uproar among research scientists; furthermore, even if the cost increase requires cutting other titles, it can be justified by the fact that 900 Elsevier titles may be preserved by sacrificing say 200 titles taken from other publishers, that are not part of another “Big Deal”. The pricing system essentially acts like a ratchet device; it can never be rolled back and even keeping it in the same position appears more than problematic.

Let us now examine the latter solution more closely because it brings into sight a rarely mentioned situation—namely the peculiar conditions under which scientific publishers compete with each other. Scientific publishers do compete with each other, for example, in trying to attract the winners of Lotka’s race, the Einsteins of the scientific world to their titles; but they also compete by striving to create better visibility for their journal, the idea being to make them move up the pecking order ladder. Let us see how it works.

If the acquisition of the “Big Deal” forces canceling access to a number of titles from other publishers, this corresponds to direct competition, based on some price/title comparisons. Elsevier can win some of these confrontations, because of the pricing scheme of the “Big Deal”, but there is a cost. Without the need for “Big Deals”, revenues could reach even higher levels. Competition here does manage to keep some prices down.

However, competition among scientific publishers does not stop there; there are more subtle and indirect consequences to the “Big Deal” tactic. By discounting titles at a price that makes the offer irresistible, Elsevier contributes to creating a scholarly landscape that is distorted compared to the normalized scholarly landscape of the “core journals”. A concrete example will demonstrate this.

OhioLINK has contracted a “Big Deal” with Elsevier and, as a result, Elsevier journals quantitatively dominate the lot; so does the number of articles published in the Elsevier journals. Innocuous in appearance, the consequences of this situation are quite startling: as reported in the September 2000 newsletter from OhioLINK,<sup>39</sup> 68.4% of all articles downloaded from the OhioLINK's Electronic Journal Center came from Elsevier, followed far behind by John Wiley articles (9.2%), Academic Press (8.5%), Kluwer (6.5%) and Springer (4.8%). What is remarkable is that Elsevier, although it controls only about 20% of the core journals, manages to obtain 68.4% of all downloaded articles in Ohio.

How can we account for such an overwhelming use of Elsevier papers compared to other publishers? Could it be quality? Possibly, but quality differences in core journals could never claim to account for such a huge distortion level. On the other hand, if the number of available Elsevier articles is large compared to the articles from other publishers—and this is actually the case thanks to the combined effect of Elsevier possessing about 1,200 titles and its peddling them *en masse* through “Big Deals”—then it stands to reason that a Ohio-based scientist has more chances of hitting Elsevier articles than articles from any other publisher.<sup>40</sup> In other words, there is some direct (although not necessarily linear) relationship between the proportion of articles a publisher manages to inject in a given database such as OhioLINK's and the usage these articles enjoy in the database.<sup>41</sup>

“So what?” might you ask? I believe the implications are profound and, as claimed earlier, they have much to do with the competition between publishers. If, through the manipulation of the number of articles in a given database, a publisher manages to affect the rate of use of its own articles, it also stands to reason that this publisher is able to affect the citation rate of its articles. If this situation leads just one Ohio scientist to cite one more Elsevier article in one of his/her articles, this affects the impact factor of the journal where the article appears. Of course, with one citation, the effect is too small to be detected, but imagine now that event repeated an untold number of times in Ohio and across other similarly structured consortia. It will lead to increasing the number of citations to Elsevier articles. As a result, the impact factor of Elsevier journals should begin to go up.<sup>42</sup> As a consequence, these

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<sup>39</sup> See <<http://www.OhioLINK.edu/about/update/>>. These documents are in PDF format.

<sup>40</sup> The OhioLINK booklet “OhioLINK Snapshot 2000” reports on the number of articles available from each publisher. According to this 2000 data, Elsevier Science led the way with nearly 58% of the articles. Academic Press, which merged with Elsevier in 2001, represented another 8% of the articles. This means that in 2001 Elsevier represents somewhat over two thirds of the articles available via OhioLINK.

<sup>41</sup> This point would also deserve a good research project....

<sup>42</sup> See the Amin and Mabe article cited in note five. This may explain in part why Elsevier appears so interested in impact factors.

journals begin to attract more authors; but then, with a greater choice of authors, the quality should go up. In effect, a kind of quality pump has been successfully primed and it begins to propel the journal up the pecking order ladder among the core journals. In all probability, the same tactic can also help a new journal reach core level and thus graduate to the charmed circle of the SCI list.

If this reasoning holds any water, it means that a publisher like Elsevier knows how to use consortia to improve the direct and indirect marketing of its publications. Of course, a scientific publisher's role is to give the greatest possible visibility to its authors and Elsevier might reasonably argue that it is just doing its basic job; it might even add that this is part of the "added value" publishers are supposed to bring to their publications. In other words, Elsevier has designed a dynamic strategy that directly aims at its competing publishers, probably with the idea of weakening them enough so as to swallow them eventually, and thus reinforce its grip on the core set of scientific knowledge. The European Union has quietly blocked the merging of Reed-Elsevier with Kluwer that had been announced in October 1997, but the Anglo-Dutch giant has recently acquired Academic Press. Just adding the year 2000 usage percentage of Academic Press to Elsevier would bring the OhioLINK statistics to 76.9%. Moreover, if Academic Press is thrown into some "Big Deal" basket, one may expect the numbers to rise even higher. The oligopoly presently controlling the core journals could easily turn into a very strong monopoly.

Ken Frazier's criticism of the "Big Deal" is, therefore, valid and, as I have tried to show, it can be understood to include marketing and competing strategies against other publishers. Due to Elsevier's size, the "Big Deal" strategy is particularly effective. In other words, and at one blow, Elsevier locks the libraries in (the ratchet effect described earlier) and begins to undermine the impact factors of publications coming from other publishers.

From all that precedes, it is legitimate to ask whether libraries still control what they offer to their patrons. Are they not being temporarily assuaged by a kind of compulsory buffet approach to knowledge whose highly visible richness disguises the distorted vision of the scientific landscape it provides? In other words, are not "Big Deals" the cause of informational astigmatism, so to speak?

As we have seen, CNSLP, as a consortium, has succeeded in negotiating with various vendors only in hardening the "Big Deal" approach. The only advantage, really, of this approach, beside some, alas probably temporary, price pressures on vendors, is that, in this case, it has shut out Elsevier from the Canadian Consortium. As a result, the eroding of other publications' impact factors cannot occur so simply on a national scale in Canada; in fact the reverse could happen, but this is nothing more than a different form of cognitive astigmatism. And through individual "Big Deals", Elsevier will try to compensate for that loss, and it may well succeed.

The strength of large commercial publishers does not stop there. We shall meet them again in their attempts to counteract newer forms of publishing that threaten their monopoly. However, even before we do this, I would like to introduce the notion of panoptic vision. Since Michel Foucault's famous book on prisons,<sup>43</sup> we pay more attention to Bentham's panoptic architectural structure which was incorporated into the design of prisons in the early 19<sup>th</sup> century. Not only is such a structure endowed with a strong surveillance capacity, but, in turn, it induces new kinds of knowledge. It is as if Bacon's aphorism had been translated into stone: knowledge can be power exactly as power can generate knowledge.

That publishers own a panoptic site with regard to site licensing negotiations is obvious. Through dozens of negotiations and almost as many deals with various libraries and consortia, publishers acquire a rich stock of experience, elements of comparisons and so on that amount to occupying the center of a powerful panoptic site. And they make good use of it. But they also own another panoptic site that appears even more important—that provided by usage statistics. Scientometrics specialists would die to lay their hands on such figures; governmental planners also. With usage statistics you move faster and stand closer to the realities of research than with citations. Usage statistics can be elaborated into interesting science indicators of this or that, for example how well a research project is proceeding on a line that might prepare the designing of new drugs or new materials. The strategic possibilities of such knowledge are simply immense. They resemble the marketing possibilities emerging from the study of consumer habits and profiles.

It is somewhat disquieting to note that such powerful tools are being monopolized by private interests and it is also disquieting to imagine that the same private interests can monitor, measure, perhaps predict. They can probably influence investment strategies or national science policies. In short they could develop a secondary market of meta-science studies that would bear great analogies with intelligence gathering. Is that the role of publishers?

Compared to the widely advertised "Echelon"<sup>44</sup> project of global communication surveillance, I find this second panoptic site much more threatening: it deals with cutting edge, fundamental knowledge and publishers are presently unaccountable for it. It is, after all, their database. In short, the movement toward the privatization of databanks of fundamental science that has coincided with the digitization of commercial scientific journals is opening untold new opportunities for the Elseviers of the world. It would be surprising to discover that Elsevier has not thought about such perspectives, and, as we shall see in the

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<sup>43</sup> Michel Foucault, *Discipline and Punish: The Birth of the Prison*, Tr. by Alan Sheridan (New York: Pantheon Books, 1977). French original, 1975.

<sup>44</sup> See, for example, <<http://www.echelonwatch.org/>>, a site organized by the ACLU in the U.S.

discussion of that publisher's ChemWeb Chemical Preprint Server (CPS) that will be found later, a few details point exactly in that direction.

In any case, the possibility should be discussed in the open. Who better than the librarians can bring this to light?

## 11 Open Archives and Other Subversive Undertakings

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Scientists, as we have seen earlier, read in two different ways: they access archived, refereed articles to check on earlier results at footnoting time in order to give back to Caesar what rightly belongs to him; on the research front, they seek information in any way possible, preprints, recent personal communications, etc., and these documents circulate in a variety of ways, although nowadays e-mail tends to supplant most other modes of transmission. Actually, the distinction between the two modes of reading may have been accentuated by the digital context; in any case, the ease with which digitized documents can be copied and transmitted has made them an invaluable tool of contemporary research.

Not surprisingly, with preprints exchanged at increasing rates, more efficient means had to be developed than the traditional, laboratory-based, preprint and off-print collections. Paul Ginsparg was apparently the first to set up a server to that end in 1991. He then invited his colleagues in high-energy physics to place their papers in it. With the new server, physicists could check many new developments in their field with just one stop. With the growth of the server and its use by an ever-larger proportion of physicists, the one-stop shopping began to approximate an exhaustive search of high-energy physics. The solution rapidly appeared very promising.

Had Ginsparg wanted to look for inspiration, he would now have had to look far afield to find it. Back in 1969-70, ARPAnet had discretely initiated a publishing reform of its own when it inaugurated a series of papers that were simply (and quite respectfully) called "RFCs" (request for comments). The scheme was simple. Anyone could submit papers to a network of servers—the notion of one single server is not even needed here, as most everything else at the heart of the Internet distributive philosophy—so long as he/she adhered to a few basic presentation rules.

Two points must be noted with regard to the Internet RFCs.

1. The act of publishing is totally meshed with the desire to communicate, and vice versa, thus putting back in sync the two functions which print had gradually pried apart.
2. The fate of the published idea depends strictly on the way it is being received. If the proposal or idea sinks, it quickly disappears from



people's view and memory; if it floats, it generally leads to more discussions, refinements and extensions; sometimes a further RFC appears. Eventually, some ideas end up taking a life of their own. Again, there is nothing really new in all of this, except for the fact that new communication channels had been invented in the margins of the ARPAnet project.

Analogies to the Ginsparg server can be found further afield and even reach areas of work that are far from obvious. Consider, for example, the free source code movement harboring developments such as GNU<sup>45</sup> and Linux. It does nothing more than adopt the RFC or the pre-print system. Various people who do not even have to know each others' existence write code and place it in a public depository. The idea is to have the result checked by others. There, it is either forgotten or improved by someone else. And so on. The whole creative and productive dynamics rests on the search for public visibility, fame and authority, exactly as in science—recognition or symbolic capital, in short, rather than hard cash, at least as a first phase—and by the knowledge that the public scrutiny of results is the best guarantee of quality and fast evolution. Linus Torvalds, the “father” of the Linux kernel, has coined a nice aphorism to explain this situation: “With enough eyeballs, all bugs are shallow”. Although improved and rationalized by the use of devices such as the Concurrent Version System (CVS), these forms of collaboration do little more than extend Oldenburg's basic idea of a public registry of ideas so as to protect intellectual property and simultaneously ensure the rapid evolution of scientific knowledge.

The Republic of Letters or of Science, the Internet RFCs and the free source code movement all display a common principle, generally known as a distributed intelligence. Henry Oldenburg in the Royal Society of London, Jon Postel, Vinton Cerf and their colleagues inside the ARPAnet, and later, Internet projects, Paul Ginsparg and the high-energy physics community, and Linus Torvalds (as well as Richard Stallman) in the GNU/Linux world, have all tried to build better human intellectual collaboration on the basis of distributed intelligence principles.

The Los Alamos archives (recently transferred to Cornell) did not go unheeded. Other, similar archives began to be contemplated and implemented in a variety of fields and along various organizational schemes (i.e., disciplinary, institutional, etc.). In short, the movement began to grow and expand until the need for some kind of federative action became obvious. This call found its response in Santa Fe, in October 1999, when the foundation for what was at first called a “Universal Preprint Service” (UPS—the pun is obvious) was laid and the Santa Fe Conventions were outlined.<sup>46</sup> The spirit that presided over that

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<sup>45</sup> GNU is a recursive acronym. It stands for “GNU Not Unix”. Those with a mathematical bent or a pronounced sense of the absurd will fully appreciate the joke.

<sup>46</sup>A useful summary of the October meeting can be found at the following URL: <<http://iubio.bio.indiana.edu/R63470-81653-/news/bionet/journals/note/9910.newsm>>.

meeting is reminiscent of the spirit that guided Vint Cerf and Bob Kahn when they drafted a first version of what was to become TCP/IP at the end of 1973: optimize the ratio results/technical constraints while striving to keep the latter (constraints) as low as is possible. This is nothing more than distributed intelligence again, and in this case, it revealed the need for some elements of interoperability that would allow any registered archive to be easily harvested through a common search instrument.

In this kind of vision, the devil is in the details; wisely, I believe, I will leave the ugly creature on the doorstep of our discussion, but not before remarking that interoperability was indeed achieved; it even included some consciousness of the librarians' concerns (and expertise) by including elements compatible with OCLC's well-known Dublin Core metadata approach. The final result has been the Open Archives Initiative,<sup>47</sup> financially supported by U.S. institutions (Digital Library Federation, Coalition for Networked Information, and National Science Foundation), but which would deserve getting some additional aid from Canada, Europe, and Japan as well. No need to recount here what its history, scope and objectives are, as these are well explained in a fine (and freely available) paper penned by Carl Lagoze and Herbert van de Sompel, both then from the Digital Library Research Group at Cornell University,<sup>48</sup> I will just underscore that the philosophy of interoperability adopted here, unlike that embedded in by Z39.50 for example, aims at simplicity. The point is to offer something easy to implement and easy to deploy, similar in this regard to the initial design philosophy of HTML.

Let us look more closely at Ginsparg's project. Because it was so sharply focused on the simple, fast, efficient, communication of scientific papers, the physics server inadvertently managed to bring several important, yet unintended, points to light:

1. Journals are rather inadequate when it comes to communicating quickly and efficiently; they are much better at validating and evaluating the relative worth of scientific authors. They are adequate to preserve the memory of science over the long haul (several centuries in the best of circumstances).
2. Ginsparg's server also showed that the swift communication of new papers, the validation of ideas and the long-time archiving of articles did not need to rest on one single tool, object, or process. Our civilizational love affair with print had gradually erected this

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See also <[http://www.openarchives.org/meetings/SantaFe1999/sfc\\_entry.htm](http://www.openarchives.org/meetings/SantaFe1999/sfc_entry.htm)>.

<sup>47</sup> <<http://www.openarchives.org/>>

<sup>48</sup> C. Lagoze & H. Van de Sompel, "The Open Archives Initiative: Building a Low-Barrier Interoperability Framework," <<http://www.openarchives.org/documents/oai.pdf>>. Note in passing that the Open Archives Initiative dovetails very nicely with Provost Shulenburg's "NEAR" proposal fielded in October 1998 at the ARL membership meeting. See <<http://www.arl.org/arl/proceedings/133/shulenburg.html>>.

conflating of functions into some kind of dogma. Digitization, in effect, was helping to unpack the various functions of print (and shatter the dogma).

3. As science goes through various phases—research investigation, giving credit, validation—it relies on different documentary units. Short- and long-term scientific memory really relies on articles, authors' names, and keywords; journals are of secondary importance in this regard. However, in the validation phase, the journal counts more than anything else because evaluation procedures rely very strongly on them, especially since the introduction of the impact factors. It also showed that if journals acted as a handy tool of evaluation, in no way could they ever hope to be equated with it. Journals can help to evaluate; they are not evaluation *per se*.

In short, the advent of Ginsparg's pre-print server has demonstrated that the act of publishing could easily and safely be dissociated from evaluation and from long-duration archiving. Each question could be examined in isolation, on its own—a perspective that had remained long blocked by the towering presence of print.

The distinction between article and journal, and the relationship each bore to a different phase of the life of science was clearly underscored when several physicists found themselves pressured by publishers to remove their articles from Ginsparg's server after they had been published in a traditional, paper journal. Physicists resisted and insisted on keeping their papers in the server. Why? Simply because, for these scientists, print publication was related to career management, while the digitized version placed on the public server dealt with the management of intellectual quests. That the two processes are intertwined is obvious; but it is just as obvious that intertwining does not mean equating. With Ginsparg's server, all the emphasis was placed on the article, so that the journal's hold on evaluation appeared less obvious and secure than before.

Ginsparg's project also contributed to driving a second point home: in the print world, the large up-front investment in time, equipment, skills, and money means that a severe selection process has to precede the printing process.<sup>49</sup> In fact, selection and printing have become so closely tied together that it is sometimes difficult to dissociate one from the other. To print necessarily means to select and, therefore, all that is printed at least connotes and often denotes some singling out process. The converse is false, of course, but with the long-staying power of print technology, it looked as if this simple point had definitely been forgotten.

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<sup>49</sup> The real problem of piracy is that only successful works are pirated; as a result, pirates avoid all the costly attempts to field a variety of works before finally hitting upon a profitable combination.

In the digital world, the selection process works differently. It allows publishing (the equivalent of printing and disseminating here) at much lower prices and with much greater flexibility to add, remove, and correct, should any error occur. With the lowering of economic and skill barriers, a form of publishing empowerment takes place and, as a result, the need to select is no longer significantly tied to technical and economic constraints. In fact, in the digital world, selection through usage becomes the dominant question to be addressed and solved. In other words, the peer review process tends to extend to the whole community almost immediately.<sup>50</sup>

The separation of the selection process from technical and economic constraints allows one to look at it in a new light and to recast it in ways that might serve the scientists' need better than the older forms of selection/evaluation that emerged as a tacit compromise between various concurring forms of constraints in the print world. **In short, with the digital world, the evaluation process stands ready to be reinvented in a clear, rational way by the relevant research communities themselves.** But this is nothing more than saying that the evaluation process will have to be torn out of the publishers' grip and we must not forget that the concrete form of this grip is made of gatekeepers, i.e., colleagues! Ways to tear this detrimental alliance apart while relying on the real expertise of these gatekeepers must therefore be sought. It is not an easy problem to solve, but it should clearly be on the agenda of learned societies, university administrators, and, of course, librarians.

The commercial response to Ginsparg's initiative was not long in coming. In particular, commercial variations on the archive movement appeared—for example, BioMedCentral,<sup>51</sup> HighWire Press, Bepress, and BioOne. It is worthwhile to try and classify these trends.

HighWire, Bepress, and BioOne are really attempts to create what might be called the equivalent of an "electronic printer", with various degrees of task delegation to authors and editors. These organizations

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<sup>50</sup> Of course, in the print world, the peer review process of selection amounts to a *de facto* delegation of responsibility to editors, and they, in turn, further delegate the evaluation task to reviewers. However, in the last analysis, the whole of the community is ultimately involved in the evaluation process because scientific work consists largely in confirming, refuting, or extending published work. Karl Popper's philosophy says much the same thing in much more rigorous terms. For its part, digital publishing allows dispensing with the prior delegation of authority. In other words, it allows disintermediating the powerful layer of gatekeepers. Commercial publishers maintain a powerful hold on the scientific community by influencing the nomination of who will be a gatekeeper, at least at the beginning of a new journal. They are not the only ones involved, but they clearly hold an important, albeit veiled, position in that process. Are they really suited for this role, one may well ask? Should not gatekeepers be controlled exclusively by the scientific communities themselves?

<sup>51</sup> See <<http://www.biomedcentral.com/>>. BioMed Central distinguishes itself carefully from BioMed Net, an Elsevier project. It also positions itself carefully with regard to both PubMed and PubMed Central. See also <<http://highwire.stanford.edu/>>, <<http://www.bepress.com/>>, and <<http://www.bioone.org/>>.

help journals, particularly from small scientific societies, as well as noncommercial journals and similar publications; in particular, they try to ease the transition into the digital age and, in this manner, to improve their impact factor by taking advantage of the best resources of networking. By working with several journals at once, they try to recreate conditions suitable to the emergence of economies of scale similar to those enjoyed by many big commercial publishers. These electronic publishers stand somewhere between a commercial outfit and a cooperative. In a certain way, the SPARC venture resembles these projects; like them, it continues to place the main emphasis on journal titles rather than on articles. In fact, BioOne is a SPARC partner and ARL's President, Shirley Baker, is on the board of directors of BioOne.

BioMed Central is somewhat different; and it is a particularly interesting example, if only because of the way in which it emerged. It too is apparently moving in the direction of creating new journals; however, these "journals" really act as specialty or disciplinary boxes, while the branding through peer review is really attached to the whole BioMed operation.

BioMed Central was created in response to the partial failure of the NIH-led PubMed Central project: under the impulsion of Nobel prize winner Harold Varmus, and then NIH director, PubMed Central sought to encourage journals to free their content as quickly as possible, possibly from day one. There again, the journal level was targeted by PubMed Central, but it behaved a little too idealistically: journals, especially commercial journals, were not ready to give the store away, or even parts of it, and they strongly criticized the PubMed Central venture; in the end, the NIH-based proposal was left with very few concrete results, as could have probably been expected from the beginning.

By contrast, BioMed Central, a part of the Current Science Group,<sup>52</sup> locates itself squarely as a commercial publisher; at the same time, it sees itself as a complement to PubMed Central. It invites scientists to submit articles which are immediately peer reviewed; once accepted, they are published in both PubMed Central and BioMed Central. Most of PubMed Central "successes" actually come from BioMed Central!

The reasons given by BioMed Central to induce scientists to publish with them deserve some scrutiny:

1. The author(s) keep their copyright;
2. High visibility is promised because, so it is argued, it is enhanced by free, immediate access to all; moreover, all BioMed articles are indexed in PubMed and they are put in easily citable form (for SCI's purposes); and

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<sup>52</sup> See <<http://current-science-group.com/>>.

3. Long-term archiving is ensured by PubMed Central, a credible reference since it is covered by the National Library of Medicine.

BioMed Central's main weakness lies with its business plan. It rests on a combination of possible page charges,<sup>53</sup> publicity and relatively fuzzy intimations of innovative services. These include cross-linking of articles and optimized search engines overlaying the free open archive. Clearly, the new archive will have to be supported for a while before it can hope to be self-sustaining.

Beyond the financial details, this project is fundamentally interesting because it tries to rework the relationship between scientific and commercial objectives so as to optimize both sides of the equation, unlike present commercial models. Let us remember that scientific authors generally want to see the results of their work disseminated as widely as possible, but their desire is absurdly kept in check by the very high prices of the publications. The contradiction is patent; it is indeed a Faustian bargain, to use Stevan Harnad's famous description of the situation. In effect, BioMed Central tries to achieve a kind of Hegelian "Aufhebung", i.e., a synthesis that is greater than the sum of its parts, in order to move beyond the contradictory tensions between the scientists' and the publishers' agendas. Although interesting, this ambition involves a very tall order indeed.

Interesting experiments as they may be, neither BioOne nor BioMed Central match the importance of another development that created some expressions of surprise when it emerged in August 2000: I am referring to the open article archive in chemistry launched by Reed-Elsevier under the name of "Chemical Preprint Server" (CPS).<sup>54</sup> Why Elsevier got involved in an open archive is far from obvious; at first sight, it seems to run contrary to their basic business agenda. Yet a number of reasons have motivated this decision and it may not be too presumptuous to try and sniff them out. If I am right, it clearly shows that Elsevier should never be underestimated and that its corporate IQ is impressive. Here are three possible reasons for this move, the last one being the most important in my opinion:

1. The open archive movement began intriguing Elsevier a while back; for a company—witness the "embrace and extend" philosophy of Microsoft in this regard—there is no better way to understand a potentially threatening movement than to be part of it, the better to manipulate or deflect it in harmless directions. Creating an open archive of its own, one that may even soon become interoperable with OAI, is a clever move on the part of

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<sup>53</sup> The debate about this point is made public on the BioMed Central site. See <<http://www.biomedcentral.com/editorial/charges.asp>>.

<sup>54</sup> To place this archive in perspective, it presently (mid-August 2001) sports, after nearly one year in operation, 282 articles. Obviously, chemists, although far more numerous than physicists, react cautiously to the publisher's entreaties.

Elsevier.<sup>55</sup>

2. Elsevier is not that strong a publisher in chemistry. Opening an archive in that enormous and somewhat amorphous discipline is a good way to test waters and explore how best to challenge the American Chemical Society's (ACS) dominant position in that field. We have already seen moves made by Elsevier—e.g. The “Big Deal”—that aim directly at competitors. In this case, it looks as if Elsevier aims at competing directly against ACS, all the more so that the latter has allowed some of its publications to appear under the SPARC label. The battle, should it develop, will be interesting to follow!
3. Most importantly, I believe Elsevier is testing ways to reconstruct a firm grip on the evaluation process of science in the digital context; this may be in response to the BioMed Central project that we encountered earlier.

Elements of Elsevier's ChemWeb site—home of the CPS—I believe support the last point.<sup>56</sup> It sports a prestigious advisory board, including, somewhat ironically, a member of NIH who may have heard about Dr. Varmus and his involvement in PubMed Central and BioMedCentral.<sup>57</sup> A “Quick Find” feature is also available and this feature also harbors telling details.

“Quick Find” allows searching according to the following categories:

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<sup>55</sup> The influence of ArXiv (OAI) is openly admitted by CPS: “The CPS was developed by closely following the Los Alamos archives <<http://arxiv.org/>>, which cover physics and related disciplines” <<http://www.chemweb.com/docs/cps/CPSinfo.shtml>>. The issue of interoperability must appear tricky to Elsevier as it might make CPS dependent on standard-related decisions taken outside the firm; on the other hand, not being interoperable may also condemn CPS to becoming a ghetto. I suspect the hesitations of Elsevier in this regard correspond to the need to see how things evolve and to play for time. And, of course, ACS must be thinking about all of this too.... See the following point.

<sup>56</sup> See <<http://www.chemweb.com/>>. To examine the structure of the preprint server, one has to become a member, a simple enough task since I registered without any difficulty despite my belonging to a comparative literature department.... However, this registration process probably creates enough of a filter to diminish the risk of critical evaluation by people who are not interested in chemistry *per se*, but very much interested in the latest developments in the Elsevier world. Keeping a close, critical watch over Elsevier is always worthwhile. The preprint server's URL is <<http://preprint.chemweb.com/CPS/>>. One can click on it from within the ChemWeb site. Cookies have to be allowed, raising further privacy issues.

<sup>57</sup> See <<http://www.chemweb.com/docs/cps/advisory.shtml>>. Institutions represented include Oxford, Cambridge, NIH, IUPAC, ETH Zurich. There is also a representative from ... Procter and Gamble, Dr. Edlyn S. Simmons, whose activities are described as follows: “[Dr. E. Simmons] ... serves as Section Manager, Patent Information in Business Intelligence Services at The Procter & Gamble Company in Cincinnati, Ohio, USA.” No further comment needed!

most viewed, most discussed,<sup>58</sup> highest ranked, and most recent. Some of these categories are innocuous enough: "most recent", for example; even "most viewed" appears objective, although we do not know whether "viewed" refers to hits, readings on screen, or actual downloads. On the other hand, "most discussed" is murkier: the basis for measuring this amount of discussion remains unclear: Number of characters? Number of people involved in the discussion? A combination of both?

Most problematic, of course, and also most crucial is the "highest ranked" category. We do not know on what this evaluation or judgment rests, but it clearly refers to evaluation methods and the right to evaluate. Perhaps Elsevier, if only to cover all possible fronts, is thinking about the implications of a future where archives would replace journals. How do you carry business in such a context?

Elsevier knows that, with scholarly publishing, it is involved in the evaluation of scientists' performance, and that their business plan rests in some fundamental way upon controlling large sections of this activity. An anecdotal event I was privileged to witness at the NDLT<sup>59</sup> meeting at CalTech last spring made me suddenly sit up straight and listen. I believe it supports my thesis. A panel involving several publisher representatives was discussing various issues relating to placing online dissertations that include one or several published papers. The representative from Elsevier amiably underscored the point that his company would not necessarily object to this practice so long as it was clear that the article was part of a thesis and so long as the published source was also mentioned, including, of course, the publisher. Someone then asked the same question about academics that are beginning to archive their own papers in their own website, and keeping them there even after their being published in a journal. Again, to my great surprise, the answer appeared measured and tolerant: "not to worry," seemed to say the gentle rep, so long as, again, it is clear that this is a kind of extended curriculum vitae and the sources, again, are clearly indicated. What was up?

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<sup>58</sup> Again, note the innovative characteristics of Elsevier. They transform their archive into a discussion forum to attract more readers. Indeed, if a hot paper appears in their archives, and it generates a lot of discussion, most chemists concerned will have to register and become familiar with this archive in order not to miss potentially important remarks.

Libraries have not yet learned to use their automated system in this fashion, for example, by allowing faculty and students to post comments on the books they read. Also, in keeping with Amazon's ideas, libraries could present patrons with statements such as: people who have borrowed the book you request have also borrowed the following volumes. All this by way of stating that the digital world repositions documents, their "keepers" and readers in radical ways. In other words, digitization can also re-intermediate, but librarians have not yet exploited these possibilities, so far as I can see.

<sup>59</sup> Networked Digital Library for Theses and Dissertations. No one will sufficiently sing the praise of Ed Fox and the good people at Virginia Tech (John Eaton and Gail McMillan in particular) for all the work they have done to put theses online, free them from commercial encroachments, federate institutions that tend to behave like a herd of cats, and, more recently, for having worked at the harmonization between this effort and OAI. In fact, Ed Fox is closely involved in the OAI effort.



The answer might have been heard if a third question had been asked: what about self-archiving that uses description formats and metadata standards that allow these articles to be harvested across the Web by OAI search engines?<sup>60</sup> How about a collaborative, distributed, approach to citation linking?<sup>61</sup>

To imagine what the reaction of the Elsevier representative to the third question might have been, another recent piece of news may be useful: the Software & Information Industry Association (SIIA) has made representations to the U.S. Government to the effect that the Department of Energy, through its PubSCIENCE service:

...“enters into commerce,” and

provides access to a database of bibliographic information that duplicates and competes with databases made available by private sector publishers.<sup>62</sup>

This activity is described by the SIIA document as being of “great concern.”

Named among the commercial publishers allegedly hurt by the PubScience initiative are BIOSIS, Chemical Abstracts Services, Cambridge Scientific Abstracts, Reed Elsevier (!), ISI, and the Institution of Electrical Engineers. Amusingly, PubScience counts several important commercial publishers on its side: Kluwer, Springer, Taylor & Francis, etc. In short, the PubScience initiative appears to be dividing the publishers, according to an interesting fault line that ought to be explored further—namely, that between publishers involved purely in publishing and publishers also involved in the bibliographic indexing kind of activities that contribute to increasing journals’ visibility and pushing them into the core group. The latter publishers have felt sufficiently alarmed by this governmental tool to start lobbying Congress, with ambiguous success so far: the House of Representatives removed all budgetary provisions for this particular activity, but the Senate restored them. The future will tell how the two bills will be reconciled; and at whose expense....

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<sup>60</sup> The best way to follow closely and accurately the self-archiving thesis, its tenets, and its objectives is to consult Stevan Harnad’s personal site at <<http://www.cogsci.soton.ac.uk/~harnad/>>, in particular the following papers: “The Author/Institution Self-Archiving Initiative to Free the Refereed Research Literature Online” <<http://www.cogsci.soton.ac.uk/~harnad/Tp/selfarch.htm>>, and “The Self-Archiving Initiative: Freeing the Refereed Research Literature Online” <<http://www.cogsci.soton.ac.uk/~harnad/Tp/nature4.htm>>.

<sup>61</sup> See <<http://opcit.eprints.org/>> for details. OpCit presents itself as a potential service provider to OAI.

<sup>62</sup> This is quoted from a one-page paper sent to me by Mr. David LeDuc of SIIA. Mr. LeDuc may be contacted at <[dleduc@siia.net](mailto:dleduc@siia.net)>.

Whatever may be the outcome of the political battle that is heating up in the United States, it is easy to imagine how a system of open archives, with unified harvesting tools and citation linkages constructed in a distributed manner, can threaten vast commercial interests. If we imagine that a significant fraction of scientific knowledge should circulate through open archives structured in the OAI spirit, it is easy to see that tools to evaluate all sorts of dimensions of scientific life could also be designed and tested. These tools might be designed as public good, by a combination of specialists in scientometrics and bibliometrics—an ideal outcome in my opinion. This would amount to creating an open panoptic space—a marvelous project for librarians. But even granting that it might be done, at least in part, by private firms, it would still lead the publishers to have to compose with a new player as they did once with ISI. Of course, we can also imagine that some of the main players will try either to destroy or control what they do not already own, but, if suitably forewarned, scientific and library communities backed by lucid administrators do pack a certain amount of firepower. Unlike consortial battles, fraught as they are with many ambiguities, these are not dubious battles.

Publishers must be aware by now that the most crucial form of competition they are bound to face in the future will be found on the evaluation front. Through a clever alliance elaborated with the scientific gatekeepers, commercial publishers have become partners in the only evaluation process in town so far. With the advent of the digital world, as this extremely lucrative combination is being threatened, it is on this battlefield that the future of scholarly publishing is probably going to depend most. And librarians would do well to follow and even intervene whenever possible, if only to bring their particular skills to bear on data that they are familiar with. At the same time, librarians may be able to create a new channel of discussions between scientists and university administrations by pointing out that good evaluation tools not only make better research institutions, but also contribute to lowering the cost of scientific communication.

In passing, it is interesting to note that Ginsparg knew well what information could emerge from the use statistics of his server, but he refused to release them for ethical reasons and political prudence. If evaluation were ever to rely on his archives, it had better emerge as a conscious, collective move stemming from the whole community of scientists, and not from the initiative of a single individual. Now, time has come to build the evaluation tools on its own two feet, without meddling constraints coming from print-related concerns. Libraries can help.

Before proceeding with the rest of this presentation, it is important to state that the case of the social sciences and the humanities (SSH), has been left aside for a variety of reasons. SSH journals differ markedly from natural science journals in the way in which they position themselves in a field. They often tend to incarnate a theoretical position or even a particular group, rather than a segment of knowledge. SSH disciplines

may harbor several paradigms or orientations, while natural sciences, as a rule, do not tolerate such cognitive fragmentation. National or linguistic differences that go well beyond forms of specialization may also appear. For example, various French philosophers, such as Foucault and Derrida, reappear in departments of comparative literature in North America. The fragmentation of SSH makes the notion of “core” journals far fuzzier, all the more so that linguistic babelization continues to play an active role in those disciplines. In SSH, accessing world notoriety is far less connected with the notion of universal authority than is the case in the natural sciences. Furthermore, the degree of disconnection varies from discipline to discipline. Economics and linguistics probably tend to behave more like a natural science than philosophy, anthropology, or sociology.

For these reasons, SSH publishing will not be treated here. It will not be treated also because I believe that much more work needs to be done on this particular kind of publishing to understand how it works. In some ways, it resembles natural science; in others it does not. And the best proof that no one really knows how the political economy of SSH publishing works is that no serious serial pricing crisis has developed here. I believe this is due to the fact that commercial publishers, despite the vast intellectual resources at their disposal, have not yet figured out the way profitably to manipulate the SSH market, as they have done in the case of the natural sciences. But this is only a question of time.



## 12 A Conclusion in the Form of a Modest Proposal That is Anything but Swiftian in Spirit

Oldenburg's shadow stretches far indeed; from the original intent to simplify the management of scientific intellectual property to the subsequent possibility of evaluating scientists' performance, a common thread runs continuously, linking us back to our London-based ancestors. Much of the power structure of science rests on a double concern: public registry and pecking order. Not surprisingly, commercial interlopers have done their homework and have studied the basic publishing motives of the scientific tribes; they have done so in terms that were anything but romantic.

Oldenburg's vision nicely dovetailed with the notion of scientific excellence, but the latter gradually evolved to integrate elitist elements as well. More recently, through mechanisms adumbrated earlier, in particular the unification and materialization of the notion of "core journals", the intellectual excellence/elitism of science began to merge with economic elitism with the result that the exploitation of a well-defined, well-protected, inelastic market began in earnest. Then started the first revolution in the economics of scholarly journals: it is generally better known under the name of "serial pricing crisis", but I believe it deserves a more sanguine title.

The advent of networks, in particular the Internet, and the rise of digitized publications has led to a variety of publishing strategies. Two have been singled out here: first, the recasting of actual sales into licenses has totally subverted the traditional ways in which such business was conducted prior to the rise of a digitized society. This is what I have called the counterrevolution in scholarly publishing. Libraries in particular have seen their being and even their souls threatened by these developments. So far, they have found nothing better than regrouping in the form of consortia to try and resist these new trends better. While consortia are necessary and help buy some time, they also induce a mixed bag of effects, especially when encountering tactics such as the "Big Deal". In particular, it can produce curious forms of "cognitive astigmatism" that can be exploited by some publishers to move ahead of others.

The second major tendency to emerge from the digital era is the

growth of various flavors of open archives. The picture is quite complex, not least of all because commercial publishers have somewhat unexpectedly moved in and are trying to see how best to take advantage of a development initially designed beside or even against them. By embracing a process aiming at freeing the scientific process of communication, and extending it into various directions, such as intelligence gathering, they explore ways of generating new business plans that will carry them profitably through these new, uncharted, territories. Some of these perspectives may send one chill or two through a few spines....

Probably more clearly than ever before, the digital, networked, world reveals that refereed science publishing is closely intertwined with the evaluation of scientists; in fact, it stands closer to evaluation than to communication. As the digital world grows in importance, the evaluation question will loom ever larger. In particular, who controls it is going to be the object of intense struggles.

Librarians must develop strategies favoring the outcomes best corresponding to the deepest values of their profession, in particular the desire to keep the knowledge commons open. From that perspective, it is clear that they must throw all of their weight—and it is considerable—behind the Open Archive Initiative, for the following reasons:

1. It is the only alternative to present publishing that has a chance to develop without the economic penalties associated with present, digital publications peddled in the form of site licenses.
2. It is the only alternative that, although relying on some external, public support, has a chance to withstand the competition of the large publishers over the middle and long term, unlike most learned societies and similar, generally irreproachable, institutions.
3. It is the only way for librarians to recover responsibility over traditional concerns such as classification and conservation. In this manner, they can also get involved with the elaboration of various tools that add values to any collection of scientific articles (what I call “epistemological engineering”).
4. It is the only way to ensure that powerful panoptic effects, either already identified or to be discovered, do not remain the exclusive preserve of private, unaccountable, profit-driven companies, many of them operating offshore.
5. Open archives provide a very good way to develop new and positive relationships with scientists, particularly gatekeepers, and administrators to review in depth the processes of scientists’

evaluation now that these questions can be treated independently of print-related constraints.

Other reasons can undoubtedly be added to this list, but, as it stands, they easily justify supporting the OAI.

What does supporting the OAI mean? Actually, support can work on a variety of levels:

Description and metadata are the province of librarians; they should be more involved with the development of these tools than they presently are. They should also develop and propose, in the RFC spirit of the Internet, new tools and methods that could be gradually integrated along the lines of wisdom encountered in every system of creation based on distributed intelligence. All this should be done while keeping in mind the fact that OAI proceeds from a philosophy of easy, reliable implementation of relatively simple functions. OAI refuses to design any utopian SOE (standard of everything). As they say of the Internet, "implementation precedes standardization." The same appears true of OAI. In short, discuss seriously only that which demonstrably works.

Librarians are already designing knowledge commons; in harmony with their administrators and with the scholars and scientists themselves, they should contribute storage space on servers that would allow faculty to self-archive their publications while respecting the rules of OAI so as to allow easy, efficient harvesting of these data. Such a move would also lead universities and research centers to tackle useful reforms in the evaluation procedures of their research personnel.

Librarians, relying on their specialists in bibliometrics, information science, scientometrics, etc., should mount a concerted effort to build the best tools that could feed into a public panoptic site of science. This effort correlates with point four in the reasons why librarians should support OAI.

Librarians, with administrators and scientists, must elaborate gatekeeping networks whose task is to separate the chaff from the wheat in the open archives. Actually, such networks would no longer guard gates, but would rather award "distinction" on a fraction of the world's publications. Various networks would arrive at various conclusions, of course, but as they all would be visible, comparable, and the result would probably overlap somewhat, the quality of the evaluation would also be in full view. Rather than gatekeepers, these editor-referees are value-adders.

Such value-adder networks should be as international as possible, in order to avoid harmful accusations of provincialism and collusion. They could rely on usage statistics to justify some of their decisions. In this fashion, scientific communities would recover the initiative in creating the evaluation tools without which science cannot function and they would not have to rely on the commercial strategy of big publishers to try and enter the evaluation game<sup>63</sup>.

The *Science Citation Index* remains a wonderful tool despite all the evils it has inadvertently generated, but it remains somewhat arbitrary and limited in scope. In fact, it profits from its very limitations to sell a notion of core journals that actually makes no real sense, except as a pragmatic solution to the question: how can we practically trace citations within a meaningful fraction of the world's scientific publications. However, with a well-designed principle of distributed intelligence, with the help of scientists self-archiving their work, with the help also of selections that do not rest on the prior reputation of a brand, but on the actual quality of each selected work, **librarians hold the key to developing a total, global mapping of science.** The vision, in itself, is dizzying, but it is not new; somewhere, it has lain in the background of Garfield's (and Vannevar Bush's) thoughts and quests; we may just begin to have the tools and the social know-how (distributed intelligence again) to do it all now. Let us do it!

In parallel, librarians, while supporting the SPARC pragmatic efforts to put pressure on commercial publishers and their pricing practices, should also think about ways to synergize SPARC with the Open Archives Initiative. Several facets of SPARC work can help further this strategy, for example:

Pushing for free journals means that they can easily become parts of open archives—the Warwick University journals have already moved in that direction, thus creating a direct connection between SPARC and OAI.

Helping the efforts to free the content of journals—e.g., the NEAR proposal by Provost Shulenburg—after a certain time feeds directly into the open archives movement.

Using the so-called “gray literature”—doctoral theses, research reports, etc.—to gain practical experience with the handling of vast, digitized archives will help familiarize libraries with the building of open archives. Let us remember that open digital archives may well be the closest thing to the open public spaces of

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<sup>63</sup> Obviously, hierarchies would develop there too, and they would actually play the role of journal titles. Further work is needed to develop workable schemes in this regard.



the Gutenberg era, and that defending these open archives may well be the best way for libraries to maintain a continuity of purpose through a technical revolution.

Using the opportunity of negotiating the long-term archiving of commercial journals to free that content as quickly as possible will also enrich the open archives.

Let us also remember that if archives are open, mirrors can be established with little hassle and, as a result, the archive has more chances to survive. Frequent replication and wide distribution, not hardened bank vaults, have long been used by DNA to ensure species stability that can span millions of years. We should never forget that lesson! Vicky Reich's LOCKSS project at Stanford University appears to have taken in the implications of this "dynamic stability" vision for long-term preservation of documents. The model should be discussed, refined and extended if needed by librarians. If openness can be demonstrably and operationally linked with better long-term survival, it will have gained a powerful argument that will be difficult to counter.

A very recent event suggests yet another possibility for action. The Public Library of Science, while gathering around 27,000 signatories, managed to convince only a very few journals to free their content after six months. To provide suitable publishing outlets for researchers who have committed themselves not to publish in non-cooperating journals, the idea of creating new journals has been floated, but that does not solve the evaluation problem these scientists face. Interestingly, BioMed Central understood that and offered its own evaluation services. This move is important because it suggests that what is at stake now is not so much a competition between journals, as a competition between means of evaluation. SPARC, perhaps with BioMed Central, but also with any other suitable partner, could move to establish evaluation committees for the Public Library of Science signatories. In this manner, the dilemma of these scientists would be relieved, and the battlefield would be elegantly moved away from the concern for journal titles about the quality of the evaluation committees. And journal titles would see their power diluted by becoming one among several modes of evaluation. With 27,000 scientists, it should not be very difficult to create evaluation structures that demonstrably are equivalent, or even superior, to the present system of peer judgment as defined by various editorial boards and their associated pools of reviewers.

SPARC will play a crucial role if, a few decades hence, a number of tangible results can be pointed out, such as a fruitful extension of librarians' activities into the area of scholarly publishing, as well as the creation of new, powerful alliances with scientific gatekeepers. If, furthermore, administrators and scientists, with the help of librarians, create new and better tools to evaluate scientific performance, thus leading to improved ways of branding scientists, then commercial

publishers will have been successfully put back to their proper place—namely, a limited, yet positive, input to scholarly publishing. Let us recall that, in the 19<sup>th</sup> century, commercial publishers turned out to play a somewhat useful role in scientific transactions by (unwittingly) introducing what amounted to some checks and balances into the system. Scientists who felt that the publishing delays had become unacceptable, or that some refusals were not entirely dictated by scientific criteria, could try and find redress through the existence of alternative commercial channels.

Finally, we must not forget another problem. The somewhat embarrassing—this is obviously the year's strongest understatement—case of the poor countries has kept on haunting the minds of the more soft-hearted among us. We know that Third World countries have been increasingly shut out from up-to-date scientific and medical information; in a real sense, it is this cognitive apartheid that gives its most terrible meaning to the expression "serial pricing crisis": 70% of humanity is affected by it, at least indirectly. Now, with the generous mediation and financing of WHO and the Soros Foundation, this "slight embarrassment", like a crinkle on a beautiful satin sheet, is being ironed out, at least in the sensitive field of medicine. As a result, big publishers can add this new, unexpected, market to their collection of revenue-generating devices with the satisfaction that, for once, they can even display some degree of benevolence: imagine! If "we" had sold these journals to normal markets, "we" could have made so much more money!

But, there again, imagine how much better a worldwide system of open archives, accompanied by various networks of value-adders would be. The Third World could design blue ribbon value-adder teams that would distinguish work according to criteria involving both quality and relevance to Third World problems (e.g., malaria); the Third World would have access to this information—humanity's heritage, in fact—without having to beg for it, a move that does wonders to anyone's sense of dignity.

In the end, the access to large corpora of texts, laid out in open archives, and cross-linked in various ways, in particular through their citations, will open the way to many different and useful forms of evaluation. It will also help monitoring the crucial growth areas of science while placing this bit of intelligence gathering into the public sphere where, i.e., at everybody's disposal. It would be part of the public infrastructure, so to speak.

In short, librarians must not lose sight of the fact that they must help reconstruct cognitive infrastructures corresponding to the open collections of the Gutenberg era. In so doing, they can also repossess much of the ground lost since the beginning of the licensing disasters, and thus claim anew that, indeed, they are epistemological engineers. They can also begin to claim an active role at the side of research

scientists involved in the game of science as defined earlier in the presentation.

Librarians can (and ought to) help create a navigable, worldwide ocean of knowledge, open to all; and, like Odysseus, they will know how to help negotiate the tricky ebbs and eddies, the vortices and the undertows of chaotic knowledge flows that necessarily accompany the development of a distributed intelligence civilization—a civilization open to all that are good enough (excellence), and not only to those who can afford it (elites).

