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Open Access Archives: from Scientific Plutocracy to the Republic of Science

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I. Introduction

The recent history of science has been characterized not only by a transition from science to "Big Science", to use Derek de Solla Price's terminology, but also by a deep transformation which, in retrospect, threatens to subvert the original values of modern science. Originally, science appeared as an offspring of the "Republic of Letters", and as such, it belonged to a certain élite: the social structure of Europe in the late Renaissance would have made any other arrangement most unlikely. However, inside the scientific playground, elitism gave way to a peer-to-peer mode of behaviour.

Egalitarianism, of course, never prevailed, at least not as an outcome of scientific pursuits; on the contrary, a strongly competitive, and even contentious, atmosphere has always prevailed in science, leading to sharply defined pecking orders of reputation. However, the playing field was as close to even as it ever was, and seventeenth-century science fairly qualified as a meritocratic system of excellence. A continuum obtained, that ranged from mediocrity and insignificance to excellence and even "genius".

Not that things were altogether perfect: some remote participants would have found it a bit more difficult to keep up to date with what was going on in the established centres of Western Europe simply because distance coupled with slow means of transportation imposed important delays in the circulation of research results that were published in the various Transactions, Mémoires, etc. of the main European academies. But, on the whole, scientific research results did reach the peripheral centres in a manner sufficiently timely to allow all would be contestants to participate in the general race of all against all. In particular, no clear economic barrier interposed itself between natural philosophers, as they were then named, and scientific information: the cost of the publications was either covered by the research institution – be it an academy or, after the eighteenth century, a university -, or it was well within the means of genteel practitioners who were personally financing an activity that, more often than not, stood closer to a hobby than to a profession¹.

¹Thomas Shadwell lampooned this situation in his play, **The Virtuoso**.

In more recent times, and for reasons that I have tried to recount elsewhere², the situation has greatly changed. The "serial pricing crisis", as it is commonly named, emerged about thirty years ago: it refers to the rapid and steep increase of the subscription costs of scientific journals. More recently, the advent of licensing schemes has done nothing to relieve the cost pressure, quite the contrary, and it has severely limited ownership, access and permissible forms of use. However, scientists have tended to ignore these negative transformations because they stand in the shadow of much more visible changes: digital technologies have made desktop accessibility to scientific journals a reality and this is what end users have most enthusiastically embraced, while remaining largely oblivious or indifferent to the costs. Publishers' marketeers, of course, dwell on these glittery changes while trying to keep the downsides of licensing out of sight.

All in all, well meaning librarians have shielded - too well shielded in fact - scientist and scholars from the signals sent by the rising prices of scientific publications. Ironically, the efforts deployed by librarians to serve their constituencies have helped publishers in their "divide and conquer" strategy, and they have led to disastrous results: because the buyers of scientific materials are not the ultimate users, and because scientists and scholars essentially remain insulated from the growing economic pains librarians face, publishers have been able to get away with economic murder³. It will be difficult to change this state of affairs so long as scientists and scholars are not actively involved in the buying of the scientific literature. Presently, they are bedazzled by the convenience of easy physical access while publishers use the promise of some added value or better services in order to have them petition the library and thus squeeze a few more dollars from its budget. As a result, scientists tend to ignore the ownership, preservation and circulation issues emerging from the brave new world of licensing.

Administrators, for their part, do see the financial problem more clearly, but they are caught in a difficult dilemma: while the costs of scientific information weighs down on the university budget, it affects a very small part of it, generally less than 2%. Meanwhile, the need to remain competitive at the research level in the world-wide scientific race cannot be avoided. Consequently, they generally chart a middle course between excessive spending and diminished research environments. The problem is that this pragmatic position generally does not leave much room or time to go at the bottom of the serial pricing crisis.

The cost of research essentially depends on two different kinds of inputs that often require being imported from abroad, even in the richest of countries: laboratory equipment and scientific information. It would be most interesting to study how the cost of laboratory equipment has evolved since the Second World War. However, in this paper, I shall limit myself to scientific information. The reasons for this choice are many but the most important one is that here I am addressing librarians: having rapid access to up-to-date, validated, scientific information is crucial for any research effort and librarians take this as one of their main tasks.

On a world wide basis, the sums devoted to buying scientific information are considerable – around ten billion dollars/year are spent across the planet if we limit ourselves to scientific periodicals. Also, the rapidly increasing information costs have begun to reach such proportions that even rich institutions in rich countries are beginning to feel the pain. Because they are rich institutions from rich countries, their complaints, although similar to those voiced for many years in poorer countries and research centres, are being heard better at last. However, the reaction has been sufficiently delayed that, in the meanwhile, poorer research institutions, particularly in poorer countries, have essentially been left out of the scientific loop and condemned to a marginal form of existence. As a grave consequence, teaching in those countries must essentially proceed without a living contact with research – a situation which severely limits the possibility of training doctors and perhaps even masters in science.

²See "In Oldenburg's Long Shadow: Librarians, Research Scientists, Publishers, and the Control of Scientific Publishing," <http://www.arl.org/arl/proceedings/138/guedon.html>.

³This very point was made by John Cox, from Blackwell's, in his concluding remarks to the First Nordic Conference on Electronic Publishing held in Lund and Copenhagen from the 22nd to the 24th of October, 2002.

It is these kinds of trends that allow me to claim that many recent transformations in scientific publishing have been detrimental to science - so detrimental in fact that they threaten to undermine its basic ethos. The meritocratic system of intellectual competition originally designed to identify and reward excellence is increasingly being replaced by an elitist-building system that adversely affects not only the workings of the scientific enterprise, but also its fundamental meaning. The quest for fundamental knowledge about nature is in the process of being superseded by the quest to control scientific knowledge itself and limit its access to a privileged minority. Furthermore, the circumstances leading to the present situation are not the product of serendipity; on the contrary, I believe that a few commercial publishers shrewdly understood the peculiarities of scientific publications and of their distribution, and they learned to capitalize on it, especially after the Science Citation Index prepared the way for the creation of an inelastic market of scientific journals⁴. The serial pricing crisis, in short, was not born; it was engineered. Furthermore, as the rising prices of scientific periodicals did not hit the very richest laboratories and other research institutions as hard and as early as the rest of the scientific world, it actually contributed to improving their competitive advantages. As a result, wondering whether some tacit, largely unconscious, convergence of interests emerged between large commercial publishers and a fraction of the scientists themselves does not appear completely fanciful. I am not talking about a conspiracy theory here; rather, I am referring to these kinds of gradual trends that simply make some choices marginally more appealing than others, especially if one pursues one's selfish interest at the expense of some of the basic tenets that stand behind a Republic of excellence.

The rest of this paper will examine one particular facet of this question: given the growing restlessness generated by the spiralling cost of scientific journals, alternative strategies are being suggested and even initiated. Among them, the open access archives appear most promising. However, given the importance of scientific journals for the scientific pecking order, how should open access archives be designed and organized? The objective here is designing a publishing device that would be better at identifying and encouraging excellence than present journals are, while avoiding to restrict access and slow down the circulation and percolation of cutting edge scientific information in the direction of any brain susceptible of making good use of it. In short, what is at stake here is nothing less than the restoration of a "Republic of science" that appears to have been occulted too long by an elitist *Ersatz*.

Scholarly publishing and copyrighting: an uneasy relationship.

The recent development of open-access archives and their active promotion under various names⁵, has led to a series of questions which can be classified under three main headings:

1. Where do open access archives fit in the publication process of scientific research?
2. How do open access archives deal with copyright issues?
3. What are the strategic implications of 1 and 2 for the successful implementation of these open access archives?

The first question is best approached from the perspective of the peer-review process. In the print world, the publishing phase must follow some form of filtering, some form of editorial choice, for example peer review. This is simply the consequence of high printing costs and it has led to the connotation of authority attached to many if not most printed documents. By contrast, open access archives deal exclusively with digitized documents and would be unthinkable without them. As a result, quality control can now be so wholly disconnected from the publishing phase that it no longer has to precede it. Correlatively, the publishing costs of digital documents are no longer high enough effectively to prevent anyone's entry into the publishing game, as

⁴See again my "In Oldenburg's Long Shadow...", *op. cit.* (note 2).

⁵For example, "Open Repositories". See Raym Crow, "The Case for Institutional Repositories: A SPARC Position Paper" <http://www.arl.org/sparc/IR/ir.html>.

the explosion of the World Wide Web amply demonstrates. With it, most everyone can be his or her own publisher, an opportunity that the pornographic industry and minority political movements understood apparently faster than anyone else, but which scholars are beginning to recognize as well.

Because it lowers the entry barrier to publishing, the Web holds the very real potential to disturb delicate equilibria within academic and research circles because the ability to be published lies at the very heart of one's reputation, authority, prestige, and, ultimately, one's scientific career. Tenure and promotion rest on publications. It is no wonder, therefore, if open access archives have led to cautious and protracted academic debates, and it is no surprise either if these issues have often been finessed or addressed in tangential fashion: rather than talking about careers and power relations, academics prefer to focus on carefully distinguishing between various categories such as pre-prints, e-prints, etc. Public access and publishing have also been distinguished, particularly in the case of scientific publishing. Because documents can now be easily accessed from all over the world by about anyone connected to the Internet, the need to distinguish scientific publishing from digital vanity presses increases. Behind these apparently ethereal debates, lie concrete and crucial concerns such as: how will these open access archives affect my present institutional situation, my status, my reputation, etc.?

Motivations aside, these discussions have turned out to be quite useful. In particular, they have led to the gradual emergence of important definitions, clarifications and distinctions. Salient among them are the following:

1. "An eprint is the digital version of a peer-reviewed research article.
2. Before refereeing and publication, the draft is called a "preprint."
3. Once published, the refereed, final, paper is called a "postprint"⁶.

Note in passing the irony of retaining the "print" category to describe the passage from pre-peer-reviewed to post-peer reviewed. Apparently, some security seems to be derived from the familiarity of the print world and it is clearly carried over into the electronic world, with results that may appear somewhat oxymoric or even a little confusing. Note also how transient an eprint can be: it briefly exists between positive peer review and publication. In the digital environment, this phase may be extremely short, and even vanish altogether if the paper has been made public before it is peer reviewed. By contrast, in the print world of the humanities and social sciences, that period of limbo may easily extend for months and even years;

Among all of these distinctions, that between pre-print and e-print stands as the most important: it is a rigorous distinction and it sometimes serves to define "scientific publishing" (SP) not as an activity or an event, but as a real concept. Let us use Stevan Harnad's words in this context:

...for scholarly and scientific purposes, only meeting the quality standards of peer review, **hence acceptance for publication by a peer-reviewed journal**[my emphasis, J.-C. G.], counts as publication. Self-archiving should on no account be confused with self-publication (vanity press). (Self-archiving pre-refereeing preprints, however, is an excellent way of establishing priority and asserting copyright)⁷.

While Stevan Harnad encourages all of us to archive "all significant stages of one's work, from the pre-refereeing preprint to the peer-reviewed, published postprint, to postpublication updates...", he also reminds us that these forms of archiving do not all amount to scholarly publishing. In other words, he carefully distinguishes "making public" from SP.

Is the criterion that Stevan Harnad uses to distinguish SP from other forms of publications completely satisfactory? In particular, and in order to be distinguished from other forms of publishing, does SP need to rely on a peer-review process located exclusively within existing and recognized "scientific journals"?

⁶<http://www.eprints.org/self-faq/#What-is-Eprint>.

⁷<http://www.eprints.org/self-faq/#self-archiving-vs-publication>.

The reason why the journal requirement finds such a prominent place in Stevan Harnad's thesis appears mainly tactical: by linking the peer review process to existing journals, and only to them, Harnad apparently hopes to sidestep the tricky question of designing new forms of peer review. Because such discussions tend to be complex, he believes they can only delay reaching the objective of reforming scholarly publishing; they will detract would-be reformists from more urgent issues; they may even end up being counterproductive by generating needless worries on the part of practicing scientists who manage their career on the present, well known, publication system. By insisting that existing journals should remain as the heart of the evaluation loop, he can claim that his own strategy, based as it is on self-archiving, can simply begin by taking advantage of existing peer review processes.

Stevan Harnad's strategy generates a few difficulties of its own: if an article is evaluated and accepted by a journal (and ultimately published), and if the copyright has been signed away, how can one also deposit it in an open access archive? The objection is important because it has led Harnad to suggest solutions that rest on untested legal matters. For example, he suggests that, once the peer review process is successfully completed and the pre-print draft is stabilized into an accepted, publishable, form, the author can legitimately archive an earlier preprint draft and add the corrigenda distinguishing that draft from the publishable manuscript. Although amusing – it reads a bit like a funny prank played on greedy publishers - and quite flawless from a strict interpretation of the copyright tradition, Stevan Harnad's strategy remains questionable in practice: beyond the fact that its Jesuitic tonality may generate a smile plus some skepticism, one may also legitimately wonder how a court of law would actually react if faced by such a ploy.

Copyright, let us remember, rests on the form or the expression and not the themes and ideas of a text, but it can also deal with plagiarism, thanks to notions such as "substantial similarity" and the "meaning and feel of a totality"⁸. Stevan Harnad's suggestion would probably have to overcome this obstacle and, in the absence of an actual, favorable precedent, it is not surprising that his tactic has not rallied large numbers of scholars and scientists: authors, particularly young, vulnerable researchers – e.g. untenured faculty – fear transgressing or even coming close to transgressing copyright laws. They also worry about antagonizing powerful editors and publishers, thereby risking losing access to important, prestigious, journals. In short, legal uncertainties and (all too) human career concerns get in the way of Stevan Harnad's otherwise impeccably logical argument.

These objections notwithstanding, Stevan Harnad's suggestion remains useful as a thought experiment. In particular, it allows us to look more precisely into the complicated SP process and to distinguish more clearly what depends on copyright *per se* and what depends on the editorial or publishing policies of particular journals. For example, the refusal to publish an article because a preprint has already been made available on the Web cannot be justified on the basis of a blanket recourse to copyright because nothing in copyright laws prevents the contracting parties to decide that self-archiving is also permissible. Publishers may well wish to hide behind the copyright laws but such decisions are really part of an editorial policy; unlike copyright provisions, it can be negotiated⁹.

In conclusion, Stevan Harnad's self-archiving proposal shows that, within the present publishing system, there is room for negotiating a number of rights. However, it must also be remembered that if scientific authors do not try harder to capitalize on these opportunities, it is because, in the last analysis, they value the author status far

⁸Peter Jaszi, "On the Author Effect: Contemporary Copyright and Collective Creativity" in Martha Woodmansee and Peter Jaszi, **The Construction of Authorship. Textual Appropriation in Law and Literature** (Durham, N. Carolina: Duke University Press, 1994), pp. 40-1

⁹The refusal to publish articles previously made public on the Web is often referred to as the "Ingelfinger Rule", named after the former editor of the **New England Journal of Medicine**. See Stevan Harnad, "Ingelfinger Over-Ruled: The Role of the Web in the Future of Refereed Medical Journal Publishing", <http://www.ecs.soton.ac.uk/~harnad/Papers/Harnad/harnad00.lancet.htm>.

above the owner's benefits¹⁰. For scholarly authors, the risks involved in challenging editors and publishers simply appear too great in view of the fact that the present equilibrium essentially satisfies them.

A few months ago, thousands of signatories to the Public Library of Science (PLS) petition threatened to refuse publishing in journals that would not free their content within six months after publication. The recourse to a threat, although it dramatically publicized the scientists' frustration with the present situation of scientific publishing, was probably ill advised: publishers ignored it, safely betting on the inability of the majority of the signatories to follow up on their promise. The fear of losing ground in the great international prestige race prevailed, and submissions to prestigious, yet expensive, journals continued unabated.

The unrealistic dimension of Harnad's suggestions and the failure of the PLS threat must be remembered and integrated into future strategies. One very simple principle must be kept in mind: open access archives will succeed only if they can satisfy the scientists' needs in terms of their careers. This means **incorporating certification or branding tools that are at least as authoritative as those presently provided by existing journals**. In other words, it means squarely tackling the branding issue that Stevan Harnad wants to avoid. As a consequence, there is an urgent need to discuss how best to integrate evaluation tools within open access archives. The question is all the more important that a revised evaluation system will strongly contribute to weaken the scientific plutocracy that presently reigns and will help restore the Republic of science.

II. Where intellectual and financial matters mesh.

Because they are so important for the evaluation of scientists, it repays examining closely how scientific journals reach that objective, and how satisfactorily. How do they evaluate and what do they evaluate? Deans and research managers have come to rely on them to grant promotions and tenures to researchers and faculty. Do they have in hand what is needed to do the best possible job? The answer is largely negative, as we shall see.

A - How do scientific journals evaluate?

Journals present us with two opposite façades: on the one hand, the editor-in-chief and the editorial board are made up of recognized specialists in some scientific domain - in other words, peers. By exercising "peer review", they provide scientific credentials whose value rests on the fame and reputation of the journal. In turn, their decisions loop back into the reputation of the journal. On the other hand, journals also rely on a variety of skills that have nothing to do with the "peer" category: linguistic editors, layout specialists, printers, marketing specialists, and foremost among them, a financial director. The director of a journal may take on several of these tasks and he may end up being more of a manager than a scientist; as such, he or she may be part of a publisher's structure rather than part of the editorial function. The relationship between the management and financial dimensions of a journal and its intellectual content may thus vary greatly from case to case: sometimes these functions are merged within one person – typically an over-worked faculty member who takes on tasks that reach well beyond academic or research skills –, sometimes they involve a complex division of labor between an academic group and the personnel of a publishing house. In other words, a scientific journal can be totally under the control of the relevant scientific communities but it can also be a strong commercial shell within which a number of scientists are allowed to act as gatekeepers.

The word "peer" itself deserves some comments: while commonly used in a descriptive way, it does refer to a situation where some people appear as distinctly "more equal than others" to use George Orwell's satirical phrase. By exercising peer review, the academic fraction of any scholarly journal enjoys a degree of power that their colleagues simply do not possess. This is simply because directing or playing a significant role within a journal provides extra visibility and an authority that is clearly tied to the journal as social institution. In fact,

¹⁰The terms are borrowed from Mark Rose's excellent study, **Authors and Owners ...**

acting as editor-in-chief, or as senior editor of an important journal is such a good way to advance one's career that it can easily be compared to receiving something like an important prize. Moreover, both can reasonably be assimilated to a kind of extra academic promotion. A "peer" has actually been silently promoted to the status of a gatekeeper – a fact that, ironically, the word "peer" tends to hide rather than reveal.

What is particularly interesting is that the promotion to gatekeeping does not lie squarely within the research institutions or communities; on the contrary, the process involves a mixture of academic and non-academic factors. For example, in the case of a new journal being created, a group of scholars or scientists band together to try and compensate for what they feel is a lack of publishing outlets or opportunities for their specialty. Note that although this step is purely academic in nature, it already involves a process of self-selection that must not be forgotten. Moreover, behind the arguments in favour of a new journal often lurk a wide variety of interests and situations : it may be that a new specialty is trying to emerge; perhaps the available publishing outlets are perceived as being in the hands of foreign colleagues and this may be viewed as a competitive disadvantage¹¹; sometimes growing institutional ambitions suggest that locally supporting an international journal will bring some prestige and visibility, as well as opportunities for fruitful alliances with other powerful institutions; at other times, particularly in the social sciences and the humanities, new schools of thought emerge within established specialties and they must find ways to overcome or circumvent the resistance of the established schools. Linguistic considerations may also play a role, especially in the humanities and a number of social sciences. In short, the reasons standing behind the creation of scholarly journals can be very varied, but they all relate in one way or another to questions of autonomy, power, visibility, etc. The spectrum of agendas is extremely broad and many are not strictly connected with purely intellectual or scholarly motives; ideological or even disguised economic objectives can also interfere; yet, the rhetoric almost invariably alludes to some regrettable dearth of publishing opportunities.

Whatever their motives, the promoters of a new journal quickly bump into material concerns: money is of the essence, of course, but so is space and secretarial time, etc. At precisely this juncture, intellectual dimensions begin to mesh with practical worries. While a journal will never rise simply because a number of material conditions are present – the academic side of the incipient publication must play its role here -, these material factors certainly act as sufficient conditions: **only if** money is available can the new venture begin; **only if** the support is maintained for a few years, can the new publication hope to reach sustainability.

It is from that angle that a publisher, by virtue of its financial resources, can have an important say in the creation of a new journal. In other words, the extra-promotion to gatekeeping may occur only if publishers or research institutions provide the material means to bring the journal to existence. Often, research institutions do not have those means and the task ultimately falls into the lap of a publisher. However, in divesting themselves of the economic responsibility for scientific journals, scientific institutions do not seem to realize that they are giving up any role they might play in the promotion to the gatekeeping role. This is a curious situation because, while they rely on journals to evaluate their scientists, they act in such a way as to lose most of their input in the fate of these journals. In effect they delegate the evaluation of scientists to a mixed institution – the scientific journal – without examining closely how this affects the quality of the evaluation.

The reasoning applied to new, emergent, journals can be easily extended to on-going publications. It is known that commercial publishers monitor existing journals. They look for intellectually valuable publications that might enjoy forms of economic support that are less than optimal. Actually, they are evaluating whether such journals ought to be drafted into the company's "stable" and with what inducements. For them, it is an

¹¹ I have heard this argument used in relation to **Tetrahedron Letters** (Elsevier): European chemists, according to this opinion, need this "European" outlet to compensate for the "advantage" allegedly enjoyed by American chemists in **Organic Letters** (American Chemical Society)! I will let the reader evaluate the validity of such a claim...

investment strategy aiming at capitalizing on a worthwhile journal, even if it means operating at a loss for a few years, because publishers know that the return on this investment will be ultimately significant and steady¹².

Some academic editors would resist such a shift because they would tend to consider it as a betrayal of their role; others on the contrary may see moving into the collection of a well-established and powerful publisher as an opportunity and even a form of valuable recognition. As it also generally translates into a series of tangible advantages - more financial support for the gate keeping function, more secretarial help, better equipment and software to work with – it is quite difficult to resist. It is even more difficult to resist if it involves some personal financial rewards that are not altogether symbolic¹³. In short, scholars and scientists may begin to feel dependent on their good relationship with a publisher to maintain their gatekeeping advantages, some of which are purely academic, others not.

In conclusion, being "promoted" to gatekeeping entails a complex combination of factors where the specific values and forms of authority that characterize a particular specialty tend to mesh with some of the financial and infrastructural underpinnings without which a journal just could not exist. This does not imply that a scientific editor does not enjoy intellectual freedom; on the the contrary, publishers know better than to tread in an area that could only discredit the reputation of the journal they publish. However, the editor of a journal that does not pay any attention to his/her publisher's concerns would do so at the risk of being quickly "demoted".

The status of a scientific journal is largely measured by its impact factor. Improving this impact factor has become an obsessively present task. For its part, a commercial publishing house will constantly be monitoring how best to allocate its resources to maximize profit. As a result, a publication with a modest and stagnating impact factor may come to be seen as a problematic business proposition. The touch stone, in this case, will be the profit margin. If the journal, although of decent intellectual or scientific quality¹⁴, does not manage to be sufficiently profitable, it will be dropped. But this amounts to demoting a particular group of gatekeepers, and for reasons very removed from issues of scientific excellence. Demotion ensues because when a journal is left to its own devices, it may lead to the journal working within much reduced circumstances and even to its ultimate disappearance. Deprived of the marketing push that large publishers can provide¹⁵, the cast-away journal will probably see its impact factor decrease and this will translate into a lessened ability to attract first rate authors. The impact factor, as a result, goes further down, and so on. The quality of the financial underpinings of a journal can feed directly into the ups and downs of the impact factor, thus catalyzing a self-sustaining, upward or downward, trend.

Financial concerns and quality control issues do interact in journals, and not only at their inception. As a result, it is easy to understand that a complex chemistry made up of financial and intellectual concerns lies at the heart of the device on which rests the whole evaluation process of scientists and scholars. While it is impossible to get rid of the financial dimension dimensions of scientific publishing – it will always cost something – one may

¹² A full analysis of the publishers' roles should clearly distinguish between the profit motives of large, international, commercial publishers and the scholarly concerns of university and some (not all) learned societies' presses. One must not confuse businesses driven by profit or professional objectives with presses simply seeking to recoup costs or, at best, turn a modest profit.

¹³ In a recent conversation, a colleague that will remain unnamed, acting as a journal editor for a commercial publisher, revealed to me that he was receiving US\$ 50.00 for every article that he dispatched for peer-reviewing Another colleague, also acting in that capacity for the same publisher, mentioned that he was treating around 3,000 articles per year. I do not know whether the editorial inducement in the second case were the same as in the first case, but if this should be the case, it would amount to US\$150,000/year...

¹⁴ Again, discretion prevents me from citing names, but this thesis reflects actual conversations with people very familiar with the publishing business and, therefore, very much "in the know".

¹⁵ Again, see my "In Oldenburg's Long Shadow...", *op. Cit.* (note 2), particularly the part dealing with the distorting lens created by the big deals, as exemplified by the statistics provided by Ohio Link.

question the usefulness of adding the profit motive in this operation. All indications point to the possibility of very unhealthy forms of commercial interference into the process of scientific communication.

B - What to evaluate?

One further question must now be raised: what is the exact function of peer review? What does it really evaluate? Based as it is on the judgment of very few people, can peer review claim to be much more than a filter allowing scientific players to engage in the scientific game? Actually, it cannot, of course, and, from this perspective, peer review amounts to a mere entry ticket into the great scientific debate. At that stage, the evaluation of excellence has hardly begun.

Surprisingly few tools exist to measure excellence once an article is published. In fact, impact factors are about the only forms of measurements that may claim some usefulness in this regard, which probably explains its wide (and somewhat overextended) popularity. Moreover, impact factors are not always used felicitously or even judiciously¹⁶.

Other ways to estimate the level of use and the impact of a given paper can certainly be devised, but such statistics are hard to come by with large commercial publishers. However, the possibilities offered by various forms of usage statistics for the appreciation of excellence have not escaped the attention of a number of commercial players and some have begun to explore new possibilities in this regard. Clearly, new forms of measurement of use and impact are needed and are being investigated. Elsewhere¹⁷, I have discussed how Elsevier, with its open access chemical archive, appears to be testing new models of evaluation based on factors such as downloading, discussions, commentaries, etc. Elsevier, probably inspired by ISI's Science Citation Index, appears to understand that controlling the tools of evaluation has a high strategic value which hold the promise of considerable cash rewards.

Looking for new forms of excellence measurements, therefore, is not a far-fetched chimera; in fact, its presence on the radar screens of commercial outfits involved in scholarly publishing proves the contrary thesis.

Other forms of evaluation that already exist must not be neglected either. Scientists are submitted to forms of evaluation that are a good deal more complex and varied than the simple peer review process found in scientific journals. Many institutions need to identify and hierarchize various forms of scientific excellence and several of these processes keep some distance away from the publishing process *per se*. For example, research grant agencies rely on some form of excellence measurement, but they do not limit themselves to journals: while publications count, of course, the candidate must often select his/her best papers and the quality of the proposal is often peer reviewed within the context of a jury. Likewise, scientific prizes and distinctions are attributed on jury-based peer review. They matter a good deal and they certainly enter into the art of successful grantsmanship. Finally, being named on important commissions and being invited as an important speaker within high-profile conferences also provide useful indications of excellence. Again, peer review is at work here, but in yet another setting: organizing committees acting as a jury select future speakers on the basis of previous experience and reputation.

Some of the evaluations mentioned above are obviously more rigorous than others but journal-based peer review does not entirely avoid criticisms in this regard. In the end, one constant lesson emerges from even a quick survey of the existing range of evaluation procedures: the evaluation of scientists's work has been going on for a long time and it has relied on a wide variety of techniques and indices among which the presence of peer-reviewed articles is but one possibility. Interestingly, as time goes on, not only do more centers and forms of

¹⁶For example, institutions use journal impact factors to evaluate individuals' publications. This makes about as much sense as using the size of a limousine to gauge the worth of its passengers ... Moreover, within a given publication, impact factors of individual articles can vary by as much as two orders of magnitude and, finally, impact factors vary quite a bit from one discipline to another.

¹⁷See "In Oldenburg's Long Shadow..., *op. Cit.* (note 2).

evaluation keep on appearing, but the filtering function of the peer review process seems to lose some of its importance. In some fields such as theoretical physics, such filters appear secondary as the ArXiv bank of articles demonstrates: actual use, comments and criticisms play a far more useful role for science. Given these trends, one may begin to wonder why journals have maintained such a tight grip on the evaluation process: incomplete, unreliable, fraught with interferences from non-academic concerns, they only represent a small and diminishing fraction of the tools that ought to be systemically deployed to achieve a full and fair estimate of research excellence. Open access archives may well be the tool to demonstrate this point fully and rapidly.

III. Open access archives and the restoration of the Republic of science.

As more and more institutions develop open access archives and institutional repositories, one may surmise that the first wave will be made up of wildly varying documents. My own university provides a rather strong example of this chaotic situation: while the library is beginning to test waters with a carefully designed self-archiving system¹⁸ and is proceeding with some caution to include only recent works, presumably peer-reviewed, that authors want to promote more widely in this fashion, a competing group located within the computer services is promoting a "prepublication" site as part of a wider e-publishing scheme. In it, documents as old as 1986 can be found, as well as old newsletters from neighbouring research centres¹⁹, commentaries on governmental reports, etc. The desire to fill the archive quickly has obviously led its designers to resort to pieces of work of doubtful value and, in some cases, probably best forgotten. In the absence of a classificatory scheme that would be a little more sensitive to the career and research needs of scientists, such heaps of materials are of questionable value and may even nourish the skepticism of serious scholars who do not want to see their work buried in such unseemly neighbourhoods. While old working papers from local research teams may generate some local nostalgia for the "good old times" among a few sentimental types, they will gain little respect from the outside world. Best ignored, they will tend to look quite silly and they may actually do more harm than good for institutional repositories.

In order to avoid such problems, institutional repositories respecting the OAI meta-data guidelines should draft guidelines for the selection and the classification of documents. In this manner, we may collectively hope to avoid the kinds of problems that overly eager or competitive groups will generate simply because they are trying to occupy some territory first. Institutional repositories and open access archives should not be confused with the Wild West or some "gold rush" territory with its free-for-all land claims.

By contrast, the following structure might be more interesting for institutional repositories in open access:

1. A first entry level would roughly correspond to what the papyrus project at the University of Montreal is trying to achieve: scholars select papers that they consider to be interesting either to give them a second life, if they have already been published in a paper, or to promote some discussion around a paper-in-progress. In the first case, the author may do so either because he/she never signed away the digital rights to a journal – a situation almost universal until quite recently which makes most of us owners of the digital versions of our articles and books. Alternatively, permission has been secured from the publisher either through some blanket statement or through an individualized request. In either case, the fact that this work has been peer-reviewed should be clearly indicated in the document and the metadata that accompanies it. In the latter case, where an author is fielding a working paper that has not been peer-reviewed, this situation should also be clearly established in the document and in the metadata that accompanies it. Note in passing that this entry level agrees well with the self-archiving procedure advocated by Stevan Harnad. Some authors may even want to test the pre-print plus the corrigenda technique he advocates to circumvent the provisions contracted with a publisher, but this will probably require some legal (and political) support on the part of the home institution as we are bound to see court cases testing this approach in the near future. A bit in jest, I will call

¹⁸<http://papyrus.bib.umontreal.ca/>.

¹⁹<http://www.erudit.org/prepub/index.html>. Five texts are from 1986. About thirty come from local newsletters (Bulletins) that fairly qualify as gray literature.

this entry level archive IH or "institutional hell". It does not carry much prestige or symbolic value, but it does tell the world that a serious member from a serious institution is putting out some work seriously done, some of which have been peer reviewed in other contexts, and some have not. Harvesting machines can then be configured to include or exclude these sub-categories according to the desires and needs of individual investigators.

2. A second level can now be initiated. This level does not generally exist in our institutions but I believe that its presence could do a great deal to help universities and research labs regain a better control over the evaluation of their own researchers. In effect, each institution could decide that it stands behind all the publications that are located in this level. In so doing, it would have to decide whether it will simply rest on the traditional tools (peer review of established journals, impact factors, etc.) or whether it wants to set up particular procedures that would amount to creating an independent evaluation process for pieces of work that local faculty would submit for acceptance at that level. A given university, for example, might decide to set up an internal jury to deal with these selections, or it could build a mixed jury with external members from other universities, as is already the case for the defense of theses and dissertations. In short, it could devise whatever policy it wants to bring pieces of work to this level, but the important point is that, once admitted at that level, this particular piece of work would have the full backing of the institution. In particular, it could legitimately be used to support promotion or tenure. The point here is that the institution would consciously decide how to evaluate its members rather than lazily delegating the task to outside entities such as journals, with all the attendant problems and ambiguities that have been fleshed out earlier in this text. An institution might even decide simply to use the existing procedure, but even that would require a conscious, lucid and transparent act that would certainly lead to some interesting debates within any given university senate worth its salt. That in itself would be healthy. Let us call this second level IP or "institutional purgatory".
3. Paradise is, of course, the next step. How can we characterize a publication paradise? Simply by having this publication submitted to an evaluation procedure that is demonstrably the best possible, given what is available elsewhere. It should be so good as to put an article in **Nature** or **Science** to shame, so to speak. The way to achieve this is to constitute extremely prestigious, international, juries that pass judgment on submitted papers with the utmost rigour. For example, the top schools of the world in any given field form a consortium to evaluate economics papers or sociological papers and they publish their procedures, their minutes and their results so as to create the greatest possible transparency. And if a given institution feels slighted because it has not been asked to join the first prestigious consortium, let it organize the second prestigious consortium: competing world-wide juries could set themselves up and the quality of their judgment would be quickly evaluated by the kinds of usage and comments their choices would elicit. Being selected by one or several of these juries would indeed be, for any paper, the equivalent of reaching Paradise. Metadata, again, would clearly indicate the presence of a UP or "universal paradise".

Structuring an institutional repository in this fashion would clearly indicate that not only open access is of the essence, but that evaluation is also a central concern of such a repository. Obviously, these evaluation procedures will not replace the existing ones, particularly those grounded in journals, but they will allow for a very different mechanism of gatekeeping – one that is no longer so dependent on much financing to work, as is presently the case with journals. Poor countries with good minds will be able to make their voices heard and good minds in peripheral institutions will also be able to participate in the world-wide concert of opinions. In other words, against the unjust, elitist form of globalization that has affected science so negatively, this approach would begin to offer a path toward a better form of scientific globalization. At the same time, the diluting of the evaluation monopoly presently enjoyed by the core journals would gradually translate into a looser grip on the best minds of the planet and, as a result, real competition would have a chance to emerge, and the evolution of serial prices would begin to slow down and would perhaps even begin to fall.

One must realize that if most of the major research libraries begin to build institutional repositories, as seems to be the case right now, and if such evaluative schemes begin to develop, the distributed power of such institutional networks can quickly become so enormous as to become the defining criterion of excellence. In

other words, the creation of the open access archives accompanied by a suitable evaluation scheme holds the promise of relocating the centre of scientific evaluation squarely within academic and research environments. The peer review process is, after all, done by peers that belong to our institutions: why abandon part of the privilege to promote some of our colleagues to this important role to bodies that share few, if any, of the basic academic values. It simply does not make sense to see profit-based companies having an indirect, insidious, ill-defined, yet effective, voice in elevating or keeping a few of our colleagues in important gatekeeping roles.

This also means that, because they are open access and because they are networked and harvestable as if they were a single gigantic unit, these institutional repositories will offer an enormous amount of information on usage statistics that, presently, are kept in the hands of private companies. As a result, new evaluation tools based on various form of usage become possible and will be developed and tested by academics and for academics. This too is important.

Open access archives should also leave room for commentaries, criticisms and rebuttals, so long as they are conducted within the rules that prevail within conferences, colloquia or seminars, and with similar rules of access: we are talking here about scholarly debate and not street side discussions or chat rooms. In this fashion, threads of discussions and debates will emerge that will help identify which questions, concepts, objects appear most interesting at any given moment. And these threads and lines of discussion will begin to implement what Manuel Castells likes to call a "space of flux" and which he sees as a fundamental element of any networked situation. Obviously, these comments, judgments and criticisms will enter into the evaluation of a given scientist's work because his/her ability to defend, extend or even correct a piece of work in an interesting manner are obviously what is expected of a good scientist or scholar. At the same time, one may see looming over the horizon, a few decades down the line – academics tend to be a very conservative lot and some even pride themselves on that score – the gradual demise of the batch production of knowledge which articles incarnate so vividly. In its place may gradually emerge a more fluid and flexible mode of scientific communication, one where a given individual could contribute as little or as much as he/she wants, so long as it is significant and accepted as such by his/her peers.

If open access archives develop in this fashion, the academic networks, on a world wide basis, will be able gradually to reclaim their control over the circulation and the evaluation of scientific communication and, correlatively, commercial publishers will tend to move back to their original position when they were marginal players, looking for interesting authors that could write some specialized products such as treatises, manuals and reference works. Their involvement in scientific journals would then become a secondary task but their presence would be as useful as it was in the past, when they helped to keep in check the corporatist tendencies that sometimes haunt academic circles.

IV Conclusion

The thrust of this presentation has been to show that the evolution of science had gradually led to an elitist plutocracy, but that open access archives could become part of a solution aiming at restoring what I like to call a "Republic of science". However, to do so, a precise study of the evaluation processes presently used in various scientific communities has been necessary. In this regard, the discussions around self-archiving have been illuminating: by focusing clearly on the act of scientific publishing, as a concept, they served clearly to delineate what was of the essence of scientific publishing. At the same time, they show that the intellectual rigor of Stevan Harnad's analysis does not always take into sufficient account some of the fuzzier realities of scientific publishing, such as the legal interpretations of plagiarism. Nonetheless, these discussions have been useful in showing that rebuilding scientific evaluation would have to accompany the effort to build open access archives. Present evaluation procedures as managed by journals, including periodicals placed within the ambit of vast profitable companies, display a complex of motives that cannot serve science or the management of research in an optimal manner.

At the same time, the quasi-monopoly exerted by these large commercial publishing houses over scientific evaluation partially explains their ability to manipulate prices as they do: scientists simply need these publishers too much and they cannot conceive of an alternative way to manage a successful career; as librarians have been

too good at protecting them against the bad effects of high prices, at least for those located within rich institutions in rich countries, they have not paid all that much attention to resources that, to them, appear "freely" accessible (but at a high cost to their institution). However, if open access archives are built as suggested, with an associated evaluation system, then scientists, scholars and academic administrators may have a chance to wrestle a sufficient fraction of the evaluation potential as to create a real competition for commercial publishers and, thereby, create change, as the SPARC pamphlet admonishes us to do. To this end, a three-tier structure fully harvestable according to the rules of the Open Access Initiative has been suggested and it should move us a long way in the right direction while avoiding debasing open access archives into meaningless heaps of shoddy ware as appears to be the case in a few careless ventures.