

# 10. The public domain and the economist

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## 1 INTRODUCTION

The initial brief for this chapter – to examine from an economist’s perspective the triad of open access, open science and open source – was daunting in two different ways. First, there is now an economics literature on this triad large and varied enough to make a survey article barely feasible within the space constraint and thus ultimately unsatisfactory, as interesting policy issues and personal experiences would have to be left out. Secondly, the public domain is an area where economists tread very carefully and rather uncomfortably, as they have to walk (or is it surf?) without the aid of some of their most trusted points of reference, such as well-defined property rights and individual incentives.<sup>1</sup>

I have thus redefined my brief, confining it to the examination of the relationship between the public domain and scholarly and scientific communication with special reference to one case-study which throws up interesting questions on the wider issues of open access, open science and open source: the case of economics journal publishing.

## 2 THE WEB AND THE DISSEMINATION OF RESEARCH OUTPUT: A PERFECT MATCH?

A key feature of ‘research output’ that distinguishes it from the rest of the material available on the Internet is that any piece of research, in order to qualify as proper ‘output’, has to go through a well-defined process of quality control and certification: the peer review mechanism. Although readers of this book are probably well acquainted with the concept, it may be useful to take as an example a piece of economic research and follow it through its three basic stages of development: (1) the *working paper*: this is the first draft circulated infor-

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<sup>1</sup> For a recent analysis, see Lerner, J. and Tirole, J. (2004), ‘The Economics of Technology Sharing: Open Source and Beyond’, NBER Working Paper 10956, December.

mally to a set of potentially interested fellow researchers and/or published (typically online) as a departmental/research centre discussion paper; (2) the version submitted to a journal (henceforth referred to as the *pre-print*); and finally (3) the *published article*.

This is indeed the *iter* followed by a piece of research in *any* academic discipline, but the meaning, status and timing of each phase show vast differences across disciplines.<sup>2</sup> I shall argue that these differences are extremely significant and indeed are at the root of some of the key problems that beset the relationship between the diffusion of academic research and the public domain.

On the face of it, the almost universal access to the Internet by researchers (definitely in the developed world and increasingly in developing countries) may seem to provide the ideal solution to the problem of dissemination of research. What could be simpler than using the Web to deposit and retrieve research output, without restrictions, tolls and barriers? In fact, if one examines more closely the incentives underlying the actions of researchers as *producers*, *monitors* and *consumers* of research output, the case for what is commonly called 'open access' appears irresistible:

- as producers, academic researchers supply their output without any expectation or prospect of *direct* economic gain (in terms of royalties and so on); indeed, sometimes potential authors *pay* submission fees in order to have their paper screened and reviewed by their peers. The main incentive is to maximise the *impact* of their research, by having it disseminated as widely as possible, hence gathering citations and peer-recognition, which eventually turns into career advancement, greater likelihood of research funding, and so on.
- as monitors (that is, as referees and editors), researchers provide their services either for free or for monetary rewards that are substantially below the opportunity cost of their time and effort. Again the incentives are not directly pecuniary and are provided by increased prestige within the profession/discipline. In the jargon of academic production, referees and especially editors act as 'gate-keepers', regulating access to, and defining the boundaries and direction of, the frontiers of the discipline.
- as consumers of research output, academics demand the widest and fastest access to publications.

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<sup>2</sup> For an interesting overview of the refereeing process (especially with relation to the public domain), see Rowland, F. (2002), 'The Peer-Review Process', *Learned Publishing*, 15 (4), 247–58. For a study on differences across disciplines regarding access to, and publication of, research output, see JISC Disciplinary Differences Report, August 2005, available at [www.jisc.ac.uk/uploaded\\_documents/Disciplinary%20Differences%20and%20Needs.doc](http://www.jisc.ac.uk/uploaded_documents/Disciplinary%20Differences%20and%20Needs.doc).

Thus the key aspects of the production, regulation and consumption of academic research, namely the lack of (direct) pecuniary gain, the collegiality of efforts and the desire for the widest and fastest dissemination appear to match perfectly the ethos of the Internet within the context of the 'public domain'.

This congruence of incentives and ethos is at the foundation of the open access movement, which advocates toll-free universal access to all scientific and academic output produced without any expectation of direct pecuniary reward. In fact, so perfect is the match that any external observer might have predicted that open access to all research produced not for pecuniary gain but for peer recognition would have taken place well before the advent of open source, where software with a market of potentially paying customers is instead made available by developers both to fellow developers and to users without any *direct* pecuniary gain. On the contrary, it can be argued that the open source movement has made far more significant inroads into the market of commercial software than open access into the field of scientific communication (I shall return to the relationship between open source software and open access to research output later in this chapter).

The reasons for this paradox and thus for the apparently inexplicable lack of widespread success for the open access campaign can be found in the specificities of scientific communication across different disciplines.

### 3 THE WEB AND OPEN ACCESS: A CAUTIONARY TALE?

In this section I shall try to examine how the specific interactions between the various phases of a typical piece of research (working paper, pre-print and published article) and between the various roles of researchers (producers, monitors and consumers of scientific communication) may explain why the road to open access is far more tortuous than some of the more evangelical advocates of open access are prepared to admit.

For reasons that will become apparent shortly, in the case of economics the main motivation for producing a *working paper* is (a) to establish priority of discovery, and (b) to elicit comments from peers. Although there is quite wide variation here, it is not uncommon for a working paper to be in circulation for quite some time before being turned into a pre-print and even to appear in different versions. The main reason for this lag is that the author's main aim is to produce a submission as polished as possible and to aim it at the highest-ranking journal with reasonable chances of being published. In order to achieve this aim, potential authors have a strong incentive to disseminate their work as widely as possible so as to signal their presence in a particular field, making their peers aware of their contributions, and expecting their peers to return the

favour. It is crucial to realise that in this specific phase toll-free dissemination is feasible, desirable and virtually cost-free; and thus it is not altogether surprising that, as far as working papers are concerned, almost universal free access is a reality in economics. A good example is RePEc,<sup>3</sup> an open-access repository of over 164 000 working papers in economics. In spite of repeated assertions to the contrary by open-access advocates who should know better, the wide availability of working papers in economics is *not* synonymous with open access, as the latter refers to the free availability of the text *of the published article*.

To explain the difference between working papers and published articles we have to consider the role of editors and referees in economics. For a rather complicated series of reasons (to do with the nature of the discipline, which has become increasingly mathematical and specialised, and with the emergence of over-strict refereeing norms and conventions), refereeing in economics is a very protracted and generally painful process, involving successive rounds of resubmissions and substantial lags, and, on average, ends with the rejection of the submitted version.<sup>4</sup> Economics is one of the very few disciplines where rejection rates above 90 per cent are common among the highest-ranked journals. Indeed, even success at the lowest rung of the peer-recognition ladder – the acceptance of a paper for a conference – is far from certain, with some conferences having rejection rates (50–60 per cent) that in other disciplines are restricted to high- to medium-ranking journals. The experience of having one's papers rejected is a recurrent and widespread occurrence and even future (and current!) Nobel laureates are not immune.<sup>5</sup> One significant effect of the refereeing ethos in economics is the lag between submission and publication, which very rarely is less than two years and may be as long as seven years, a delay that is unheard of in many other disciplines, where submission-to-acceptance lags are measured in months. There is however a positive side to this lag, namely that, on average, the accepted version (which may have gone through several revisions and resubmissions) is typically rather different from the initial one. The same cannot be said of other disciplines where the referee-

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<sup>3</sup> At [repec.org](http://repec.org).

<sup>4</sup> In two companion articles published in the (top-ranked) *Journal of Political Economy* ('The Slowdown of the Economics Publishing Process', 110 (5), 947–93; 'Evolving Standards for Academic Publishing', 110 (5), 994–1034) Glenn Ellison provides both a fascinating insight into the inner workings of some of the top-ranked journals in economics and an explanation for the progressive slowing down of refereeing.

<sup>5</sup> I very much doubt that there are many disciplines that have produced as long a list of rejected 'classic articles' as that compiled in the field of economics by Gans, J.S. and Sheperd, G.B. (1994), 'How Are the Mighty Fallen: Rejected Classic Articles by Leading Economists', *Journal of Economic Perspectives* 8 (1), 165–79.

ing process (and here I oversimplify somewhat) has more of a binary nature, whereby a submission is checked for novelty and correctness and approved or rejected on that basis.

In economics the refereeing lag is a sociological problem, not a technological one: the advent of electronic submission and the use of software for manuscript reviewing (both increasingly popular in economics, but by no means universally adopted) seem to have had only a marginal impact. The lack of urgency applies also to the final stage of the process, when the lag between the final version being accepted for publication and the actual publication may extend to several months.

So, to recap the story so far: in economics the initial incarnation of a piece of research (the working paper) and the final one (the published article) perform significantly different roles in the chain of research communication and thus interact with the public domain in different ways. The working paper is used as a means to stake the priority of the author's contribution and to advertise his/her presence in the field. As a consequence, the speed and reach of the dissemination are essential, and it is no surprise that a very efficient mechanism for the posting, archiving and retrieving of working papers has developed wholly within the public domain. As far as the published article is concerned, the priorities are wholly different: speed is definitely unimportant and even reach is not of direct relevance. The paramount preoccupation of the published author in economics is the *prestige* of the publication. There are about 300 journals in economics (broadly defined), not only with a huge variation in their prestige but also in a very strict and codified ranking order. In economics there exists a very close correlation between the peer recognition of a researcher and the publication record of the said researcher in an extremely narrow set of top-ranked journals. Considering that in economics citations tend to accumulate over time often with a very slow start (unlike other scientific disciplines, where the citation impact is highest in the first couple of years after publication), articles are judged almost exclusively by the prestige of the journal they are published in, and not by short-term citation impact. A number of important consequences follow, as far as the relationship between the published economist and the public domain is concerned.

Although begrudged by many economists, the stranglehold of the top 5 per cent of journals on the journal market is a deeply entrenched phenomenon which has been strengthened in the last few years by the appearance of formalised research assessment mechanisms. The arrival and success of the Internet has had no impact whatsoever in terms of facilitating entry of new journals into the top echelons of the economics journal hierarchy.

Contrary to the mantra infinitely repeated by some advocates of open access that the sole/main/paramount aim of researchers is to maximise the impact of their published research, economists appear not to be at all bothered by the

size of the potential readership of the journal, as long as it is a prestige journal. This should not be surprising. Once I asked the most strident and uncompromising of all open access advocates – Professor Stevan Harnad – where he would choose to deliver a paper if forced to choose between an audience comprising the 5 per cent top researchers in his discipline and an audience with all the rest. He could but admit that he would choose the former, but, of course, stressed that in the post-Gutenberg era it should be possible to reach a universal audience. This, in my view, is a fundamental misunderstanding of the role and status of journals in the Internet era (at least in disciplines such as economics).

#### 4 OPEN-ACCESS PUBLISHING: A VIABLE ROUTE?

One of the routes to toll-free access to refereed academic work is by submitting one's research output to open-access publishers, where the costs of refereeing, online publishing, and distribution are not levied on the readers but on the authors (or rather on their institutions). The fact that open-access journals in economics are a minuscule fraction of the total and likely to remain the hobby-horse of a tiny minority disposes of open-access publishing as a viable strategy – at least for disciplines like economics. The reason is easy to see.

Let me start with an analogy. At a recent 'celebrity' event one of the many assembled paparazzi had the bright idea of furnishing himself with a step-ladder to gain a better view. Very quickly all other fellow photographers scrambled to equip themselves with step-ladders, too, thereby achieving the suboptimal equilibrium of everybody retaining their relative position but at a cost. The statement that in the Internet era there exist more efficient mechanisms to disseminate peer-reviewed scholarly and scientific work than reliance on a system of toll-access, while being objectively correct, is as useful as a policy prescription as the suggestion to the paparazzi of my example that they should come down from their inefficient step-ladders. No paparazzo would and should follow the advice (in itself very sound and well intentioned) unless he/she can assure him/herself that all others would follow suit.

It could be argued (correctly, as it turns out) that my analogy is imperfect in so far as, assuming that somehow all paparazzi could be persuaded to get rid of their step-ladders, each one of them would still have the incentive to acquire one, as it would give him/her an advantage over his/her rivals. In the case of research publishing, on the other hand, if somehow all researchers decided to move to the promised land of toll-free open access, there would be no incentive to restore inefficient toll barriers. While correct, this argument fails to grasp the deep-rooted nature of the problem of the *transition* to open access, which can be summarised in a single word: *coordination*.

The devastating effects of the *coordination trap* were made dramatically evident by the Public Library of Science debacle. Very briefly, what happened was that nearly 34 000 scientists (mainly from the bio-medical sciences) signed a petition-ultimatum whereby each signatory threatened not to submit their work for peer review to any journal that did not undertake to grant (delayed) open access to the published articles.<sup>6</sup> The initiative received wide media coverage and, as any economist would have predicted, ended in a humiliating retreat: publishers called the scientists' bluff and, when the threatened deadline arrived, the great majority of the signatories meekly backed down and duly submitted their work to non-open-access publishers.<sup>7</sup>

The PLoS story shows that the existence of a better and feasible alternative (open access) to the status quo (toll-access) in itself does not imply that the transition to the superior equilibrium is feasible. Indeed, unless there exist credible mechanisms whereby individuals can commit (that is, force) themselves to the better alternative, the tyranny of the status quo will prevail.<sup>8</sup>

In conclusion, if one looks at the range of open-access journals one cannot but be struck by two overwhelming facts: (i) in spite of being probably the most efficient way of disseminating peer-reviewed research, open-access journals constitute a tiny minority of the universe of refereed publications; and (ii) even within the minority of open-access journals, there are significant differences across disciplines.

## 5 SELF-ARCHIVING: A PANACEA?

This leaves self-archiving of *the accepted version of the article* as the only potentially feasible route to open access. By examining the logic of self-ar-

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<sup>6</sup> 'we pledge that, beginning in September 2001, we will publish in, edit or review for, and personally subscribe to only those scholarly and scientific journals that have agreed to grant unrestricted free distribution rights to any and all original research reports that they have published, through PubMed Central and similar online public resources, within 6 months of their initial publication date.' Predictably, the PLoS website ([www.plos.org](http://www.plos.org)) does not dwell on the failure of its Open Letter.

<sup>7</sup> I was told by a leading open-access publisher in biomedical sciences that out of the 34 000 PLoS signatories the number of scientists who followed through with their 'threat' and did submit their work to open-access publishers instead could be counted on the fingers of *one* hand.

<sup>8</sup> The PLoS story has an interesting coda: some of its leading lights, following the failure of the petition, decided to become open-access publishers themselves and, thanks to a \$10m donation from a charitable foundation, have launched a handful of open-access journals. Unfortunately, in the absence of philanthropists willing to donate billions of dollars, this is not a template that can be reproduced for all scholarly and scientific communication.

chiving, it may be possible to identify some of the fundamental problems of the relationship between the public domain and scientific and scholarly communication.

Another analogy may be useful here. In the UK telephony market (and probably elsewhere in the world) there are companies that offer completely free calls to fellow subscribers, that is, if you subscribe to company A, all your calls to all of company A's other subscribers are gratis.

Now consider the following statement: 'if all potential customers subscribe to company A, then the free-calls-to-fellow-subscribers outcome is not a sustainable equilibrium'. This statement is not a hypothesis nor does it require empirical corroboration. It is the only logical conclusion from the premises. I would argue that precisely the same argument applies to the statement, repeated ad nauseam by proponents of the self-archiving route to open access, that '100 per cent open access can be achieved overnight by all researchers self-archiving all their accepted articles'.

I would argue that one of the main reasons why economists (and possibly the majority of researchers, except for some sub-disciplines such as high-energy physics and some fields of mathematics and computer science) are reluctant self-archivers is that they regard the above strategy as inherently self-defeating as a long-term policy for the attainment of open access.

The reason is obvious: the accepted (as yet unpublished) version of an article, once self-archived in a repository whence it can be searched and retrieved, is at least as good a substitute for the published article in so far as its content is identical but it is, by definition, made available *before* the published version, and therefore, being available at a zero price, necessarily drives the *economic* price of the published article to zero, thereby making publication unsustainable (even if the article is priced *at cost*). The argument, however, is made subtler and more complicated by the fact that articles are not published individually but are bundled into journal issues, which in turn are bundled into annual subscriptions, which in turn are bundled into multi-journal 'packages'.

This complication explains the apparently paradoxical unholy alliance between the most radical proponents of self-archiving as a route to open access, on one side, and some of the most rapaciously commercial multinational publishers, on the other. 'Self-archivangelists' rank publishers according to how 'permissive' the latter's policies are in terms of allowing authors to self-archive the first submission (good), the final submission (very good) or the published version (divine). The sad irony is that self-archivangelists proudly announce a victory for the cause if a publisher joins the list of the 'good' guys or moves up the scale of self-archiving permissiveness, blissfully unaware that this, far from being a sign of success for the self-archiving cause, is evidence that academic journal publishers rightly perceive the self-archiving strategy as inherently unsustainable, thereby making a 'liberal' stance on their part not only a cheap PR



stunt but also a diversionary tactic to prevent or delay badly needed regulation of their very profitable industry.<sup>9</sup>

The relationship between self-archiving as a dissemination strategy on the one hand and academic economists *as a profession* on the other is, in my view, a good example of how complex and subtle is the nexus between the public domain and scientists and scholars more generally.

As we have seen above, economists have been among the first and most enthusiastic self-archivers as far as early (that is, *as yet un-refereed*) research work is concerned. Therefore the commonly advanced suggestion that the lack of a self-archiving ‘culture’ may be due to inertia, lack of technical skills and so on clearly does not apply to economics as a discipline. So, why is it that economists, who, by training and inclination, ought to be keen on exploiting the benefits of cost-free wider dissemination, have shown so far no strong inclination to adopt *as a professional norm* the policy of self-archiving accepted articles (refereed pre-prints)?

The main reason, as I argued above, is that self-archiving is considered a policy for the dissemination of refereed research that cannot be sustained in the long term. It is not at all surprising that a discipline that, for good or ill, relies almost exclusively on the refereeing customs and ethos of a handful of journals at the top of a strictly codified hierarchical publication structure as its mechanism for apportioning recognition and prestige should promote behaviour that does not threaten in any way the long-term survival of ‘the ranked journal’. Indeed, as we are going to see in the next section, this ‘protectionism’ extends to other areas of the relationship between academia and the public domain.

## 6 OPEN SCIENCE, THE PUBLIC DOMAIN, AND ECONOMICS AS A DISCIPLINE

In line with the strategy deployed in this chapter, namely to try to extrapolate from a specific case some conclusions of potentially more general applicability, in this section I wish to focus on one particular aspect of the relationship between open science<sup>10</sup> and the public domain. I would argue that the very specific

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<sup>9</sup> For evidence and analysis of the highly inefficient but extremely profitable market of academic journals in economics, see La Manna, M. (2003), ‘The Economics of Publishing and the Publishing of Economics’, *Library Review*, 52 (1), 18–28.

<sup>10</sup> One of the main preoccupations of open science advocates (especially in the biomedical sciences) is the free access to datasets on which research papers are based. Although in economics, too, researchers tend to be rather protective of any datasets they may have collected, often at some considerable cost, the editorial policies of journals are moving in an open-science direction. The top-ranked *American Economic Review*,

case of what we might call ‘refereeing technology’ in economics journals is worth exploring.

It could be argued that the advent of the Internet provided not only the means for wider and faster dissemination for traditionally refereed research output but also the opportunity of improving the very process of peer review and its relationship with the academe and the general public.

At a rather superficial level the Internet allows journal editors to make the pre-Web system of refereeing more efficient by replacing paper transactions with online communication, with obvious gains in terms of speed, ease of retrieval and so on. In this respect, it is somewhat surprising to note that economics journals have been singularly slow in adopting even simple and well-established best practices such as the electronic submission of manuscripts. In spite of the wide availability of both commercial and open-source software for the electronic reviewing of manuscripts, again economics journals have not been at the forefront. Perhaps this slow start could be ascribed to the general phenomenon of institutional inertia that surrounds the adoption of new technology and indeed there are some encouraging signs that at long last economics journals are slowly joining the 21st century.

There is, however, one important aspect of the quality-control process where economics shows no sign whatsoever of using *new* opportunities offered by online technology – I refer here to wider and more innovative peer review on the one hand and to interactions with the readership on the other.

Although economics is by no means an isolated phenomenon in its rejection of these new technological opportunities (which is common to most scientific disciplines), one would have expected economics journals to avail themselves of any available chance both to enhance refereeing as a process and to raise the status of referees. What are the innovations in refereeing that the Internet has made possible and that are apparently steadfastly eschewed by journals? I shall list briefly some of them:

- especially in disciplines such as economics where (repeated) resubmissions are the norm, the value added by peer review to the quality of the published article can be enhanced by allowing direct (but anonymous) contact between author and referees;
- online refereeing offers a simple and effective solution to the long-standing problem of unbundling *assessment* from *evaluation*. Let me explain. It could be argued that referees perform two main tasks: (i) they assess

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for example, explicitly states that it will publish papers ‘only if the data used in the analysis are clearly and precisely documented and are readily available to any researcher for purposes of replication’.

submissions in terms of a number of criteria, such as originality, correctness, technical/methodological advance and so on and (ii) they evaluate submissions by assigning weights to, and trade-offs between, various assessment criteria. It is perfectly possible (and in the case of economics very likely<sup>11</sup>) that some referees, while very scrupulous and accurate in their assessment, may apply the 'wrong' criteria, that is, may attach excessive importance to certain criteria to the detriment of others. Online submission assessment/evaluation forms make it possible to distinguish between the two tasks (assessment and evaluation), thereby making better use of referees' reports.

- online refereeing software makes very easy the relative and absolute *evaluation of referees*, thereby raising the status of referees themselves. Editors who wished to signal to the profession the performance of their outstanding referees (according to a set of publicly announced criteria) could easily do so.

More importantly, appropriate use of online technology could turn journals from one-directional documents into interactive knowledge exchanges.<sup>12</sup> What I mean here is that instead of viewing the published article as the terminal point of a uni-directional transfer from the author, through the review process, to the reader, one could envisage an interactive process whereby readers can interact both with the author *and* with the referees, thereby turning the published article into an intermediate stage in the process of knowledge exchange. To consider but a simple example, readers could provide their own assessment and evaluation, using the same online forms designed for referees, thereby providing potentially very useful feedback on the quality (and bias, if any) of both the article and the refereeing process.

In my experience of promoting more efficient publishing modes to the economics profession, I have come across not just indifference to the suggestion of moving economics towards more innovative refereeing and interaction between authors, referees and readers but mainly outright *hostility*, especially from the community of past and current editors of both well-established and new journals. The reason, I would surmise, is yet again the fact that the availability of technologies and practices that could be regarded as superior alternatives to the status quo is no guarantee that such technologies and practices will be adopted, unless they fit the ethos and incentives of the would-be adopters: the

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<sup>11</sup> See, for example, Ellison, G., 'Evolving Standards', op. cit.

<sup>12</sup> See La Manna, M. and Young, J. (2002), 'The Electronic Society for Social Scientists: from Journals as Documents to Journals as Knowledge Exchanges', *Interlending and Document Supply*, 30 (4), 178–82.

desire to protect a peer-review mechanism grounded on the journal as the ‘focus of energy’ and ‘nexus of interactions’ may and does easily turn into a justification for resisting *any* efficiency-enhancing reform by the stakeholders in the status quo.

## 7 CONCLUDING REMARKS ON OPEN SCIENCE/ SOURCE/ACCESS AND SCHOLARLY COMMUNICATIONS

This chapter has taken as its point of departure the remarkable and yet largely neglected similarities between the open source movement and the process of peer review of research output.<sup>13</sup> In both cases, (i) substantial amounts of time and effort are devoted to activities yielding no direct financial reward; (ii) the main motivation is peer recognition and prestige; (iii) the outcome is a *joint* production of the original authors/developers and their referees/fellow developers; (iv) both producers and users have a common interest in the widest dissemination of new ideas.

In view of these remarkable similarities, then, how can one explain the substantial difference between the success of open source in the software market and the failure (or, more charitably, the lack of progress) in achieving open access to scientific and scholarly research output?

The answer, I have argued, is two-fold. First, there is no single homogeneous ‘research output’: a typical piece of research goes through different phases where the incentives of the parties involved may be different and it may be perfectly individually rational to combine open access at one stage (such as the working paper stage) and toll-access at a different stage (the published article) if the latter is more congruous with the aims of the stakeholders (prestige for the authors, preservation of a hierarchical mechanism of peer review and ‘gate-keeping’). Secondly, the relationship between open access and ‘the researchers’ varies according to the *specific role* played by the players in the process who are producers, assessors and consumers. Again, there are important trade-offs here: while the researcher as consumer would clearly benefit from having toll-free access to all published articles, the researcher as producer and assessor may perceive open access *to the published output* as a threat to the viability of the

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<sup>13</sup> The otherwise exhaustive analysis of ‘commons-based peer production’ by Y. Benkler (2002), ‘Coase’s Penguin, or, Linux and *The Nature of the Firm*’, *Yale Law Journal*, 112, 369–446, hardly mentions the similarities between open source and academic peer review. Similarly, Lerner and Tirole (2004) also treat the relationship between open source and academia in a paragraph or two.

(not cost-free) process of peer review, namely the mechanism that guarantees the evaluation and eventually certifies the prestige of research.

This ‘unpacking’ both of research outputs and of multi-role researchers renders policy-making more complex to design and difficult to implement but all the more necessary and urgent: precisely because individual players may have conflicting incentives and, more importantly, because there exists a gap between individual and collective incentives, there is ample scope for welfare-enhancing government intervention. Such intervention is likely to be discipline-specific and would involve a subtle mixture of competition policy, advocacy, training, changes in grant-awarding rules, and so on.