# What do journals do? – Voluntary public goods and the doomsday of commercial science publishing

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## Abstract

Commercial (and non-commercial) science publishing has evolved as a solution to a number of problems in the market for research results. It has reduced transaction costs by bringing together authors and readers, which is just the simple advantage of market intermediaries. It has delivered added value to readers by filtering out bad work. It has added value to authors by delivering signals of high quality work. It has added value by sorting, relieving readers from the necessity to identify relevant work in some field of interest. And it has contributed to the value of published work by delivering guidance from reviewers to authors. But technological changes already have and will continue to erase the value of these services. These services can be provided in much better quality and at much lower costs via open access science networks like SSRN. All we need to make this work are some simple technical improvements and a few new but simple modes of peer interaction. My conjecture is that commercial science publishing will not survive for more than a couple of years.

## The journal technology

I started my inquiry into this topic by asking my old friend Google: "What do journals do?" I got no answer! Then I asked his well educated daughter Google Scholar the same question. I got an answer. After asking her to put me through, I had presumably a scientific paper at the other end of the line. The title of this paper was: "What do journals do?" It has been published in a journal called "Progress in Orthodontics". The paper was not directly accessible because the journal is not an open access one. So I had to pay to get the paper. I did. The first lesson I learned on what journals do was: they do let you pay. And they let you pay serious amounts of money.[2] After paying, what I actually got was not a paper, it was a 2-page editorial. Two passages in this editorial seemed noteworthy: "Journals separate truth from facts…" and "Journal articles are reports of scientific studies conducted with rigorous standards as judged

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[2] Sample (2012).

through the peer-review process".[3] I have some doubts about the general applicability of these claims, and I feel that there is something missing. So here is what I think what journals do and why they will fail to continue doing what they do.

**What journals actually do**

Science publishers act as middlemen between consumers (i.e. readers) and producers (i.e. authors) of research results. Therefore, science publishing is a distributive trade. As market intermediaries they reduce transaction costs.

Beyond trading, publishers utilize an incoming goods inspection technology called "peer review". This technology is employed to ensure the quality of products delivered to consumers. Comparable technologies are used in many other industries as well. A labor market example is temporary employment companies that test workers and then "sell" them to other firms. This screening technology limits a reader's risk to spend time on reading low quality papers or even papers that present faked results. Quality screening is therefore not only about filtering out bad work but also about verification of good work. Reduction of transaction costs and the guaranty of high quality can be assumed to be the main reasons why readers would be prepared to pay for these services.

But there must be more to it. This is because authors have much work with preparing their papers, obeying the formatting rules, dealing with editors and reviewers and eventually rewriting their papers over and over again. So what motivates authors to take part in this market? Surely not money, as authors get typically not paid by publishers and eventually even have to pay submission fees. The most important reason can be assumed to be the signaling value of having a paper accepted by a good journal. If authors get higher pay or better jobs after their papers have been accepted, submitting papers is an investment that may be worthwhile even if there is no direct compensation and even if submission fees have to be paid.

Still another reason for the existence of journals may be that the reviewer process offers guidance on how to improve the quality of submitted papers. That is, journals themselves take part in the production of scientific results.

[3] Tuncay (2001)

Since all these services cannot be delivered without incurring expenditures, journals have to charge a price. While the monetary price of journals is paid by the readers, authors also pay a price by delivering costless inputs to the journal industry and eventually by paying submission fees. And of course, reviewers pay a price by providing costless reviews.

## A brief history and a bad outlook

The technologies of scientific journals have been developed a few hundred years ago with the French "Journal des scavans" and the English "Philosophical Transactions of the Royal Society" starting in 1665.[4] The industry has provided a solution for the couple of problems mentioned above and therefore was able to charge prices. However, we actually see the advent of alternative technologies that will be able to solve the problems better at lower costs. One of these innovations is promoted by science publishers themselves. It is the switch to electronic publishing. By electronic publishing, distribution becomes much cheaper and there are huge savings on physical assets like paper, on energy in production, and transportation of printed material. Electronic publishing drives marginal costs of serving another customer down to zero. By switching to electronic publishing, firms in the industry cannibalize their own returns from physical publishing. Of course, this is not a wrong decision, because firms in the industry cannot escape from this development driven by a new technology. If the market would become fully competitive, prices would fall to zero and the industry would vanish. If publishers are to survive, they must find a way to charge a markup on marginal costs. In what follows, I will argue that they will not find that way. Therefore, commercial science publishers will vanish and this will happen at short notice. The reasons for this conclusion are as follows.

## Voluntary public goods

Public goods are defined by two properties.[5] One property is non-rivalry in consumption. This means that consumption of the good by agent A does not reduce the quantity available for agent B. If scientific work is published electronically, reading a paper by A does not reduce the availability of that paper for agent B. With regard to the non-rivalry property, electronically published papers fulfill this criterion. The other property of public goods is that

[4] See http://en.wikipedia.org/wiki/Scientific_journal, December 10th, 2012.
[5] The definition of a public good is taken from: http://en.wikipedia.org/wiki/Public_good, November 15th, 2012

agent A cannot be excluded from consumption even if A does not pay for that good. In physical as well as electronic publishing, readers can be excluded from consumption if they do not pay. However, technologies of exclusion are not perfect. Printed journal papers can be copied and distributed at marginal costs only slightly above zero. Electronically published papers can be copied and distributed at zero marginal cost. To preserve positive returns, different technologies of exclusion from consumption have evolved. There are at least two technologies to increase the price of making and distributing copies.

One technology is the definition of copyrights, prescribing that those violating the copyrights of others get punished. As far as can be seen, the copyright technology does not perform well. The music industry is a good example for the failure of copyrights. Music gets copied and illegally distributed around the world and the success of prosecution is rather limited. The most it has achieved yet is to turn millions of children into criminals.

Another technology is to increase the price of copies. For example, firms selling photocopiers in Germany must pay fees to collecting agencies that pass this money to copyright holders. Since the copyright markup on prices of copiers is low, the marginal costs of producing a copy of some paper are still almost zero. The same procedure of copyright markups is employed for computers as well. But these markups also are no significant hindrance of copying electronically. What is more, the markups also hit people who do not copy and thereby these markups produce a negative externality.

The conclusion from this is that technologies of exclusion from consumption in electronic publishing do not work well yet. I doubt that they ever will unless big brother would always watch everything. Maybe in some distant Orwell-type future, there will be smoky bars around the world where people meet and ask the barman where to get the right stuff. They will find their ways to circumvent science prohibition, although sometimes they will get only dangerously blended cannabeconomics.

There is an ongoing battle between those who make and distribute illegal copies and those who want to stop the former. Up to now, the former seem to have the first mover advantage on their side. In effect, given the technological innovations in electronic publishing, papers already have turned into public goods. The investments needed to keep them private will not be regained by the prices that can be charged.

But the business outlook for commercial science publishers is even worse. This is true since even an effective technology to keep the papers private would presumably not help. The

reason for this conjecture can best be explained by comparing musicians and scientists. Musicians live, at least partially, from selling their music, while scientists do not live from selling their papers. Musicians have a fundamental interest in keeping their music private which scientists have not. Scientists want to share their ideas with other scientists. They do not want to exclude their colleagues from the consumption of their work. Since charging a price excludes others, scientists do not want prices to be charged. And they do not want to pay for the ideas of others because they know that others also do not want them to be excluded. In what follows, I will label goods that can be delivered in a non-rivalry way and that are delivered by producers that do not want prices to be charged as "voluntary public goods" (VPGs). I will not go into the details of regulation of VPGs here, but it seems clear to me that some VPGs may deserve public subsidization at least as much as some other public goods. A wonderful example of such a VPG that in my eyes deserves subsidization is Wikipedia. This is especially true in an economic context where Billions of Dollars of public funds are abused to subsidize purely private goods with highly questionable benefits.

So if science publishing is a VPG, the situation of science publishers is much worse than the situation of music publishers. Neither authors, nor readers nor reviewers make money, it's only the publishers that do. Besides tradition, what has kept the industry alive up to now despite its adverse effects on authors, readers, and reviewers?

**Results Production**

Journals take part in the production of scientific results. They do this by providing peer reviews that offer guidance on how to improve a paper. But this guidance is not limited to the conventional reviewer process. Guidance can also be given by readers of early drafts. Improved papers can then be published at zero marginal costs. This improvement can continue forever. Since those formerly involved as reviewers can still continue to give guidance, nothing will be lost. But additional comments can be obtained from the scientific community. Taken together, the part that journals play in the production process of science is rather unimportant. The value added by the conventional reviewer process is limited to the contribution of typically two reviewers. It is not reasonable to assume that this procedure gives all the guidance that is possible. An open access science network can offer much better opportunities for the scientific community to take part in the production of results.

## Filtering

The reviewer process is also an incoming goods inspection technology. Why should such a technology be employed? One obvious reason is to protect consumers from buying low quality products. Filtering out is especially important if delivering bad quality products significantly harms consumers. If the damage done by bad products is rather low, incoming goods inspection may be too expensive.

Over the last decades, one kind of damage done by delivering bad papers to readers has significantly declined. With the advent of electronic publishing, there is no more need for physical printing and physical distribution. Since readers do not have to pay anymore for the use of physical assets, this kind of damage done by delivering a bad paper has declined.

Still, there remains an opportunity cost of time that would be wasted on reading bad papers. And since the marginal costs of delivering a bad paper electronically are zero, thousands of bad papers could eventually reach each reader. Therefore, filtering is still important and it may have become even more important than before. But it is rather unlikely that we need journals just to do the filtering. For example, there are billions of documents in the internet, most of them completely irrelevant to most of us. For example, there are horrible sites on breeding hamsters but nonetheless these sites do not foist opportunity costs of time on my internet searches. I obviously do not need reviewers to protect me from hamster sites. So I guess I neither need reviewers to protect me from bad papers. All I need are some simple electronic and peer filtering technologies. I will discuss this conjecture in the next section in more detail in combination with the signaling value of filtering.

## Signaling[6]

Filtering out bad work has another positive side effect. It not only protects readers but it delivers a signal of high quality to authors. I guess this may be the main reason for the survival of journals up to now. As long as an A-journal acceptance is worth more than the cost of producing the paper and the losses incurred by exclusion of colleagues from consumption, authors have sufficient incentives to keep publishing in journals. But this

[6] The seminal paper on signaling is Spence (1973). For a brief definition of signaling see: http://en.wikipedia.org/wiki/Signalling_(economics), 15th November, 2012

signaling value will vanish. This is true since there are cheaper opportunities to signal high quality. And over time, even more precise signaling technologies will be developed, that will outperform the signaling value of a journal publication.

One technology already in heavy use is citation counting. Being cited is interpreted as a signal of high quality. But since for example any SSRN-paper can be cited as any journal paper, the signaling advantage of journals has already diminished considerably. If an author wants to be cited, publishing via SSRN is a viable strategy. But a science network like SSRN even delivers at least on highly valuable advantage over journal publications with respect to citations: Time! If an author wants to be cited and submits a paper to a journal, the time passing by until publication may be years. And it may take another couple of years until papers are published in other journals that cite the original paper. With SSRN, authors can publish immediately and others can immediately react. This advantage is especially relevant for young researches that need citations to signal the value of their work. Some journals have already reacted to this time pressure by publishing papers electronically long before the printed issues reach the market.

Of course, counting citations does not deliver a perfect signal as bad work may also be cited heavily and good work may be a victim of ignorance. What is more, citations may be subject to manipulation, e.g. in the form of citation cartels. But there are other signaling technologies that can complement citation counts. One of these complements in electronic publishing is the number of downloads. A high number of downloads indicates at least that the paper is interesting.

The download signal is also far from perfect. This is true because downloads are also subject to easy manipulation so far. Authors can use simple computer software to download their own papers and papers of friends over and over again at zero marginal costs. But this problem can be overcome by registration. If only downloads of registered members are counted as downloads, and only one download per member is counted, the impact of an automated download procedure is already eliminated. It is still possible to form download and citation cartels, but it is hard to see how such cartels should be able to beat really good work. What is more, cartels formed by registered members can easily be detected, further limiting opportunities of manipulation.

Remains another problem of the download signal: forwarding. If people download a paper and then send it around to colleagues, a paper may be read much more often than it is

downloaded. Right now, I have no idea how to control for this problem, but maybe it would be sufficient if we just promise each other not to send downloaded papers around but only the link where the paper can be found. So if you like this paper of mine, please do not forward it, forward the link only.

There is an intermediate procedure between counting citations and downloads on the one hand, and giving detailed advice in a peer review process on the other. That is rating. If you buy goods via internet, most sites offer opportunities to rate the quality of products delivered. Whether you buy books from Amazon or you book a hotel or a journey, you are given the opportunity to rate what you have bought. Lots of sites even actively seek those ratings from their customers. So far, these ratings are also subject to manipulation. But within a scientific community, there are easy ways to handicap manipulation. First of all, the opportunity to rate papers can be limited to registered members. As the status, e.g. affiliation and position, of members can be quite easily verified, the opportunities of manipulation are already severely limited. And even if someone manages to infiltrate the system by bringing his grandma in, the effect will be negligible since the GPC-figure (Grandmas Per Capita) is typically rather low in OECD countries. In fact, rating has already arrived at scientific networks. For example, at www.getcited.org you can rate papers, but only if you are a registered member.

Can manipulation-free citation counts, download statistics, and ratings replace the signaling value of the journals' reviewer processes? Maybe not exactly, as good reviewers invest their time to explicitly evaluate the content of the paper. And they provide a detailed account of the merits and shortcomings of the work at hand. But the reviewer process has some severe shortcomings also.

The first problem is the potential abuse of reviewers. Reviewers eventually read and evaluate papers that they would not have read outside the reviewer process. And people that would read the papers anyway are excluded from the process. They can still comment on the paper in their own papers later on, but this may only produce marginal contributions and citations. It could be much more valuable for science to have the opportunity to give guidance, comments and critique on an already existing paper than to produce a new one that contains only such guidance, comments and critique. Of course, there is an incentive problem. Those that give guidance directly instead of publishing this guidance in their own papers are not rewarded under the current system. But this incentive problem could eventually be overcome by evaluation procedures that acknowledge valuable reviewers. My conclusion is that open peer reviews by a group of those interested in the topic may be much more valuable than reviews

given by two anonymous reviewers that waste their time on reading things they would not otherwise have read. What is more: authors also get their papers eventually reviewed by people they do not want as reviewers but can't help it. This implies that the allocation of papers to reviewers is likely not a stable allocation.[7]

The signaling value of a journal acceptance depends, over the long run, on the journal's ability to attract and identify good papers. But identification is not done by journals, it is done by reviewers. This implies that the signaling value of the journal is a weighted average of the quality of its reviewers. An A-journal can only survive as an A-journal as long as it is able to employ A-reviewers that really can distinguish between good and bad work. In effect, the A-status of a journal is actually the A-status of its reviewers. The question is therefore why this transfer of status from reviewers to journals should be necessary. Why shouldn't papers be A-reviewer papers instead of A-journal papers? If we switch from the journal status to the reviewer status, the precision of the signal would even increase since journals can only provide the signal value of its average reviewer quality. A direct reviewer rating would therefore improve the precision of the signal. Just assume that one of your papers is anonymously reviewed by a Nobel laureate who loves it. Would you prefer that the reviewer is disguised by a journal or would you prefer that anyone comes to know that a Nobel laureate loves your work?

Of course, if reviewers have strong incentives to stay anonymous, an open reviewer process might not work. But I doubt that good reviewers have such incentives. It is the bad ones that might and should have those incentives. But crowding out the latter by an open reviewer process will only improve the situation. What is more, the anonymity of reviewers has become an anachronism anyway because the double-blind procedure itself has become one. My own experience from acting as a reviewer is clear: In about 70% of all cases I knew who has written the papers. These papers have been presented at conferences and they have been published as discussion papers before submission. From this experience I know that reviewers of my papers also know that these papers are mine. But then I can expect them to publicly stick to what they have to say about my work. If they don't want to do that, I don't want them as reviewers. In the preceding paragraph I discussed the incentive problems of offering open guidance instead of making suggestions for improvement disguised as a new paper. If

[7] The Nobel prize in Economics 2012 was awarded to Alwin E. Roth and Lloyd S. Shapley for their concept of "stable allocations". For an introduction to stable allocations see: Economic Sciences Prize Committee (2012).

someone gives his reviews openly, it should be easily possible to reward good reviews. Such a procedure of recognition should also help to diminish the incentives to stay anonymous.

I have to admit that the experiences with open reviewer mechanisms so far are rather discouraging.[8] As Michael Nielsen[9] writes:

*"The problem all these sites have is that while thoughtful commentary on scientific papers is certainly useful for other scientists, there are few incentives for people to write such comments. Why write a comment when you could be doing something more "useful", like writing a paper or a grant? Furthermore, if you publicly criticize someone's paper, there's a chance that that person may be an anonymous referee in a position to scuttle your next paper or grant application."*

But these experiences are based on two premises that are subject to possible changes. First of all, if people are just given the opportunity to deliver an open review, the incentives to give one may not be sufficient. But being members of a scientific community we can force ourselves to deliver reviews. For example, if you do not provide a number of reviews that matches at least the number of your submissions, you could be excluded from further submissions. It should be possible to find a combination of incentives and punishments that make open peer reviews work.

The other problem, namely that people whose papers you have openly criticized, may become anonymous referees of your papers rests on the premises that anonymous reviewing will survive and that you take care. If you decide to publish all of your work via open access science networks, there will be no anonymous reviews that can block your papers. And even if anonymous reviewing of grant applications could be a problem, granting institutions can do at least two things: They can make sure that applicants stay anonymous and they can watch out whether or not the reviewer may be prejudiced.

But my most important objection against this "fear of consequences" argument is: I don't see the difference between reviewing and writing your own papers. If you think that other researchers produce bad work, do you remain silent about it in your own papers? No. Science often enough is a hard battle and it is fought with sharp tongues. My last paper published with

[8] For some examples see: http://p2pfoundation.net/Open_Peer_Review, 14th December 2012
[9] http://p2pfoundation.net/Open_Peer_Review, 14th December 2012

SSRN is titled: "The Managerial Power Approach – A Tautology Revisited".[10] The title speaks for itself. I guess that the proponents of the managerial power approach will not be especially happy about the things I say about their work. Nor will I be especially happy about what they will presumably answer. But obviously none of us has enough fear to keep quiet. Since we discuss our peers' papers in our own papers we already have open peer reviews and make heavy use of it. Therefore, the "fear of consequences" argument is not valid. The reason that explicit open peer reviewing has not worked well so far is just a lack of incentives. This problem can be solved.

When we comment on other papers in our own ones, could we make better use of our comments? I think the answer must be yes. We could develop technical standards that allow for something that could be labeled "automated short reviewing". Whenever you comment a colleague's paper in your own work, this comment could be extracted and made available as a short review. We could also introduce rated citations. Each time you cite another paper, you mark that citation with a plus or a minus sign to indicate your opinion about the cited paper. These types of information could also be used as signals, further diminishing the need for anonymous peer reviews from journals.

Since the signaling value of having an A-journal acceptance may be quite huge, the A-journals are bombarded with papers. If we switch to an A-reviewer technology, the reviewers would be bombarded instead. But since the A-journals react to their bombardment by bombarding reviewers, there would be no significant changes due to the switch. What is more, an open science network could accommodate prospective reviewers with some simple or more elaborate filtering technologies. The simplest would be to allow reviewer submissions only via some internet site where the reviewer sets a maximum of possible submissions she is willing to review. Of course, the filtering mechanism can be improved by incorporating information on topics, authors, and so on.

How do we know that a journal is an A-journal? The basic procedure to answer this question is to compute impact factors. Actually, I think the invention of journal impact factors is one of the most ridiculous things that scientists have done to themselves ever. The impact factor has absolutely no effect on the quality of a paper. I agree that there is or might be a correlation between the quality of papers and the impact factors of the journals that publish those papers. But since we are able to evaluate each single paper on its own, I cannot see why we should

[10] Winter, Stefan and Michels, Philip, The Managerial Power Approach – A Tautology Revisited (November 1, 2012). Available at SSRN: http://ssrn.com/abstract=2179036 or http://dx.doi.org/10.2139/ssrn.2179036

employ correlations instead of causal inferences about paper quality. The impact factor of journals has an impact factor of exactly zero on my live.

There is another source of information we completely ignore so far. When we use peer reviews, we seem to forget that we are peers ourselves. So why don't we expect authors to evaluate their own papers? I would greatly appreciate to have some information on what authors really think about their papers. How compares the present paper to what the author has done so far? How does it compare to the work of others? For example, we could ask authors to rate their papers on a given rating scale. For example, assume the following PSES (paper self evaluation scale):

PSES:

1 = "complete nonsense"

2 = "marginal contribution"

3 = "work of a craftsman"

4 = "art"

5 = "fine art"

6 = "Nobel prize level"

7 = "Above"

Now assume that I tell you that I have written and published lots of category 1 papers. But this paper at hand is in my opinion a 5+. Does this self appraisal have any value for you? I think that it must have. If you think that this paper is complete nonsense, you know that my benchmark is another one than yours. But if you agree, you know you should read my other 5+ papers also, but not my 1- papers. I'm your peer and if I offer a review opinion on my work, this opinion is a peer review.

Stevan Harnad[11] writes:

"*Peer review is a quality-control and certification (QC/C) filter necessitated by the vast scale of learned research today. Without it, no one would know where to start reading in the welter of new work reported every day, nor what was worth reading, and believing, and trying to build one's own further research upon.*"

[11] Harnad, (1999).

I agree. Our future will not be a future without peer reviews. But it is hard to see why we should need journals anymore to organize these reviews. And we definitely do not need commercial journals for this task.

**Product Bundling**

Can product bundling save commercial publishers? Product bundling refers to a strategy where products are not sold individually but rather in combination with other products. The music industry has recently started with product bundling. There are three main sources of income in the music industry. It is income from the sales of recorded music, income from ticket sales, and income from merchandising.[12] In former times, labels contracted with artists only for a share of returns from sales of recorded music. But since these returns have fallen considerably due to illegal copying, labels ran into trouble. The new deal is the 360° contract, whereby labels also contract for a share of revenues from ticket sales and a share of revenues from merchandising. Although superstars were rather reluctant to accept these new deals, young talents are more receptive. Still another resort for labels was to simply buy artist-management agencies which also helped to get a share from merchandising and ticket sales. If all revenues cumulate in one hand, the advantages of pricing product bundles can be exploited. Recorded music eventually is given away for free as a promotion tool, promoting the sales of tickets and merchandising. At the same time, live performances have lost their potential to trigger additional revenues from sales of recorded music, so there is no more reason to subsidize ticket prices. Accordingly, ticket prices have climbed considerably over the last decade. All in all, the music industry still has a business model that could work.

But this route is not accessible for science publishers. Although scientists meet for live performances once in a while, typically called conferences, nobody is ready to pay high prices just to hear what you have already read or will be able to read soon. You do not attend a conference because hearing what you already know would make you happy. But if it doesn´t make you happy, you will not pay.

Merchandising will also not work. After reading a paper I certainly will not buy the author's portrait printed on a black satin pillow case. And a leaflet with photos of the author's wedding will also not be on my shopping list. It is thus not possible to secure the business model of

[12] In what follows with respect to the music industry, my references are: The Economist (2007) and Krueger/Conolly (2005).

science publishers by integration of other sources of income, since these other sources simply do not exist.

**Sorting**

Another service provided by science journals is sorting. If you buy an issue of the Journal of Finance, you will usually find papers on finance. But from my own experience I conclude that the efficiency of this sorting algorithm is rather limited. I cannot remember when I have read a complete issue of any journal, maybe except some special issue. For my taste, journals deliver too many useless byproducts. Sorting simply fails. I'm not prepared to pay a meaningful price for this questionable service. This is especially true since there are much more efficient sorting algorithms already available and these algorithms are much cheaper.

One fine example is Amazon's list of recommended books. If you buy a book, you get a list of books that other people have bought along with the book you are buying. This easily gives you access to something you may be interested in. There is a heated debate in Germany on the dangers of data collection by firms like Amazon. But all the people around me do not care. They simply enjoy that someone does their sorting for them. This joy may even go much deeper. If you transact in a market like the book market, you need guidance because it is impossible to oversee this market. It is not even possible to get only an idea of what is available and what is worthwhile reading. Of course, you can base your decisions on recommendations offered by critics or on lists of bestsellers. While employing these technologies protects your personal data, they do not deliver precise information, because these technologies don't make use of your own tastes. What is more, even if firms like Amazon influence your decisions, you may still be better off as compared with deciding yourself. Maybe some time from now we will simply give firms budgets to fill our refrigerators with the food we prefer without knowing that we do. And they will fill our bookshelves with the books we want to read. We will then rather make meta-decisions about what firms to trust instead of deciding what to buy. The savings in transaction costs would be immense. This idea has already reached the supermarkets, where you find suggestions for complete meals including the recipes and a complete package of the things needed. And people buy it, because it saves much time. Rich people have employed such meta-decision technologies for long. They hire a cook and delegate their decisions what to eat, they hire people to plan their vacations, and they hire people to manage their investment portfolios. So

why shouldn't we hire our colleagues to identify what we should read and why shouldn't they hire us?

Yet another wonderful people driven sorting technology is the invention of the playlists you can find on youtube. I have no clear idea yet whether the concept of a "journal" can survive in any reasonable meaning of the word. But one form I can think of is a "journal" that only produces playlists: "Editors" identify papers of interest for the subscribers of their playlists. Thereby they reduce subscribers' transaction costs of finding relevant material. I can see no reason why this youtube technology should not work with science. We would then have playlists in finance, game theory, econometrics, and so on. We could also have playlists covering highly specialized topics. And we will have handbook playlists where the first entry of the list contains the preface and table of contents. Along with an open reviewer process as described above we could have reviewer playlists that document all the papers a reviewer has reviewed. And of course: What we already have on SSRN is personal playlists. Just click on your author ID and the playlist pops up.

**Journal Failures**

Journals do good things. That is what I have discussed above. The problem with these good things is that they can be done better. But journals do not only do good things, they also do bad things. One bad thing is clear: They charge money. But there are other failures as well.

A thing journals completely fail to deliver is ordering. It is not only relevant to know what there is on some topic of interest. It is also relevant to know in what order these things should be consumed. When we organize a program for students, we do not only select topics, but we present the topics in an ordered fashion. We tell students what to read first and what to read last. By this procedure, we make sure that students can understand what they are reading. A playlist could serve this requirement in that it not only lists what is relevant but also what should be read first. If a good survey is published 20 years after some topic has gained attention, the survey could be put on the playlist's pool position. According to Formula 1 racing results, starting from the pool position seems to have some advantages.

Journals plague their authors with formatting rules. Obeying these rules consumes time and if a paper is rejected, it is typically submitted with another journal and more time is consumed. I do not see how these formatting rules contribute to the ends of science. Once upon a time I

have thought about submitting a paper to an American law review. After reading the formatting rules, I gave up. I simply didn't understand them.[13]

**Impersonal conclusion**

The journal inflation we have suffered over the last decades is nothing more than a market bubble. But along with a bubble there is always a bear hanging around as any grey-haired stock broker can tell you.

The emergence of the commercial publishing industry was a necessary reaction to the problems of transaction costs, quality verification, and the other problems discussed above. But now, better and cheaper technologies to solve these problems are available. With respect to physical publishing, science journals consume scarce natural resources, and they consume the limited budgets of libraries. The industry's electronic products can also be substituted by better products of higher quality. The industry's doomsday is not far away.

**Personal conclusion and recommendations**

It's completely funny that by now science publishers try to sell me the copyrights of my own work so that they can afford to distribute my work for free and still make money. Being an economist, working for free is crazy enough, but additionally paying to get back what I have created myself feels way too strange. Sorry, no deal. Never.

Now, what will I do? I don't know exactly. But I do know what I will not do anymore. I will never submit a paper to any journal again, unless this journal is no journal. Whatever it is, it must be available in electronic form, freely accessible, and is must be fully integrated into a science web like SSRN. I just canceled my job as an editor of a renowned German business journal. And I certainly will never again produce an anonymous review. Of course, I have tenure and I'm free to do all these things. I cannot yet recommend this avenue to young researchers as their job opportunities may depend on having some good journal publications. But maybe in ten years the world will have changed and having a personal list of journal publications will be regarded as something obscene, violating the codex of VPGs. At least that's my dream.

[13] What the … means "Ibid"?

I guess some recommendations have become quite apparent by now. The most important thing that journals do is they provide signals. If we want to get rid of commercial science publishing, we need good signaling technologies. Therefore, I would greatly appreciate the introduction of rating opportunities and the opportunity to provide openly visible, personalized reviews and self appraisals at SSRN. I would also like to be able to "auction" some of my time to review papers whose authors want me to review their work: By such means we should be able to improve the matching of authors and reviewers. We should try to find a stable allocation. I would also appreciate if I would be able to construct playlists of papers and subscribe to the playlists of peers.

Of course, most of what I have written here also applies to non-commercial journals edited by our scientific societies. I think that the time of these journals is over, too. The financial contributions to our scientific societies should better be allocated to the development and improvement of electronic science networks. I'm afraid to say that. Breaking up with a tradition of almost 400 years hurts a little.

But there also will be huge rewards: We all know who has the pole position on the commercial publishers' elimination playlist. The start of the race will be price discounts. Keep in touch with the folks in your libraries. If they happily announce that they managed to get huge discounts even from companies that formerly where rather reluctant, sit back, open a bottle of good wine, and enjoy the show down.

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