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4-20-2016

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Poynder, Richard and Gowers, Timothy K.B., "The Open Access Interviews: Sir Timothy Gowers, Mathematician" (2016). *Copyright, Fair Use, Scholarly Communication, etc.*. 18.

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The Open Access Interviews: Sir Timothy Gowers, Mathematician

RICHARD POYNDER

20th April 2016

If you wish to go straight to the Q&A please click [here](#).

After following the open access movement for over fifteen years I have become shy about making predictions. Nevertheless, I do feel OA is approaching a watershed moment.

I say this for two reasons. First, despite the growing number of “green” open access policies being introduced, green OA increasingly looks like a failed strategy, not least because of publisher embargoes, which can delay access for up to 2 years, and sometimes beyond. As [Sander Dekker](#), the Dutch State Secretary for Education, Culture, and Science, [puts it](#), “Access delayed is access denied.”

It is no surprise to me, therefore, that we are already seeing some scepticism about the green OA policy introduced just this month by the Higher Education Council for England ([HEFCE](#)). A blogger at the Open University [wrote](#) recently for instance: “As we deal with the increased deposit in the institutional repository precipitated by the HEFCE Open Access Policy it rather strikes me that our increased activities meeting the policy can be seen as *doing* Open Access but is it really *being* Open Access?”

For this reason, (and others) it seems likely that green OA policies will more and more be complied with by means of gold OA, not green. In fact, the Head of Scholarly Communication at the University of Cambridge Danny Kingsley has [suggested](#) that HEFCE’s green policy will prove to be a Trojan Horse for gold OA.

What could be more symbolic of the fading of green OA than a [tweet](#) posted in March by self-styled “archivangelist” and high-profile green OA advocate Stevan Harnad. Apparently signalling his retirement from OA advocacy, Harnad wrote: “I fought the fight and lost and now I’ve left the #OA arena.”¹

Second, by imposing green embargoes while simultaneously offering [hybrid OA](#), legacy publishers have now effectively co-opted the OA movement. And this has been done in a way that will enable them to continue to control scholarly communication, and to continue making what many believe to be [obscene profits](#) from taxpayers (who ultimately foot the bill).

The king is dead, long live the king!

What is odd is that the triumph of gold, and the emasculation of green, is being facilitated, nay directly encouraged, by governments, research funders and universities – especially those in Europe – who are demonstrating a surprising willingness not just to prioritise gold OA, but to pay publishers their asking price for providing it.

¹ Harnad is however still happy to [comment](#) on OA. Explaining his position in an email to me he said: “OA advocacy is no longer my priority. I’ve had my say. That does not exclude an occasional post or tweet – nor the possibility of a future development that might change things ...”

This is most evident in the policies of [Research Councils UK](#), the [Wellcome Trust](#), the Association of Universities in the Netherlands ([VSNU](#)) and the German Max Planck Society. The latter in particular is currently touting a [proposal](#) to “flip” all subscription journals to an open access model based on expensive pay-to-publish article-processing charges ([APCs](#)).

And it seems the EU may adopt the Max Planck strategy, or a variation thereof. At an Amsterdam meeting organised by the Dutch EU Presidency in April a [Call for Action on Open Science](#) was drafted in which one of the two primary goals proposed is to achieve “full open access for all scientific publications” by 2020.

The only way this timescale could be met would be by utilising some kind of gold OA “flipping” model.

The important point here is that while the emerging European model *could* eventually usher in a world of open access, this would be achieved in a way that suited legacy publishers more than the research community. And in the process it would see the monopolistic power of the so-called [Academic Publishing Oligopoly](#) relocated into the new environment.

What is particularly egregious about this strategy is that it offers no realistic hope of addressing the affordability problem that the research community has been experiencing for the past few decades², and which led many to join the OA movement in the first place. In fact, SpringerNature CEO [Derk Haank](#) told delegates in Amsterdam that gold OA will be more expensive than the subscription model.

The upshot will be that a small group of large multinational commercial publishers will continue to hold the research community to ransom, with the serials crisis replaced by an APC crisis. Instead of struggling to afford access to third-party research, universities will struggle to afford the fees necessary to publish their own research.³

Perhaps most worryingly, rather than reinventing scholarly communication for the networked world, the European model will lock the outdated journal model into the new environment, along with discredited evaluation systems like the [impact factor](#).

The king is dead, long live the king!

Writing on a French mailing list the day after Harnad’s resignation tweet, green OA advocate – and architect of green OA’s poster child, the [Liège open access policy](#) – Bernard Rentier described the current situation in [this way](#): “Le mouvement de l’Open Access a gagné la première bataille (un long combat de 20 ans). Il est en train de perdre la suivante.”

Rentier’s view is that while the OA movement may have won the first battle (convincing the powers-that-be that OA is desirable), it is losing the second battle (solving the affordability problem by wresting back control of scholarly communication from legacy publishers). The OA movement, he argues, has succumbed to [Stockholm Syndrome](#), and as a result is now conspiring with legacy publishers to emasculate cost-effective green OA in favour of a publishing model (gold OA) designed to protect publishers’ profits, rather than seek to increase the efficiency and affordability of science and the communication of science.

² Historically referred to as the [serials crisis](#), a phenomenon that has seen research libraries increasingly unable to afford subscriptions to all the journals their faculty need.

³ A problem that will be that much greater for researchers in the developing world.

When challenged in Amsterdam about the advisability of a gold OA flipping strategy, a Max Planck representative exhorted delegates: “Let’s focus on achieving open access. We can seek to lower prices once we have done that.”

The problem is that such a strategy is not just a very expensive way of doing things, but it will leave publishers in the driving seat. Before it can hope to re-engineer scholarly communication for the network, therefore, the research community will have to try and win back what the OA movement has given away, a task that will be that much harder once publishers have embedded themselves – and their outdated methods – into the new landscape.

In short, before the affordability problem can be solved, and before scholarly communication can be reimagined for the online era, it will be necessary to fight a third battle.

This is why I believe the OA movement is approaching a watershed moment. The question is: who will fight this third battle, and how? Experience suggests that we cannot rely on those who have driven the OA movement thus far to do so.

Needed: Foot soldiers and leader

One remarkable thing about the OA movement is that it has primarily been driven by people other than researchers.

The President of the Royal Netherlands Academy of Arts and Sciences, [José van Dijk](#) drew [attention](#) to this recently when she pointed out that the debate about open access has been mostly about what university administrators, librarians, government, funding organizations and publishers think, not what researchers think, or need. Yet it is researchers who create, quality check, and consume the papers that make up scholarly journals. They are the originators of, and primary audience for, the literature, so should they not have a large say in how scholarly communication develops?

As the financial consequences of gold OA become apparent, and as researchers are confronted with ever more onerous bureaucratic rules (policies) requiring them to make their work OA, however, this is likely to change. Certainly we can see researchers beginning to take more of an interest in the topic, and the signs are that they are not at all happy with the mess and confusion created by the OA movement.

Might we, therefore, see researchers become the foot soldiers of the next battle in the revolution the OA movement began?

We can but hope so, both for the sake of the taxpayer, and for the efficiency of science. Either way, new generals will be needed. What will also be needed is a leader – since another oddity of the OA movement is that it never acquired a leader. And doubtless this power vacuum has helped publishers exploit open access for their own ends.

One obvious leadership candidate, I would submit, is British Mathematician Sir Timothy Gowers.

Gowers is a talented and accomplished researcher who has a great deal of credibility amongst his peers. He is a member of the Royal Society, a [Research Professor](#) at the Department of Pure Mathematics and Mathematical Statistics at the University of Cambridge (where he holds the [Rouse Ball chair](#)), and a Fellow of Trinity College.

He is also the recipient of prestigious prizes and awards. In 1996 Gowers received the Prize of the [European Mathematical Society](#); in 1998 he won the [Fields Medal](#) for research on [functional analysis](#) and [combinatorics](#); and in 2012 he was [knighted](#) by the British monarch for services to mathematics.

Gowers has also demonstrated a keen understanding of the potential of the network to revolutionise the scientific process. In 2009, for instance, he utilised the Web to [test](#) whether mathematicians could be coordinated to crowdsource solutions to specific mathematical problems. In what he dubbed the [Polymath Project](#), Gowers invited colleagues to work together on finding new combinatorial proof to the density version of the [Hales–Jewett theorem](#). Seven weeks later he was able to [announce](#) that the problem was (probably) solved.

The Polymath Project, which has sparked a number of similar initiatives, has been widely cited as an example of open science – a way of working, says Gowers, in which “the entire discovery process, and not just some paper that summarizes (and to a large extent conceals) it at the end, is out in the open.”

As Gowers implies, however, whatever potential open science projects like Polymath might have, at the end of the process researchers are still expected to publish papers in prestigious scholarly journals, journals most likely owned by the Publishing Oligopoly, and that use a publishing form and process that has changed little in the past 350 years.⁴ If science is to realise the full potential of the network, sooner or later someone is going to have to take on the Oligopoly. Currently the OA movement is simply rewarding it by providing a new revenue stream for its legacy model.

Here too Gowers has a worthy track record. In 2012, in protest at the pricing policies of the largest scholarly publisher (Elsevier), Gowers announced that he was no longer prepared to publish in or peer review for Elsevier journals, or to sit on their editorial boards. As he [put it](#), “I am not only going to refuse to have anything to do with Elsevier journals from now on, but I am saying so publicly.”

Gowers’ message struck a chord with his peers, and within days a website had been set up under the banner [Cost of Knowledge](#). Here researchers were invited to signal their commitment to also boycott Elsevier.

But after attracting nearly 16,000 signatures the Cost of Knowledge endeavour has lost steam, and in 2012 Gowers had to conclude that the boycott had not achieved what he had hoped. Writing on his blog, he said: “There were rumblings from the editorial boards of some Elsevier journals, but in the end, while a few individual members of those boards resigned, no board took the more radical step of resigning *en masse* and setting up with a different publisher under a new name (as some journals have done in the past), which would have forced Elsevier to sit up and take more serious notice.

⁴ True, scholarly journals are now published digitally, but in the main this involves little more than shovelling print versions on to the Web. Moreover, many believe the very notion of the journal is now redundant.

In addition, some researchers later reneged on their promise, even [self-proclaimed advocates for open access](#).

But Gowers is not someone to give up. “I have come to the conclusion that if it is not possible to bring about a rapid change to the current system, then the next best thing to do, which has the advantage of being a lot easier, is to obtain as much information as possible about it,” he wrote in 2014.

With this in mind, Gowers submitted a series of Freedom of Information requests to the UK’s 24 [Russell Group universities](#) asking for details of how much they spend annually for access to Elsevier journals. He then published his findings on his blog [here](#).

Deaf ears

Again, however, while [others](#) have followed Gowers’ lead and sought to [shine a light](#) on subscription costs, attempts to shame publishers and research institutions into behaving more responsibly with taxpayers’ money tend to fall on deaf ears. The harsh truth is that scholarly publishers are as good as impervious to criticism over their pricing policies. Only too aware that researchers have little option but to continue publishing in their journals – since jobs, funding and promotion depend on doing so – they feel little pressure to address the affordability problem.

In short, the research community has become addicted to publishing in expensive, prestigious journals, and there is little willingness to give up the drugs. Nor is there much in the way of methadone to help wean them off them. “It is difficult to change a system if there is no alternative system,” says Gowers below. “In particular, we need enough high-quality cheap journals to remove any need for people to submit to expensive ones.”

True, open access publishers have been creating alternatives to subscription journals for some 15 years now, and increasingly these are acquiring the prestige of having impact factors. In addition, legacy publishers now offer hybrid OA. But with APCs often costing as much as \$3,000 - \$5,000 per article, this is no solution to the affordability problem. Indeed, OA journals appear to be as susceptible to above-inflation price increases as subscription journals – as indicated by a recent [study](#) of [BioMed Central](#) prices.

So the next logical step is to encourage the creation of new low-cost non-commercial OA alternatives, which is exactly what Gowers sought to do in March when he [launched](#) a new open access journal called *Discrete Analysis*. Since it uses the physics preprint server [arXiv](#) to host its papers⁵ (at no cost), *Discrete Analysis* is able to publish papers at a fraction of the cost of other OA journals. True, there is still need for a web site and a submissions platform, but by using technology provided by [Scholastica](#) the cost to *Discrete Analysis* is just \$10 per paper – a far cry from \$3,000.

Moreover, thanks to a grant from Cambridge University, *Discrete Analysis* does not currently have to pass on the costs to authors (or their institutions). As such, the journal imposes no access fees and levies no publishing charge, making it a good example of an emerging new model for providing open access called [diamond](#) OA.

⁵ Technically, therefore, *Discrete Analysis* is an [overlay journal](#).

Clearly, a single journal can achieve little on its own. But Gowers hopes that *Discrete Analysis* will provide an attractive model for others to emulate. As he puts it, “I hope that by doing it myself and then telling people how easy it was, I can persuade others to do the same.”

Time will tell whether Gowers’ new initiative will become a catalyst for significant change. What will clearly be key is whether a sufficient number of his peers prove willing to follow in his footsteps, particularly those outside the physics, math, and astronomy disciplines, where arXiv is understood and routinely used to post preprints.

Gowers also hopes to see more editorial board defections similar to the one that occurred last year, when the board of Elsevier’s *Lingua* [resigned en masse](#) and set up an alternative, less expensive journal.

Ultimately, however, success will surely depend on whether funders, governments and research institutions can be persuaded to stop using the impact factor when evaluating researchers, and so help dissuade them from routinely seeking out expensive, prestigious commercial publishers like Elsevier when publishing their work.

With regard to the latter, the signs are currently not great. Three years after the San Francisco Declaration on Research Assessment ([DORA](#)) called for a halt to the practice of correlating the journal impact factor to the merits of a specific scientist’s contributions little appears to have changed. In fact, DORA has attracted fewer signatures than the Cost of Knowledge boycott.

Finally, we should note that Gowers’ vision for scholarly communication is somewhat more radical than implied by *Discrete Analysis*. As he says below, “I was always interested in more radical ideas such as not bothering with journals at all and having websites devoted to open peer review.” Consequently, he does not see much of a future for commercial scholarly publishers.

What is surely clear is that if scholarly communication is to be maximised for the network a root and branch revolution is still needed. The only question is when and how this revolution will take place, and who will oversee it. Sadly, before that revolution can get properly underway it seems likely that it will first be necessary to recover territory that the open access movement has given away

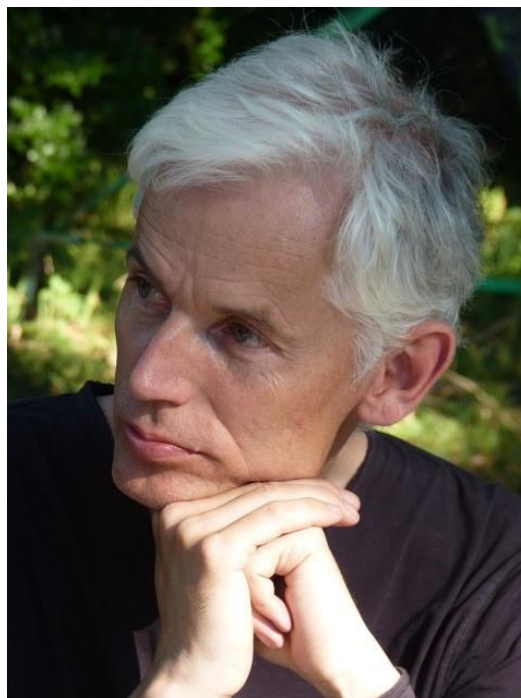
In truth, the real battle has yet to begin.

Given his credentials, who could claim to be better qualified to lead the troops over the top than Sir Timothy Gowers? True, he has not said he wants to be a leader *per se*, but he does clearly want to lead by example. Here at last is someone to whom researchers can usefully listen, and follow – if they want to see scholarly communication made fit for the 21st Century, and at last made affordable.

The interview begins ...

RP: *You are a highly regarded mathematician who has, amongst other things, proved mathematical conjectures, resolved mathematical problems, presented new mathematical tools, and introduced new mathematical notions. This has rightly earned you a number of prestigious prizes and honours, including in 2012 a knighthood from the Queen for services to mathematics. Is it possible to explain your research in a way that a layperson would understand?*

TG: It is difficult to describe in a satisfactory way to a layperson what my research is about, or why anyone should find it interesting, but maybe it helps if I try to describe my mathematical tastes. I am drawn to what mathematicians call “elementary” problems. This does not mean easy problems, but rather problems that it is realistic to attack from first principles.



For many mathematical problems, even the questions are hard to understand unless you spend a long time being trained in the right area of mathematics. There are others with statements that can be easily understood but that it would not be realistic to tackle without first mastering a large amount of theory – [Fermat’s Last Theorem](#) was an extreme example of that.

The ones I like have simple appealing statements and some chance of being solved by someone who just sits down and directly attacks the problem.

Having said that, I don’t like my problems to go too far in this direction. My ideal mathematical experience, which has occasionally happened to me, is to be drawn in by an appealing statement and to find that in the process of trying to prove it I come to understand that I need to use interesting tools that I did not know about, and sometimes also to develop new tools of my own. So the resulting proof may not look completely elementary, but I will have got there by just directly attacking the problem and going where it takes me.

RP: *So what you do is a branch of pure mathematics I guess. Does that mean your work doesn’t have real-life applications, that the aim is rather to better understand the world?*

TG: Yes, my research is indeed a branch of pure mathematics. That means that it does not have direct applications.

I see myself as contributing to a huge and highly interconnected body of knowledge: the body as a whole has many very important applications, but it is hard to predict in advance where these applications will come from, and it is therefore counterproductive to try to direct the field towards potential applications.

Another very important point is that the way the subject develops is such that pieces of work

can influence other pieces of work in subtle ways, and end up contributing almost invisibly to very important applications years later.

For example, insights from an area I used to work in – the geometry of [Banach spaces](#) – were closely related to some famous work of [Candes](#) and [Tao](#) on [compressed sensing](#), which has extremely practical applications in medicine that can make a difference between life and death.

I do not know to what extent Candes and Tao directly used that earlier work, because by the time of their papers those insights were firmly established as part of the collective knowledge of mathematicians. If you now look back at some of the papers in the geometry of Banach spaces, you might be tempted to say that they had no applications, but that is very misleading because it ignores how mathematics progresses

RP: In 2009 you experimented (successfully) with crowdsourcing a mathematical problem (the first [Polymath Project](#)). Subsequently you set up [Tricki](#), a Wikipedia-style site intended to develop a large store of useful mathematical problem-solving techniques. These two initiatives have been characterised as “open science”. Would you agree with that definition? If so, how would you characterise open science? And what benefits does it provide over traditional methods?

TG: I would characterize open science as being science where the entire discovery process, and not just some paper that summarizes (and to a large extent conceals) it at the end, is out in the open.

Polymath fits the definition almost perfectly, since the idea is that the participants should report on their private thoughts whenever they have anything even slightly interesting to say.

The Tricki was slightly different, because it was a place for sharing research tips rather than a place for actually carrying out research.

What they have in common is that they are attempts to make the research process more efficient by avoiding duplication of effort. In the case of Tricki, the idea is that as people do more research, they build up a box of “tricks” that they use over and over again. With a little thought, one can often give quite a good explanation of when and why a certain trick is likely to be useful.

Why not share this information, so that people can develop more quickly as researchers? There can be a lot of benefit in discovering a trick for oneself, but it is a slow process, and there should at least be the option of learning about tricks from other people.

Polymath offers various potential efficiency gains. One is the possibility of specialization – not so much in subject matter expertise (though there is that too) as in the kind of roles that people play, including generating ideas, assessing the likelihood of already generated ideas being fruitful, making existing ideas more precise, summarizing what has been said so far, reformulating ideas, and so on. Different people excel at different roles, and Polymath makes it possible for people to benefit from this.

The fact that it takes place completely openly means that the people who participate in a Polymath project are self-selecting. That too is efficient: it means that you do not have to

guess who would be a good and enthusiastic contributor to any given project. Experience has shown that some of the most valuable contributors are people I would never have thought of approaching, or in some cases people I had not even heard of.

Another way in which Polymath is efficient is that it allows a mathematical dialogue to take place at a very quick, but not too quick, pace. If you can only communicate through journal articles, then you miss out on a lot of very useful informal thinking that would not be suitable for publication, and you have to wait a very long time for somebody to polish their ideas and get them into publishable form. One way to get round that is to talk to people, but sometimes it is very hard to keep up with a mathematical conversation.

With an online project the basic unit of discourse is a comment of two or three paragraphs, which can either be read with no difficulty or can be understood after a small amount of thought – far less than is needed for digesting a typical journal article.

And people can put forward ideas in an imprecise or incomplete form, get feedback from other participants, and then either abandon those ideas or refine them. The whole process is much quicker than working on one's own, sometimes startlingly so.

RP: *What led you to experiment in this way, and what would you say you have learned from doing so. Also, what expectations do you have for large-scale collaborations like the Polymath projects in the future, both in maths and in other disciplines? Will it become commonplace?*

TG: I think the reasons I have just given lay behind my decision to try out the Polymath experiment. The rather striking success of the first attempt led people to wonder whether this new way of doing mathematics was going to take over.

With the benefit of a few years since then, which has included various Polymath projects that have not succeeded in solving the problems they set out to achieve (which, by the way, I do not see as a failure for the method, since most conventional attempts to solve problems also fail), expectations have become more realistic. It looks as though Polymath projects will occupy an important niche but without dominating mathematical research.

However, it is still early days, and it could be that slight modifications to the approach, perhaps including carrying it out on platforms cleverly designed for the purpose, would make a big difference and greatly increase participation.

Boycott and beyond

RP: *In 2012 you publicly boycotted the scholarly publisher Elsevier, citing the high prices it charges for its journals, its preference for expensive and inflexible journal bundling (through the use of so-called “Big Deals”), and its support for the Research Works Act (RWA), which if it had passed would have prohibited open-access mandates for federally funded research in the US (and presumably would have rolled back the NIH Public Access Policy).*

Your boycott led to the setting up of the Cost of Knowledge web site, where approaching 16,000 researchers have now also committed to boycott Elsevier. Although Elsevier responded by making a few changes, including setting up its Open Archive, I suspect you

would say that little has changed. So how would you characterise the achievements of the boycott, and what did you personally learn from it?

TG: You are right that I think that little has changed. Elsevier's opening up of mathematics papers over four years old is very welcome, and it is disappointing that the other big publishers, in particular Springer, have not done the same (and indeed that Elsevier has not done it for most subjects).

But the fundamental problem, that universities are paying huge sums for a service that should be much, much cheaper, remains.

***RP:** I believe you have said that you became an advocate for open access by accident. When and how did your OA advocacy begin? Was there a specific event or incident that triggered your interest in open access?*

TG: Basically the event that triggered my interest in open access (as opposed to open science, which I was already interested in) was the surprising amount of attention I received for my [blog post](#) in which I said publicly that I was not cooperating with Elsevier and suggested that others should do the same. After that, I felt a certain responsibility, and still do.

One can go back further and ask what prompted the blog post. Over the preceding years I had read various articles about the problematically high cost of journals, and of Elsevier journals in particular, which had left me boycotting Elsevier privately.

The main triggers that made me go public were (if I remember correctly) [an article](#) by George Monbiot in the *Guardian* about the extraordinary profits of the commercial publishers, and an initiative by the [International Mathematical Union](#) to do something about journal prices.

The latter led to an IMU blog, which attracted very little attention. I knew that my blog would be far more widely read, and so part of my reason for writing the post was impatience with the slow rate of progress on this problem.

***RP:** Recently you launched a new open access journal called [Discrete Analysis](#), which has been described as an overlay journal. What is an overlay journal and in what ways is it different from/better than a regular journal? Is the main aim to lower costs, to leverage the technical possibilities of the Internet, or what?*

TG: An overlay journal, at least in the sense I use the term, is one where the articles live in an existing repository such as [arXiv](#), rather than being hosted by the journal itself. The “journal” then consists of arXiv URLs of the articles that have been accepted.

I would not say that there is a profound difference between an arXiv overlay journal and a more conventional electronic journal that hosts its own articles. However, I think it is symbolically important: we are acknowledging the reality that a large proportion of what mathematicians read is in the form of arXiv preprints rather than journal articles, because you get to read the articles sooner, and because even if you have access to the journal it is far more convenient just to type the title into Google and click directly on a pdf.

Therefore, we are declaring that almost all that is needed from a journal is the parts that cost nothing: the editorial work and the peer review.

That said, we were able to make small innovations that we hope will make *Discrete Analysis* better in certain respects than traditional journals. In particular, our website is designed first and foremost as a website, rather than as a kind of feeble electronic imitation of a paper journal.

We also include “editorial introductions” to each article, so that the website is more than just a list of papers, and becomes something that one can conveniently browse to get an idea of the content of the journal. A small but important detail is that if you click on the words “editorial introduction” the browser does not open a new page: in general, we try, as all well-designed websites should, to minimize the amount of clicking people have to do.

Another feature of the journal is that the website is very attractive to look at. Credit for this and for several of its design features belongs to [Scholastica](#), an organization we use that was set up in order to make it very cheap and easy to start electronic journals.

RP: *Can you say how the idea for Discrete Analysis developed and what issues arose during the planning process. Where there any particular surprises?*

TG: The idea of arXiv overlay journals was in the air for a long time. I think one impulse behind *Discrete Analysis* was the very hostile reaction from many people to the setting up of the open access journal [Forum of Mathematics](#) by Cambridge University Press, which (after a three-year free period) charges £750 per article.

It seems that a large proportion of mathematicians are implacably opposed to article processing charges, no matter what assurances are given that authors themselves will never be expected to pay out of their own pocket, and that ability to pay will not affect the choice of which articles to publish.

A difficult aspect of the conversation that took place at the time was that I found myself defending a model that I was never wholeheartedly in favour of. I was always interested in more radical ideas such as not bothering with journals at all and having websites devoted to open peer review.

We have not done anything remotely that radical with *Discrete Analysis*, but I think it is important to move one step at a time.

Anyhow, at some point I wrote to a number of people who I thought would make good editors and who could suggest further people, and we then discussed details of how the journal should operate. It took a couple of years from initially thinking about it to actually going ahead.

One thing that helped a lot was that I received a small grant from Cambridge University to use on the journal. That suddenly meant that, for example, using Scholastica was a realistic possibility: they charge \$10 per submission, and our grant would cover that for a few years, which would give us time to find more funding.

I wouldn't say that anything unexpected happened during the planning process. In fact, the

only thing that really surprised me was just how smooth it all was. I think that's an important point to make to anyone else who might be considering a similar initiative: it has not taken up huge amounts of my time.

RP: *You referred to the hostile reaction to the launch of the [Forum of Mathematics](#) (for which I believe you personally [took some flak](#)), and you said, “A difficult aspect of the conversation that took place at the time was that I found myself defending a model that I was never wholeheartedly in favour of.” You are, I think, still on the editorial board of FoM. How does your involvement with FoM fit with what you are doing with *Discrete Analysis*?*

TG: That's a slightly tricky question. What complicates matters is that FoM is a very good journal. I don't just mean the standard of papers it publishes, but also the quality of the editorial board and the excellent way that the editors interact and take communal decisions. (This isn't visible of course, but as an editor I find it striking.)

Also, although the APC model is one that has too many problems for me to want to promote it, I think that FoM is managing to avoid those problems, at least for now, by having APCs that are modest by the standards of the commercial publishers, and by having a generous no-questions-asked waiver policy.

We are at a crucial stage now, since the initial three-year period of not charging at all has just come to an end. So now we will see whether enough mathematicians are ready to accept this publication model for the journal to continue, or whether they will vote with their feet and force the journal to change.

I have always felt that different models should be tried, since it is very hard to guess in advance what will work. So I suppose the answer to your question is that although I would prefer the *Discrete Analysis* model to be the one that spreads, I don't want to put all my eggs in one basket just yet.

Also, I think that if FoM does not get enough submissions to be considered economically sustainable by Cambridge University Press, there would be the potential to continue the journal on a different basis that mathematicians might prefer, and I would like to be around to influence that.

[Taking responsibility](#)

RP: *Would I be right in thinking that your decision to launch *Discrete Analysis* was informed by a belief that if they really want the scholarly communication system to change, and to become more affordable, researchers will need to move beyond boycotts and petitions, and take responsibility for scholarly communication themselves?*

TG: Yes, that is right. It is difficult to change a system if there is no alternative system. In particular, we need enough high-quality cheap journals to remove any need for people to submit to expensive ones.

At the moment, many people would seriously damage their careers if they boycotted all unreasonably expensive journals, and one cannot expect them to make such a big sacrifice.

RP: *I assume it is important for you that Discrete Analysis is open access. OA advocates deploy a number of different arguments when calling for open access – e.g. that subscription journals are too expensive and so impose unnecessary barriers between researcher and research, that research is funded by the taxpayer and so should be freely available to the public, that open access can enfranchise researchers in the developing world etc. – but what for you are the most compelling reasons for making research open access?*

TG: I have always been more interested in cost than in open access. That is because the people who write papers that interest me routinely post them to the arXiv, which means that I've got all the open access I want (except to papers published many years ago, where it can still be inconvenient to get hold of them).

However, that is a rather narrowly selfish point of view, and I realize that there are all sorts of potential benefits of open access to papers in other subjects.

I would say more generally that, like many people, I dream of a world where pretty well all intellectual property of any kind – books, music, films, academic articles, databases, etc. – is right there on your laptop, very easy to find, free at the point of use.

The easier it is to get access to information, the easier it is to profit from and add to that information, so I would like to see barriers removed as much as possible. Obviously in some cases that will be harder to achieve than in others, but academic articles, which are not written to make money, should be an easy case.

Unfortunately, the publishers have found a way of making huge profits from our articles: the resulting financial incentives are the source of all the problems.

RP: *You said that you have not done anything very radical with Discrete Analysis. I guess that the main objectives are to provide a proof of concept for a cheap high-quality journal and to make a transitional step towards the larger goal of re-engineering scholarly communication for the networked world.*

How will you measure success, and to what extent are you willing to adapt your plans in order to achieve and sustain that success? For instance, you say you are currently funding the journal by means of a grant. If you did not manage to get a follow-on grant could you envisage charging authors an APC? What if Scholastica increased submission fees significantly?

TG: My main measures of success will be whether we continue to receive a regular supply of high-quality submissions, and whether other journals start up with a similar publication model.

I think that libraries stand to gain so much from a cheaper publication model becoming the norm, and our costs are so low, that there is almost no chance that we will not continue to be able to obtain enough funds to continue as a completely free journal.

However, if, contrary to expectations, that did happen, then I would not mind too much passing the \$10 submission charge we pay to Scholastica on to the authors, given how small an amount of money that is.

I think it is very unlikely that Scholastica will significantly increase their fees (unless they are bought by a commercial publisher, but I think that is unlikely too), since that would be completely contrary to their mission. But if it happened, then we would simply have to move to a different platform, which would be annoying but possible.

RP: *You rightly say that overlay journals have been in the air for a long time. I believe Paul Ginsparg [first suggested](#) the idea in 1997. Since then there have been a number of experiments, but while a number of overlay journals have come and gone I am not sure we could claim that the idea has taken off. I also think it fair to say that the widely-applauded [Episciences Project](#) is still some way off becoming the global platform [described](#) by Nature in 2013.*

And yet as you imply, the idea of the overlay journal is extremely simple and you say you were surprised how smoothly the process of developing Discrete Analysis was. Why does it appear to be proving quite so challenging to make the overlay journal mainstream? What are the real challenges? Is it mainly an issue of sustainability?

TG: From my experience, I think the main challenge is probably just having enough people who are ready to do the work to create overlay journals. I hope that by doing it myself and then telling people how easy it was, I can persuade others to do the same.

But it is important that people will not be afraid to submit to these new journals, which, at least to start with, requires an editorial board with several editors with well-established reputations.

So one needs to find very good mathematicians who are also keen enough on the publication model to do what it takes to make it work. We are lucky enough to have this with *Discrete Analysis*, but finding enough such mathematicians to create an entire publication infrastructure may be more difficult.

RP: *In its 2013 coverage Nature indicated that you personally were planning to start an Episciences journal. Did that happen, or did you opt for the Scholastica platform instead? If so, why?*

TG: I opted for the Scholastica platform instead.

There were three reasons. The first is that the Episciences platform took a long time to be ready, the second is that it is mainly connected to the [HAL archive](#) rather than arXiv, which I thought some mathematicians might find off-putting, and the third is that a small grant fell into my lap, so to speak, which meant that I became ready to consider Scholastica with its small charge.

Impact factors and APCs

RP: *You said, “many people would seriously damage their careers if they boycotted all unreasonably expensive journals, and one cannot expect them to make such a big sacrifice.” I am thinking that this in part relates to the [impact factor](#) which, although now entirely discredited as a measure of the quality of individual papers, remains hugely*

important to the research community, and is a metric on which jobs, tenure and promotion often hang.

Can we expect the research community to ever end its addiction to the IF? If so, how? Do you expect Discrete Analysis to gain an impact factor? If it did would the editorial board advertise the fact, or perhaps abjure it?

TG: We are applying to be listed on [Web of Science](#) and thus to obtain an impact factor. That is not because we approve of impact factors, but because it is an unfortunate necessity if we want our journal to be a realistic possibility for all mathematicians working in relevant areas.

I already know of an example of a paper that an author wanted to submit to us but ended up not doing so because he had a Korean co-author, for whom it would not have counted as a publication because of our (current) lack of an impact factor.

If we are successful in obtaining an impact factor, I think we should probably state somewhere on our website that we have done this for practical reasons and not because we wish to endorse the system in any way.

I find it so obviously stupid to take impact factors seriously that I am a bit nonplussed that anyone does, and am left not knowing what to suggest to tackle the problem. The San Francisco Declaration was one attempt, but it takes more than a declaration to change people's behaviour.

I tried to get the [Royal Society](#) (which has signed the [San Francisco Declaration](#)) to state prominently on its website that it would decide its appointments on the basis of article quality rather than journal metrics, but that has not happened.

But explicit declarations by individual departments that they will take no account of impact factors and the like when making hiring decisions might be helpful.

RP: *In [your 2013 defence](#) of Forum of Mathematics you wrote that funding a journal with APCs is acceptable if authors do not have to pay the money themselves. You added, “I think that there is a case for having APCs at least as a transitional arrangement.”*

You then added, however: “I had a horrible fantasy the other day, when it occurred to me that publishers could try to reintroduce the bundling concept in connection with APCs. Suppose that Elsevier made an offer to a university that for a flat fee all academics at that university could publish free in Elsevier journals for the next five years. If the flat fee was set in such a way that the university expected to save money, then it would be a tempting offer. But what would happen then? The university would say to its academics, ‘If you have the choice between an Elsevier journal and a comparable journal published by someone else, please go for the Elsevier journal.’ And once Elsevier (and other big publishers with similar arrangements) had driven the smaller journals out of business, it could start upping the fees, and it would be very difficult for new journals to compete. In other words, the major problem with subscription journals could be reborn in a new guise.” You added however: “forewarned is forearmed ... we can tell our universities to have nothing to do with them. Any sign that a publisher is trying to introduce them can be met with widespread negative publicity.”

Are not Dutch universities currently acting out this horrible fantasy, and not just with [Elsevier](#), but with [Wiley](#), [Sage](#), and [Springer](#). Moreover, rather than attracting negative publicity, the initiative is being applauded by many in the OA movement. Does this concern you? If so, what can/should be done about it?

TG: I think this is beginning to happen, and that publishers are finding ways to create an APC-based market that will be as dysfunctional as the subscription-based market is.

The basic problem with APCs is that publishers can charge what they like, knowing that if universities start to tell academics that they must publish in cheaper journals, there will be an uproar about the perceived threat to academic freedom.

I have never seen a convincing explanation for how a properly free market in APCs could work.

RP: *You indicated earlier that you support the concept of open peer review. I don't think *Discrete Analysis* has implemented this. What in your view are the pros and cons of open peer review, and why did you choose not to adopt it?*

TG: The main pro is that the information provided by referees, which is often very interesting, is not lost if the peer review is open.

Also, with open peer review it is possible for people to make valuable remarks some time after publication, such as noticing that a part of the argument can be simplified. On the other side, some people may be less frank if what they write is to be public, especially if it is not anonymous.

Also, many people are uncomfortable with exposing themselves to public scrutiny online, and a disproportionate number of them are women. It was this last reason that was the strongest one for not having open peer review for *Discrete Analysis*, but more generally I thought it was sensible not to try to introduce too many innovations at once, to maximize the chances of the journal being accepted.

With our editorial introductions, we have tried to provide some of the contextual information about papers that we learn from referees' reports, so in a small way I think we have some of the advantages of open peer review without the disadvantages.

RP: *I sometimes think that the open access movement may have inadvertently slowed the process of re-engineering the scholarly communication system for the networked world – by for instance, importing out-dated print habits into the digital environment, including perhaps pre-publication peer review, and indeed the traditional journal format itself.*

And as we have discussed it is looking as if the APC model will enable legacy publishers to create an OA market that is just as dysfunctional as the subscription system. As you will doubtless know, The Netherlands, which currently holds the Presidency of the Council of the EU, has set open science as one of its priorities and to that end, in early April it hosted a conference where a [Call for Action on Open Science](#) was drafted.

Amongst other things, this Call for Action sets a goal of full open access for all scientific publications by 2020. Given the very short timescale I must assume that if successful this

will see the triumph of the OA Big Deal approach that Dutch universities have been pushing. What makes this more likely is that Max Planck was at the conference promoting its [Open Access 2020](#) proposal, which envisages all subscription journals being “flipped” to an open access model based on the APC system.

As I see it, were the EU’s goals to be achieved we would see the traditional journal locked into the new environment, along with legacy evaluation systems like the impact factor, journal prestige etc. I assume you would deprecate such a development. But what if anything can be done to prevent this happening, and who should be working to prevent it?

TG: I don’t know whether it is the best solution, but one possibility that I think should definitely be pursued is the “[fair OA](#)” idea, which is what the linguistics journal [Lingua](#) did. They obtained a guarantee of funding to support APCs of [400 Euros per article](#), and then asked Elsevier to switch the journal to an open access journal with APCs at that level.

Elsevier refused, so the editorial board resigned and set up the journal with [Ubiquity Press](#) instead, with the new name [Glossa](#). Although I am not sure articles need to cost that much, a system where submission and reading of articles was free and the academic community was paying 400 Euros per article would be much cheaper and more open than the system we have now.

Of course, I would also like to see more very cheap journals like *Discrete Analysis* being set up.

Eventually, if there are enough high-quality cheap places to submit articles, universities will surely begin to ask serious questions about why they should pay for much more expensive options.

***RP:** Earlier you outlined your vision of open science, and indicated that you would prefer researchers stopped bothering with journals and started using websites devoted to open peer review.*

How likely do you think it is that this would ever happen to any meaningful degree? If it did, how would you expect the roles of the current “stakeholders” of scholarly communication to change (here I am thinking of researchers, research institutions, research funders and publishers)?

Would there still be a role for commercial publishers? If so, how confident can we be that an efficient market place for the services they provided would ever emerge, and that there would be any reliable mechanism for containing prices?

TG: I think it is unlikely to happen soon, since it would be very hard to switch to such a radically different system. I think the change probably has to be continuous, which I think it can be.

The first generation of new journals could be pretty conservative – we have already discussed how this is true of *Discrete Analysis* – but once there are many respectable electronic journals out there it would become easy for some of them to experiment with comment pages associated with articles.

And once people became used to those, the stage would be set for “journals” that did not accept articles in quite the way that journals do now, but simply opened them up for comments.

One change that this would force on academics would be to stop relying on “metrics” and think more about actual quality of research. In general, I think it would give more power to academics, and less to publishers and bureaucrats.

There would almost certainly be distortions and unfairnesses in the new system, but I do not think they would be anything like as bad as the distortions and unfairnesses of the current system.

Ultimately, I don’t see much of a role for commercial publishers. I like to do the following thought experiment. Imagine (admittedly rather implausibly) that the internet had come into existence before people started doing research in mathematics in any great volume. People would have posted their mathematical findings online, and after a while would probably have found that there was some need to organize the literature. But nobody would have thought of using the print journal, or anything like it, for that purpose.

Something more like the arXiv would have been much more likely, perhaps combined with websites that provided commentary on particularly interesting articles. And I don’t see what conceivable role commercial publishers would have had. So the huge role they have now is, I think, mainly the result of status quo bias.

RP: How specific do you feel your vision for open science is to mathematics (and perhaps physics and astronomy)? Do you have a sense of whether it would be appropriate for all academic disciplines?

TG: I think there are big cultural differences between subjects, so what I say is particularly true of subjects where most of the dissemination takes place before publication, through the arXiv and other repositories.

This has become the case in much of mathematics, physics and astronomy. I don’t see any reason in principle that it couldn’t happen in all subjects, but with those three it would be a much smaller change from what happens at present.

RP: Thank you for your time, and good luck with Discrete Analysis.

Richard Poynder 2016



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