

PLoS ONE, Open Access, and the Future of Scholarly Publishing

Open Access (OA) advocates argue that [PLoS ONE](#) is now the largest scholarly journal in the world. Its parent organisation — Public Library of Science (PLoS) — was co-founded in 2001 by Nobel Laureate [Harold Varmus](#). What does the history of PLoS tell us about the development of PLoS ONE? What does the success of PLoS ONE tell us about OA? And what does the current rush by other publishers to clone PLoS ONE tell us about the future of scholarly communication?

The most important thing is to find out what is the most important thing
Shunryu Suzuki

Our story begins in 1998, in a coffee shop located on the corner of [Cole and Parnassus](#) in [San Francisco](#). It was here, Harold Varmus [reports](#), that the seeds of PLoS were sown, during a seminal conversation he had with colleague [Patrick Brown](#).¹ Only at that point did Varmus realise what a mess scholarly communication was in. Until then, he says, he had been “an innocent person who went along with the system as it existed”.

Enlightenment began when Brown pointed out to Varmus that when scientists publish their papers they routinely (and without payment) assign ownership in them to the publisher. Publishers then lock the papers behind a paywall and charge other researchers a toll (subscription) to read them, thereby restricting the number of potential readers.

Since scientists crave readers (and the consequent “[impact](#)”) above all else, Brown reminded Varmus, the current system is illogical, counterproductive, and unfair to the research community. While it may have been necessary to enter into this [Faustian bargain](#) with publishers in a print environment (since it was the only way to get published, and print inevitably restricts readership), Brown added, it is no longer necessary in an online world — where the only barriers to the free-flow of information are artificial ones.

Physicists, Brown said, have overcome this “access” problem by posting preprints of all their papers on a web-based server called [arXiv](#). Created by [Paul Ginsparg](#) in 1991, arXiv allows physical scientists to ensure that their work is freely available to all. “Should not the biomedical sciences be doing something similar?” Brown asked Varmus.

It was doubtless no accident that Brown — who had previously worked with the Nobel Laureate — chose Varmus as his audience for a lecture on scholarly publishing: at the time Varmus was director of the National Institutes of Health (NIH) — the [largest source of funding for medical research](#) in the world. He was, therefore, ideally placed to spearhead the revolution that Brown believed was necessary.

Fortunately for the [open access movement](#) (as it later became known) Varmus immediately grasped the nature of the problem — aided perhaps by some residual Zen wisdom emanating from the walls of the coffee shop they were sitting in, which had once been the Tassajara Bakery.² Varmus emerged from the café persuaded that it would be a good thing if publicly-funded research could be freed from the publishers’ digital padlocks.³ And he went straight back to the NIH to consult with colleagues to that end.

Again fortuitously, one of the first people Varmus broached the topic with was [David Lipman](#) — director of the NIH-based National Center for Biotechnology Information (NCBI). NCBI was home to the OA sequence database [GenBank](#), and Lipman was [an enthusiastic supporter](#) of the notion that research should be freely available on the Web. By now Varmus’ conversion was complete.

¹ Patrick O Brown is a geneticist at [Stanford University School of Medicine](#).

² The Tassajara Bakery was a Zen Center venture promoted by [Richard Baker](#), and an extension of the baking practices at [Tassajara Zen Mountain Center](#). It was sold off in 1992 and eventually closed, presumably then becoming the café in which Varmus and Brown met.

³ In his book [The Art and Politics of Science](#) Varmus heads the section describing his meeting with Brown as “My Conversion: The Making of a Radical”. p. 255.

This conversion was to see Varmus embark on a journey that would lead to the founding of a new publisher called [Public Library of Science](#), the launch of two prestigious OA journals ([PLoS Biology](#) and [PLoS Medicine](#)), and subsequently to the creation of what OA advocates [maintain](#) is now the largest scholarly journal in the world — [PLOS ONE](#).

Varmus' first attempt to liberate biomedical research from behind publisher paywalls took place during his tenure at the NIH, when he proposed a service called [E-Biomed](#). Varmus [envisaged](#) E-Biomed as being “a huge digital library in which we would place existing journals, plus possibly some new journals, and then make it all fully searchable and available at the click of a mouse.”

As we shall see, Varmus' journey was to prove no walk in the park, and some believe his project lost its bearings on the way. Rather than providing a solution, they argue, PLoS may have become part of the problem.

Certainly *PLoS ONE* has proved controversial. This became evident to me last year, when a researcher drew my attention to a row that had erupted over a [paper](#) the journal had published on “wind setdown”. Even some of the journal's own academic editors appeared to be of the view that the paper should not have been published (in its current form at least). As the row appeared to raise questions about *PLoS ONE*'s review process — and about *PLoS ONE* more broadly — I contacted *PLoS ONE* executive editor [Damian Pattinson](#).

The response I got served only to pique my interest: While Pattinson invited me to send over a list of questions, I subsequently received an email from *PLoS ONE* publisher [Peter Binfield](#) informing me that it had been decided not to answer my questions after all.

Explaining the decision Binfield said: “We've given this more thought, and I'm afraid that we don't wish to engage in the long Q&A you have proposed. You have raised many questions now about this one paper along with various broader questions about *PLoS ONE*. I don't think we have anything further to say about the article at this point, and so it doesn't seem appropriate to use the discussion that surrounds this article as a way to build a much more extensive discussion about *PLoS ONE*.”

He did however attach a statement addressing one of the questions I had asked. (See page 22)

I took Binfield's position to be that the row over the wind setdown paper raised no larger questions about *PLoS ONE*, its review process, or PLoS more generally. If that's right, I disagree, and I hope I can demonstrate why in what follows.

But even if Binfield were correct, one must wonder why a non-profit organisation that exists courtesy of the public purse, and which presents itself as an open and transparent organisation, should decline to answer questions sent to it by a journalist.

That I found odd.

Plan A: Advocacy and activism

But first let's back up to 1998 again. As noted, the [initial outcome](#) of Varmus' conversation with Brown was a proposal for E-Biomed — an outline of which Varmus published in May 1999.

At first glance, E-Biomed appeared to be a biomedical version of arXiv, and would include a variety of un-refereed material, including preprints, data sets and “conversations with authors of previously published papers.”⁴

⁴ As Varmus puts it in his book (p. 256), “E-Biomed would also display, in a separate section, reports that were not peer-reviewed in the traditional manner, including papers that might normally not be published at all, such as those describing a new method or idea, or those reporting negative results from experiments or clinical trials.”

On closer examination, however, it was clear that the proposal went further: arXiv assumes that papers posted in it will subsequently be peer reviewed and published in a traditional journal.⁵ It was envisaged that E-Biomed would review and publish (online) at least some of the papers itself.

As the proposal explained, content would be posted into E-Biomed in two ways: "(i) Many reports would be [submitted to editorial boards](#). These boards could be identical to those that represent current print journals or they might be composed of members of scientific societies or other groups approved by the E-biomed Governing Board. (ii) Other reports would be posted immediately in the [E-Biomed repository](#), prior to any conventional peer review, after passing a simple screen for appropriateness."

Naively, Varmus failed to appreciate that his plan would prove controversial. So far as he was concerned he was simply proposing a way of using the Internet to improve scholarly communication.

More politically astute colleagues at NIH, however, were startled, both at the audacity of his proposal, and the way in which he was going about things. As one insider [put it](#) to me in 2006, "Varmus wrote the proposal himself and just sent it out to a bunch of people as a rough draft. For us it was like 'Oh, my God: he clearly doesn't know what he is getting into'."

The truth of this became apparent almost immediately. To Varmus' surprise, reaction was loud, visceral and highly critical. He was accused of trying to dispense with peer-review, and of seeking to destroy traditional journals. Critics also [complained](#) that he was planning to organise "a take-over by the US government of an activity that should be international in character and belong in the private sector."

Varmus later [joked](#) to *New Scientist*, "I must have known that I was not going to be at NIH for much longer, because this caused a tremendous political argument: what the hell was I trying to do to destroy the publication industry."

Opposition was so virulent, and so vocal, that by the time Varmus' initiative saw the light of day it was a pale shadow of what he had planned. Rob Kling *et al.* were later to [characterise](#) the process in this way: "[I]n less than a year, the E-Biomed proposal was radically transformed, eliminating the preprint section, instituting delays between article publication and posting to the archive, and changing the name to '[PubMed Central](#)'."

This, King added, was a consequence of "highly visible and highly influential statements made by publishers and scientific societies against the proposal."

PubMed Central was eventually launched in February 2000.

But however naïve he may be, Varmus is not a man to give up. Within a year of leaving NIH he had co-founded — with Brown and [Michael Eisen](#)⁶ — the advocacy organisation Public Library of Science, and launched a new front in his campaign to free the refereed literature.

Initially PLoS focused on trying to strong-arm publishers into accepting OA, and to that end in 2001 an [open letter](#) was published that read: "We support the establishment of an online public library that would provide the full contents of the published record of research and scholarly discourse in medicine and the life sciences in a freely accessible, fully searchable, interlinked form."

Members of the research community were invited to sign the letter, and in doing so to make the following commitment: "[W]e pledge that, beginning in September 2001, we will publish in, edit or review for, and personally subscribe to only those scholarly and scientific journals that have agreed to grant unrestricted free distribution rights to any and all original research reports that they have published, through PubMed Central and similar online public resources, within 6 months of their initial publication date."

⁵ Although, as we shall see, it does have a "moderation" process.

⁶ Eisen is an associate professor of genetics, genomics and development at the University of California.

Initial response to the letter was extremely encouraging: Within a short space of time nearly 34,000 scientists from 180 different countries had signed it.

But while a handful of publishers complied with the demands outlined in PLoS' letter, most blithely ignored them. Even more discouraging, most of the scientist signatories proved themselves happy to forswear their own pledge and continued publishing in the very journals that had turned a deaf ear to them.

Plan B: If you can't beat them ...

In retrospect it's clear that the wider research community wasn't ready for the kind of bottom-up revolution that physicists had embarked upon. So Varmus and his colleagues had again to re-group.

Fortunately, in December 2002 the [Gordon and Betty Moore Foundation](#) awarded PLoS a [\\$9 million grant](#). This allowed Varmus and his colleagues to adopt a completely different approach, and reinvent PLoS as a publisher. Not any old publisher, but an open access publisher — one committed to making every paper it published freely available on the Web.

To achieve this PLoS adopted a very different business model.⁷ As the Gordon and Betty Moore Foundation press release [put it](#), “The PLoS journals will retain all of the important features of scientific journals, including rigorous peer-review and high editorial standards, but will use a new business model in which the costs of these services are recovered by modest fees on each published paper.”

In other words, rather than charging readers (or their institutions) a subscription to read the papers, PLoS would charge authors (or their funders or institutions) a fee to publish them. Later dubbed the article process charge ([APC](#)), PLoS' modest fee was set at \$1,500 per article — a payment designed to defray the publisher's costs, and so obviate the need to charge for access.

Even more ambitiously, PLoS founders set out to compete with the world's most prestigious subscription journals — journals like [Nature](#), [Science](#) and [Cell](#).

PLoS Biology was launched in 2002, and *PLoS Medicine* the next year.⁸ [Writing](#) in the first issue of *PLoS Biology*, the three founders explained: “Today, with the launch of *PLoS Biology*, we take on a new role as publishers, to demonstrate that high-quality journals can flourish without charging for access.”

They added, “Our aim is to catalyse a revolution in scientific publishing by providing a compelling demonstration of the value and feasibility of open-access publication. If we succeed, everyone who has access to a computer and an Internet connection will be a keystroke away from our living treasury of scientific and medical knowledge.”

In fact, creating OA journals is not difficult. The challenge lay in ensuring that PLoS became financially self-sufficient before its grant ran out, and so demonstrate that author-pays publishing is sustainable.⁹

About this there was no shortage of sceptics, particularly amongst publishers. In October 2003 *Nature's* Declan Butler [argued](#) that \$1,500 per paper would never provide sufficient revenue to keep PLoS afloat. To support his argument he quoted Yale University cell biologist [Ira Mellman](#), then editor of [Rockefeller University Press's *Journal of Cell Biology*](#), who said: “I feel that PLoS's estimate is low by four — to six fold”.

⁷ PLoS was not the first to use the author-pays model. It was pioneered by OA publisher BioMed Central, a private company founded in 1998.

⁸ Since then a number of [other journals](#) have been launched, but it is *PLoS Biology*, *PLoS Medicine* and *PLoS ONE* that have attracted most attention, and are the key products.

⁹ OA advocates [object](#) to the term “author-pays”. For that reason I shall use the term “author-side fees” from henceforth.

Of course, *Nature* had an axe to grind. Not only was PLoS by now poaching staff from the journal, but OA posed a potential threat to the subscription business that sustained *Nature*: By deciding to charge \$1,500 per paper, for instance, PLoS had breathed new life into a long-standing belief that scholarly publishers make excessive profits from the research community (and thus the taxpayer, who funds most research).

This was evident a year later, when *Nature* gave evidence to a UK [Science & Technology Select Committee](#) enquiry into scientific publishing. Richard Charkin, then CEO of Macmillan (owner of Nature Publishing Group's), [told](#) British MPs that it costs *Nature* \$10,000–\$30,000 to publish a paper.¹⁰

British politicians could not help but wonder: If PLoS can make ends meet by charging \$1,500, then how could incumbent publishers justify levying a subscription that, they said, assumed a per-article cost of \$10,000 to \$30,000?¹¹

Consequently *Nature* was keen to convince the world that PLoS would not survive if it did not increase its prices substantially. Butler returned to his theme again in June 2006, [reporting](#) that an analysis of PLoS' accounts "shows that the company falls far short of its stated goal of quickly breaking even."

He added triumphantly, "In an attempt to redress its finances, PLoS will next month hike the charge for publishing in its journals."

Butler was right on that point: shortly afterwards PLoS announced that it was raising its APC to \$2,500.

But Butler still believed that PLoS would struggle to make ends meet. "The figures show that PLoS lost almost \$1 million last year," he wrote. "Moreover, its total income from fees and advertising currently covers just 35% of its total costs. And although this income is increasing — from \$0.75 million in 2003–04 to \$0.9 million in 2004–05 — it lags far behind spending, which has soared from \$1.5 million to around \$5.5 million over the past three years."

Were Varmus and his colleagues about to prove themselves naïve again? Had they significantly underestimated the costs associated with running a publishing house, particularly when competing with the world's most prestigious journals?

Or were they simply being canny, conscious that it is better to win the hearts and minds of customers before handing them the bill?

Those who had been following Varmus' OA activities since 1998, however, may have been more inclined to conclude that by creating traditional peer-reviewed journals (OA or otherwise) PLoS had lost its way, or even sold out.

PLoS ONE: Return to first principles?

Shortly afterwards, however, PLoS announced the launch of *PLoS ONE*. This was to be very different to *PLoS Medicine* and *PLoS Biology*, and has served to change the nature of the debate — both about PLoS and about OA. While some saw *PLoS ONE* as a return to first principles others were to take a more cynical line.

The [press release](#) announcing the launch of the new journal described it in this way: "[V]irtually everything about *PLoS ONE* is new: the peer-review strategy, the production workflow, the author experience, the user interface, and the software that provides the publishing platform."

¹⁰ As Charkin [put it](#) to the UK Science & Technology Select Committee, "Very crudely, £30 million of sales: we get income of £30 million and we publish 1,000 papers a year."

¹¹ This figure is sometimes quoted in pounds rather than dollars. See for instance [here](#).

What was most distinctive about *PLoS ONE* at first glance was its scope. As Varmus put it in his book, *PLoS ONE* was to be “encyclopaedic”.¹² This was radical because the long-standing trend in scholarly publishing had been for journals to become increasingly specialised and narrower in scope, not broader.¹³ *PLoS ONE*, however, planned to accept papers from all disciplines within science and medicine.

[Speaking to me](#) in 2006 the then managing editor of *PLoS ONE* [Chris Surridge](#) pointed out that there was in fact no scientific topic that *PLoS ONE* would not consider. As he put it, “[I]f someone came to *PLoS ONE* with nuclear physics we would welcome them with open arms, and go and find some editors who knew something about nuclear physics who could make a decision about whether the paper was worth publishing. And if they felt it was worth publishing we would then publish it.”¹⁴

In retrospect, however, it is *PLoS ONE*'s approach to peer review that has proved most significant. Varmus [hinted](#) at this when I spoke to him in October 2005. *PLoS ONE*, he said, was envisaged as, “a very large compendium of papers that have been vetted for scientific quality, but which will not be confined in terms of their likely importance.”

Surridge [expanded on this](#) when he spoke to me at the time of *PLoS ONE*'s launch. “Traditionally a lot of the work that goes into peer reviewing consists of asking questions like: ‘How significant is this? How surprising are the conclusions?’ Essentially, these are subjective questions. A more objective question to ask would be: ‘Is this properly done science?’”

To that end, Surridge added, *PLoS ONE* reviewers “will be asked to answer a simpler question than has traditionally been asked. Essentially, he added that question is: ‘Has the science in this paper been done well enough to warrant it being entered into the scientific literature as a whole’.”

As a consequence, he added, “[W]e are looking at an easier decision on whether to publish.”

It is important to note that telling reviewers to ask a simpler question was based on PLoS' belief that a scientific paper's significance can only be properly established after publication. “We believe that the more subjective questions about how a paper relates to other work, and where it fits into the whole corpus of scientific literature are still important questions,” said Surridge. “But we feel that these can be better answered via an open peer review process that takes place after the paper has been published.”

In other words, in introducing a simpler review process *PLoS ONE* assumed that reviewing of the papers would continue after publication, and would be done by the wider research community. As the launch press release put it, “All readers will have the tools to add comments, annotations, and ratings to each article, so that post-publication review forms an integral part of the review process.”

It added, “*PLoS ONE* will empower the scientific community as a whole to engage in an open discussion on every piece of published work, capturing the varied and extremely valuable assessment of published papers that occurs after the work has been published.”

We shall come back to that point.

Meanwhile, for authors, Surridge told me the main attraction of *PLoS ONE* would be its ability to maximise the number of people (eyeballs) able to read their work, while at the same time providing a quicker, simpler, more efficient publication process.

As he put it: “If you want to spend months revising and re-revising your paper to satisfy the demands of two or three experts who may or may not be the most appropriate people to judge your

¹² On p. 264 of his book Varmus describes *PLoS ONE* as an, “encyclopaedic, high volume publishing site”.

¹³ Indeed, the history of the scholarly journal (mapping the development of science in general) has been one of increasing specialisation.

¹⁴ Today *PLoS ONE* publishes papers in subjects [that range](#) from biochemistry to medicine, to ecology, computer science, and the marine and aquatic sciences.

work (and so face a small chance of eventually being published), and if you are happy for your paper to be inaccessible to many of the readers who might wish to read it, then conventional journals are for you.”

If, on the other hand, he added, “you want to share your results — as soon as possible — with the whole scientific community in a way designed to stimulate discourse and so scientific advance, then *PLoS ONE* will be your journal of choice.”

It was an exciting and logical move on the part of PLoS founders. Importantly, the new journal appeared to fit well with Varmus’ initial vision. It also held out the promise of rapidly increasing the number of papers freely available on the Web.

This last point was important because it had become clear that highly-selective subject-specific journals like *PLoS Biology* and *PLoS Medicine* would not be able to catalyse PLoS’ revolution on their own — not least because in order to compete with high-quality prestigious subscription journals like *Nature* they were having to [reject around 80%](#) of the papers submitted to them.

As a *PLoS Medicine* [editorial](#) explained in 2006, “We feel that the unwanted side-effect of this rejection rate — and the authors of rejected papers would agree — is that we are turning away many valuable contributions.

It added, “As an organisation whose vision is a world of scientific publishing where there is an open-access journal for every paper worth publishing, it is essential that PLoS continues to create additional open-access venues to help reach this goal.”

It was for this reason, the editorial pointed out, that *PLoS ONE* had been launched. The aim, it seems, was to flood the world with OA papers — an ambition that was to become something of an *idée fixe* for PLoS employees.

So was *PLoS ONE* a return to first principles? It was, after all, an arXiv-like service (Varmus’ “huge digital library”), although unlike arXiv (but envisaged with E-Biomed) it would review papers itself rather than leave the task to others.

It is worth pointing out that arXiv does itself operate a “moderation” system. It assumes, however, that the papers will subsequently be published in traditional journals. *PLoS ONE*’s review process was therefore (in theory at least) more traditional than arXiv’s moderation process¹⁵. Moreover, it planned to send most papers out for review rather than moderate them internally in the manner of arXiv. Unlike both arXiv and E-Biomed, however, *PLoS ONE* planned to publish papers in all disciplines within science and medicines¹⁶ — so its ambitions were broader.

Certainly when he spoke to me in 2006 Surridge appeared keen to portray *PLoS ONE* as more in keeping with PLoS’ founding mission than *PLoS Medicine* and *PLoS Biology*.

As he put it, “[T]he system of disseminating scientific research has become extremely inefficient, and the concept of the journal has been eroded by the Internet ... [S]imply publishing journals and distributing them electronically isn’t using the full potential of the Web to make the dissemination of scientific information efficient and effective. It just isn’t the best way to do it anymore.”¹⁷

Sceptics, meanwhile, saw a more self-serving motive behind *PLoS ONE*. As Butler had pointed out, competing effectively with journals like *Nature* and *Science* would require more money than PLoS had access to. Cynics concluded, therefore, that *PLoS ONE*’s broad disciplinary approach and simplified peer review was an attempt to create a cash cow.

¹⁵ arXiv does not peer review the papers submitted to it but, as we note, it does have a [moderation system](#).

¹⁶ It is worth noting that the scope of arXiv too has grown over time. Today it [covers](#) Physics, Mathematics, Computer Science, Quantitative Biology, Quantitative Finance and Statistics.

¹⁷ We should note, however, that *PLoS ONE* does describe itself as a journal, where many view it more as a database, or archival service.

After all, they reasoned, *PLoS ONE* planned to charge \$1,250 per paper (subsequently increased to \$1,350) for providing, by its own admission, a no-frills stripped-down service. As such, they viewed *PLoS ONE* as a mass market product, but one designed to operate on high margins.

Even if that were true, however, *PLoS ONE* would first need to demonstrate that it was sufficiently appealing to the research community to prove successful. In 2006 success was by no means certain.

Bulk, cheap publishing

Certainly some observers had [doubts](#) about *PLoS ONE*. [Jason Kelly](#), for instance, [wondered](#) how the journal expected to “build up a community of editors/reviewers/annotators?” For instance, he asked: “What is the incentive for scientists to take the time to provide this feedback?”

As we shall see, Kelly’s scepticism on this last point was to prove justified. However, always keen to bulk up their CVs, researchers were soon signing up as editors, and submitting papers, to *PLoS ONE* in droves. Today the journal has over 1,000 academic editors, and the number of papers submitted is growing exponentially.

Consider that in 2006 *PLoS ONE* published just 138 articles. By 2007 this had grown to 1,230 and to 2,716 in 2008, making *PLoS ONE* the largest open access journal in the world. In 2009 it published 4,405 papers (a 65% increase on 2008), taking the total since launch to 8,495.

And this rapid growth continued in 2010. By the end of the year *PLoS ONE* had published [6,749](#) papers (a further 50% increase), out-publishing both [Physical Review B](#) and [Applied Physics Letters](#).

As a consequence, [OA advocates](#) now boast that *PLoS ONE* is the largest journal in the world. As it was only launched in 2006 it is not in fact the largest journal in terms of total numbers of papers published (rather than annual growth). But to have published 15,238 papers in a little over four years is a significant achievement. And at the time of writing this figure had grown to 17,099, up 1,861 on the year.

Why has *PLoS ONE* proved so successful? It surely helped that by the time the journal was launched PLoS as an organisation had spent a great deal of money marketing itself as an upstart right-on publisher willing and able to prove a David to the Goliaths of the industry. It doubtless also helped that PLoS was co-founded by a Nobel Prize winner and former director of NIH.

And as more and more authors concluded that having a paper published in *PLoS ONE* was sexy, so the journal entered a virtuous circle. This was to see it [win](#) the [Association for Learned and Professional Society Publishers’](#) Publishing Innovation Award in 2009 — for being “bold and successful and shaping the future of publishing” — and gain its first Impact Factor ([IF](#)) in 2010, providing authors with an even more compelling reason to publish in the journal.

For traditionalists, *PLoS ONE*’s IF came as a shock. Describing it as “a stunning 4.351”, OA critic [Phil Davis wrote](#) on the [Society for Scholarly Publishing](#)-sponsored blog [The Scholarly Kitchen](#): “[T]here seems something very strange about a journal that accepts nearly 70% of all submissions yet achieves such a score, especially with its first assessment.”

OA advocates, however, nodded wisely and asserted that it was a good demonstration of the so-called “[open access advantage](#)”.¹⁸ This was a rather unsatisfactory explanation for Davis — not just because he is a long-time critic of OA, but because his [PhD thesis](#) concluded that the OA advantage is a chimera.

But doubtless the fundamental appeal of *PLoS ONE* was exactly as Surridge had predicted: a journal that publishes [nearly 70%](#) of the papers submitted to it is a godsend for ambitious researchers looking for an outlet for their papers. That they can do so without having to spend months revising

¹⁸ By making their papers freely available on the Web, proponents of the open access advantage argue, researchers can expect to attract a far greater number of citations, and thus have a greater impact.

and re-revising their papers to satisfy the often quixotic demands of anonymous reviewers — “and so face a small chance of eventually being published” — made *PLoS ONE* all the more attractive.

For PLoS itself the most obvious benefit of the new journal’s success was that claims that it was not financially sustainable began to look increasingly hollow — for as submissions to *PLoS ONE* climbed so did PLoS’ revenues.

In 2010 PLoS was able to announce that it was well on the way to self-sufficiency. A [progress update](#) published last July, for instance, reported that PLoS “continued on its path towards operating profitability in 2009, exceeding plan expectations on all fronts”.

Specifically, total revenues for the period grew to \$8.8 million — a 36% increase over 2008 levels. This, PLoS explained, had been “fuelled mainly by strong growth in publishing volumes.”¹⁹

But while the sudden inflow of income was good news for PLoS, critics merely used it as another stick with which to beat the publisher. And a new charge emerged: PLoS’ primary aim, they argued, was not that of “shaping the future of publishing”, but keeping itself afloat at all costs.

In another [article](#) published in July 2008, Butler suggested that in order to make ends meet PLoS had decided to go down market, “relying on bulk, cheap publishing of lower quality papers to subsidise its handful of high-quality flagship journals”.

Entitled, “PLoS stays afloat with bulk publishing”²⁰, Butler’s article set off a firestorm of criticism and moral outrage amongst OA advocates. “This clumsy hatchet job from *Nature* reporter Declan Butler is beneath him, a poor excuse for journalism and an affront to the respect with which many of his colleagues are regarded by the research community,” [bridled](#) then PLoS employee [Bora Zivkovic](#).

“With this scurrilous parroting of anti-OA [FUD](#), *Nature* makes pretty clear where its interests and its allies are,” [snorted](#) Bill Hooker.

Costs

The backlash from OA advocates was sufficiently fierce that *Nature*’s [Timo Hannay](#) felt compelled to [join the discussion](#). Rather than seek to placate OA advocates, however, he repeated Butler’s claims, and then added another charge: PLoS, he said, was guilty of anti-competitive behaviour.

While stressing he was not claiming that PLoS was being intentionally anti-competitive, Hannay argued that the publisher had nevertheless created “effects that run directly against its stated aims of spreading the open access model across all of science publishing.”

He elaborated: “The situation back in 2002 was that existing publishers (Nature Publishing Group among them) were very sceptical that author-pays economics could ever work for high-end journals. Fast-forward to 2008 and far from bringing other publishers along with them, PLoS has helped to confirm these suspicions.”

Echoing Butler, Hannay added that PLoS’ flagship journals had only survived courtesy of subsidies, first from the Gordon and Betty Moore Foundation, subsequently from *PLoS ONE*’s bulk-publishing

¹⁹ The update added: “Consistent with our sustainability strategy to achieve operating profitability in 2010, operating revenues increased 45% over 2008 levels (\$8.9MM) while expenses increased only 24% over the same period, substantially narrowing our operating gap. Public support for the year was \$0.5MM, slightly lower than the previous year but consistent with expectations as we fund more of our growth through our publishing operations. PLoS posted its first profitable quarter in Q1 2010 due to strong growth in publishing activity, and we anticipate meeting or exceeding our financial targets for 2010.” The 2009 figures are available on [GuideStar here](#)

²⁰ The term “bulk publishing” was doubtless coined for *PLoS ONE* in order to attack it. We should note, however, that in his book Varmus himself describes *PLoS ONE* as “a high volume publishing site”. (p. 264)

activities. And by cross-subsiding its journals, he concluded, PLoS had blocked other publishers from the market.

PLoS, said Hannay, “has created heavily and perpetually subsidised journals that act as a strong disincentive for anyone else to try a similar publishing model. Which other publisher will enter the high-end author-pays segment now that there is an incumbent with 'private income' that will always be able undercut any competitor who needs to be economically self-sustaining? In short, the effect has been to reduce competition in an industry that, for the good of science as a whole, ought to have more competition.”

Hannay concluded with a barbed “hypothetical” scenario: “[Y]ou might imagine setting up a small series of loss-making open-access journals, thus killing off any chance of a competitive market emerging in that segment. You'd fund them through donations for as long as possible, but such income is vulnerable to the vagaries of donors so eventually you'd have to cross-subsidise them from a profitable business line. And all the while you'd explain that you're doing the world a favour.”

While critics preferred not to labour the point, in claiming that it was subsidising its sister journals they were, by implication, also accusing *PLoS ONE* of overcharging authors.²¹ For perhaps self-serving reasons, they preferred to construct a narrative that said *PLoS ONE* was generating excess revenue by providing low-cost, low-quality peer review services.

But however the narrative was nuanced, it encompasses two intersecting claims: First, in order to subsidise its flagship journals, *PLoS ONE*'s APC of \$1,350²² had been set higher than warranted for the service provided (a cost issue). Second, *PLoS ONE* was able to do this by providing minimal, possibly inadequate, peer review (a quality issue).

The underlying premise to this double claim is that the more papers a journal publishes the lower it will have set its bar of scrutiny, and thus the cheaper it will be to publish a paper. Or to put it the other way round: The more papers a journal rejects the higher will be the quality bar, and the more costly each paper will be to publish – because publishers incur costs not just for the papers they accept, but for those they reject too.²³

While this argument is widely accepted it is a very crude one, and certainly [open to challenge](#). Suffice it to say that *Nature*'s criticism of *PLoS ONE* was postulated on the claim that if a high-quality journal (like *Nature*) [rejects 90%](#) of the papers submitted to it, then the cost of reviewing 100 papers will – for accounting purposes – have to be borne by the 10 accepted papers.²⁴ If, by contrast, a less picky journal (like *PLoS ONE*) rejects only 30% of the papers submitted to it, then the cost of reviewing 100 papers can be distributed amongst the 70 accepted papers – significantly reducing per-paper costs.²⁵

In the traditional subscription publishing environment the relationship between rejection rates and per-paper costs was not overly dwelt upon, or much discussed – not least because publishers did not feel compelled to justify their costs. When authors are asked to pay to publish their papers,

²¹ Why would they? It is not in the interests of subscription publishers to draw attention to the research community's fears that scholarly publishing can be extremely lucrative.

²² Initially \$1,250.

²³ In a footnote on p. 264 of his book Varmus says, “Since *PLoS Biology* generally publishes fewer than twenty articles a month, the rejection rate is high; this factor both ensures prestige and drives expenses up, since well-paid professional editors must monitor the review process for rejected as well as accepted papers.”

²⁴ As *Nature* [put it](#) in 2004: “Economics dictates that high-quality journals like *Nature* have a high unit cost per paper published, because for every article published more than ten have been reviewed and deselected.”

²⁵ It is worth pointing out that most papers in high-quality journals are not actually sent out for review. A *Nature* editor recently [confessed](#), for instance, that he rejects “at least four in every five manuscripts straight off the bat, before review.” How much does it cost a publisher to hire someone to throw most of the papers it receives straight into the bin? In this light one is inclined to be highly sceptical of *Nature*'s claim that it costs \$10,000 to \$30,000 to publish a paper.

however, per-article costs become much more significant (and in theory transparent). Certainly they are thrown into sharp relief, and inevitably invite questions about costs not previously asked.

What none of this tells us, of course, are the actual costs the publisher of a journal incurs in publishing a paper – since even in an OA environment what the author pays may be entirely unrelated to the costs incurred by the publisher (which is what, by implication, *PLoS ONE* critics argue). So a key question to ask is, does *PLoS ONE* really incur the circa \$1,000 worth of processing costs for each paper implied by the above argument; or is it, as critics claim, overcharging authors in order to subsidise its sister journals?

In this respect, it is worth comparing the cost of *PLoS ONE*'s APC with the costs that arXiv estimates it incurs for every article posted to its database. Last year Cornell's [Simeon Warner reported](#) that this has been calculated at \$7 per paper.

As noted earlier, while arXiv does not operate a formal review process like *PLoS ONE*, it does nevertheless subject all papers to a process of [moderation](#). In other words, arXiv is not simply a dumb repository into which documents can be thrown willy-nilly; all the papers submitted are [checked](#) — for suitability, appropriateness, format, and what arXiv calls “refereable quality” amongst other things. If any document submitted does not meet the necessary criteria it is removed.

A relevant question to ask, therefore, is: To what extent does arXiv's moderation process differ from the reviewing done by *PLoS ONE*, and is this difference reflected in the estimated costs incurred. Put simply, is the additional work undertaken by *PLoS ONE* 193 times more costly than arXiv's moderation process?²⁶

We should also note that while publishers casually boast about the costs they incur in “doing peer review”, all they actually do is *manage* the review process. The reviewing itself is done by the research community, without charge to the publisher.

So is *PLoS ONE* overcharging in order to subsidise its sister journals? In the hope of finding out one of the questions I emailed *PLoS ONE* was: “What are the costs per paper for publishing in *PLoS ONE*?”

Unfortunately, I received no reply to my question.

The suspicion is that it *is* overcharging. And if it is, then *Nature*'s estimate that it incurs costs of between \$10,000 and \$30,000 to publish a paper begins to look unreasonably high.²⁷

There is another cost issue we will need to consider later. That is, even if publishers' charges were demonstrated to be fair and reasonable, can the research community still afford them, particularly at a time of global financial crisis?

Quality

We will come back to costs but, as indicated, the issue of costs is bound up with that of quality. This is a point Butler made in 2008 when he [said](#) that PLoS' financial future looks brighter, “thanks to a cash cow in the form of *PLoS ONE*”. One, he added, that “uses a system of 'light' peer-review to publish any article considered methodologically sound.”

Davis made a similar point last June on *The Scholarly Kitchen*. Arguing that *PLoS ONE* had been launched to fund the other PLoS journals he [said](#), “In financial terms, the purpose of *PLoS ONE* is to subsidise the cost of publication for their two flagship journals, *PLoS Biology* and *PLoS Medicine*, two highly-selective, high-cost journals that would be unable to continue in their current state

²⁶ A 2009 JISC-funded [report](#) estimated that a “self-archiving with overlay services” OA model would allow savings of £260 million per annum in the UK alone.

²⁷ As claimed [here](#).

without financial subvention. *PLoS ONE* is able to do this because of low selectivity — rejecting only 3 out of every 10 manuscripts.”

As noted, the assumption behind this claim is that the more papers a journal accepts the lower will be the quality of those that it publishes. That *PLoS ONE* states publicly that its reviewers are told to make an easier decision obviously fuels such claims.

Kent Anderson, publisher of [The Journal of Bone and Joint Surgery](#) and another frequent blogger on *The Scholarly Kitchen* is a sceptic too. And he worries about the impact *PLoS ONE* could have on scholarly communication at large. Last April he [reminded](#) readers that *PLoS ONE* publishes 70% of the papers it receives. Describing it as “not a tight filter,” he added: “In its early days, PLoS had an opportunity to break free from the herd. Instead, it joined the herd. That’s poignant enough. Now, it’s potentially carrying a business model with *PLoS ONE* which could make people doubt the quality or safety of the herd.”

The debate about *PLoS ONE*, therefore, is not just about the costs of publishing, and financial sustainability. It is also a debate about the quality of peer review. At bottom it is a question of whether the papers published by *PLoS ONE* are indeed of an acceptable standard to, in Surridge’s words, be entered “into the scientific literature as a whole.” If not, then what might be the likely impact on scholarly communication at large?

So let’s look more closely at *PLoS ONE*’s peer review. Is it indeed “light” in the way Butler claimed? What does light peer review imply anyway, and what are the implications if *PLoS ONE*’s review is indeed light?

We should note that PLoS has never described its peer review as light although, as we’ve seen, Surridge told me that when evaluating papers reviewers are instructed to ask a “simpler question” and make an “easier decision”. It would therefore seem fair to call the process light. PLoS has also never made any secret of the fact that it publishes the majority of papers submitted to it. Indeed, it prides itself on the fact.

The key question, therefore, is whether *PLoS ONE*’s review process is felt to be adequate. Unfortunately, this is not an easy question to answer — for two main reasons.

First, trying to assess the quality of peer review is very difficult because there are no universally agreed methods, standards, or formal processes against which to measure adequacy, or that specify how editorial decisions should be made. If one were to talk to ten different editors one would likely get ten different explanations of how peer review ought to be conducted, and exactly what is required. To a great extent, therefore, good peer review is in the eyes of the beholder

Second, peer review is invariably a non-transparent process: reviewers usually remain anonymous and their reports are rarely made public. And while *PLOS ONE* initially trumpeted a commitment to openness and transparency, and promised to make reviewer reports publicly available, that commitment appears to have waned over time. Indeed, at some point there may have been a change of policy. That at least would seem to me to be the implication of a 2008 blog [comment](#) by *PLoS ONE* academic editor [Cameron Neylon](#).

When I emailed Neylon — a senior scientist at the [Science & Technology Facilities Council](#), a UK funding body²⁸ — and asked him to clarify his comment he replied: “I’m not sure it was so much a change in policy as a change in the practicalities. It was just proving to be too much work. However this happened before I became an AE. I’m pretty sure the hope is that the review process can be opened up to make that information available. But the best person to ask about the details would be Pete Binfield.”

As we have seen, however, Binfield appears to be a little selective about what questions he answers, and by the time I emailed Neylon Binfield had in any case refused to answer the questions I had emailed to PLoS.

²⁸ Neylon is also now editor-in-chief of the new BioMed Central OA journal [Open Research Computation](#)

In August 2010, however, Binfield did say something about transparency. Asked by *The Scientist* whether reviewers should reveal their identities he [replied](#), “I think at some level there should be this transparency to the production process. The question is whether it puts off reviewers from being as frank with their comments, or even [from] review[ing a manuscript] in the first place.”²⁹

Be that as it may, the best we can hope to do in terms of assessing the adequacy of *PLoS ONE*'s review process is to try and measure its practice against its stated standards and procedures.

Let's remind ourselves how *PLoS ONE* describes its review process. As we saw, *PLoS ONE* reviewers are told to ask a “simpler” question than when reviewing for traditional journals. That question, Surridge told me is: “Has the science in this paper been done well enough to warrant it being entered into the scientific literature as a whole?”

We should note that this begs the question of how one defines “well enough”.

As it happens, Surridge may subsequently have regretted describing the review process in the way he did when speaking to me. After the interview he [wrote](#) on the PLoS blog “I'm quite distressed to discover how inelegantly I speak. Richard was asking some tough questions and I'm no politician.”

Others also picked up on Surridge's discomfort, and one commentator [concluded](#) “Perhaps there may have been some initial ambiguity about where the threshold for publication would be set.”

This confusion might however be seen to support the view that *PLoS ONE*'s review process is problematic: If the managing editor of a journal struggles to explain how its review process works, and what standards are applied, will not reviewers be confused too?

If we turn to the description of the *PLoS ONE* review process in the [press release](#) announcing its launch we read that: “[S]ubjective considerations like 'likely impact,' 'degree of advance,' or 'interest to a general reader' will not play a role in deciding whether an article should be published or not.”

The press release added, “Papers published by *PLOS ONE* will be held to rigorous standards of scientific quality.”

The *PLoS ONE* web site [describes](#) the review process in this way: “Too often a journal's decision to publish a paper is dominated by what the Editor/s think is interesting and will gain greater readership — both of which are subjective judgments and lead to decisions which are frustrating and delay the publication of your work. *PLOS ONE* will rigorously peer-review your submissions and publish all papers that are judged to be technically sound.”

We are bound to wonder how *PLoS ONE*'s “rigorous standards” are defined, and exactly what scientific quality and technical soundness are.³⁰ We might also wonder if any potential contradiction arises from applying rigorous standards while making a simpler decision — at least in the minds of reviewers.

In any case, the key concepts for reviewers appear to be the need to avoid making “subjective judgements”, to maintain “rigorous standards of scientific quality” and to ensure that papers are “technically sound”.³¹

²⁹ Note that a recent *BMJ* paper [concluded](#), “Telling peer reviewers that their signed reviews might be available in the public domain on the *BMJ*'s website had no important effect on review quality. Although the possibility of posting reviews online was associated with a high refusal rate among potential peer reviewers and an increase in the amount of time taken to write a review, we believe that the ethical arguments in favour of open peer review more than outweigh these disadvantages.”

³⁰ *PLoS ONE*'s [Guidelines for Authors](#) adds, “The research must have been performed to a technical standard high enough to allow robust conclusions to be drawn from the data.” But is this clearer?

³¹ [Elsewhere](#) PLoS says of the *PLoS ONE* review process: “For *PLoS ONE*, peer review is handled by academic editors who seek the opinions of other relevant experts, but the question asked is only whether the submission

One is inclined to point out that interpreting these terms would in itself require making a subjective judgement. What exactly do they mean?³²

As it is, one can envisage reviewers scratching their heads, and then simply ignoring the guidelines all together. But with words like “easier” and “simpler” resounding in the minds they would hardly be likely to take a hard line.,

Authors not readers

One frequently voiced concern about *PLoS ONE*'s review process is the journal's insistence that reviewers make no judgement about the significance or importance of the research they are evaluating. The problem with this, they argue, is that busy researchers don't have time to read everything published in their field. If the journals don't alert them to the papers they really need to read then how can they keep up to date?

As one researcher [asked](#), “Is a faster publication time vs. no (immediate) research significance assessment of papers really a good trade-off? Faster publication is good for authors but not a must for readers.”

This draws our attention to an important feature of “author-side” fees publishing: it radically changes the publisher's perception of who the customer is. In an OA world journals inevitably focus much less on the needs of readers, and much more on the needs of authors. And in today's “[publish or perish](#)” environment the priority for researchers (as authors) is to have as many papers published as possible. Only by doing so can they hope to advance their careers.

But while a journal with a 70% acceptance rate is very attractive to authors, it is less so for the researcher-as-reader — because if (as claimed) high acceptance rates lead to a large number of low-quality papers then the filter problem will surely grow over time.³³

So while other publishers have tended to focus on *PLoS ONE*'s business model, and the issue of pricing, for the research community the journal's rapid “bulk publishing” model (what Varmus calls “high volume publishing”³⁴) raises concerns that the quality of published research will decline, and the task of identifying papers worth reading will be that much harder as a result. This implies a vicious circle rather than a virtuous one. And even some of *PLOS ONE*'s own academic editors have begun to worry about this.

Meanwhile from the taxpayer's point of view a bulk publishing model inevitably raises questions about both quality and cost e.g. how can we be sure that tax-payer's money is being spent wisely? If the review process is lighter, how can the public be confident that when a researcher pays \$1,350 to publish a paper he or she is getting value for money, and that the paper is being adequately vetted before being placed in the public domain? What exactly is being provided for taxpayer dollars?³⁵

Finally, any discussion about *PLoS ONE* inevitably leads to the topic of publicity too. As we shall see, *PLoS ONE* papers are frequently used as marketing tools to promote not just the paper itself, but both *PLoS* and OA too — an activity that fosters exaggerated claims about the research in the paper and (ironically) its significance. The concern must be therefore that promoting research

has been rigorously and ethically conducted and properly reported; no account is taken of the potential significance or target audience for the work.”

³² There is a slightly more detailed [description](#) on the *PLoS ONE* Web site, but I do not think this really addresses the ambiguities implicit in *PLoS ONE*'s definitions.

³³ The increasing focus on the author rather than the reader may also partly explain why journals are finding it far more difficult to find reviewers. See for instance the comments of this [Nature](#) editor: “over the past couple of months I have seen an increase in the incidences of referees declining to review manuscripts because they are too busy.”

³⁴ On p. 264 of Varmus' book he describes *PLoS ONE* as an, “encyclopaedic, high volume publishing site”.

³⁵ As my experience suggests, Binfield is not of a mind to discuss this.

papers can hinder rather than aid the public understanding of science. And when the public is misled about the significance of published research it inevitably raises doubts about the underlying science, and thus the quality of *PLoS ONE*'s review process.

But is there any evidence that *PLoS ONE*'s peer review is inadequate, or fails to meet its own standards? Can we be confident that the journal consistently demands "rigorous standards of scientific quality" from the papers it reviews? Can we be sure that it only publishes research that is "technically sound"? And can we be sure that its standards are in fact adequate? Finally, is there any reason to be concerned about the way in which *PLoS ONE* papers are promoted and presented to the public?

Some have certainly expressed doubts on these matters.

Drivel?

In June 2007, for instance, *PLoS ONE* published a [paper](#) by a former investment banker on the relationship between male circumcision and AIDS. A [press release](#) written by the author publicising the paper stated: "male circumcision is found to be much less important as a deterrent to the global AIDS pandemic than previously thought ... [and] ... The new study finds that the number of infected prostitutes in a country is the key to explaining the degree to which AIDS has infected the general population."

It added: "prostitute communities are typically very highly infected with the virus themselves, and because of the large number of sex partners they have each year, can act as an engine driving infection rates to unusually high levels in the general population."

And it concluded that the study had "a number of important findings that should impact policy decisions in the future."

Shortly after the paper was published, however, AIDS researchers began to express a very different point of view, claiming that the paper was flawed in a number of important ways.

[Reporting](#) on the controversy, *Nature* explained: "[A]ccording to several angry AIDS researchers the paper merely shows that the peer-review system of the journal that published it, *PLoS ONE*, failed on this occasion." And added: "The study, they say, is flawed and, moreover, concerns a debate over statistical techniques that, in this instance, have been largely superseded by more powerful clinical trials."

[Tim Farley](#), a Geneva-based official in the World Health Organisation's HIV-prevention team put it more bluntly, [telling](#) *Nature*, "The paper is total drivel, it should have been picked up in the review process." He added, "In public health there are severe dangers in such stuff getting through."

In short, critics concluded that *PLoS ONE*'s review process had failed. They also felt that the paper had been inappropriately promoted by means of a press release put out by the author. True, a disclaimer from *PLoS ONE* had been attached to the press release, but as Farley pointed out, since the paper had been published in a peer-reviewed journal the views expressed in the press release inevitably acquired "a public perception of validity."

It did not help that the press release included the florid statement: "To argue we should do nothing about infected prostitutes during an AIDS epidemic because of a fear of creating a stigma against the infected would be like an animal rights activist claiming that a rabid dog must be allowed to run free in a neighbourhood regardless of how many men women and children he infected and killed."

Asked by *Nature* to comment Surridge responded, "There are lessons to be learned from all papers that we publish; we are a young journal ... We are feeling our way."

Controversies like this clearly do not inspire confidence in *PLoS ONE*'s light review system. It is also striking that rather than producing its own press releases (although it does do that), *PLoS ONE*

appears to prefer (and indeed [encourages](#)) authors to promote their papers by means of press releases, as apparently happened in this case.

We might also be tempted to conclude that making self-serving claims about the importance or significance of a paper makes a mockery of *PLoS ONE*'s claim that these things can only be established after publication – and presumably by other researchers rather than the author.

Of course one cannot generalise from a single paper, and an incident that took place nearly four years ago. However, a number of other *PLoS ONE* papers have subsequently attracted controversial attention, and there is growing suspicion that asking reviewers to make an “easier decision” on publication may be inherently problematic.

Technically sound?

Last April, for instance, Phil Davis [drew attention to another *PLoS ONE* paper](#); a paper, he suggested, that cast some doubt on the journal's review process.

The paper was entitled “[Do Pressures to Publish Increase Scientists' Bias? An Empirical Support from US States Data](#)”, and authored by the University of Edinburgh's [Daniele Fanelli](#). It was, said Davis, “a great example of a paper that may be technically fine, but fails miserably on theoretical grounds, and the methodology reveals it.”

Davis was not the only one to express doubt: [John Timmer](#) raised a number of questions about the paper on [ArsTechnica](#). Fannelli's paper, Timmer [explained](#), sought to test the hypothesis “that researchers in a competitive environment, who are most sensitive to the ‘publish or perish’ mentality that prevails in the sciences, would be less likely to publish papers that describe negative results.”

While conceding that the issues discussed were problematic, Timmer concluded that the new data provided by the authors did not appear to offer an adequate explanation for their conclusion. “The authors themselves raise some significant questions about their interpretations without even going into some of the basic issues with the data,” he complained.³⁶

Anderson also commented on the Fanelli paper. In a [separate post](#) on *The Scholarly Kitchen* he concluded, “The study critiqued [by Davis] shoves a sloppy hypothesis into a forced positive.”

Also critical was [David Crotty](#) — another habitué of *The Scholarly Kitchen*, and executive editor of [Cold Spring Harbour Protocols](#). In a comment on Davis' post Crotty [suggested](#) that it was not actually accurate to call the paper technically fine. “As you point out [he said to Davis], the theory, the methodology and the conclusions drawn are flawed. I'd go one step further and state that this is more likely a failure of *PLOS ONE*'s peer review process to do even the primitive job to which it aspires.”

Alerted to the criticism Fanelli posted several responses on *The Scholarly Kitchen*. Eventually giving up in frustration he [said](#): “[P]lease, if you really have nothing better to do than try to demolish my work, at least read both papers properly before.”³⁷

“Welcome to the future of science, at least according to *PLOS ONE*,” [snapped](#) back Crotty. “What you view as ‘demolishing’, *PLOS ONE* promotes as ‘post-publication review’.”

Crotty added: “You seem surprisingly uninformed as to the process *PLoS* encourages. Articles are reviewed solely for methodological accuracy – did the author do what the author says he did, no conclusions are made regarding the significance of the research. That's meant to be determined

³⁶ *PLoS ONE*'s “Guidelines for Authors” [require](#) that conclusions, “are presented in an appropriate fashion and are supported by the data.”

³⁷ Fanelli had pointed critics to a [second paper](#) of his called “‘Positive’ Results Increase Down the Hierarchy of the Sciences.” (This paper too was published in *PLoS ONE*).

after publication, in public forums such as this one, by the endless, faceless yobs of the internet, regardless of their level of expertise or understanding.”

It is noteworthy that [not a single comment](#) on the paper was posted to the *PLoS ONE* web site itself. As Jason Kelly had anticipated, researchers are not inclined to spend valuable time commenting on published papers — unless, apparently, doing so on their own site, a blog, or a social networking service.

This is significant since it draws our attention to what might be viewed as a serious potential flaw in *PLoS ONE*'s review process. As we saw, *PLoS ONE* asks reviewers to make an easier decision on whether a paper should be published — on the assumption that the real evaluation will take place after publication.

So the question arises: If a *PLoS ONE* paper receives no ratings, and attracts no comments, then what is its status as peer-reviewed research? After all, when a paper is published by a traditional peer-reviewed journal the public assume it to have been checked and certified as sound research by qualified specialists, who in the process will give some guidance on its importance. If no commentary is ever attached to a *PLoS ONE* paper, then by the journal's own criteria is not the review process incomplete?

True, the Fanelli paper did attract comments, but these took place not on the *PLoS ONE* site but on a blog. Moreover, many might feel that the tone and content of the comments was insufficiently objective, or scientific, to justify their being described as post-publication review — Crotty's claims about the “faceless yobs of the internet” notwithstanding.

It seems pertinent therefore to ask how many *PLoS ONE* papers do attract comments. Mulling this question over on *The Scholarly Kitchen* last April [Kent Anderson said](#): “*PLoS ONE* believes that its ‘innovative’ post-publication peer-review approach alleviates some of the obligations of publishing a scholarly journal. This innovation is essentially a form of commenting. But as a [just-released report on peer-review's role in academia](#) states in discussing *PLoS ONE*:

While submitted papers undergo a form of internal pre-publication peer-review, all ‘technically sound’ papers are published. (A scan of articles suggests that reader comments are, in fact, rare.)”

One of the questions I emailed to *PLoS ONE* therefore was, “Would it be accurate to say that it is generally only those papers that gain media attention that attract comments. What's the proportion of articles that get more than 1 comment? How many articles have 0 comments?”

Unfortunately my question was not answered.

Last December, however, PLoS did release some [figures](#) that offer insight into this. These suggest that sceptics are correct: post-publication reviewing is a rarity. [Writing on The Scholarly Kitchen David Crotty](#) commented: “[Q]uick analysis of the data shows a total of 23,934 articles in the set.³⁸ Of these, a mere 1,291 (5.3%) received a ‘rating’ (a user assigned assessment of the article's value). 1,042 of those had only one rating, and 208 had two ratings.”

He added, “As many science journals send articles out for review to three reviewers, that means that only 0.17% of PLoS articles received that same level of post-publication scrutiny as far as star ratings go. The numbers are similar for articles that received comments (2,606 total papers with comment threads, with 1,803 seeing only one comment thread and 512 two comment threads yielding 1.2%) and notes (745 papers with notes, 536 with one note, 116 with two yielding 0.38%).”

Crotty concluded: “Clearly, the research community has yet to embrace post-publication peer-review. If traditional methods are indeed in crisis and this is the immediate solution, the result

³⁸ Crotty's figures will be skewed since the data released cover all PLoS journals. Nevertheless, Crotty's conclusion appears reasonable.

then is a literature that is almost entirely unreviewed. I'm not sure that many researchers would see that as an improvement."

Flooding the media

PLoS ONE's habit of using its papers as a tool to promote both itself and OA have tended to fuel criticism of its review process. As we said, it also encourages its authors to engage in self-promotion too. This inevitably leads to concern about objectivity, and about the quality of the underlying science – particularly when exaggerated claims are made.

PLoS ONE's [guidelines](#) for authors who want to write their own press releases include the following advice: "The press often enjoys slightly quirky studies (from [flying fungi](#) to [grunting worms](#)), paleontology (including the [Nigersaurus](#)) and those on topical issues, such as [climate change](#) and [biodiversity](#)."

Members of the public might be inclined to ask, "Is this the purpose of science: titillating and amusing laypeople, and/or feeding their fears and neuroses? Is this indicative of the degree of respect that the scientific community has for nonscientists?"

Certainly some have been surprised at the extent to which *PLoS ONE* will go to help authors hype their papers. Last year, for instance, *PLoS ONE* published [a paper](#) reporting on the discovery of the [Darwinius masillae](#) fossil (dubbed "Ida"). The authors – who were already in discussions regarding the paper with a production company ([Atlantic Productions](#)), several television channels (History Channel, BBC1, ZDF, NRK) and a book publisher ([Little, Brown and Co.](#)) – began hyping the paper before it had even been published.

This led to [a flood](#) of wildly overstated stories in the media, including one (in *The Daily Mail*) [claiming](#) that the fossil was "a vital 'missing link' in human evolution", an assertion apparently encouraged by one of the authors ([Philip Gingerich](#)) – who told the press that the paper, "is going to advance our knowledge of evolution."

While it was not *PLoS ONE* that made these claims, the publisher showed itself more than happy to exploit the media frenzy set in train by the paper's authors. On the day of publication (May 19th 2009), for instance, it [posted a message](#) on its blog saying that in order to assist the authors in their publicity campaign it had published the article ahead of time.

The post explained, "We published the paper at 10.30 a.m. Eastern Time today, rather than our usual publishing time of 8 p.m. ET, because at 10.30 this morning, the fossil was unveiled at the [American Museum of Natural History](#) (AMNH) in New York, to an audience which included Ellen Futter, the President of the AMNH and Michael Bloomberg, the New York City Mayor."

The post continued: "The findings have already made and continue to make a significant impact in the media (we will post a summary of the coverage later in the week); you can read the freely available *PLoS ONE* article, in full, online [on our website](#) (we encourage everyone who writes a news story or blog post about the study to link to this URL). A special documentary film, *The Link*, will be screened on US television on May 25th and in the UK on May 26th and a major book (also called *The Link*) is published on May 20th."

With language like this one wonders where a blog post merges into, or itself becomes, a press release. (*PLoS ONE* did itself also put out a press release about the paper, but this no longer appears to be available).

As soon as the paper was published other researchers began to pooh-pooh the exaggerated claims that had been made, and to deplore the way the paper had been hyped. "Ida is undoubtedly a spectacular fossil," [wrote](#) science writer [Brian Switek](#) in *The Times*. "A nearly complete fossil primate, with a body outline and stomach contents, she is the sort of discovery palaeontologists dream about. It may come as a surprise, then, that Ida does not change everything we thought we knew about human evolution."

PLoS ONE was criticised both for fanning the flames and, more significantly, for withholding the paper from journalists who had attended a press conference given by another of the authors – [Jørn H. Hurum](#). Without access to the paper, journalists complained, they could not obtain any independent scientific views on it, leaving them little option but to recycle the hype, or not report at all.

Writing on his blog [Carl Zimmer commented](#): “[I]t appears that both PLoS and Atlantic Productions did not give journalists any time to consult with outside experts before launching a major press conference with a huge blitz of media attention. In other words, science writers who were *trying to do their job well and responsibly* were actively hindered. Those who declared ridiculous things, such as claiming that human origins were now solved once and for all, were not.”

[Responding](#) on Zimmer’s blog, Binfield insisted that *PLoS ONE* had done everything it could to make the paper available as soon as possible: “Once the paper was accepted we made a strenuous effort to publish the article in time for the Press Conference which was happening on May 19th – only a week later. We were not involved in the Press Conference, but felt it was clearly in the public interest to have the article publicly available in time for that conference. Our production team managed to get the article published more than two weeks quicker than normal, so that it would be ready for the 19th. However, it was only on the afternoon of the 18th May that we knew the paper would definitely be available in time and until that point, no final copy of the paper was available.”

He added, “We do regularly help *PLoS ONE* authors with the distribution of press releases under an embargo, as do many other journals, but when we do this we only ever issue that information on a date that is acceptable to the authors. The authors of this paper requested that we did not issue a press release, or reveal any other information about this paper, until 10.30 EST on the 19th May (the time of the press conference). We respected their wishes, and at the time of publication also issued our own press release about this article.”

Zimmer, however, remained sceptical: “I’m curious what scientists, journal editors, reporters, and other readers think of Binfield’s response. Other prominent journals don’t leave these matters up to the scientists (or their television producer pals). They inform the scientists of which issue a paper is scheduled for, and they put the paper on a press list a few days earlier.”

Mark Henderson of *The Times* subsequently emailed Zimmer to report: “[T]he PLoS paper WAS made available under embargo to the press – but only to selected individuals and under very unusual restrictions. I was invited to read it by Atlantic Productions on Tuesday morning (I’m Science Editor of *The Times* in London), but I had to go to their offices to read it and wasn’t allowed to take a copy away. I also had to sign a non-disclosure agreement, which meant I wasn’t able to approach anyone else for comment until the embargo lifted. *The Guardian* also had advance access (they got to see more in advance than we did, and earlier). So – obviously because they bought the film rights – did the BBC. But other UK papers (*Independent*, *Telegraph* etc.) got nothing. This is a very weird (and in my experience unprecedented) way to manage the release of published science.”

PLoS ONE does appear to be a little too keen to court the press. As noted, it also encourages its authors to do the same. And Henderson, Switek and Zimmer are surely right to question the wisdom of this. The upshot in this case, Switek concluded, was that: “What could have been a unique opportunity to communicate science has quickly developed into a fiasco. Science proceeds through discovery and debate, and hypotheses do not become accepted by flooding the media with press releases.”

Nevertheless PLoS remains keen to court the attention of the media. In its [progress update](#) referred to earlier it cites the *Darwinius masillae* “fiasco” as evidence of the high media interest *PLoS ONE* research gets. And it added that more than 200 articles published in *PLOS ONE* in 2009 were covered by international media and bloggers.

Could it be that PLoS is struggling to separate its original advocacy role from its new role as publisher?

Of course, the controversy over the *Darwinius masillae* paper was not about the quality of the research, or of *PLoS ONE*'s review process, but the overblown way in which the paper was promoted, and the willingness of *PLoS ONE* to collude in the hoopla — even to the point of changing the publication date to fit around press conferences and media events. The problem is that this tends to raise doubts about its objectivity and about its priorities; doubts that do not sit well with the role of a non-profit science publisher.

It may be that this has become a sensitive issue for *PLoS ONE*: It is noteworthy, for instance, that the only question Binfield directly addressed from the list I emailed was one relating to the hyping of papers. (See page 22)

Hurray for Science!

Is there a causal relationship between hype and allegations of low quality? The controversy over the *Darwinius masillae* paper would suggest not. But the two things do seem to have a habit of going hand in hand. They coincided, for instance, in the controversy over the wind setdown paper that was drawn to my attention last August.

Entitled [Dynamics of Wind Setdown at Suez and the Eastern Nile Delta](#) the paper was authored by two researchers at the National Center for Atmospheric Research ([NCAR/UCAR](#)) at the University of Colorado at Boulder ([CU](#)). The lead author Carl Drews is a software engineer at NCAR/UCAR; the second author, [Weiqing Han](#), is an associate professor in the Department of Atmospheric and Oceanic Sciences (and Drews' supervisor).

What exactly was the issue? In making a scientific case for a natural process referred to as “wind setdown”, the paper's authors used as an example the 3,000 year-old biblical story of the parting of the Red Sea to allow Moses to lead the Israelites out of Egypt.

In a short space of time other scientists, including two of *PLoS ONE*'s own academic editors, began posting critical remarks about the paper. One of those editors — [Björn Brembs](#) from Berlin's [Freie Universität](#) — made the following [comment](#): “[T]his paper presents a reasonable scientific explanation for a documented event that some people for some time took for a religious miracle. Hurray for science!”

The problem, he added, is that, “there is scant, if any, evidence that the documented event ever took place, as there appears to be little archaeological evidence of any Jewish habitation in Egypt, nor, hence, any exodus. Clearly, scientific phenomena cannot be defined merely in some bestseller, or we'd be reviewing heaps of vampire and alien studies; which, in this case, leaves us with a paper that explains a phenomenon that most likely never existed.”

Another researcher called Steven Smith [agreed](#): “Because there was never an Exodus, an attempted physical proof of the parting of the Red Sea is beyond silly. The archaeological evidence is conclusive: The Jews were never in Egypt, there was never an Exodus, and no Jewish conquest of the land of Israel.”

What seemed to especially annoy critics was that while Moses and the Exodus are mentioned only in the abstract (just once and in passing) Drews appeared keen to imply that the paper had provided some sort of scientific proof that a mythical event had indeed taken place — notably by means of a [press release](#) and a [video](#) — both of which focused almost exclusively on the biblical story (In fact, the paper discussed three different sites).

As the press release put it, “The biblical account of the parting of the Red Sea has inspired and mystified people for millennia. A new computer modelling study by researchers at the National Center for Atmospheric Research (NCAR) and the University of Colorado at Boulder (CU) shows how the movement of wind as described in the book of Exodus could have parted the waters.”

As with the publicity surrounding the *Darwinius masillae* paper, a flurry of breathless and exaggerated [media stories](#) followed, with coverage by the [BBC](#), [Wired](#), [The Los Angeles Times](#) and

[CNN](#), amongst others. [AFP](#) was sufficiently excited by the press release that it headlined its report “[Moses had help parting the Red Sea: study](#)”. Drews appears to have encouraged such a conclusion, and is quoted saying: “So now there’s a scientific basis for a 3,000-year-old story that we’ve seen movies of and read books, and that’s really exciting.”

This was hype. But did the incident reflect badly on *PLoS ONE*’s peer review process?

Critics thought it did, concluding that the reviewers had allowed the authors to practice a sleight of hand. Commenting on the single reference to the biblical story, another *PLoS ONE* academic editor — [Ramy Aziz](#) of [Cairo University](#) — [said](#): “I find this statement irrelevant to the scientific topic and also culturally biased.”

Aziz added, “Normally, the Abstract should summarise the paper and not advance a new (here pivotal) idea that is not explicitly mentioned in the paper.”

[Responding](#) to his critics Drews commented: “The historicity of the Exodus is outside the scope of this paper, which deals with the dynamics of wind setdown at several coastal sites, modern and ancient. Since this is a scientific paper in a scientific journal, we have omitted references to what a deity did or did not do.”

In short, Drews’ argument was that his paper made no judgement about the truth of the biblical story, or the likelihood of divine intervention, but simply used the mythical event as an example of a natural phenomenon. As he [put it](#) to one cynic, “We [only] addressed Egyptian history in the context of reconstructing the likely topography of the eastern Nile delta circa 1250 BC.”

The unanswered question of course was, why build a paper around a biblical story in the first place? Drews’ critics pointed out that he runs [a web site](#) promoting [theistic evolution](#). Amongst other things, the web site mentions the Red Sea crossing; and the strap line on the home page reads: “faith and science are compatible”.³⁹

Again, it was not *PLoS ONE* that had put out the press release. Nevertheless, it does encourage authors to market their own papers. And as Farley pointed out in connection with the HIV paper, if a paper is published in a peer-reviewed journal the views expressed in any associated press release inevitably acquire “a public perception of validity.”

In fact, the wind setdown press release was the work of NCAR/UCAR. I asked the head of media relations David Hosansky whether he felt that there had been any sleight of hand in the way the paper had been presented to the media. “We are comfortable with the press release,” Hosansky replied. “Neither the study nor the press release attempted to make conclusive statements on the historicity of the Exodus which, as Carl Drews has noted, is outside the scope of the paper.” Had the aim been perhaps to find the oceanic science equivalent of grunting worms and flying fungi?

So was there a failure on the part of the reviewers?

Brembs believes so. “I think two mistakes were made,” he emailed me last year. “For one, there was no proper reference to the particular mythology the authors referred to. Second, there was no reference to alert the reader that the mythology in question lacks empirical support. Both mistakes should have been caught by the reviewers, or by the academic editor.”

One way of judging how effectively a paper has been reviewed would be to examine the reviewers’ reports. The *PLoS ONE* Guidelines for Authors [states](#), “upon publication *PLoS ONE* routinely posts the referees’ reports as comments accompanying the online version of papers.” And it adds, “By posting the referees comments, with their permission, *PLoS ONE* hopes to make the review process more transparent as well as stimulating informed debate of the published papers.”

³⁹ This information was in fact listed in the competing interests section of the paper.

In the case of the wind setdown paper, unfortunately, neither of the reviewers' reports for the wind setdown paper were published on the web site. But as Neylon flagged in 2008, very few are now posted anyway.

I nevertheless asked to see the reports. Binfield later replied: “[W]e did ask both reviewers if they would share their reports with you. One replied and expressed that he did not want to share his report with you, and the other has not yet replied.”

I heard nothing further.

Whether there was a causal relationship between the controversial reviewing of the wind setdown paper and the way it was hyped is a matter for speculation. But it is surely not advisable to put out press releases with exaggerated claims, or claims that do not accurately reflect the contents and conclusions of a paper, as doing so (rightly or wrongly) tends to raise doubts about the quality of the underlying research, and therefore of the peer review process it has been subjected to — regardless of how much the release might titillate the media.

Crucial role in expanding open access

As indicated, it was as a result of the controversy surrounding the wind setdown paper that I contacted *PLoS ONE* executive editor Damian Pattinson.

One of the questions I sent was this: “*PLoS ONE* appears to be very keen to spread news about its research (not least via its blog), and in ways — some might argue — that gloss over the complexities of the research in question. As a result every now and then exaggerated interpretations of data are widely disseminated (e.g. the paper we are discussing and, last year, [the paper](#) on *Darwinius masillae*), thereby generating negative publicity for the publisher. Yet when this happens *PLoS ONE* always distances itself from what the media say about the articles. Given that *PLoS ONE* courts publicity, and could be said to be partially responsible for the exaggerated claims that sometimes emerge about its articles, is it not guilty of hypocrisy?”

When I received the email from Binfield declining to answer my questions he at least did address this point, sending me the following statement (which he requested I quote in full):

“As regards the media and blog coverage surrounding this article, it occurred several weeks after it was published, and was not the result of PLoS issuing a press release about the article. We issue press releases on a very small number of the approximately 150 articles that we publish in *PLOS ONE* each week and we did not issue a press release for this article.”

He added, “There is much further information about *PLoS ONE* on the journal web site (data on the [peer review process](#) are provided and the *PLoS ONE* editorial criteria are [detailed](#)), and readers are encouraged to contact us if they have any questions.”

And he concluded, “We believe that *PLoS ONE* has a crucial role to play in expanding open access to new research findings, and tens of thousands of researchers are supporting this effort as editors, peer reviewers and authors.”

Those familiar with the publisher will recognise in the last sentence PLoS's organisational *idée fixe* about expanding OA — leading one to wonder whether critics might be right to suggest that the primary goal of PLoS is no longer that of “shaping the future of publishing”, but “OA at all costs”. If correct, PLoS' priority has presumably shifted from developing high-quality OA journals, to seeding the world with so many freely-available papers that OA becomes the default for publishing research papers.

That is undoubtedly a worthy goal, and it seems likely to succeed. But is there a danger that adopting such a narrow strategy in trying to solve a problem as multi-faceted as the scholarly communication crisis could cause collateral damage — not just in terms of hampering the public understanding of science but, as we shall discuss, undermining the very *raison d'être* of scholarly communication? Could “OA at all costs” be in danger of doing as much (or more) harm as good?

Certainly the researcher who drew my attention to the wind setdown paper saw the row it had sparked in a wider context. And one suspects that all the paper's critics did too.

Scientific rigour

It would however be wrong to imply that *PLoS ONE* did not address the issues raised by the wind setdown paper: On 30th September last year Pattinson posted an official [comment](#). But I found the comment somewhat ambiguous, which is partly why I emailed over my questions.

Pattinson began by saying that “the scientific rigour of the work has not been questioned”, and that the paper had been assessed by an academic editor plus “two relevant experts”.

Since scientific rigour is one of the main criteria for acceptance by *PLoS ONE*, it is unsurprising that Pattinson stressed the point. He did however appear to concede that the review process had not been adequate. Specifically, he suggested that the paper “could have been framed, interpreted and discussed differently.” Nevertheless, he added, *PLoS ONE* did not feel there were “any grounds to retract or formally correct the article.”

Again we find ourselves wondering exactly what scientific rigour is. And if scientific rigour is the litmus test for whether a paper should be published, can we be confident that the review process is adequate?

Let's remind ourselves that Varmus' description of *PLoS ONE* was “a very large compendium of papers that have been vetted for scientific quality, but which will not be confined in terms of their likely importance.” One question to ask with any controversial paper published by *PLoS ONE*, therefore, is whether it could be said to have met that standard (assuming we are confident we know what scientific quality is).

Pattinson added: “there are lessons that we can learn from the reaction to this article (and to other recent articles that have generated criticism)”. However the implication seemed to be that ultimate responsibility lay with the academic community. As he explained, while *PLoS ONE* staff “screen” incoming papers for such things as potential conflicts of interest, “[o]nce the articles are assigned to academic editors, we rarely intervene in the peer-review process but we provide as much support to the editors as we can, and we will redouble our efforts to do this in future, so that we can ensure that all articles, especially controversial ones, are presented as clearly and accurately as possible.”

Stressing this point, Pattinson said that *PLoS ONE* “is run by the broader academic community, using a distributed editorial model whereby individual academic editors take responsibility for specific editorial decisions.”

Cynics might have concluded that Pattinson was trying to pass the buck, implying that if any criticism is due it should be directed at the reviewers, and the academic editor, not at the publisher. But that would be to ignore the fact that *PLoS ONE* defines what criteria must be applied when reviewing papers, not the reviewers, and not the academic editor. In other words, it is *PLoS ONE* that tells reviewers to ask simpler questions and to make easier decisions. And it is *PLoS ONE* that insists on a fast turnaround (inevitably putting pressure on reviews to also deliver fast, rather than necessarily thorough, decisions).

Pattinson concluded by pointing out that *PLoS ONE* expected to publish around 7,500 articles by the end of 2010, becoming “the largest peer-reviewed publication in the world.” For that reason, he said, “It is inevitable that *PLoS ONE* will publish articles that occasionally touch on controversial topics”.

That would seem to imply that the row had been over the *topic* of the paper, not its quality, or the adequacy of the review process it had been subjected to. It may be that critics disliked the topic, but their real beef surely was with the way that the paper had been written, what they viewed as a

sleight of hand on the part of the authors, and the failure of *PLoS ONE*'s review process to see that this was corrected before publication.

Production values

As we see *PLoS ONE*'s appear to have a habit of attracting criticism. But is it simply a by-product of publishing so many papers (as Pattinson implied) or is *PLoS ONE*'s review process inadequate — as critics imply? The papers discussed above certainly raise questions, both about *PLoS ONE*'s review process, and the way in which the publisher communicates science — not least by encouraging authors to engage in self-promotion.

In terms of adequacy, there are a number of more specific issues one could look at too. For instance, *PLoS ONE* does not copyedit the papers it publishes, only the abstracts. But it would appear that even this minimal service is not always provided. That this can be problematic became apparent to me in November 2008, when a researcher drew my attention to [an article](#) whose abstract was barely comprehensible.

The paper was called “Incompetence of Neutrophils to Invasive Group A *streptococcus* Is Attributed to Induction of Plural Virulence Factors by Dysfunction of a Regulator”. The abstract was riddled with spelling errors, malapropisms, and grammatical errors, and included the following incoherent sentence: “In addition, rare outbreaks of invasive infections and their distinctive pathology in which infectious foci without neutrophil infiltration hypothesized us invasive GAS could evade host defense, especially neutrophil functions.”

When I asked Binfield to comment he agreed that there was a problem, replying: “Having looked at this example, clearly the language here wasn't up to scratch. I queried our production department and it seems that this abstract slipped through the cracks and was not copyedited properly.”

He added, “I am going to be following up with the Academic Editor who handled this paper ([Dominik Hartl](#) — as named on the paper) to see what his opinion is, and whether we should make a formal correction to PubMed as a result.”

When I looked at the abstract again last October I noticed that the errors were still there. This time I contacted Hartl, and I asked him what the outcome of his conversation with Binfield had been. Hartl, however, seemed unaware of the issue, and asked: “What do you feel is in its current version incorrect in this abstract?”

I suggested he start by reading the sentence cited above, and go on from there. Later I received a follow-up email from Hartl. He wrote: “That definitively needs to be corrected, I agree.”

He added, “Normally, *PLoS ONE* proof-read. Editors should have corrected this prior to submission.....”

Once again, however, my enquiry appeared to fall into a black hole. At the time of writing the abstract remains in its uncorrected form, both on the *PLoS ONE* site and in the [PubMed](#) and [PubMed Central](#) records two years after I first contacted Binfield, and over four months after I contacted Hartl.

Perhaps this is unsurprising: When I contacted Binfield he seemed to be signalling that he does not take such matters very seriously. Explaining *PLoS ONE*'s “production values”, he said: “Speaking for *PLoS ONE* we do not copyedit content (other than a very light clean up). We do a light (but real) copyedit on the abstract; and at time of submission one of our (many) Quality Control checks is on the quality of the English. However, as a general rule, if the language is intelligible, and passes QC and passes peer review etc., then it will be published as is.”⁴⁰

⁴⁰ It is worth noting that the *PLoS ONE* [guidelines](#) state quite clearly, “Perfect, stylish English is not essential but the language must be clear and unambiguous. If the language of a paper is poor, Academic Editors should recommend that authors seek independent editorial help before submission of a revision.” [Elsewhere](#) it says, articles must be “presented in an intelligible fashion”, and “written in standard English.”

Maybe Binfield's views are general amongst scientists today, and so unremarkable.⁴¹ However, members of the public — who fund most of the research published in *PLoS ONE*, and who fund PubMed and PubMed Central — might wonder why it is thought acceptable for the abstracts of scientific papers to verge on the incomprehensible. They might also wonder how, if this is the end product, *PLoS ONE* can justify charging \$1,350 to publish a paper.

Since the taxpayer additionally pays for researchers' time when they do the reviewing (via their salary), the public might be forgiven for concluding that it is getting pretty poor value for its tax dollars. Should not charging a fee of \$1,350 at least require the publisher to run a spellchecker over a 267-word abstract? Should not professional scientists be expected to describe their research in an articulate and coherent manner when putting it into the public domain?

And if Butler and Davis are right to claim that *PLoS ONE* is overcharging for its services in order to subsidise its sister journals is it not unfair on *PLoS ONE* authors (and their funders)? Why should scientists who have failed to get their papers published in a prestigious journal be expected to subsidise authors who have succeeded in doing so?⁴²

Pointless?

Back to the question of whether *PLoS ONE*'s review process might be deemed inadequate or problematic. As we've said, some believe that it is — not just critics of *PLoS* like Davis, Crotty and Anderson, but *PLoS ONE* academic editors too.

Indeed, at least one *PLoS ONE* author has his doubts too. Last year a researcher whose first paper had just been accepted by the journal copied me into an email he sent to his co-authors. He wrote: "My opinion of *PLoS ONE* (which I usually praise) has gone down considerably given this very superficial and purely cosmetic refereeing, which gives no evidence of having understood the paper or even having read it critically rather than just self-interestedly."

If *PLoS ONE*'s own customers believe that the service it provides is inadequate then should we not all be concerned?

But if there is a problem what exactly is it?

There are a number of issues we could explore further — not least the implications of asking reviewers to make easier decisions, and the ambiguity of some of the instructions given to referees — but one of the most frequently cited concerns relates to *PLoS ONE*'s insistence that reviewers take no account of significance when evaluating papers.

This is the issue that critics like Davis and Anderson most frequently raise — not (just) because it means abandoning the filtering role that journals have traditionally played, but because they believe that attempting to review a paper purely on the grounds of technical soundness (the consequence of ignoring significance) can lead to official certification of pointless research. As Davis [argued](#) last October, it means reviewers are required to accept papers that have little or no inherent value.

If correct, this would suggest that *PLoS ONE* is in danger of encouraging researchers to waste their own time, and thus the time of their reviewers, and any readers. One might also wonder why the taxpayer is expected to pay \$1,350 to publish a paper that has little or no worth.

The question to ask, suggested Davis, is: "Can a scientific paper be methodologically sound, but just not report any significant or meaningful results?" He added, "What constitutes whether a paper that is 'technically sound' is much more nuanced and much less clear than it appears. In fact, you will not find discussions of what makes a sound methodology in any methods textbook."

⁴¹ [This](#) discussion, however, suggests otherwise.

⁴² One assumes that, all things being equal, any researcher would prefer to be published in *PLoS Medicine* than in *PLoS ONE*

In other words, what is the value of a paper — even if it is technically sound — if its publication is pointless?⁴³

To make his point about methodology Davis asked a number of colleagues from different disciplines to define what they understood by sound methodology. The replies he received varied considerably, but were generally too vague to be useful (e.g. “appropriate methodology is what your community accepts as appropriate methodology.”)

So we are left wondering whether scientific rigour and technical soundness are adequate tests. It could be that *PLoS ONE*'s approach would work with a journal addressing a small sub-discipline. But in aiming to cover “all disciplines within science and medicine” can it really hope to provide a uniform approach to peer review? How can reviewers from different disciplines know what *PLoS ONE* means when it asks for scientific rigour and technical soundness if there is no science-wide consensus on sound methodology? And in the absence of clear definitions, how can we judge whether *PLoS ONE* is measuring up to its own standards?

And where a journal has an editorial board as large as the one now deployed by *PLoS ONE* its review process must surely start to become unmanageable, and at some point impossible to police. As we saw, *PLoS ONE* has around 1,000 academic editors. This figure, I am told, is set to double in the near future.

One of the questions I asked *PLoS ONE* was: “Would you agree that when an editorial board gets as large as *PLOS ONE*'s it is very difficult to have unified standards? Academic editors are inevitably like judges who will always make different rulings. If the number of academic editors keeps increasing there will surely come a point where there will be no standards at all. What one editor rejects will be perfectly acceptable to another editor. This is surely demonstrated in the case of the wind setdown paper: Two *PLOS ONE* academic editors have signalled that they would not have accepted it — in its current form at least — and yet it was accepted by another editor?”

I received no answer to this question.

I also asked this question: “I understand that *PLOS ONE* currently has 1,000 academic editors, and this is set to increase to 2,000. Is there not a danger of creating ‘families’ within the editorial board? An editor could, presumably, invite a bunch of friends who are not affiliated with him directly to submit papers and guarantee that if they submit a paper s/he will guarantee it will go to friendly reviewers. After all with numbers like that nearly every author will have a friend in the board. Is that not a recipe for potential corruption?”

Again I received no answer to my question.

And as I had been told that *PLOS ONE* academic editors can, if they wish, simply waive papers through without sending them out for review, I also asked *PLOS ONE* the following question: “I understand that academic editors are able to accept a manuscript right away without seeking a second opinion. Is that true? If so, is that not also likely to encourage corruption? Do you have any figures on the number of papers that are immediately accepted by academic editors?”

Alas, no answer to this was forthcoming either.⁴⁴

All in all, one is inclined to conclude that *PLoS ONE*'s review process must be somewhat arbitrary and, let's face it, rather unscientific.

⁴³ For authors, of course, it will not be pointless, since publication adds another line to their CVs, and so helps in terms of tenure and promotion. But the paper may nevertheless be pointless in terms of adding value to mankind's scientific endeavour.

⁴⁴ In the links that Binfield sent me, however, I did find this [statement](#): “[Academic editors] can conduct the peer review themselves, based on their own knowledge and expertise.” Another link led to [a note](#) saying 8% of the articles in *PLoS ONE* are reviewed by the editors themselves.

Journal form at risk?

Where does this leave us? We've suggested that *PLoS ONE* raises two interconnected issues for scholarly communication — one relates to quality, and the other to costs. And in considering these issues our attention has also been drawn to the problems inherent in promoting scholarly papers as though they were products. Doing so seems inevitably to lead to exaggerated claims, and hinders rather than aids the public understanding of science.

In considering the topic of quality the perennial issue raised by OA critics is that in an author-side fees model the primary customer becomes the author rather than the reader. This, they say, has implications, not least that it inevitably exerts a downward pressure on quality, since publishers face much greater pressure to accept papers than they do in a subscription model.

In a memorandum submitted to the UK [Science & Technology Select Committee](#) in 2004 [Elsevier described](#) the danger this way: “By introducing an author-pays model, Open Access risks undermining public trust in the integrity and quality of scientific publications that has been established over hundreds of years. The subscription model, in which the users pay (and institutions like libraries that serve them), ensures high quality, independent peer review and prevents commercial interests from influencing decisions to publish. This critical control measure would be removed in a system where the author — or indeed his/her sponsoring institution — pays. Because the number of articles published will drive revenues, Open Access publishers will continually be under pressure to increase output, potentially at the expense of quality.”

In other words, because in an OA model authors buy a service from the publisher, the author becomes the ultimate customer. Since authors need to have their papers published in order to advance their careers, and OA publishers know that every sale will increase their income, the pressure to prioritise income and careers over quality becomes too great to ensure scientific objectivity.⁴⁵

What Elsevier did not predict is that OA would spark a “[gold rush](#)” of new author-side fees publishers offering services more akin to vanity publishing than scholarly communication. That, however, appears to have happened. In [a 2010 review](#) of some of these companies US librarian [Jeffrey Beall](#) dubbed them “[predatory publishers](#)”. They are predatory, he argued, because their mission “is not to promote, preserve, and make available scholarship”, but to “exploit the author-pays, Open-Access model for their own profit.”

Elsevier likewise did not predict (although it was perhaps implied in its memorandum) that author-side fees publishers might deliberately introduce a “simpler” or “lighter” review process as *PLoS ONE* has done.

We should not doubt that PLoS' primary motivation is to further the cause of OA, not (as is doubtless the case with predatory publishers) to make a financial killing. But if, as critics claim, *PLoS ONE*'s APC has been deliberately over-priced in order to subsidise PLoS' flagship journals we should be concerned — not least because it has implications for scholarly communication at large.

What are these implications? First, there is a danger that quality will be sacrificed on the altar of profit. For that reason, some have concluded there is little to choose between *PLoS ONE* and Beall's predatory publishers. This is implied, for instance, in an April 2010 post on *The Scholarly Kitchen* by Kent Anderson, who [wrote](#): “Author-pays journals make money when authors publish. Some, like *PLoS ONE* and [Bentham Science](#), work to get as many papers as possible published in order to bolster their bottom lines.”

⁴⁵ OA publishers deny there is any commercial pressure on the quality of the papers they publish, arguing that publishing decisions are always taken by academic peer reviewers and editors, independently of the publisher. Editors of OA journals [concur](#). Critics, however, are sceptical about such claims, particularly in the wake of a number of incidents in which OA journals have accepted flawed, or even nonsense, papers for publication. In [one case](#), the editor [resigned](#) after a computer-generated paper was accepted, claiming that he had not even been aware the paper had been submitted, let alone sanctioned its publication.

In the case of *PLoS ONE*, added Anderson, the revenues generated are far from insignificant. “As of this writing, the *PLoS ONE* site lists 122 articles published in the past week. At \$1,350 per article, that’s ~\$165,000 last week in publication charges, equating to ~\$8.5 million per year. This doesn’t factor [in the institutional charges that can offset some of these](#).”⁴⁶

And *PLoS ONE* submissions continue to grow. As we saw, by the end of 2010 the journal had published 15,238 papers since launch. At the time of writing this had grown to 17,099.

Let’s say it again: there is no reason to assume that *PLoS ONE*’s motivations are in any way dubious, or that it has ever done anything unprofessional (and let’s assume for a minute it is not overcharging *PLoS ONE* authors). Even so, in making author-side fees publishing and “light” reviewing respectable *PLoS* has helped drive down the quality of published research, if only by enabling the so-called predatory publishers to prey on the research community.⁴⁷

Anderson believes that high-volume publishers like *PLoS ONE* (along with its less reputable clones) pose a second, greater, danger. In lowering the quality of published research, he argues, they threaten to destroy the very notion of the scholarly journal. “Noise, chaff, and pollution in science should be controlled upstream by scholarly publishers,” he wrote. “Journals are well-positioned to make a difference here, but the way *PLOS ONE* and similar publishers are bulk-processing manuscripts into journal dress could possibly devalue the journal form at its roots.”

He [added](#): “If being published in a journal no longer immediately carries the imprimatur of having cleared a high bar of scrutiny, then the form itself is at risk. Journals could become merely directories of research reports. And publishers who are truly setting standards should take notice of the risk the drift toward directories poses.”

Davis makes the same [point](#). However, he believes that the benefits of moving towards “directory publishing” are sufficiently compelling that the *PLoS ONE* model will prove highly contagious.⁴⁸ [Reporting](#) on the launch of *BMJ Open* last year, for instance, Davis pointed out that even traditional subscription publishers are succumbing to the downward drift.

[Citing](#) an advertisement for a managing editor for the new *BMJ Open* journal, Davis [commented](#): “*BMJ* clearly sees a market for their new open access journal. But that’s it — a market, a business opportunity. The ad is devoid of any lofty goals and aspirations for how this journal is going to improve medicine. *BMJ Open* represents a business decision to recapture manuscripts (and article processing fees) that would have been lost to other publishers. It is a bulk publishing model, not unlike *PLoS ONE*.”

Davis’ point is that subscription publishers like *BMJ* are coming to realise that by introducing new OA journals operating light peer review they can offer what he calls “cascading peer review” services. And doing so would allow them to earn a fee from papers that have been rejected by their more fussy subscription journals. Once a paper is rejected, the model assumes, the publisher could invite the authors to pay to have it published in a lower-quality (i.e. less-choosy) OA journal instead. This would allow the publisher to earn income on papers that would otherwise end up being published by an exclusively OA publisher like *PLoS ONE*.

⁴⁶ This refers to the various membership schemes OA publishers have introduced to help research institutions defray APC costs for their scientists e.g. [here](#).

⁴⁷ We should note that the handful of journals named in Beall’s article represents just the tip of an iceberg. These new journals come from companies whose ownership, status and peer review quality are usually obscure or unknown. Yet researchers publish in them — such are the “publish or perish” pressures on them today. Consider, for instance, that on 24th February I received, out of the blue, a manuscript from one of these predatory publishers inviting me to review it. I have never had any dealings with the publisher, and am not an academic but a journalist. And for this the author is expected to pay a \$550 handling fee.

⁴⁸ As we shall see, Davis has been proved right: publishers are now rushing to copy *PLoS ONE*.

If this were to become common an inevitable side effect would be that (for all practical purposes) every paper authored would be published, regardless of its quality. It would also mean that OA would, in the eyes of researchers at least, become synonymous with low quality.

“For many authors resigned to the fact that they have been rejected by a top-tier journal, a second-tier specialist journal will do just fine,” said Davis. “[A]nd barring those, a general archival journal may be better than no publication at all.”

Not new

But hang on a minute. Is this not a bit simplistic? Let’s not forget that the critics of *PLoS ONE* we have been citing — Butler, Anderson, Davis, Crotty *et al.* — are not objective observers. They are advocates of (and have a vested interest in) subscription publishing, and in the traditional notion of peer review. This does not make their criticism invalid, and it does not mean that they are wrong, but it does overlook an important fact: Many of the weaknesses and faults that they draw attention to in OA journals are prefigured in subscription journals.⁴⁹

First, cascading peer review has been a characteristic of scholarly publishing for many years, although papers have tended to cascade from publisher to publisher rather than from journal to journal within a single publisher’s portfolio. OA advocate [Stevan Harnad](#) uses the term [hierarchy of journals](#) to describe this phenomenon, but cascading peer review would be equally descriptive. Both terms denote a spectrum of quality, and both imply that practically every paper is sooner or later deemed publishable. As Harnad [put it](#) in 2007, “[E]verything *does* get published, eventually, but there is a hierarchy of journals, with a corresponding hierarchy of peer-review quality standards.”

In practice, this means that when trying to get a paper published researchers will first submit it to a top journal. If that journal rejects it they re-submit it to a less prestigious journal, and so on down the ladder of quality until their work is accepted.

What’s new is that in their restless pursuit of profits publishers are assembling ever larger portfolios of journals. This opens up the possibility of their being able to offer their own quality hierarchies, eventually becoming one-stop publishing shops for researchers. So any scientist submitting a paper to a big publisher could be confident that it would be accepted, although perhaps not in their journal of choice.

In a subscription environment the ability to offer cascading peer review is appealing, but not compelling. In an OA environment, however, the logic becomes irresistible, especially for subscription publishers. Confronted by growing competition from OA publishers they have now learned [to love OA](#) — not least because they have come to realise that by offering [Hybrid OA](#)⁵⁰ they can now earn both subscription revenues and APC fees from the same journal. Cascading peer review promises yet another attractive new revenue stream for them.⁵¹ Importantly, it could neutralise the threat posed by the new generation of OA publishers, particularly those like *PLoS ONE* whose encyclopaedic approach allows them to offer the equivalent of hundreds of journals as a large database service.⁵²

⁴⁹ As we also saw, some of *PLoS ONE*’s own editors and authors have concerns about the journal too.

⁵⁰ Hybrid OA is a publishing model in which a subscription publisher offers researchers the opportunity to pay to have their article made freely available on the Web. While most of the contents of the journal will remain behind the subscription paywall, the researcher is able to ensure that his or her individual paper is OA. The first Hybrid OA initiative ([Open Choice](#)) was launched by Springer in 2004.

⁵¹ With *PLoS ONE* as a model publishers can reduce their costs by providing a minimal service, while continuing to overcharge the research community for what they provide. Meanwhile Hybrid OA publishers feel able to charge over twice as much — \$3,000 in the case of Open Choice.

⁵² It is interesting to note that OA publisher [Hindawi](#) was recently driven to publish a [press release](#) boasting of the total number of papers submitted to it in December 2010 (as though its more than 200 journals were one big box like *PLoS ONE*).

Neutralise how? As we said, authors rejected by a prestigious subscription journal could be gently shepherded to a lower-quality author-side fees journal and invited to pay to publish. Unlike subscription publishers, however, OA publishers don't generally have any prestigious journals to offer authors, so they have no honey pot with which to draw them in. By launching new OA journals subscription publishers could gain a potentially significant competitive advantage.

Second, while critics like Davis and Anderson highlight the problems *PLoS ONE* faces in trying to offer meaningful review standards, the truth is — as we pointed out earlier — there have never been any universally agreed methods, standards, or formal processes setting out how peer review should be conducted.⁵³

Partly for this reason, no doubt, subscription journals often discover that they too have unwittingly published low-quality, wrong, sometimes even fraudulent, research (see, for instance, [here](#), [here](#), [here](#), and [here](#)). Indeed, even the most prestigious journals in the world are not immune from publishing bad or falsified research, and the occasional drivel too.⁵⁴ And the number of incidents has been growing in recent years. Peer review scandals, in short, are on a growth curve quite regardless of OA.⁵⁵

OA aside, therefore, something else is going on. That something is an exponential growth in papers produced (since the end of WWII at least), accompanied by an apparent decline in the quality of peer review. The latter is doubtless a consequence of the research community's inability to cope with the growth. In the same year that Varmus sat down with Pat Brown in a San Francisco coffee shop *The New York Times* [concluded](#) that the number of scientific articles and journals being published around the world “has grown so large that it is starting to confuse researchers, overwhelm the quality-control systems of science, encourage fraud and distort the dissemination of important findings.

The *NYT* cited the deputy editor of the *Journal of the American Medical Association* ([JAMA](#)) [Drummond Rennie](#), who [said](#): “There seems to be no study too fragmented, no hypothesis too trivial, no literature too biased or too egotistical, no design too warped, no methodology too bungled, no presentation of results too inaccurate, too obscure, and too contradictory, no analysis too self-serving, no argument too circular, no conclusions too trifling or too unjustified, and no grammar and syntax too offensive for a paper to end up in print.”

Amplified

In short, peer review was in trouble before *PLoS ONE* appeared on the scene. The absence of peer-review standards, and an inability to keep pace with the constant growth in research papers, has caused the system to begin to creak. Rather than being an issue unique to OA journals, therefore, quality control is now plaguing scholarly communication at large.

What *is* new, however, is that in propagating a new, lighter, review process *PLoS ONE* has amplified the pre-existing problem. Importantly, *PLoS ONE* has done this in the name of a worthy cause (making research freely available on the Web). In short, it has formalised and legitimised a process that looks set to accelerate the current decline in the quality of published research.

⁵³ If there are no clearly understood rules, of course, essentially everything is publishable, somewhere, somehow.

⁵⁴ It's a real case of the more you look, the more you find. In 2005, for instance, *Science* withdrew a number of papers co-authored by South Korean scientist [Dr Hwang Woo-suk](#) after it was revealed that he had faked the results of key stem cell research. On February 2nd 2010, *The Lancet* withdrew a 1998 paper co-authored by controversial former British surgeon [Andrew Wakefield](#) in which a connection was claimed to be made between the MMR vaccine and autism. *The Lancet* noted that elements of the manuscript had been falsified. Last December *The Scientist* published [an article](#) headed, “Top Retractions of 2010”. And so the list goes on ... See also [here](#) for instance and [here](#).

⁵⁵ In an attempt to map this last summer a blog was launched called [Retraction Watch](#). This aims to monitor and publicise papers being retracted.

In addition, *PLoS ONE* has demonstrated to other publishers that lowering the bar of scrutiny offers compelling financial benefits. And it justifies this on the grounds that papers can only be properly reviewed after publication, and that testing for “technical soundness” is the same thing as conducting “rigorous peer review” — leading one to suspect that it is promulgating a form of [doublethink](#).

Sooner or later, critics argue, it will become apparent to all that the [emperor has no clothes](#).⁵⁶ At that point a system (peer review) that was always little more than a compromise, and primarily form over substance, will become sufficiently discredited that the current system of scholarly communication will collapse.

Indeed, the alarm bells are already ringing: In January the UK Science & Technology Select Committee [announced](#) a new enquiry into peer review.

Whether systemic collapse would be a good or bad thing depends on one’s point of view. One could argue, for instance, that if scholarly communication was viewed simply as a way of making research visible to peers, rather than a process in which papers were checked, approved and certified, this would not matter.

But since the current system assumes that published research *has* been checked and certified — when in reality this may not have been done adequately — one might conclude that the public is being misled, and it is therefore time for the research community to go back to the drawing board.

In passing, we should also point out that the habit of promoting research papers as if they were products is not unique to *PLoS ONE*. Last December, for instance, a [paper](#) published in *Science* by a group of NASA-based scientists claimed to have discovered an arsenic-based life form (bacterium).

The paper was the recipient of considerable hype. This included an advance [press advisory](#) trumpeting that NASA scientists would soon reveal an “astrobiology finding that will impact the search for evidence of extra-terrestrial life”, followed a few days later by a [press release](#) announcing that “NASA-funded astrobiology research has changed the fundamental knowledge about what comprises all known life on Earth.”

In a short space of time, however, the blogosphere had given the paper a big thumbs-down, and a wave of criticism spread across the Internet — spearheaded by [Rosie Redfield](#), a microbiology professor at the University of British Columbia. The paper, she [declared](#) contained “lots of flim-flam, but very little reliable information.”

When I emailed Redfield she added that, in her view, the paper was based on faulty research, was inadequately peer reviewed, and represented, “bad judgement by the authors, reviewers and editor”.

Once again, however, *PLoS ONE* appears to be taking hype to the next level, not least by encouraging authors to produce their own press releases. And once again we are tempted to conclude that an “OA-at-all-costs” strategy could simply amplify the pre-existing problems of scholarly communication.

Affordability

But let’s return to the question of costs. As we indicated, this is tightly bound up with quality. By demonstrating that it is possible to charge authors to publish (rather than levy a subscription on readers to read), and by showing that it is possible to charge \$1,350 for a service sufficiently minimal that papers are not even copyedited, *PLoS ONE* has pioneered a model that holds out to

⁵⁶ None of this is to imply that where reviewers are committed to providing a fair-minded critique, and have the time to do so effectively, and where paper authors have submitted a paper in a genuine attempt to communicate valuable scientific results (rather than simply add another notch to their publication record), peer review cannot, and does not, provide a valuable service, and an effective filter. Such motivations, and the necessary time, however, appear to be increasingly rare in today’s stressful publish-or-perish environment.

subscription publishers the promise of being able to migrate to an OA environment without any loss of revenue (perhaps to earn even greater revenues), and without any reduction in their high profit-margins.

It might of course seem entirely reasonable for publishers to expect to continue earning a good living in an OA environment. But we should note that one of the benefits of OA promised by OA publishers was that it would reduce the costs of publishing. We should also remember that publishers have long been criticised for making excessive profits from the research community. And we should note that the research community is approaching the point where it will no longer be able to afford publishers' asking price.⁵⁷

It could therefore be that by taking too narrow an approach to the crisis afflicting scholarly communication ("OA at all costs"), *PLoS ONE* will not only encourage a decline in quality but exacerbate rather than ameliorate the financial difficulties confronting the research community. In an environment where research output is constantly growing,⁵⁸ the implications of this could be grave, and could on their own trigger systemic collapse.

In other words, although *PLoS ONE* is rapidly increasing the quantity of research that is freely available on the Web, it is doing nothing to resolve a financial crisis that has had scholarly communication in its grip for several decades.

Known historically as the [serials crisis](#)⁵⁹, in the context of OA it would be more accurate to call it an [affordability](#) crisis. Rather than address the problem, *PLoS ONE* has opened up a new seam of gold for publishers, and provided them with a migration path that will allow them to lock their current profit levels into the burgeoning OA environment, regardless of the savings they make from OA, and from electronic distribution.

What has become apparent is that simply shifting the costs from the end of the publishing process to the beginning – from reader to author, from subscription to APC – has not removed any costs from the system, but simply reallocated them. Indeed, as we shall argue, there are reasons to believe that the author-side fees model may exacerbate the affordability problem.

To do PLoS justice, when Varmus left the San Francisco coffee shop 13 years ago his focus was not on the costs of the scholarly communication system, but on improving access to research. Nevertheless, PLoS assumed that replacing subscriptions with APCs would also provide costs savings.

In a [written statement](#) to the UK Science and Technology Select Committee in 2004, PLoS said, "New technology — most notably the Internet, with its capacity for distribution, storage, and retrieval of information in digital form — has brought us to the threshold of an era that scientists, philosophers and scholars have hoped for and dreamed about for millennia."

That PLoS assumed this would mean lower costs was evident in what Varmus [told](#) the Committee when giving oral evidence: "There is no doubt that when you create one copy in digital form which

⁵⁷ The affordability problem is composed of several phenomena. First, as research continues to grow exponentially so the costs of communicating that research grow. Second, the amount of money research institutions can afford to pay to support scholarly communication (subscriptions, APCs, OA memberships etc.) continues to fall. Third, publishers have nevertheless (until now at least) continued to earn excessively high profits from publishing research. And they can do this because the scholarly journal market is dysfunctional. See the analysis of the market, for instance, by anti-trust economist Mark McCabe: <http://www.infoday.com/it/dec02/poynder.htm>.

⁵⁸ Recently the CEO of Springer (owner of BMC) [estimated](#) growth at between 6% and 7% per annum.

⁵⁹ As Wikipedia puts it, "The term serials crisis has become common shorthand to describe the chronic subscription cost increases of many scholarly journals. The prices of these institutional or library subscriptions have been rising much faster than the Consumer Price Index for several decades, while the funds available to the libraries have remained static or have declined in real terms. As a result, academic and research libraries have regularly cancelled serial subscriptions to accommodate price increases of the remaining current subscriptions."

can be used by everybody, you have a simpler and less expensive system than if you require the publisher to carry printed copies; every copy costs an additional fee to the system.”

The implication was that electronic publishing (which OA assumes) would be significantly cheaper, and that publishers would automatically pass the savings on to the research community.⁶⁰ It was also assumed that new automated editorial and production systems would provide cost savings;⁶¹ and it was argued that further savings would be realised as a result of publishers no longer having to administer expensive subscription systems etc.

Asked by the Select Committee how much it costs to publish a paper in an OA environment, PLoS [said](#), “We estimate that a typical article processed through our journal management system and production process costs between US\$1,000-1,200, not including costs for peer review and print”.

It added: “[F]or our future PLoS [community journals](#) (with staffing and publication standards similar to most society journals), we estimate that peer review will cost no more than US\$200 [£100] per article”.

In other words, PLoS argued that it would be possible to publish a traditional peer-reviewed paper for between \$1,200 and \$1,400. We can assume it would have predicted significantly lower costs for a journal like *PLoS ONE* that utilised a light review system (*PLoS ONE* was launched two years later) — particularly where it did not even copyedit papers and, by publishing nearly 7,000 papers a year, would inevitably benefit from significant economies of scale.

Today PLoS charges [\\$2,250](#) to publish in its community journals, and \$2,900 to publish in *PLoS Biology* and *PLoS Medicine*. These costs continue to rise, mirroring the serial price inflation characteristic of traditional subscription publishing. As indicated earlier, PLoS ONE has also increased the cost of its APC since launch.

The same inflationary process can be observed with *PLoS ONE*’s main OA competitor BioMed Central ([BMC](#)). When BMC launched it charged \$525 to publish an article. Today its standard APC is \$1,605, and authors can pay up to \$2,440 to publish in a BMC journal like [Genome Biology](#).

Meanwhile, the cost of publishing in a hybrid-OA journal [ranges from](#) around \$3,000 per paper with Springer’s Open Choice, to \$3,850 with *BMJ*. Per-paper costs, in other words, are rising inexorably. All things being equal, at some point we could expect the price to rise to the figure *Nature* cited when speaking to the UK Science & Technology Select Committee in 2004.

Author crisis

The mistake PLoS made, it seems, was to assume that the savings made possible by OA would be passed on to customers.

In doing so it overlooked two important facts. First, the scholarly journal market is not a proper market. As such, the mechanisms that operate to control and restrain prices in a true market are absent. For this reason, antitrust economist [Mark McCabe](#) has described it as a broken market.⁶²

Second, PLoS has surely discovered that if you don the robes of a publisher you acquire the trappings, the tastes, and the associated costs of being a publisher, even if you are inspired by a vision of something better. As a consequence, PLoS appears to be as focused on maximising its income today as any other publisher.

⁶⁰ Of course subscription publishers were already making their journals electronic. In doing so, however, they were locking in their prices, and forcing libraries to buy large bundles of journals by means of the infamous “[Big Deal](#)”.

⁶¹ As the launch press release [put it](#), “The software is open source, and will form the first part of an innovative and flexible publishing system that will be developed over the next two years and will be available to all groups for storing, disseminating, and sharing literature and data.”

⁶² For an analysis see [this interview](#) with McCabe from 2002

This was all the more likely given PLoS' determination to compete with the world's most prestigious journals. This saw it, for instance, locate its HQ in one of the most expensive cities in the world (San Francisco), poach high-salaried staff from journals like *The Lancet*, *Cell* and *Nature*, and hire a chief executive from the financial services industry on a salary of [\\$338,000](#). Most bizarrely, it led to PLoS burning money on expensive [TV air time](#) to promote itself (to whom it is not clear).⁶³

In short, in order to meet the costs arising from competing with the world's top journals, PLoS has found it necessary to tax the research community at the same high level as traditional publishers. And, argue critics, the bulk of this comes from overcharging *PLoS ONE* authors.

As indicated earlier, there is a further complication: Even if publishers' charges were demonstrated to be fair and reasonable, the research community would still be unable to afford them in the long-term. Grappling with static or falling budgets, research institutions simply cannot meet the rising costs of scholarly publishing, not least because of the constant growth in the number of papers⁶⁴ – or at least they cannot do so within the confines of traditional journal publishing.

For librarians — who have until now borne the brunt of the serials crisis, and who joined the OA movement in the expectation that it would solve their budgetary problems — inevitable disappointment lies ahead,⁶⁵ as it does for all those OA advocates who assumed that OA would be cheaper. In fact as indicated, some observers believe that OA will turn out to be a more expensive method of publishing in the long run.⁶⁶

This suggests that for research institutions OA simply changes the question from: “How can we afford to pay the constantly rising subscription costs that publishers are demanding”, to “How can we afford the constantly rising APC prices?” No longer is it a question of, “How can we ensure our scientists have access to all the research being produced by other institutions?”, but “How can we ensure our scientists have sufficient funds to enable them to publish their research?”

In short, OA appears destined simply to replace the serials crisis with what one might term an author crisis.

For some authors this could have very serious implications. Those, for instance, based in the developing world could find themselves even further disenfranchised from the international research community. This point was made in 2008 by [Raghavendra Gadagkar](#) of the [Centre for Ecological Sciences](#) at the [Indian Institute of Science in Bangalore](#). In a letter to *Nature* Gadagkar [argued](#) that author-side fees publishing, “does more harm than good in the developing world”.

He explained: “Page charges may be waived⁶⁷ for authors who cannot afford to pay, but a model that depends on payment by authors can afford only a few such waivers. And why should anyone want to survive on charity? The argument that it is the granting agency and not the author that pays does not wash either. If anything, the playing field for grants is even more uneven.”

⁶³ As *Nature's* Declan Butler [put it](#), “To the average viewer, it must have been perplexing stuff.”

⁶⁴ As Springer CEO Derk Haank said to me recently, “[T]he reality is that our journals are growing in volume by 6% to 7% per year. We have been doing all that is possible over the last couple of year, and will continue to do so to ensure that our price increases are lower than the volume increases. But not increasing our prices is not an option in the long term.”

⁶⁵ On the other hand, if the financial responsibility moves from the library to another cost centre within the institution some librarians might rejoice that it is no longer their responsibility to pay for scholarly communication.

⁶⁶ Keith Jeffery, Director IT and International Strategy for the Science and Technology Facilities Council at the Rutherford Appleton Laboratory (RAL), for instance, believes that OA will prove more expensive than the subscription model — certainly for research-intensive institutions. Calculations done at RAL a few years ago, he says, demonstrated as much.

⁶⁷ Most OA publishers promise to waive APC costs for researchers in the developing world. However, these waivers have been somewhat controversial, and can clearly be [withdrawn, or reduced](#) whenever a publisher chooses.

Gadagkar concluded, “Page charges make extra difficulties for authors, while the old problems associated with peer review persist. They could be disastrous for the underdeveloped world, encouraging people to remain as consumers (readers), rather than to become producers (authors) of knowledge.”

As promised by PLoS in 2004, we are surely on the threshold of a new era in scholarly communication. But how many researchers will be able to afford the entrance fee?

Author-side fees OA was intended to usher in a revolutionary new way of disseminating research — one, as PLoS put it that would ensure “everyone who has access to a computer and an Internet connection will be a keystroke away from our living treasury of scientific and medical knowledge.”

With that revolution now in full flow, however, it turns out that — improved access aside — OA is failing to resolve a number of grave problems confronting scholarly communication; rather it appears to be amplifying them. While PLoS can rightly boast that it is significantly increasing the number of OA papers available, critics argue that it is driving down the quality of those papers,⁶⁸ and enabling publishers to embed their traditionally high profit levels into the new publishing environment — to the point where the research community will sooner or later no longer be able to communicate its research effectively.

As the responsibility for paying the costs shifts from readers to authors, and prices continue to rise, some authors will find they simply cannot afford to publish their work.⁶⁹ If that were to happen, then the victory that OA supports [are claiming](#) for PLoS will prove to have been a pyrrhic victory.

The death of importance

So what does the story of PLoS tell us about the likely future of scholarly publishing? The first clue lies in the fact that the *PLoS ONE* model is beginning to be cloned by subscription publishers, not just *BMJ* but others as well. Last December, for instance, Sage announced the launch of a new multidisciplinary journal for humanities scholars. Like *PLoS ONE* [Sage Open](#) will be very broad, covering “the full spectrum of the social and behavioural sciences and the humanities”.

And like *PLoS ONE*, *Sage Open* reviewers will be instructed to ask a simpler question, and to reach an easier decision, when evaluating papers. As the web site [puts it](#), “*SAGE Open* evaluates the scientific and research methods of each article for validity and accepts articles solely on the basis of the research. This approach allows readers greater access and gives them the power to determine the significance of each article through *SAGE Open*’s interactive comments feature and article-level usage metrics.”⁷⁰

Similar services have also recently been announced by the American Institute of Physics ([AIP Advances](#))⁷¹ and even *Nature* ([Scientific Reports](#)).

Nature’s [press release](#) announcing *Scientific Reports* said that the new journal, “will not set a threshold of perceived importance for the papers that it publishes; rather, *Scientific Reports* will publish all papers that are judged to be technically valid and original.”

Like *PLoS ONE* and like *Sage Open*, *Scientific Reports* will also offer article-level metrics, and will encourage post-publication commentary. As the release put it, “To enable the community to

⁶⁸ As we have argued, quality is being driven down not just by *PLoS ONE*, but by the growing number of copycats keen to exploit the business opportunity that the *PLoS ONE* model provides.

⁶⁹ While the APC costs of *PLoS ONE* are lower than some, the sheer number of papers it is publishing, and the apparently limited amount of work put into the reviewing process, would certainly suggest it is over-priced, as argued earlier. And as we are about to see, it has also sparked a gold rush that is pushing prices up further.

⁷⁰ PLoS also pioneered the use of [article-level metrics](#) in 2009.

⁷¹ The APC for *AIP Advances* is \$1,350, and the journal’s [pre-publication peer review process](#) “focuses on whether the manuscript is technically correct and original. Concepts of ‘timeliness’, ‘significance’, or ‘importance’ are evaluated by the community post publication through the implementation of web 2.0 commenting and ranking tools.”

evaluate the importance of papers post-peer review, the *Scientific Reports* website will include most-downloaded, most-emailed, and most-blogged lists. All research papers will benefit from rapid peer review and publication, and will be deposited in PubMed Central.”⁷²

And like *PLoS ONE*, the APC for both *AIP Advances* and *Scientific Reports* will initially be set at \$1,350. We should however note that the fee for *Scientific Reports* will [increase](#) to \$1,700 in 2012 – demonstrating once again the inflationary forces that drive the scholarly communication market.

OA advocates were [quick to point out](#) that *Scientific Reports* represents a breath-taking U-turn on the part of *Nature*. Where a few years ago it was deriding bulk-publishing and denigrating cash cows it is now embracing them – fearful, presumably, of missing the OA gold rush. Critics [dubbed](#) the new service “Nature ONE”.

But who can blame *Nature*? If Hannay is right to argue that by subsidising its flagship journals PLoS has blocked other publishers from developing top-tier OA journals it is only rational for *Nature* to move into the lower-tier publishing market, particularly as that sector appears to offer greater profitability.⁷³

Of course, one could argue with Hannay’s analysis, but it does explain *Nature*’s decision, and it surely increases the likelihood that the quality of published research will continue to fall in coming years, with OA being seen as a rapid – but low-quality – publishing option for researchers. Moreover as we indicated, by offering cascading peer review services subscription publishers can hope to ameliorate the threat posed by OA publishers.

Our conclusion must therefore be that, however laudable PLoS’ aims (and the aims of the OA movement at large), and however desirable it is to make research freely available, PLoS failed to appreciate that the greatest problem confronting scholarly communication today is not access but affordability.

After all, if there were no affordability problem there would be no access problem; and as we have discovered, OA does not guarantee affordability.⁷⁴ Indeed, as indicated, some fear that it will exacerbate the affordability problem, and reduce many scientists to passive consumers of research information, rather than active producers.

In reflecting on the launch of *Scientific Reports*, Cameron Neylon [recently](#) predicted “the future of most scholarly publishing will be in publication venues that place no value on a subjective assessment of ‘importance’.”

Again, whether this would be good or bad depends on one’s point of view. If it meant adopting a more realist approach to peer review (the Emperor has no Clothes) some will welcome it.

If, on the other hand, bulk OA publishing with light review becomes the norm, but publishers overcharge for the minimal service they provide, it would be bad news for the research community. It would also continue to perpetuate the myth that peer review offers more than it does in reality. And it would create a very unfair system, not just for those in the developing world.

⁷² Note that *Scientific Reports* talks in terms of blog commentary rather than the provision of comment features on its own site.

⁷³ As an anonymous consultant [told Nature](#) in 2008: “BioMed Central knows well that much of the journal middle order is more profitable than the great brands because of the lower editorial costs and the cheaper marketing costs for bundles of journals. I suspect that *PLoS ONE* is a result of learning the same lesson.”

⁷⁴ I am aware that the discussion in this document is not particularly relevant to self-archiving, or Green OA. But here we are discussing Gold “author-side fees” OA, not [Green OA](#) — which assumes that the access problem can be resolved without changing the current system, or even necessarily migrating from subscription to OA journals. As such, it could be argued that Green OA doesn’t address the more serious problems confronting scholarly publishing today. On the other hand, one could make a case that it would allow the research community to transition to an OA environment in a way that resolved both the access and the affordability problems — in so far as it might force publishers to downsize: see <http://poynder.blogspot.com/2009/03/open-access-who-would-you-back.html>).

As one disgruntled grad student [commented recently](#): “My problem with *Scientific Reports* is that authors have to pay such an outrageous fee (\$1,350 per article) to have their peer reviewed article published. This is when I realised that it doesn't matter whether or not I would be ashamed to publish in something like *Scientific Reports* because as a grad student I couldn't friggin' afford it! Apparently, this fee ultimately goes to help fund the publishers' alpha journals. So people who do omega work have to pay exorbitant fees to have their omega work published in an omega journal, to help fund the publication of alpha work by alpha researchers. So the alpha keep getting alpha-ier while the omega keep getting omega-ier....”

If this is a true picture of the future, it is no surprise that its critics have come to describe PLoS as a “squandered opportunity”. An opportunity, as Anderson [put it](#) on *The Scholarly Kitchen* last year, “sacrificed at the altar of open access, of a radicalism blunted into tradition, of audacity channelled down the path of least resistance”.

Anderson added, “It seemed possible that PLoS might be the group to reimagine scholarly communication — from peer-review to publication practices to form and function. Advocates claimed that their aspirations extended beyond merely creating an alternative economic model for publishing ... [but then] ... it went old school, publishing a good traditional journal initially and then worrying about traditional publisher concerns like marketing, impact factor, author relations, and, of course, the bottom line.”

In short, PLoS is now charged with having joined the system it was intended to overthrow, and becoming the dragon it was meant to slay.⁷⁵

On the other hand, if it — along with its clones — was indeed to trigger systemic collapse the revolution it predicted might yet be realised, with a more rational system rising from the ashes of the old.

That radical change is coming now seems certain. The quality crisis has reached the point where scientists are concluding that, rather than being the handmaiden of science, peer review is its scourge.

Last October, for instance, neurologist [Martin Raff said](#) to *The Scientist*, “Publishing has become a nightmare. Particularly for young scientists, who can spend a year or two doing the experiments that referees demand. I think this is a terrible thing, because referees' experiments in my view can be dangerous. One, because you've put the student or postdoc into a position where they know that, if they get the results the referee wants, their paper will get into the journal and their career is on its way. If they don't, they won't get it into a high impact journal and they're in real trouble. That is just putting enormous pressure for selecting your data and cutting corners. So it's very dangerous.”

Meanwhile, the affordability problem is reaching its own tipping point. Due to the global financial crisis it is probably only a matter of time before the research community is forced to say to publishers, “Enough is enough; we can no longer pay what you are demanding”.

As equity analyst Claudio Aspesi [pointed out](#) to me last June, publishers have “built a business model predicated on annual revenue increases supported by the steady launch of new titles to justify the increases. It worked well (at least for them) as long as the librarians were able to find the money (regardless of their growing agitation over the increasing spending). It cannot go on if the libraries' budgets continue to be under pressure.”⁷⁶

In short, both the quality and the affordability problems are coming to a head.

⁷⁵ Indeed both *PLoS Biology* and *PLoS Medicine* offer a [print subscription](#) costing between \$365 and \$415 (\$45 for a single issue).

⁷⁶ Aspesi talks in terms of subscriptions but, as we've seen, whether the costs are paid by means of a subscription or an APC currently appears to make no difference in terms of affordability.

Publish then filter

So what is the answer? That is really a topic for another day. We should note, however, that Raff's solution sounds very like *PLoS ONE*. "In the best of all possible worlds," he [told *The Scientist*](#), "what you would like is for all papers to be published, just making sure that the conclusions are reasonable given the observations. And then retrospectively assess the impact and importance of the paper."

Assuming Raff envisages a system still based on pre-publication review, would not his solution take us down the same road as that travelled by PLoS?

Given *PLoS ONE*'s inability to attract meaningful post-publication commentary, and its failure to resolve the affordability problem, Raff's proposal does not seem radical enough. As we saw, in donning the jewels and ermine of a publisher PLoS appears to have been side-tracked and absorbed by the system it was meant to challenge – a victim of some kind of "[regulatory capture](#)" perhaps.

Former *BMJ* editor [Richard Smith](#), however, may have the answer. Smith's starting point is the escalating quality problem, not affordability. But his proposal could solve both problems in one.

Smith argues that peer review is now so broken we need to completely rethink the scholarly journal. On the *BMJ* blog in October, for instance, he [argued](#), "The job of journals should not be to publish original research but rather to assess critically the research that is published in full on databases. Find the comparatively few studies that matter, test them to destruction, and place them in their full context."

Elsewhere Smith has [put it](#) this way: "For journal peer review the alternative is to publish everything and then let the world decide what is important. This is possible because of the internet, and [Charles Leadbeater](#) has illustrated how we have moved from a world of 'filter then publish' to one of 'publish then filter' and a world of 'I think' to one of 'We think'."

Smith added, "The problem with filtering before publishing, peer review, is that it is an ineffective, slow, expensive, biased, inefficient, anti-innovatory, and easily abused lottery: the important is just as likely to be filtered out as the unimportant. The sooner we can let the 'real' peer review of post-publication peer review get to work the better."

In other words, Smith [believes](#) that we should abandon pre-publication review all together and "Instead, let people publish and let the world decide. This is what happens anyway in that what is published is digested with some of it absorbed into 'what we know' and much of it never being cited and simply disappearing."

How would such a system work?

One could envisage a model, for instance, in which researchers were mandated to post all their papers (as preprints) in their institutional repository⁷⁷ — or in a central repository like arXiv or PubMed Central. In most cases, this would be the end of the publication process, as only those papers deemed to be sufficiently ground-breaking, important, or otherwise worthy of greater attention, would be picked out and published in so-called [overlay journals](#).⁷⁸ These papers would in most cases not be published until their significance had been established (a reversal of the *PLoS ONE* model in fact).

This would also address the problem of researcher reluctance to engage in post-publication review, as it would be the task of the editors of overlay journals to identify interesting papers — aided no doubt by the constant chatter on blogs, and on social networking sites, where the cutting edge analysis and reviewing now seems to take place.

⁷⁷ As [proposed](#) by advocates of Green OA in fact.

⁷⁸ I.e. journals that harvest papers from multiple online sources in order to create a virtual publication.

The editors of overlay journals would then have the papers they had selected reviewed — possibly in ways not very dissimilar to how more picky journals do it today, or more likely using a model like that pioneered by [Faculty of 1000](#) (but on a pre-publication as opposed to a post-publication basis)⁷⁹.

Papers not selected for review (the vast majority) would continue to reside in the repositories they had been deposited in (living what one might call a “dark archive”) — still discoverable via search engines, but not peer reviewed.

Smith’s proposal would by its very nature resolve the access problem. And since most papers would never be formally peer reviewed publishing costs would plummet.

As it happens, this is exactly the kind of model [envisaged](#) by the founders of [LiquidPublications](#), an EU-funded project looking at ways of creating new kinds of overlay journals. As University of Trento Professor [Fabio Casati explained](#) when the project was launched: “Suppose I want to create a journal on, say, interesting findings in peer review, that I want to use to collaborate with my research group and my peers. I will go fish for interesting papers that are on the Web. People don’t submit to my journal, they just post to their webpage or to an archive. I find the paper and include it in my journal. Everybody can do this.”

Peer review purists will of course throw up their arms in horror. But as we’ve seen peer review is deeply flawed and getting worse. Casati puts it this way: “We’ve studied this and found that peer review doesn’t work, in the sense that there seems to be very little correlation between the judgement of peer reviewers and the fate of a paper after publication. Many papers get very high marks from their peer reviewers but have little effect on the field. And on the other hand, many papers get average ratings but have a big impact.”

We should note that Casati’s model is not so different from Varmus’s 1999 E-Biomed proposal, although perhaps more radical. It is also more radical than arXiv, which still assumes that most papers posted to it will also be published in a traditional journal (and so go through traditional peer review).⁸⁰ Nevertheless, Casati’s proposal is inherent in both. One is, therefore, inclined to suggest that PLoS might have been better to stick to its knitting and not become a publisher.

The most important thing

The fundamental issue that PLoS appears to have overlooked is that all publishers – not excluding PLoS, and not excluding learned societies – are so focused on maximising their income that they have become blind to the true nature of the crisis confronting scholarly communication.⁸¹

While publishers are constantly seeking out new ways of making money from the research community, the research community is having to accept that unless the costs of scholarly communication begin to fall rapidly something is going to break. And it is hard to see how this conflict of interest can be resolved within the confines of a journal-based system utilising traditional pre-publication review.

⁷⁹ As Wikipedia [explains](#), “Faculty of 1000 is a website for researchers and clinicians that provides ratings of and commentary on [scientific research](#) papers. The service acts as a filter, identifying and evaluating the most significant articles from biomedical research publications. A peer-nominated ‘Faculty’ of scientists and clinicians rate the articles they read and explain their importance.” F1000 is itself not without its critics. As Bruce Alberts, editor in chief of *Science* and a professor of biochemistry at the University of California at San Francisco, [told The Chronicle of Higher Education](#) in January, F1000 is prone to dominance by particular individuals within subfields, limiting its usefulness as an arbiter of quality.

⁸⁰ But not all: In 2002, for instance, the Russian mathematician Grigori Perelman [bypassed the peer-review system](#) and posted a landmark paper to the online repository, arXiv. Perelman later won the Fields Medal for his contribution to the [Poincare conjecture](#), one of mathematics’ oldest and most puzzling problems.

⁸¹ Last year PLoS [launched a new service](#) called *PLoS Currents*. This currently has no APC, but PLoS reserve the right to charge one at a later date. It is not clear what the long-term plan is for *PLoS Currents*, but currently it operates pre-publication peer review, and has already attracted criticism and [controversy](#). We should note that PLoS responded to this criticism in a conservative rather than radical way.

It may be, however, that PLoS editors at least are beginning to reach the same conclusion as Smith. In January, for instance, Neylon [told Nature](#) that so far as he was concerned it made much more sense to “publish everything and filter after the fact”. Neylon also suggested that the logical place for post-publication review to occur is on social networking platforms like Twitter, rather than journal web sites.⁸²

Academic editor-in-chief of *PLoS Biology* [Jonathan Eisen](#) appears to be of the same mind, [asking Nature](#) rhetorically, “Who in their right mind is going to log on to the *PLoS ONE* site solely to comment on a paper? I guarantee that there are more comments on Twitter about a PLoS paper.”

Intriguingly, we should note, Richard Smith is on the PLoS [Board of Directors](#).⁸³

Whether we are witnessing the death throes of the traditional journal form is not yet clear, but traditional peer review is surely in the process of being strangled in its sleep.

Let’s finish by going back to the beginning: In declining to answer my questions Binfield appeared to suggest that to make a connection between the controversy surrounding the wind setdown paper and the larger PLoS story would be inappropriate. I hope I have demonstrated why I disagree with this; and I hope I have also explained why I believe that, instead of being a solution to a problem (the dire state of scholarly communication), PLoS is in danger of becoming part of that problem.

For that reason I believe it is both logical and reasonable to draw a straight line from E-Biomed, the founding of PLoS, the *PLoS ONE* business model, the way in which *PLoS ONE* organises its peer review, and the controversy surrounding the wind setdown paper.

But whatever the reason for Binfield’s reticence, it is surely regrettable that an organisation that boasts of its commitment to openness and transparency, and whose business exists courtesy of the taxpayer, should decline to answer a list of questions emailed to it by a journalist?

Should we conclude from this that PLoS has lost its way? If it has, it is probably a consequence of the publisher becoming too narrowly focused on “OA at all costs”. Indeed, I am told that that is the mantra that PLoS employees repeat to one another when talking at the bar.

And as we have suggested, the price of “OA at all costs” is likely to be the continuance of an unsustainable affordability burden on the research community, and an apparent decline in the quality of refereed research.

Once again we should stress that the story of PLoS is not of one of greedy opportunism or predatory intent, but one of unintended consequences. We should likewise stress that Open Access is not only desirable but, in today’s scientific environment, [essential](#). *PLoS ONE*-style OA, however, may not be the solution.

For that reason, it might be timely for PLoS to re-examine its mission, and take a broader approach to the crisis it was founded to resolve.

Time, perhaps, for PLoS founders and managers to gather in the San Francisco café where Varmus and Brown met in 1998, and re-appraise their strategy. What better location than a former Tassajara Bakery: The [Tassajara Zen Mountain Center](#) was founded by the [Sōtō Zen roshi](#)⁸⁴ [Shunryu](#)

⁸² The key issue, of course, is whether researchers would still be expected to pay \$1,350 or more in order to have every paper they produce “published” on the Web.

⁸³ We should also note that Harold Varmus is no longer listed as a member of the PLoS Board of Directors. His former role as chairman was [taken on](#) by Gary Ward at the end of last year. However, the press release apparently failed to point out that Ward was replacing Varmus. Moreover, Varmus is still listed as chairman on the [about page](#).

⁸⁴ A Roshi is a Zen Master.

[Suzuki](#)⁸⁵ – who is much-cited for his saying: “The most important thing is to find out what is the most important thing”.

Sitting in San Francisco sipping their *caffè latte*, PLoS founders and their employees could again absorb some Zen wisdom while mulling over three questions: First, “What, in the context of the current crisis in scholarly communication, is the most important thing?” Second, are they — possibly without realising it — now operating an “OA-at-all-costs strategy?” Third, “If so, do they not feel that ‘OA at all costs’ might be a mistaken approach for an organisation wanting to ‘catalyse a revolution in scientific publishing’?”

And they should make sure that Richard Smith is present.

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RESPONSE FROM PLoS:

We were disappointed to see Richard Poynder’s current article about PLoS, but grateful that Richard sent us a draft, so that we could prepare a brief response.

Richard is absolutely correct about one thing – we at PLoS are really committed to open access, and we are doing our absolute best to inspire a broader transformation in scientific communication. We make no apology for that. We expect to be watched and scrutinized and indeed have been the subject of some criticism over the years, but not at the length or with the amount of negativity that we see in Richard’s essay.

Much of the article focuses on PLoS ONE and Richard uses some selected examples (about 5 of them) from the more than 17,000 peer-reviewed articles that we’ve published in PLoS ONE to draw much broader conclusions about the quality of its content. Although we would be the first to agree that PLoS ONE isn’t perfect, neither is any journal, as Richard points out – although not until around 30 pages into the article. But, just to quote one statistic, is it not more striking that of the 4400 articles published in PLoS ONE in 2009 around 55% of them have been cited 3 or more times (Scopus data)? The evidence points to the fact that PLoS ONE is attracting a vast amount of high-quality content.

PLoS ONE is attempting to challenge the conventional model of a journal. The peer review criteria for PLoS ONE are focused on [rigour, ethical conduct and proper reporting](#). Reviewers and editors are not asked to judge work on the basis of its potential impact. Our argument is that judgments about impact and relevance can be left (and might be best left) until after publication, and this argument is clearly resonating with the tens of thousands of researchers who work with and support PLoS ONE as authors, reviewers or editors. It’s now also resonating with many other non-profit and for-profit publishers who are [exploring the same model](#).

⁸⁵ Amongst other things, Suzuki popularised [Zen Buddhism](#) in the [United States](#).

We do not argue that the PLoS ONE approach is the only way to publish research, and indeed we view PLoS ONE as just one aspect of a much more fundamental transformation of scholarly communication. Another aspect of that transformation is in the assessment and organization of research findings, which is currently done using conventional journals. That's why we have launched [article-level metrics](#) and [PLoS Hubs](#) as new and alternative approaches to post-publication evaluation. There will be much more to come from PLoS and [many other innovators](#).

At several points, Richard's article uses quotes from staff, press releases and so on that are now several years old and misses the point that much has changed even in the short few years since PLoS ONE launched. We are learning all the time from PLoS ONE. His frequent quotes from PLoS staff also show that we've answered many of his questions (including some less than friendly ones) over the years. Nevertheless, he places great emphasis on the fact that we declined to answer a set of more than 20 detailed and complex questions about general aspects of PLoS ONE, as a follow up to a series of exchanges about the peer review process on a [particular PLoS ONE article](#) about which there was some disagreement. Indeed we [posted a comment](#) to try and clarify the issues in light of Richard's questions, and comments from researchers. We were surprised by the number and wide-ranging nature of Richard's subsequent questions about PLoS ONE, and chose not to answer them because we felt that the issues surrounding the PLoS ONE article were closed. If Richard had signaled his intention to write a lengthy article about the history and status of PLoS at the outset of the exchange, our response might have been rather different.

But the more significant point is that PLoS ONE has evolved since its launch. We did originally place a lot of emphasis on 'commenting' and 'rating' as tools for post-publication assessment, but we rapidly realized that much commentary and other activity happens elsewhere. If we could capture this activity and add it to the PLoS articles (in all our journals), that could be a powerful approach to post-publication assessment, and could also be used to filter and organize content. Thus, the article-level metrics project was born. The PLoS ONE editorial and publishing processes are also under constant review and revision as the journal's size and complexity has grown, and we post some updated general information about these processes [on the PLoS ONE web site](#).

Another theme in Richard's article is whether PLoS ONE represents value for money. PLoS Journals are an ecosystem and they all contribute to the whole, both financially and to PLoS' reputation and brand. Looked at in isolation, PLoS ONE, as well as the Community Journals (PLOS Genetics, PLoS Computational Biology, PLoS Pathogens and PLoS Neglected Tropical Diseases), all make a positive financial contribution to PLoS. They help to support PLoS Biology and PLoS Medicine, as well as the development of the journal websites and other important initiatives such as article-level metrics, PLoS Hubs, PLoS Currents and our work on advocacy. From the PLoS authors' perspective, wherever they publish in PLoS their work will reach anyone with an interest in it, and their work will be stamped with a brand that is associated with social change, innovation and quality. There's much more to value than the direct costs of publishing a single article.

And a final point on value. Publishing in the conventional system is estimated to cost the academy around [\\$4500 per article](#). What PLoS (and for that matter BioMed Central, Hindawi, Co-Action, Copernicus and other successful open-access publishers) is showing is that high-quality publishing can be supported by publication fees that are substantially less than the costs of the conventional system.

There is a revolution in the making, and despite the wealth of support that we are seeing, it won't be comfortable for everyone. We need constructive criticism, but also some optimism and creativity to make it work. There are [grounds for hope](#) that we've moved beyond the antagonism that has characterized many discussions around open access. There really is a [lot to celebrate](#).