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More means less: managing overflow in science publishing

Sabina Siebert, Robert Insall, and Laura M. Machesky

Overflow (also referred to as surplus, excess, or overspill) is the opposite of scarcity. Yet as Czarniawska and Löfgren (2012) noted, overflow can be construed as either positive (more means better) or negative (too much of a good thing). But no matter how it is defined and whose perspective one considers, they contend, overflow must be managed. Earlier studies revealed a variety of practical definitions of overflow and a variety of managing devices and ways of coping with overflow. Acknowledging the value of earlier contributions to the study of overflow and drawing on those insights, in this chapter we examine the phenomenon of overflow in biomedical science publications.

What does overflow in science mean?

We begin by summarizing the findings from our 2015 study published in *eLife* (Siebert, Machesky, and Insall, 2015), in which we identified strong perceptions about various types of overflow in biomedical publications. Our interview accounts were redolent with complaints about various forms of overflow. The scientists we spoke to mentioned the increasing number of words, figures, and tables in the average biomedical science article; the rising expectations of rich data to underpin the publications; and the introduction of new 'mega-journals', such as *PLOS ONE* (23,020 papers published in these journals in 2016). One scientist commented on the 'Figure 7 phenomenon', which symbolized to her a drive to expand papers by adding information that is likely to make them noticeable (Siebert, Machesky, and Insall, 2015).

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One reason for many of those overflows, according to our interviewees, was increasing competition for jobs, grants, and prestigious publications, a trend that a number of scientists considered detrimental to science. It can lead to lower morale, we were told, and a general sense among students and postdoctoral fellows that there is little or no chance of becoming a leading researcher in the hypercompetitive environment. Furthermore, the increase in job and grant applicants competing for a relatively stagnant pool of jobs and funds can be perceived as a decrease in funding for science, which lowers morale even further (Bourne, 2013; Alberts et al., 2015). After all, high-impact publications are often used as a measure of scientific achievement, despite many arguments to the effect that it is a flawed measure (Seglen, 1997; Curry, 2012). Publication in 'good' journals is seen as the scientist's primary goal.

The scientists we interviewed perceived the world of academic publishing as a seriously overflowing area. They spoke of the ever-increasing number of manuscripts being submitted to the top-tier journals, making it nearly impossible to publish a study in one of them. Our interviews abounded in such phrases as deluge, flood, mass, and influx. This exponential growth of scientific outputs stands in contrast to the artificial scarcity of prestigious publication outlets (Young, Ioannidis, and Al-Ubaydli, 2008; Eisen, 2011). The scientists we spoke to also commented on a rapid proliferation of journals of 'questionable quality'.

According to the interviewees, this increase in submissions was caused partially by an influx of scientists from countries that have not been known historically for biomedical research. What proportion of the exponential growth of outputs comes from these newcomers is not known to us, but this was a common perception among our interviewees.

The results of our study led us to conclude that scientists and policymakers need to be made aware of this common perception of overflow and the potential problems it causes to science. We believe that an understanding of this phenomenon may also underlie some of the reported increases in fraudulent behavior among scientists, as discussed by Steen and his colleagues (Steen, 2011; Steen, Casadevall, and Fang, 2013). When the exponential growth of science is considered, it may be that a reported increase in paper retractions is caused by an increase in the total number of papers published, rather than by an increase in fraud.

As many fingers pointed to the world of publishing, we set out to take a closer look at this world, this time exploring the perspective

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of journal editors. Our goal was to assess the perceived overflow with their help and discover whether the number of submitted manuscripts is really increasing, and, if so, how the editors deal with the increase. We wanted to learn about the structure and governance of science publishing and how publishers and editors manage the perceived overflow.

Methods

We interviewed 14 editors and one publisher of journals in areas related to the biomedical sciences. The editors were either practicing scientists working in research labs who were engaged in editorial activities as part of their jobs, or professional editors with science education – usually a PhD – employed by the publisher. All but two journals represented by these editors were listed on Thomson Reuters' scientific list of impact factors, and none of the journals was listed in Jeffrey Beall's List of Predatory Journals and Publishers (more about these later). The impact factor of their journals varied from IF 2 to over IF 30.

We asked the editors if the number of publications in the field covered by their journal was increasing or decreasing over time. If so, by how much, and when had they noticed this trend? How big was the increase/decrease per year? We also asked about competition among journals for submissions, and the main reasons why a scientist chooses to submit a paper to a particular journal.

Apart from interviewing journal editors, we tried to collect some information on submission numbers to journals over the past 10 years. This task was more difficult than we imagined, as numerous editors considered these data commercially sensitive and declined to reveal them. Only six editors or publishers agreed to provide their submission numbers. We were also able to obtain submission numbers for *Nature*, as these data are publicly available online. Some editors provided us with the submission numbers but declined our invitation for an interview; others agreed to be interviewed, but would not share their submission numbers.

To measure the overflow of publications, we analyzed PubMed's (2017) informative research output index, counting the number of indexed publications between 1900 and 2015 (either in total, or for specific journals) and plotted them using Prism.[1]

1 https://www.graphpad.com/scientific-software/prism/.

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Science at the point of saturation

Before proceeding with our diagnosis and analysis of overflow in science, we go back 50 years to the 1960s, when a British historian of science, Derek de Solla Price, predicted that the rapid expansion of science would one day reach a point of saturation. In his 1963 book *Little science, big science*, he speculated that the social organization of science and quality-control systems would have to be adapted to accommodate the exponential growth of 'Big Science' – the term he used to describe 'the large-scale character of modern science, new and shining and all-powerful'. The Big Scientist, in turn, is part of an intellectual elite in Washington or Boston, and research corporations are seeking whatever the Big Scientist produces. The Little Scientist, on the other hand, is a 'long-haired genius, moldering in an attic or basement workshop, despised by society as a nonconformist, existing in a state of near poverty, motivated by the flame burning within' (de Solla Price, 1963: 2).

De Solla Price noted that some scientists are critical of Big Science and look back nostalgically at a Little Science that was more elitist and, consequently, more manageable. He claimed that science had grown exponentially, its rate of growth being proportional to the size of the population or to the magnitude already achieved: the bigger a thing, the faster it grows. Some of the consequences of this growth are still noticeable 50 years later: loss of personal contact among researchers in the same field, a lack of cohesion in scientific communities, development of 'objective measurements' of quality, and an erosion of idealism that resulted from economic and commercial pressures (Saltelli, Ravetz, and Funtowicz, 2016). For the Little Scientist, science was a vocation, de Solla Price contended; for the Big Scientist, it has become a career, albeit an insecure one.

Is there really overflow of publications? An attempt at diagnosis

Was de Solla Price right in his prediction of exponential growth? Looking first at the overall number of research outputs captured on PubMed, the plotted line indicates that the increase was slow until the end of the Second World War; but after 1945, scientific research experienced steep, exponential growth, passing a million publications per year in 2011 (see Figure 7.1).

Replotted on a logarithmic scale, these data reveal a two-stage growth in recent years. Between the late 1960s and 2000, the number

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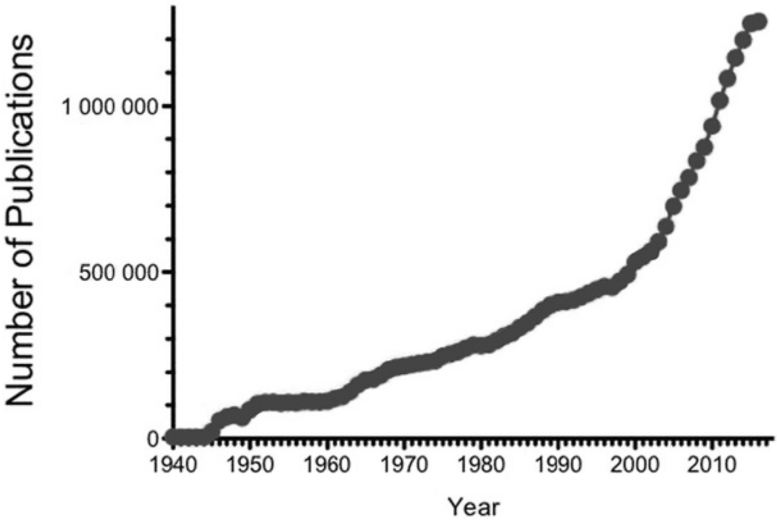

Figure 7.1 Numbers of indexed scientific publications since 1940

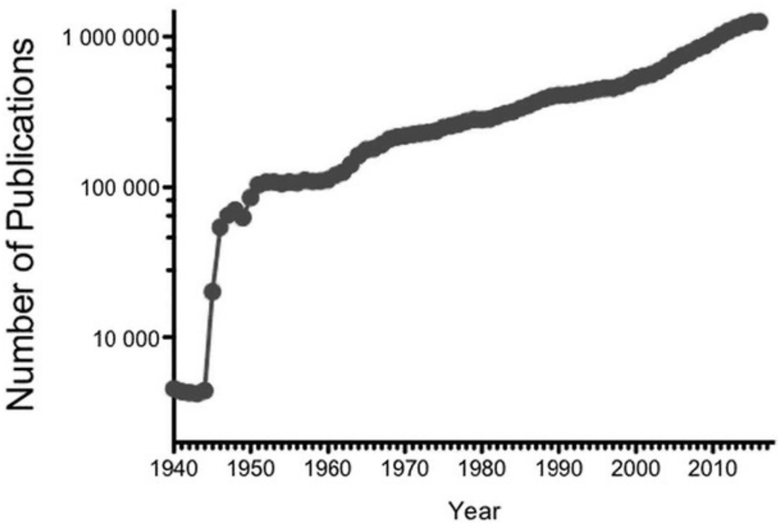

Figure 7.2 Numbers of indexed scientific publications since 1940 (logarithmic plot)

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of publications doubled approximately every 14 years, but more recently the rate has increased even further, doubling approximately every 12 years (see Figure 7.2).

In 2015, Ware and Mabe (2015) estimated that the number of articles published annually increased at the rate of 3% a year, whereas the number of journals grew steadily at the rate of 3.5% per year. Although some of the increases may be explained by more thorough indexing over time, there is still real growth visible. One explanation lies in an explosion in the production of and demand for academic research in the postwar years – a demand for which scientific societies were no longer able to provide enough publication venues (Fox, 2015). This is when commercial publishers entered the field and soon dominated the journal market. More recently, automated ('bibliometric') counting of publications has come to dominate the assessment of scientists, also contributing to the increase in publication numbers.

The figures presented here are reflected in the perceptions of journal editors. One editor we interviewed, for example, commented on the geographical expansion of biomedical science to include submissions from Asian countries, such as China or India:

> The field of science has expanded exponentially. […] Back in those days we didn't ever think about a paper being from China or India, but now we do. And now, as journal editors, we need to appeal to authors in those countries, but we also need to make sure that our journal stays solid with a good reputation and it doesn't publish poor science. So, in other words, there are more authors to sort through to make sure they're [laughs] good and honest. And I just think it's too many scientists and too many journals worldwide.

Predatory journals – harmful or harmless noise?

Accompanying the overall rise in the number of indexed publications has been the rapid rise of what Beall (2015) has called *predatory journals*. He compiled an online black list of journals with several characteristics in common: they have fake impact factors, for example, and they conduct no or very light-touch peer review that allows low-quality research to be published online within one week – all for a modest fee paid by the author – usually USD 100–200 (EUR 85–170). Beall's list developed into a significant initiative, widely discussed and used by scientific communities to verify the legitimacy of journals. Along with a regular updating of his list, Beall specified criteria for inclusion on the list, and even initiated a formal process

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for removal from the list, which involved an external board of advisors (Bohannon, 2015). Being removed from Beall's black list was almost as difficult as getting onto the 'good' list: Thomson Reuters Journal Citation Reports. The list of predatory journals contained 1294 titles, and it was widely used as a way of telling 'good' journals from 'bad' ones when, unexpectedly, in early 2017, the list disappeared. Beall is said to have removed it from the Internet and refused to explain why. This mysterious disappearance triggered various speculations about possible threats against Beall, including lawsuits and direct pressure on his employers (Chwala, 2017).

A more sensitive issue relates to the geographical regions where the predatory journals originate. Shen and Björk (2015) analyzed the journals on Beall's list and found that the regional distribution of both the publishers and authors is highly skewed. Many predatory publishers are based in developing countries in Asia – 27% of them in India. Furthermore, the authors who publish in these journals come primarily from the same regions, 75% from Asia or Africa. India leads the list with 35% of the authors (the count was of authors rather than publications).

But if Shen and Björk were correct in their analysis, and the authors who publish in the predatory journals come primarily from the regions where these journals are published, why should the Global North[2] be concerned about the rise of predatory journals? Martin Parker (2017) advanced one explanation. In his commentary on Beall's list, he argued that scientists from countries without rich traditions in molecular and biomedical science – the Global South – are vulnerable, desperate, and forced to publish 'rubbish', which consequently damages the scientific record. But, in his view, 'good' journals sometimes publish rubbish too, and the difference between the two is that submissions from academics from the Global North are rejected more often.

In practice, predatory journals are a nuisance, because they flood researchers' inboxes with invitations to submit their works, to review manuscripts, or to serve on their editorial boards. This may be why scientists experience overflow through these spam e-mails that clutter e-mail folders. According to one of the editors, predatory journals simply 'create a noise':

> There's really a lot of them […] if they were published, it would add literally nothing to our knowledge base and it does create noise. You

2 The North–South divide is a political, socio-economic divide between the richer, developed countries in the Global North and the poorer, less developed countries in the Global South.

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> do have to read them to see that there's nothing really in there […] So I've somewhat moved my position on this – that everything should be published […] Now I'm not absolutely sure that's right.

When we asked, 'Is it a harmful or harmless noise', the interviewee answered that most of it is harmless noise, or 'relatively harmless noise', but that there are some publications – and not necessarily articles in predatory journals – which are more harmful. Some of them, the editor told us, are in the high-profile journals in which 'people are having to exaggerate or select their data to make a simple story that gets published in a high-impact journal'.

But harmless noise for the Global North may be harmful noise for the Global South. For 'rich-country researchers' (Bohannon, 2015), these journals are a nuisance that creates the perception of overflow. For science in developing countries, the expansion of predatory journals may have wider consequences. Beall (2015) has argued that these predatory journals were damaging existing research cultures through the corruption of academic evaluation. Similarly, Harzing and Adler (2016) have suggested that some predatory journals appear to gain prominence in the scholarly landscape, bringing their owners significant profits. And Bohannon (2015) has reported that predatory publishers' share of the market currently amounts to USD 75 million (EUR 63.7 million).

According to the editors we interviewed, predatory journals offer publishers a market opportunity and provide market space for manuscripts that could not make it to the upper tiers of the publishing tower. In this respect, predatory journals may be seen as a practical solution to overflow, as they provide the final net 'catching the dross':

> *Interviewer:* And what do you think happens with the papers that you reject?
>
> *Editor:* Well they will eventually be published because, you know, with all the online journals mushrooming, you can publish whatever you want nowadays.

The editors we spoke to 'would not dream' of publishing in these journals, and they actively discourage their colleagues and PhD students from doing so. As one editor said, 'You must be desperate to submit to one of these journals.' Another editor agreed with Jeffrey Beall's assertion that predatory journals create overflow; but worse, they damage the scientific record.

One could pose a question: 'Why do scientists publish in predatory journals?' We hazard an explanation: The number of scientists

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worldwide is increasing, which may produce an increase in both 'good' and 'not-so-good' science. Because some of the newcomers may be unsuccessful with regard to publishing their work in the high-tier journals, they are willing to pay for publication online, in hopes that their ideas are significant enough for the world to notice them. The Internet has leveled the playing field and destroyed elitism – for better or worse. More science is available online, therefore, meaning that scientists have to sift more diligently to separate the wheat from the chaff.

Overflow within journals

Having established that there is evidence of increasing numbers of publications overall, we attempted to diagnose potential overflow in individual journals. Our goal was to discover whether the number of submitted papers is increasing. As mentioned, some journals are open about their submission figures and their publication rates. *Nature*, for example, provides its figures on its Author Guidelines website, which we partly reproduced in Figure 7.1, disclosing that the number of submissions in 1997 was 7,680, rising steadily to 10,952 in 2013. Its guidelines state: '*Nature* has space to publish only 8% or so of the 200 papers submitted each week, hence its selection criteria are rigorous. Many submissions are declined without being sent for review.' Thus competition and impact are maintained by setting limits on the publication space. Noteworthy in this quote is the expression 'has *space* to publish only …'.

The artificial scarcity of publication slots is primarily caused by editors' concerns about quality control, but also about impact factor. Impact factor is calculated by averaging the number of citations of all of papers published, so those rare, extremely highly cited papers (including review articles) that contribute hugely to the impact of the extremely high-impact journals would be canceled out if the publication numbers were to increase greatly. So increasing the number of published papers may have disastrous consequences for impact factors. One editor commented on the drop of *PLOS ONE*'s impact factor after the journal began to publish vast numbers of papers every year. A considerable share of the market was then captured by *Scientific Reports*, which may now be experiencing a similar decline in impact factor following a greatly increased number of published articles.

To verify this observation, we conducted our own analysis of the publicly available data on the number of papers published by

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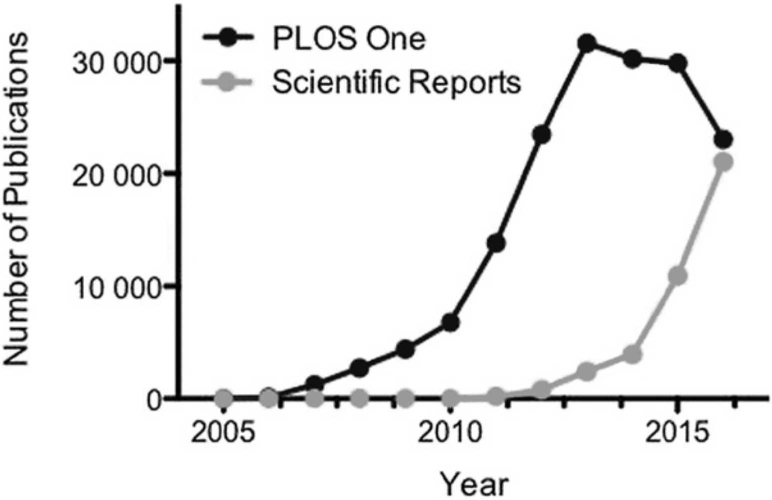

Figure 7.3 Publications in two open-access journals

these two journals: *PLOS ONE* and *Scientific Reports*. Indeed, *PLOS ONE* reduced article output from its peak of 2013, and one analysis suggests that this reduction will increase the journal impact factor (Davis, 2016). In contrast, *Scientific Reports* has maintained the increase in the number of published papers.

PLOS ONE was created to deal with the overflow from the highly selective journals and to offer a more level playing field to scientists who may have been excluded for reasons other than a lack of sound and rigorous scientific results to communicate. The huge number of articles published (Figure 7.3) attests to the great success of this model, and it inspired the creation of many other similar journals, such as *Scientific Reports*, *Biology Open*, and *Open Biology*. These journals seek to review papers rigorously on the basis of soundness and scientific merit, without requiring the level of scientific novelty or depth of mechanistic insight required by high-impact journals. Although this is a great goal, it requires a large number of rigorous peer referees to be drawn from the established scientific community, and hence probably contributes to the perceived overflow.

Even though the number of papers published overall is rising, none of the editors we spoke to reported feeling overwhelmed, which seems to stand in contrast to the perceptions of scientists.

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Even the most established, highly ranked journals do not appear to suffer from an excessive number of submissions. This is partly due to the self-selection of authors, who measure their abilities and aspirations against the requirements of the journals, realistically aiming for journals from lower tiers. One editor from one of the most highly ranked journals commented:

> For many of our journals, there's a lot of self-selection that goes on. People don't send us stuff unless they think it's good in the first place. So, you know, we have a very high standard […] So would I like to see more papers? Yes. But would I like it to double? Not really, because I'm perfectly happy with the level that it's at.

Editors of a few other journals who shared their statistics with us also noticed an increase in submissions, but it was not always a smooth upward trajectory that they observed. Rather, they had identified peaks and troughs, sometimes caused by the emergence of new journals in closely related disciplines stealing a share of their market. Efficiency of manuscript processing also affects the popularity of journals among authors. The most highly ranked journals receive over 10,000 manuscripts annually – some 50 manuscripts a day at peak times.

One editor commented on the uneven distribution of journal submissions: 'It really varies among the journals. Some are going up. I don't think any of them are falling, at least not very much, but some of them are rising and some of them are not.' The blame for this uneven distribution has been attributed to the dominance of the big players, *Cell*, *Science*, and *Nature*:

> The publishing world is in big, big trouble because those two groups – Nature Publishing Group and Cell Press – they have the most prestigious journals and […] they are squeezing out the competition. All of the other impact factors are going down. All of their impact factors are unbelievably high, and so all the other journals are getting squeezed.

Although some editors complained about the monopoly of the big three brands, the editor of one of them admitted that there is still competition between them, and a lot of effort goes into publishing 'the very best'. This editor indicated that rather than 'chasing the impact factor', their journal is interested in publishing 'influential research'.

One way of breaking the dominance of the big publishers is for funders to require that all outputs be publicly available – through

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open access, for instance. Open access is a contentious but rapidly evolving phenomenon. Many commentators have been scathing about the established model, by which science publishers retain the copyright of the paper and the data it contains, and the reader needs to pay to download a paper. This has led a number of governments and funding bodies to require that all publications deriving from their work must be freely accessible to anyone who wishes to read them. In this new model, the finances of science publication are reversed; whereas previously, publishers' costs were recouped mainly by charging readers to access the final publication, now the publishers must be paid upfront for the expense of editing, refereeing, communicating, and archiving research. This model has been applied in different ways; some fully open-access journals have been established as beneficial, non-profit establishments, whereas others are fully commercial. Obviously these two groups are subject to different pressures. Another complicating arrangement is open-access papers published for a fee in otherwise closed journals. And in the fourth variant, a legitimate, highly ranked journal makes some papers free in perpetuity without charging their authors (for example, *Science* makes public-health papers free in perpetuity).

When applied appropriately, open access can democratize science and increase participation and the distribution of results. The upfront nature of the payments has led to difficulties, however. The additional money transferred to commercial publishers has been unexpectedly large and has led to criticism about 'double dipping' by journals that charge authors for open access to their publications, then charge libraries for subscriptions.

Although many fully open-access journals, including *PLOS Biology* and *eLife*, have gained prestige, the attempts to break the commercial stranglehold of the big brands have so far failed – at least according to one editor:

> The main interest for me would be all the open access movement, all of the Wellcome Trust and Howard Hughes funding, *eLife*, and trying to break the back of all the dominance of *Nature*, *Cell*, and *Science*. It all failed, and we're heading for this worst of all worlds […]. We still haven't broken the back of the impact factor, so the impact factor totally dominates everything. But we are now paying for it, because God knows how much Wellcome Trust is pouring down the tubes in paying for open access. And for what? What does open access buy us? Is it really true that there's loads of people reading these papers who don't have access?

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Any fluctuations in submissions are easily accommodated by the journals:

> How do we manage changes in submissions? I mean, if we see significant increases in submissions, then we will look at increasing staffing to accommodate that, you know, because we're run by internal editor. I think other journals do the same thing – there's only so much, I mean. It comes back down to the volumes, and there's only so much workload that an editor can handle and still do the job; so I think that would be what we would do.

Even when the number of submissions increases, editors are reluctant to increase the number of publication slots and instead increase their rejection rates:

> Quality standards. I want to keep a certain level of quality, and that means that only a certain proportion of the papers that get submitted actually reaches that bar. […] we'd definitely be encouraged to increase [the number of published papers] if we felt that the papers were of sufficient quality. But that's really the main criterion. I mean, we want the journal to be high quality […] I wouldn't go up to like 14, 15 papers a month if I didn't feel, if I felt, I had to compromise quality to do that.

Managing overflow by rejecting papers is at times a risky business. One editor admitted receiving threats of lawsuits from disappointed authors, or letters of complaint:

> We are now very, very careful in the assessment of papers, and I always insist now on having an independent person on at least one and if possible two that assess […] So for example [some authors] are very proud, so when you reject one of their papers, you often get very nasty letters, how famous and wonderful they are, and how little we understand about research [laughs].

Some journals deal with the overflow of good-quality papers by passing them on, with the authors' permission, to what is sometimes referred to as their 'sister journals'. For example, *Cell*, *Nature*, and *Science* have launched more branded journals and designed a system for cascading articles from one tier to the next. With the authors' permission, *Science* transfers papers to their sister journals: *Science Advances*, *Science Robotics*, *Science Immunology*, or *Science Signalling*. The Company of Biologists' journals – *Journal of Cell Science*, *Development*, *Journal of Experimental Biology*, and *Disease Models & Mechanisms* – offer to transfer some of their rejected papers to *Biology Open,* an open-access

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journal that provides ‘a welcoming home for papers and helping to avoid additional rounds of submission and review’ (*Biology Open* webpage), which was created with the explicit goal of publishing papers that are scientifically sound and rigorous, but not groundbreaking. Some journals such as *PLOS Biology* also allow ‘review transfers’, whereby a transferred paper is passed on to another journal, together with the original reviews. The explicit goal of this system is to help authors find a place to publish their paper as quickly and smoothly as possible. It also makes good business sense, because the system allows publishers to capture a greater market share. Some of the editors we interviewed raised concerns that this system strengthens the monopoly of the biggest brands, as the sister journals soak up the rejected papers. One editor complained:

> The NPG [Nature Publishing Group] and the Cell Press journals, they’ve twigged […] all of this a while ago, and so they now are launching journals at every level. So when you submit your paper to *Cell* or to *Developmental Cell* or *Cell Reports*, you know there’s a cascade down all in-house. Because, of course, every paper, if you get a paper, review it, and reject it, the financial model tells you you’ve not made any money, you’ve spent money but you’ve not made any. If you can cascade it, […] it gets published, then in your open-access journal that’s a bit lower, but you now *monetize* the submission.

The authors are said to take these trickle-down arrangements for granted. As one editor explained:

> I have found that a lot of my colleagues will submit to *Nature*, knowing that they’ll probably get it into *Nature Communications*, and because it has that *Nature* name on it. […] That worries me for journals like ours, because we’re losing some of our papers to them.

So not all journals experience increasing submission numbers, and the journals that have experienced significant underflow are usually mid-tier, specialist journals (mostly with impact factors under 10) which see their share of the market being taken by the dominant top-end journals from the Nature Publishing Group, Science, or Cell Press families. One editor pessimistically commented: ‘The future of this market is fighting for submissions.’ Another editor suggested that even an upward change of the impact factor does not affect submissions, and having an impact factor in the first place makes a big difference to whether or not they get submissions.

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Peer review: an instrument for managing overflow

The main instrument for managing manuscript flow in journals is peer review, which, despite its detractors (e.g. Eisen, 2011), is perceived as the gold standard that allows a separation between science and hearsay. Yet in our earlier study (Siebert, Machesky, and Insall, 2015), we identified some scientists' concerns about the capacity of the peer-review process to cope with the number of scientific outputs requiring verification and quality assurance. We argued that with an overflow, the quality and objectivity of peer review are being damaged and posed a question: 'Should peer review be updated to cope with the demands imposed by the possible overflow, or perhaps, more radically, should it be disposed of altogether?'

When we asked journal editors about that issue, they spoke uniformly in defense of peer review. Overall, peer review does work, and it is still the best way of verifying the quality of papers:

> [Peer review] works. Honestly, I think it works fine. […] people kind of complain about it, but I actually feel it works pretty well. Is it perfect? No. But can I think of something that would be markedly better? No.

This sentiment recurred in all interviews, and none of the editors we spoke to advocated an alternative to peer review:

> I think that you know that there are lots of discussions about how you could get people to do it, you know, post things online and get people to comment spontaneously. But everybody always says that they don't have time to read even what's already out there, so you're not going to get the same level of scrutiny. It's just not going to happen.

Even small modifications to peer review – anonymizing reviews and making them publicly available (*Nature*, 2015), for example – were also dismissed. As one editor suggested, that change would damage reviewers' good will:

> You could easily make enemies in your field of research, not by doing an unjust review, but because some people feel very attached to their very own research. And when you address some points of weakness, it's not very kindly received. […] it's just not in the interest of a reviewer.

Another editor spoke in favor of anonymous reviewers:

> I personally feel the anonymity aspect is important, because it allows people to be honest without fear of reprisal, and editors have a role in making sure that people aren't being inappropriately critical.

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One modification to the peer-review system that has been praised is ‘the *eLife* model’ of reviewer consultation. After the reviewers send in their reports, they learn who the other reviewers are, and after consultation, they reach a joint decision and produce one set of recommendations. *Science* has a similar cross-review system, whereby reviewers of a paper are shown the other reviewers’ reports and invited to update or amend their own assessments of the paper accordingly. These models are seen as more transparent and constructive, although they can be more time-consuming and do not directly address the problem of overflow in the peer-review system.

Overflow is sometimes a problem in areas in which it is difficult to find enough qualified reviewers. Some journals – usually the journals with lower impact factors – struggle looking for reviewers. Highly ranked journals appear to be less likely to have that problem, because reviewing for them is said to bring scientists prestige, as an editor of one of these journals confirmed:

> We benefit from the fact that the journal is high quality and the papers are interesting. I think you know that we don’t usually have trouble finding people to review our papers, because their willingness to do it is quite high.

These journals usually recruit their reviewers from the pool of laboratory heads and principal investigators rather than postdoctoral researchers or PhD students. One editor said: ‘It should be a peer review, so I don’t want to involve my postdocs to look into it.’ Other editors, especially of the journals from the lower end, were more inclusive in their selection of reviewers and utilized younger colleagues. In their view, senior lab heads would not agree to review papers for lower-level journals. One editor strongly advocated that reviewers should be paid, on the assumption that it would solve the problem of potential deficits and increase the quality of reviews. However, problems involved in paying referees have prevented the idea from being a success. A few other editors recommended other ways of rewarding their reviewers – notifying their institutions about their good citizenship, offering ‘thank you’ lunches, or simply reaching out to them to personally to say thank you. One recommendation, which recurred in a number of accounts, was to introduce training for reviewers. Like writing papers or grant applications, reviewing papers is a specific skill that is often taken for granted and not explicitly taught. Training new reviewers and evaluating them, then, was one suggestion geared to addressing what Eisen (2011) has called the ‘peer review crisis’.

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The champagne tower of science publishing

In our search for evidence of overflow in science, we concluded that there is exponential growth of publications overall, but that this growth is related to an increase in the number of journals and in submissions to a few broad-scope, open-access journals like *PLOS ONE* and *Scientific Reports*, rather than to massive increases in submissions to most of the individual journals that we analyzed. Some journals see increases in submissions – others do not – and most editors would like to see their numbers rise.

So is overflow in science good or bad? On the one hand, the exponential growth of science publications can be seen as positive. Investment in science in the emerging economies leads to more studies being conducted, and it should mean that scientific progress moves more quickly. On the other hand, overflow can give rise to concerns about the trustworthiness of science. Our interviewees indicated that the more science is produced, the more noise there is in the system, and the more difficult it is to tell what is trustworthy and what is not. Scientists are concerned about the ability of the world of science to govern the quality of the increasing flow of scientific outputs.

The metaphor that we believe best captures overflow in scientific publishing is that of the champagne tower (Figure 7.4). Like the glasses in the tower, scientific journals are organized in tiers, with the most prestigious elite journals at the top (*Cell*, *Science*, *Nature*) and lower-ranked journals at the bottom. In between are various tiers of journals in a decreasing order of their impact factor. The Holy Grail of science is the top glass, as publishing in the top-rated journals guarantees academic positions, grants, and membership of editorial boards. A scientist's career depends on publishing as many papers as possible in the most prestigious journals (Nosek, Spies, and Motyl, 2012). Furthermore, we have been told, publishing in the top journals increases the odds of publication in the top journals in the future.

If the quality of champagne is the same at the top and the bottom of the tower, why does everyone prefer the top glass? Publishing in the top-rated journals has become the yardstick of scientific careers (Schekman, 2013), regarded by many who hold the purse strings as being the ultimate measure of scientific excellence. But these journals maintain the artificial scarcity of spaces. Neal Young and his colleagues (2008) explained this phenomenon well. In their influential article on the 'winner's curse', they compared the idea of artificial scarcity in economics (i.e., restrictions on the provision

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Figure 7.4 The champagne tower

of a commodity above that expected from its production cost) to the artificial page limits in prestigious journals. In light of their analysis, print page limits are used as an excuse to justify high rejection rates, as extremely low acceptance rates provide status signals to successful authors. This limitation on publication slots is entirely artificial, they argue. As with online publishing, there is no real need for page limits – both in relation to the length of articles and the number of slots in each issue. As Young et al. summarized their argument:

> The self-correcting mechanism in science is retarded by the extreme imbalance between the abundance of supply (the output of basic science laboratories and clinical investigations) and the increasingly limited venues for publication (journals with sufficiently high impact). […] The scarcity of available outlets is artificial, based on the costs of printing in an electronic age and a belief that selectivity is equivalent to quality. (Young et al., 2008: 1418)

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Some authors, especially those who evaluate their own research critically, will send their best work to one of the three top journals and anything they have evaluated less favorably to a lower-ranked journal. Others begin their paper's journey at the top and, if unsuccessful, will aim for a lower tier. Manuscripts that do not find their way into the top journals trickle down the champagne tower, either because their authors resubmit them to the lower-ranked journals or because the editors who rejected them offer to transfer them down to their affiliated journals. Some editors refer to the journals that accept transfers from higher tiers as 'trickle-down' journals, evoking the image of dripping liquid. The realistic view expressed by the editors and scientists is that everything will get published somewhere eventually. Even if it is not champagne, it will not end up on the table.

When considering the trickle-down arrangements between journals, it is worth examining the ownership of the journals, posing the question, 'Who owns the individual glasses in the champagne tower?' The concern expressed by some editors about the middle-tier, specialist journals is that the papers that used to be submitted to their journals now end up in the journals owned by the three big 'brands'. One journal editor commented on the power of the *Nature* brand: '*Nature* is one of the most powerful brands in the world, even more powerful than most fashion brands. People flock to these journals at all costs. The name alone stands for prestige and quality and successes in research.'

We have used a number of possibly contentious labels: Global South, Global North, Big Scientist, Little Scientist, rich-country scientist, and developing-country scientist. None of these labels is precise, because the dividing lines in science do not always follow neat geographical regions, generational lines, or tiers in the structure of scientific establishments. Whatever labels we use, we run the risk of stereotyping. Admittedly, the editors we interviewed at this stage of the project were all from the Global North, and we recognize that speaking to the editors of newly established journals from the Global South may produce a different tale of hegemony and a struggle of survival in the world that favors the established hierarchies of science.

The perspective of some of our interviewees leans toward maintaining the status quo – welcoming the newcomers but worrying about their legitimacy. Others, from journals based on new models of publishing founded on sound rigorous science and fair transparent treatment of authors, have a more optimistic outlook geared toward

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changing the system and breaking away from a heavy dependence on journal brands and impact factors. Initiatives such as the open science movement (Open Research Data Taskforce, 2017) or a move toward scientists taking greater control of publishing (Fyfe et al., 2017) are other potential ways of changing the system and making a positive change to the culture of science publishing. Open science may substantially increase the amount of shared research, including disclosure of negative or inconclusive findings. The Internet makes this sharing possible and offers more than one way to communicate new and exciting results. But of course, it creates even greater overflow.

Franck Cochoy (2012) has argued that when overflow happens, it is proof of the failure of management, as management should be channeling flows, not dealing with overflows. The most convincing way of managing overflow in science and verifying quality of published outputs is peer review – the gold standard of quality assurance, which, it seems to us, has obvious capacity limitations. If more papers are produced, more reviewers are needed, and the pool of reviewers does not appear to grow proportionally to the increase in the number of authors. A closer look at the peer-review system, construed as a 'filter failure' (Shirky, 2008), may introduce new ways of managing overflow in science. And reducing the overflow should help to reduce the noise.

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