8

Pressures to Publish: What Effects Do We See?

Daniele Fanelli

Concerns for the negative effects of pressures to publish date back at least to the 1950s (Siegel and Baveye, 2010; see also Alex Csiszar in this volume, chapter 1) and today are more widespread than ever. There is virtually no contemporary article that, in analyzing or commenting on issues of research integrity, will fail to suggest that scientists might be increasingly engaging in problematic research practices. At the very least, it is typically argued that scientists may be cynically "salami-slicing" their results (i.e., fractioning them to maximize publication output), but multiple other detrimental practices, right up to the most egregious scientific crime of data fabrication, are suggested to represent plausible strategies to "game" a system that imposes increasingly unreasonable productivity expectations (National Academies of Sciences, Engineering, and Medicine, 2017). Such "pressures to publish" might be imposed upon scientists explicitly by their employing institutions but also implicitly, through the institutional use of faulty metrics of publication quantity and quality (figure 8.1).

This narrative is logically consistent and plausible, but is it correct? The empirical evidence that is most typically invoked in support of the pressures to publish hypothesis comes from anonymous surveys and qualitative studies that observed a connection between reported pressures to publish and likelihood to observe or indulge in questionable behaviors (De Vries, Anderson, and Martinson, 2006; Davis, Riske-Morris, and Diaz, 2007; van Dalen and Henkens, 2012; Tijdink, Vergouwen, and Smulders, 2013). This evidence, however, has clear limitations. Surveys can valuably inform us about what researchers believe, what they say, and what they think they have experienced, but surveys do not necessarily tell us what actually occurs in the general population of scientists. Moreover, results of surveys and interviews are not easy to compare across studies, making it hard to verify whether the problem of pressures to publish has worsened over time as suggested.

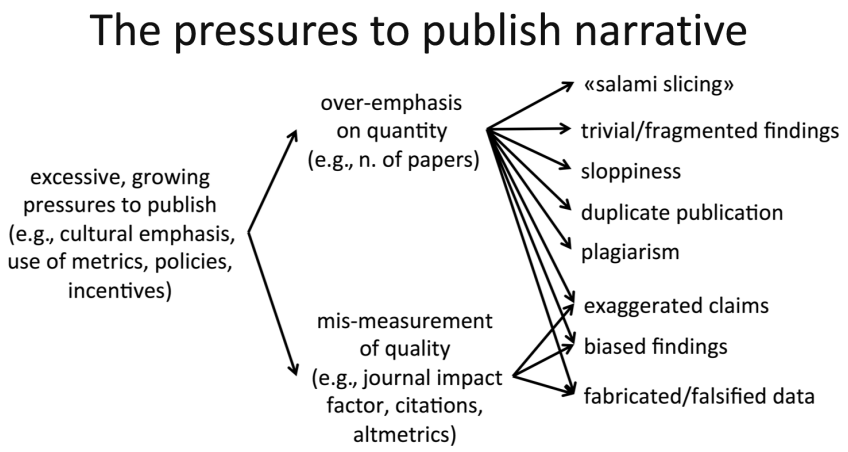

Figure 8.1
Schematic summary of hypothesized causal links between pressures to publish and research malpractice, as widely discussed in the literature.

A second and seemingly more direct source of evidence about scientific misconduct appears to be provided by data on retractions of scientific papers. Retractions are mostly the consequence of scientific misconduct (Fang, Steen, and Casadevall, 2012), and the fact that they are more frequent in high-impact journals and that their total number has grown over the years seems to confirm the worse predictions of the pressures to publish narrative (Fang and Casadevall, 2011). Such interpretations, however, are demonstrably premature and likely incorrect. Retractions are an editorial tool of recent invention, and the number of retractions issued per year reflects primarily, if not entirely, the growth in the number of journals that have retraction policies. Back in 2004, only twenty-one percent of high-impact medical journals had a policy to retract papers, whereas in 2014, the percentage had increased to sixty-five percent (Resnik, Wager, and Kissling, 2015). This datum illustrates, on the one hand, how much progress has been made in setting up a system of retractions and yet, on the other hand, how far the system still is from operating at full regime. It is easy to show that the number of retractions no longer appears to be increasing if it is adjusted by the number of journals that are actually issuing retractions, indicating, in other words, that the number of retractions per retracting journal has remained stable for decades (Fanelli, 2013). Moreover, high-impact journals were the first and most proactive adopters of retraction policies (Resnik, Peddada, and Brunson, 2009). This fact alone can explain why high-impact journals have higher

retraction rates, even ignoring additional factors like the higher level of scrutiny that these journals are subject to (see chapter by Ivan Oransky, this volume, chapter 10). Therefore, patterns characterizing the prevalence of retractions are not a valid indicator of a possible growing problem with pressures to publish.

The most solid—but still observational and indirect—evidence of a growing problem with pressures to publish comes from statistical analyses of the literature. At least three independent studies suggest that, over the last few decades, abstracts of scientific papers have reported increasingly positive or statistically significant results (Pautasso, 2010; Fanelli, 2012a, 2014; de Winter and Dodou, 2014). Furthermore, at least one study noticed that positive results might be more likely to be reported by abstract of papers from academically productive areas in the United States (Fanelli, 2010a), and at least four meta-meta-analyses in the social and behavioral sciences observed that academically productive countries, and particularly the United States, might publish findings that systematically overestimate underlying effects (Doucouliagos, Laroche, and Stanley, 2005; Munafo, Attwood, and Flint, 2008; Fanelli and Ioannidis, 2013; Fanelli, Costas, and Ioannidis, 2017).

Even meta-analytical evidence, however, offers no conclusive proof of a negative effect of pressures to publish. By drawing correlations at the national level, all the studies listed above are at risk of "ecological fallacy," because correlations observed at the national level might not reflect correlations occurring at the individual level. Moreover, higher rates of positive, statistically significant, and/or extreme results could be produced by mechanisms that have little connection to conscious biases, let alone scientific misconduct (see further discussions in Fanelli [2010a, 2010b]).

Two Empirical Blows to the Pressures-to-Publish Narrative

The most direct assessment of the pressures-to-publish narrative (figure 8.1) comes, to the best of my knowledge, from a series of studies that I recently conducted with several colleagues, which tested multiple hypotheses about determinants of research integrity, misconduct, or bias. The earliest such study, a collaboration with Rodrigo Costas from Leiden University and Vincent Larivière from University of Montréal, examined a large sample of retractions and corrections issued in the years 2010 and 2011 (Fanelli, Costas, and Larivière, 2015). We retrieved the original papers to which the retraction and correction notes referred, and for each of these papers, we retrieved two matched controls—that is, papers that

had been published in the same journal and issue but that had not been corrected or retracted. For each of these papers (a total sample of 611 retracted papers, 2,226 corrected papers, and 5,466 controls), we reconstructed the publication profile of first and last authors and recorded other characteristics of study and authors that common hypotheses made in the literature would predict to represent risk factors for scientific misconduct. We predicted that pressures to publish and other risk factors for scientific misconduct should increase the likelihood of retractions and be neutral or decrease the likelihood of corrections, because retractions are usually the consequence of scientific misconduct (Fang, Steen, and Casadevall, 2012), whilst corrections are usually spontaneously solicited by authors (Fanelli and Ioannidis, 2013).

Results showed that the likelihood to be the author of a corrected paper was similar across countries, whereas that of a retracted paper varied substantially. The variance in a country's risk of retraction was partially explained by national publication incentive policies, but not in the direction predicted by the pressures-to-publish narrative. Countries in which high-impact publications are rewarded with cash, such as China, Turkey, and Australia, registered the highest risk of retractions. Countries with career-based publication incentives such as the United States, in which pressures to publish are imposed on the individual through the requirements of tenure, showed intermediate levels of risk. Surprisingly, the risk was lowest in countries such as the Netherlands or United Kingdom, in which universities receive public funding in proportion to their ranking in national research assessments. Since in these latter countries researchers have in theory no choice but to comply with their employer's expectations, these are arguably the only countries in which "pressures to publish" are occurring in a literal sense, and in any case represent countries in which pressures are perceived to be highest (van Dalen and Henkens, 2012).

Therefore, our findings were quite different from what the classic pressure-to-publish narrative would have predicted. The countries at greater risk of misconduct appeared to be those in which researchers are not under institutional pressures to publish, but those in which researchers are lured by cash bonuses. If misconduct can be directly ascribed to a cause, that cause seems to be the corruption and greed of individual scientists.

When we looked at the publication profiles of individual authors, these surprising results were confirmed. The most prolific authors, and those who publish in high-ranking journals, were equally or less likely to produce retracted papers, and equally or more likely to author papers that were later corrected—arguably manifesting higher research integrity.

This pattern was visible even when analyses were limited to authors working in the United States.

These findings were remarkably corroborated by two later studies co-authored by Rodrigo Costas, myself, and others. These studies tested exactly the same hypotheses and the same author characteristics as the study described above, but on completely different proxies of research quality and integrity. One study retrieved a total of over three thousand meta-analyses to test if these parameters predicted the likelihood that a study would report overestimated effect sizes, possibly due to research and publication bias (Fanelli, Costas, and Ioannidis, 2017). The other study used a matched-control approach similar to the one used on retracted papers, but this time on papers that had been identified, by direct inspection, as containing image duplications that are likely to result, at least in part, from intentional misconduct (Fanelli et al., 2017). Both of these studies led to very similar conclusions with regards to the pressures to publish hypothesis: the most prolific authors, those who publish in high-ranking journals, and those working in countries in which pressures to publish are supposedly greater were less likely to report exaggerated findings and less likely to publish papers with image duplications. These results are all observational of course, and therefore do not prove that being a prolific author, or working under high pressures to publish, makes you more honest. However, they are clearly completely at odds with a simplistic narrative that associates publishing too much or too ambitiously with being a cheater.

A second blow to the standard pressures-to-publish narrative came when, in collaboration with Vincent Larivière, I assessed whether scientists are actually publishing papers at an increasing rate (Fanelli and Larivière, 2016). In order to do so, we tracked the individual publication profiles of researchers in all disciplines throughout the twentieth century. From an initial sample of over 540,000 individual authors that we could identify with relative accuracy, we selected those whose main affiliation was in North America, Europe, or Australia/New Zealand, and further limited the sample to authors who had published at least two papers over a period of fifteen years following their first recorded publication (collecting a total sample of 41,000 authors who had co-authored over 760,000 papers between the years 1900 and 2013). Our analysis focused on the first fifteen years of research productivity, because this is an early-career phase in which pressures to publish are presumably highest.

A superficial look at the total number of papers ascribed to each individual author would support the perception that scientists are publishing

more. The total number of papers associated with an author's name has increased in most disciplines, including in recent decades, a period in which pressures to publish have arguably become more intense and for which our data is likely to be more accurate. However, the average number of co-authors of these papers had increased as well, and at an accelerating rate. This factor cannot be ignored when estimating scientists' net publication rate.

When we counted publications fractionally, by dividing scientists' total number of publications by their average number of co-authors, the resulting publication rates show no marked increase, and were actually flat or declining in most disciplines. One could argue that co-authorship criteria have simply changed, and that not all names in a manuscript have contributed an equal amount of effort to the publication. However, we also limited the analysis to papers in which our sample of researchers had appeared as first author, a position that in most disciplines identifies the team member who mostly contributed to the publication. Again, we observed no increase. These trends occurred similarly across countries, and multiple secondary analyses suggest that these results are not only robust, but actually rather conservative. For example, when we extended the career time window to twenty-five years, results were very similar, whereas when we restricted it to eight years, publication rates were significantly declining for most disciplines (all robustness results and primary data are provided in the supplementary information of Fanelli and Larivière [2016]).

It must be emphasized that the conclusions of all these large-scale, quantitative studies apply to the average trend. As one should expect, our sample included cases of extremely productive individuals (e.g., researchers who managed to co-author hundreds of papers in just a few years). The number of these cases is likely to have increased during the century, if anything because the total number of scientists has increased. These highly productive individuals are, almost by definition, likely to be widely known, and their higher visibility might reinforce the perception that scientific productivity has risen to excessive levels. However, these extremely prolific authors are amply counterbalanced, at the other extreme, by individuals who publish few papers and yet seemingly do not drop out of a scientific career. On average, therefore, the publication rate of scientists has not increased.

An effect of overexposure similar to the one described above might explain why data fabrication tends to be associated with hyperproductivity. In the aforementioned study on retractions, we noticed that names

associated with multiple retractions tend to be, not too surprisingly, highly productive authors (Fanelli, Costas, and Larivière, 2015). The more retractions a case of misconduct brings about, the more exposure it will get in the media (not in small part thanks to the work of Ivan Oransky, this volume, chapter 10), and this might reinforce the public perception that scientific misconduct and unrealistic publication performance are connected.

In sum, according to our findings, scientists today are not publishing, on an individual basis, at higher rates than their colleagues in the 1950s. Today's scientific CVs do list more papers than they used to, but this occurs primarily because scientists today collaborate much more, sharing their efforts as well as their publications. Our results confirmed, with a more rigorous analysis of individual publication patterns, what a simple comparison of the yearly numbers of papers and authors had suggested long ago (de Solla Price, 1980).

Salami-Slicing Collaborations and Sandwiching Results as Alternative Gaming Strategies

In the past, I have published evidence that I interpreted as supporting the pressures-to-publish hypothesis (see Fanelli 2010a, 2012a). In light of these new findings, I am keen to revise my beliefs. Do I believe that pressures to publish are a myth and that we have nothing to worry about? Not at all. The rise of metrics-based performance evaluation is a historical fact, and if scientists claim to feel pressured to publish, there is no reason to doubt them. If these facts didn't suffice, my own personal experience leads me to believe that important and questionable changes are occurring in scientific practices, and that publication performance evaluation has much to do with these changes.

However, I believe that the popular narrative that links pressures to publish to a growing problem of misconduct might be incorrect. Upon closer scrutiny, the direct connection this narrative draws between pressures to publish and misconduct is too simplistic, and the claim that scientists today are publishing at increasing rates is not supported by evidence. It is therefore a double mistake to combine the two claims and conclude that scientific misconduct and other questionable research practices are becoming more prevalent because of growing pressures to publish.

If overproductivity is not directly distorting contemporary science, then how can we explain the aforementioned evidence that null and negative results are proportionally decreasing and especially so in scientifically

"productive" countries? I will suggest two phenomena, neglected by past analyses, that might mediate the connection between pressures to publish and questionable research practices.

The first phenomenon is the growth in collaboration size, a trend amply documented in all disciplines and confirmed by my own research, as discussed above. Scientists are likely to have increased their collaboration rates in response to pressures to publish, following a strategy that was openly recommended in the literature (for example, Hayer et al., 2013). Indeed, the growing complexity of research appears to be insufficient to justify, alone, the recent rise in co-authorship, at least in biomedical research (Papatheodorou, Trikalinos, and Ioannidis, 2008).

Larger collaborations might run a higher risk of bias and misconduct. Collaborations represent an investment of resources and professional reputation of considerable size; their members, therefore, might be under comparably high pressures to make such investment "pay off." We can therefore hypothesize that the design and conduct of collaborative studies might be exceedingly oriented toward producing "publishable" results, possibly at the expense of scientific rigor and integrity. Errors, bias, and misconduct, moreover, might be more likely to escape scrutiny in collaborations from different fields and institutions, because cultural and geographic distances impede mutual criticism and supervision. Preliminary data support this hypothesis, by suggesting that long-distance collaborations are associated with higher rates of positive results (Fanelli, 2012b) and do not protect against the risk of retractions (Fanelli, Costas, and Larivière, 2015; Fanelli, Costas, and Ioannidis, 2017; Fanelli et al., 2017).

Collaborations might be proliferating excessively because the h-index and other popular metrics of research productivity and impact are not adjusted for co-authorship or actual contribution. From a technical point of view, this is a highly questionable choice, which presumably was made not for sound mathematical reasons, but to keep publication metrics simple and to encourage collaboration, which is assumed to be a positive force in science. Paradoxically, however, by not imposing any costs to co-authorship, current metrics might discourage real cooperation, because higher "performance" scores can be accrued by scientists who fragment their effort into as many collaborations as possible and contribute the minimum possible to each (Kaushal and Jeschke, 2013). Scientists, I am suggesting, might be increasingly "salami-slicing" not their results, but their collaboration efforts.

The second phenomenon is a general increase in the length and information content of scientific papers. Such a trend has been documented

in biomedical research (Vale, 2015) and was supported in our cross-disciplinary data, which showed that the average page length of papers has increased throughout the twentieth century (Fanelli and Larivière, unpublished observation). This phenomenon, which openly contradicts the hypothesis that scientists are increasingly "salami-slicing" their results, is likely to reflect the growing complexity of scientific research and the availability of ever more powerful computational tools. Pressures to publish might reinforce this trend, by compelling scientists to "pad" their papers with unnecessarily large numbers of data points and secondary analyses, in order to boost their papers' chances of acceptance by the journal. Sheer quantity of data and analyses might be increasingly used as a cheap substitute for quality (that is, for convincing conclusions drawn from well-researched studies and cleverly designed experiments). Moreover, pressures to publish might prompt scientists to overpromote their work by emphasizing, simplifying, and perhaps exaggerating the strength and originality of their findings. Under this scenario, negative and nonstatistically significant results might not be, as usually suggested, left lying in the proverbial file drawers but might be buried instead into longer and more complex papers, which in titles and abstracts highlight only positive findings. The salami slices, in other words, are not taken apart and published individually, but used to pad large and seemingly rich panini, only the juiciest fillings of which are allowed to stick out and be exposed in titles and abstracts. Rather than "salami-slicing" and selecting results, I suggest, scientists may be increasingly "sandwiching" them.

In conclusion, contrary to what is commonly argued in the literature, there is little direct evidence to suggest that pressures to publish have increasingly undermined the integrity of scientists. However, it is reasonable to hypothesize that such pressures might have contributed to alter scientists' publication and collaboration practices. Scientists might be gaming metrics by "sandwiching" their negative results and "salami-slicing" not their publications, as commonly believed, but their collaborations. These strategies might have been effective to the point of going largely unnoticed by generations of scholars concerned by the negative effects of pressures to publish.

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Gaming the Metrics

Misconduct and Manipulation in Academic Research

Edited by: Mario Biagioli, Alexandra Lippman

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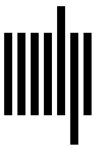

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