
Introduction: Metrics and the New Ecologies of Academic Misconduct

Mario Biagioli and Alexandra Lippman

Traditionally, the assumption has been that academic misconduct emerges primarily in response to “publish or perish” pressures. Robert Slutsky, a UC San Diego cardiologist famously caught in 1986 reporting imaginary experiments, was, at one point, putting out one article every ten days (Lock and Wells, 2001). “Publish or perish,” however, is no longer the sole incentive for misconduct. New practices are emerging that are not limited to the production of fraudulent publications but are aimed instead at enhancing, often in unethical or fraudulent ways, the evaluation of their importance or “impact” (Biagioli, 2016). “Publish or perish” is merging with “impact or perish.”¹

This is related to but different from the predictable gaming of academic performance indicators one would expect from Goodhart’s law: as soon as an indicator becomes a target, gaming ensues, which forecloses its ability to function as a good indicator.² That may take the form, for instance, of massaging the definition of what counts as a “successful student” in metrics about schools’ performance, or of what counts as a “peer-reviewed” paper in faculty evaluation protocols. It could also involve aligning one’s practices to metrics-relevant parameters, like capping classes’ enrollment to nineteen students to have them fit the *US News and World Report*’s definition of “small class,” which is rewarded in its ranking of universities. But we now find authors and editors who move beyond this kind of gaming to create (rather than tweak) metric-enhancing evidence, such as citations to one’s work or to the work published in a given journal so as to boost its impact factor. We argue that the growing reliance on institutional metrics of evaluation does not just provide incentives for these kinds of manipulations, but also creates their conditions of possibility. They would not have come into being were it not for the new metrics-based “audit culture” of academia (Power, 1997; Strathern, 2000; Burrows, 2012).

Beyond Truth and Falsehood: Innovation in Manipulation

As shown by the US federal definition, misconduct is construed in epistemic terms: fabrication, falsification, and plagiarism.³ Accordingly, misconduct is equated to producing false statements like making up data, fudging data, and faking authorship—false statements *within* a publication. Traditional fraud and misconduct continue to exist, and these definitions may be suitable to describe them. What they fail to grasp, however, are the new forms of manipulation that do not affect the epistemic status of a publication but take place *around and outside* the claims themselves like, for example, submitting fake peer reviews (often to publish in a higher impact factor journal than the article would have probably deserved), hacking journal databases (to manipulate the acceptance of one’s article or to insert one’s name in the authors’ byline of an article already in press), setting up citation rings among authors (to maximize their personal citation counts) or among editors (to maximize their journals’ impact factors), and so on. These may be called “post-production” manipulations in the sense that they concern the publication process and the impact of the claims, rather than a manipulation of the content of the publication. And while post-production manipulation may overlap with traditional misconduct, it does not need to. One can have a legitimate paper published in a journal with a good impact factor thanks to fake peer reviews.

We do not propose to simply expand old definitions of academic fraud to make room for post-production manipulations. Nor do we suggest that they are a lesser evil than traditional misconduct, or that they should be labelled as “questionable practices” rather than misconduct because they do not necessarily affect the core of a publication—its evidence and claims. The line between misconduct and questionable practice is notoriously hard to draw (Steneck, 2004; Biagioli, Kenney, Martin, and Walsh, 2019), and, more importantly, we cannot be positive that the different definitions of fraud and misconduct adopted by different countries, agencies, and academic institutions are accurate or fully commensurable with each other. This is not meant as a criticism but as an acknowledgment that the thinking, definitions, and policies about misconduct have been and continue to be the work in progress of hundreds of concerned practitioners in universities, governmental institutions, funding agencies, and journals (Jacob, 2014).

Our goal is neither to question nor to uphold existing views about traditional misconduct, but to call attention to and provide a first analysis of a recent dramatic development: the emergence of a range of new

ways of manipulating the publication process—manipulations that are qualitatively different from traditional academic misconduct. Many of the practices described in this book are obviously unethical and arguably illegal. For instance, independently of the nature of the specific manipulation involved in a given case, the goal of most of these practices is to produce an artificially enhanced curriculum vitae, which gives its holder a leg up against fair-playing competitors in gaining positions and funding that they might not have otherwise obtained. This would seem to match the legal definition of fraud, without having to mobilize more specialized definitions of academic or scientific misconduct. Our primary interest as scholars, however, is to understand the features of these new forms of manipulation, why they are emerging now, what motivates or incentivizes them, and what are the new forensic techniques and actors that are being mobilized to detect them. Ultimately, these questions are key to determining whether we are confronting new instances of old misconduct—old wine in new bottles—or something altogether different. In old Kuhnian parlance, the fact that several of these practices seem irreducible to the current misconduct taxa (fabrication, falsification, and plagiarism) does not tell us whether they are merely apparent anomalies that can be eventually massaged into our current “misconduct paradigm,” or whether, instead, we might end up having to rethink it altogether. We believe that the latter outcome is the more probable one, and hope that our book will contribute to that rethinking by providing a comprehensive map of the problem and its contours. As our many chapters show, the emergence of this new species of misconduct (or whatever more appropriate term we might develop down the line) should be seen simultaneously as a problem and a symptom of a more general shift in the academic publication system, down to the very meaning of publication, and thus of misconduct.

To put it somewhat crudely, what we discuss here are less “epistemic crimes” than “bureaucratic crimes”—practices involving the production of publications that manipulate the publishing system itself or, more specifically, what that system has recently evolved into. What is clear is that these manipulations amount to post-production activities and that, despite their many different forms, they are framed by metrics of evaluation variously based on citations and impact rather than by concerns with plain productivity (as it was in the “publish or perish” age). Conflating epistemic and bureaucratic manipulations would risk foreclosing an understanding of the conditions behind the emergence of these new practices and what that may tell us about what “publication” is becoming in the age of metrics in an increasingly global context.

Redefining Publication and Evaluation

In the seventeenth century, “publication” meant making things public by a variety of means, ranging from lectures (to students or fellow academicians) to letters, personal conversations with reputable people, and printed publications. That changed in the nineteenth century when, at least in science, the definition of publication was narrowed down to printed articles in academic journals (Csiszar, 2018). But if the definition of acceptable modalities and technologies of publication changed in time, the assumption remained that the evaluation of the claims made public was to be done by those who listened to or read them. It was a judgment made by humans, which could be contested by challenging either the protocols employed in the judgment, or the qualifications of the people making them. Today, instead, publication is no longer necessarily evaluated through reading by people but, in some contexts, through markers connected to the publication (though external to it), such as the impact factor of the journal where it is published or the number (or, in some cases, the weight) of the citations it receives (West et al., 2010; Biagioli, 2018) or article metrics on platforms like Academia.edu and ResearchGate (Lippman and Kelty 2019).

The meaning of “publication” has substantially changed, not just because its evaluation has almost ceased to require human agency, but also due to the fact that publication is no longer limited to the process of making claims public. Publication used to be separate from evaluation (which was clearly thought of and practiced as a post-publication activity), but the two may now be folded together. When it relies on the journal’s impact factor, evaluation no longer follows publication but takes place *together with* the act of publication. It involves locating the venue of the publication and attaching that location’s index—the impact factor—to the publication. A publication is born evaluated, so to speak.

The meaning of evaluation has changed as much as that of publication. It is not just that, as we often hear, nobody reads but people only count. Something more radical has happened: the traditional locus of evaluation—the publication’s claims—has become technically irrelevant to metrics regimes based on impact factors. It is not that people ought to read but have lazily stopped doing that. People still read for research and educational purposes, but reading is no longer a necessary component of *institutional* forms of evaluation because some of those metrics are independent of the epistemic dimensions of that specific publication—its claims—but rely, instead, on metadata and similar markers that can be picked out and processed by nonhumans.

From Content to Metadata

These changes in what “publication” and “evaluation” mean are not immediately evident from the external appearance of an actual publication. Whether you look at it in print or online, a journal article still looks very much like it did in the 1980s. What has changed is the role of its metadata, which has gone from descriptive to evaluative. The title of the journal where the article is published no longer simply describes where that article became public (or where it can be found on a library’s shelf), but, through the impact factor, it conveys a numerical estimation of its reception and effect. Conversely, an article published in a journal whose title appears on a list of online open-access “predatory journals” (discussed below) may be simply dismissed as having no value whatsoever by a committee reviewing a researcher for hiring or promotion. It could be effectively treated as a nonpublication despite the fact that it looks exactly like an article published in a journal that looks exactly like a journal. It is the shift of the focus of evaluation from the claims (internal to the publication) to the circumstances of the publication (external to it) that anchors all the changes discussed above: the end of reading (replaced by the scraping of metadata); the switch from qualitative human judgment to quantitative calculated indexes; and the merging of publication and evaluation.

It is worth noticing that, taken as an institutional genre, these new modalities of evaluation are an expansion of the form of library cataloguing. Like cataloguing, they do not involve the reading of a publication’s content but rather the processing of a publication’s metadata. We might say that impact-oriented evaluation becomes part of an “expanded indexing,” one that does not simply generate a call number but processes aggregate publication metadata to generate figures about the purported value of the publication as an input in a variety of institutional decisions—faculty hiring and promotion; whether the library should or should not subscribe to that journal (or to the catalogue of a given publisher); whether grants to defray open-access publication costs should be given to faculty who want to publish in those journals; and so on. If in the past the evaluation of a publication was almost exclusively undertaken to either assess the quality of its claims or the scholarly quality of its author, the new forms of evaluation based on indexes (rather than content) are aimed at informing a variety of institutional decisions, down to the national or even global ranking of the institutions themselves.

Are Journals Becoming Mints?

Also striking is what has become of the notion of impact. In common usage, impact refers to an effect, that is, to something that has happened already (like, say, the citations that an article has received since its publication). The increasingly coveted Journal Impact Factor (JIF), however, functions as an estimation of impact before it happens, as a device to give a valuation right now to a publication that can in fact accrue value (that is, impact) only in the future. This is different from saying that the value of things is bound to fluctuate in time. A house has value both when it is first built and then years after that, but, by definition, the impact of a publication is *zero* when the publication comes off the press. This means that a publication's impact cannot be measured by the impact factor because there is literally nothing to measure at the time of the publication. All the JIF can produce is purely a prediction of impact, and one that is not based on the features of that specific publication but on those of previous articles published in that journal over a certain period of time.

Like actuarial or death tables, the impact factor is based on evidence about the past. And in the same way that actuarial tables are used to estimate the likely length of one's future life to calculate today's insurance premium, the impact factor is deemed to provide an estimation of the amount of citations the article *will have received* in the future based on the fact that it was published in that journal, thus "pricing" the article (and thus its authors) *right now* rather than after it would have had actual impact. It is a rather crude tool to price futures. We are not speaking metaphorically: In China, universities hand substantial cash bonuses to their faculty for their publications, indexing them to the journals' impact factor. *Nature* and *Science* articles fetch over \$30,000 a piece.⁴ Similar schemes can be found on the other side of the Equator:

Melbourne Business School pays \$A15,000 cash for every paper published in the Top 40 list compiled by the *Financial Times*. The scheme at Queensland University Business School is more complicated. Payments, made to the departmental accounts of authors, are approximately: Tier 1 journal—\$A12,000; Tier 2 journal—\$A7500; Tier 3 journal—\$A5500; Tier 4 journal—\$A2000; Professional journals—\$A1000. (Macdonald and Kam, 2007)

There is nothing wrong in using reasonable estimates about future states of affairs, except that the impact factor is not used as an estimate, but has been reified into a positive measure of impact and, more broadly, of value. Furthermore, while the impact factor refers specifically to the journal (not the articles), the JIF has come to signify the impact or value

of each article published by that journal. It functions, literally, like money. It is the “face value” impressed on a coin or banknote, which determines its value no matter what the coin is made of (copper, silver, gold, steel), or what the exchange value that coin would have as metal. The “content” of the coin is just the medium for the stamp, which needs to be impressed on some material. What carries value is not the inside but the outside—the number inscribed on the surface. Whether the article is a piece of gold or lead (or worse), the JIF has come to determine the face value of that article. (We could probably think of journals as mints printing or coining money with a face value equivalent to their impact factor.⁵) I do not even need to know what the JIF is, how it is calculated, or how reliable it is because my institution—and probably all institutions I will ever work for—will honor that face value. An article in *Science* or *Nature* is literally money in the bank, independently from the actual impact it will ever have.⁶

Attaching an impact factor to an article at the time of publication (and thus before real impact has accrued) shows that the JIF has literally nothing to do with the evaluation of that specific article, but simply prices it in a currency that allows for *exchange*. As an author, I can “trade” articles with certain impact factors into a job, and the institution that employs me can then “trade” those publications (together with hundreds or thousand more by other faculty of the same university) into a better national or global ranking, which may be subsequently traded into more students, donors, contracts, and so on. (Conversely, these days in China, one can purchase authorship in a prewritten and preaccepted article to be published in an English language journal, at prices that vary according to the impact factor of the specific journal.⁷) The impact factor (which we are using as an exemplar of metrics) is thus literally neither about the evaluation of a *specific* article nor about making evaluation fair and transparent by removing it from the arbitrariness of qualitative judgments: It is about *creating the conditions of possibility for a market*.

No matter how accurate available actuarial tables may be, one can hardly develop a life insurance industry without them. And even if they are statistically good, they are still very unlikely to accurately predict the exact date of death of Ted the baker around the corner (in the same way that even the best of impact factors is not going to correctly predict the impact of any specific article). But that’s not the point. It does not matter that, at best, the impact factor can only capture some features of a *population* of articles published in a given journal. The role of the JIF is spreading (despite some spirited opposition)⁸ because it produces prices and a currency in which those priced goods can be exchanged and

circulated between authors, universities, libraries, and publishers. And, crucially, these prices can be determined at the time of publication, without having to wait to count the citations it will accrue in time. The journal impact factor shaves off the several years it would have taken for that article to grow its value, thus enabling more scholars—especially junior ones—to enter the market with something that looks like “hard value” rather than what they mostly have, that is, possible value in the future.

Emergent Objects, Emergent Manipulations

This shows, yet again, that post-production manipulations like those aimed at impact factors are qualitatively distinct from traditional misconduct, making it difficult to define what kind of misconduct they are, and whether misconduct is indeed the right term. They are not about manipulating knowledge claims but their institutional valuation. This means that, unlike traditional misconduct that has been boiled down to three practices—falsification, fabrication, and plagiarism—post-production manipulations can take up as many forms as there are metrics techniques and markets, which are constantly changing. The dramatic innovations in post-production manipulations that have emerged over the last few years support this view, while also suggesting that post-production manipulation may not be an object stable enough to be definable. New metrics and indicators are being constantly introduced or modified often in response to the perceived need to adjust or improve their accuracy and fairness. They carry the seed of their never-ending tuning and hacking, as each new metric or new tuning of an old one can be subsequently manipulated in different ways. Also, new “markets” and uses keep developing for existing metrics, which means that new categories of actors can get in the game, manipulating the metrics in different directions in response to specific goals. Metrics of student performance evaluation may become, for instance, tools to evaluate the performance of teachers, or of an entire school district. Closer to home, we know (as Csiszar’s chapter shows) that scientometrics emerged as a tool for mapping scientists’ works and networks, but was then turned into a tool for evaluating them. Similarly, the JIF was meant to evaluate journals, but has become the premier tool to assess the value of articles. The techniques and indicators may remain the same, but the changing significance of the outcome can be enough to create a market for new manipulations.

In sum, we do not yet have a new concept that can capture all the various manifestations of post-production misconduct, and it is not likely that

we will develop one anytime soon. All we know is that all these forms share the same *telos*: the manipulation of the metrics of academic evaluation. The means take many different forms, but the ends stay the same.

Ways of Gaming

Unlike traditional scientific fraud, post-production manipulation is no longer the purview of individuals but rather involves groups, networks, or entire institutions. Journal editors conspire to increase their publication's JIF through co-citation agreements among journals, authors organize themselves into fake peer-review rings, and editorial service providers not only help scientists write their articles in good English, but also, for an extra fee, help to line up friendly peer reviews. Up the institutional ladder, universities select or massage their data to score well on global university rankings (whose importance is growing with the increasing global scale of the higher education market).

Post-production manipulation has not only become a more collaborative effort, but it has also moved beyond the sites of traditional fraud like universities, corporate laboratories, and federal research institutions. It has spread from places where research originates to places where it goes to, like journals (especially those which librarian Jeffrey Beall and others termed “predatory journals”⁹) and “fake” conferences in vacation destinations that may use impressive-looking but possibly fake advisory committees while promising to publish the papers' abstracts (likely accepted without review) in journals that would probably fall in the “predatory” category (Brooks, 2009). You went on vacation (possibly paid for by your grant or research funds) and came back to find your vitae enriched by one additional conference talk and a publication.

Traditionally, journals have been cast in the role of gatekeepers, credited with the ability to sort good science from bad through peer review. Today, however, we see so-called “predatory journals” actively contributing to the post-production manipulation trend. While criticized mostly for their virtual freedom from the constraints of peer review, for their pay-to-play business model, and for their tendency to have fictional editorial boards (Morgenstern, this volume, chapter 15), we find the emergence of this breed of journals particularly interesting as a window on the logic of the new metrics-based regime of science publications. While light-years away from high impact journals like *Science*, *Nature*, or *Cell*, these “predatory journals” may be simply the other side, or perhaps the bottom, of the *same* metrics economy. Their impact factor is often insignificant

(or possibly made up), and yet they seem to provide a crucial service—possibly even a lifeline—to authors who are struggling to meet the quantitative publication benchmarks set by their institutions, or their strict deadlines.

These authors' institutions may be neither high ranking nor particularly ambitious, but they can hardly ignore the ranking game, which has become global and played at all levels of the “excellence” spectrum. As a result, these universities may still demand their faculty to publish a certain number of publications in nonlocal journals, that is, “international” English-language venues (de Rijcke and Stöckelová, this volume, chapter 7). The content of the article may not be crucial if the author can at least appear to be productive and able to publish in English—a fact that his/her department chair and dean can turn into a figure they can use as they pitch the steps—however modest they may be—that they are taking toward leading their institutions on the long path to excellence.

What these journals produce, therefore, are not publications but *publication effects* or *publication tokens*. More objects than texts, these publications (if publication is indeed the proper term here) are not meant to be read, but are rather generated for the sole purpose of allowing the authors to add an entry to their curriculum vitae, or for their deans to tally and include them in their annual reports to the higher administration or to prospective donors.¹⁰ These publications and journals may be virtually impact free, but they are not outside of the metrics-based economy of impact. They are simply at the bottom of that economy and, no matter the scorn they receive, no economy can function without a bottom.

The rise of “impact or perish” has also been accompanied by a marked increase in journal self-citation. In some cases, editors pursue this by asking prospective authors (especially junior ones) to cite other articles from the journal they have submitted their articles to (Wilhite and Fong, 2012). Clarivate Analytics (formerly Thomson Reuters) tracks self-citation and bans journals whose self-citation is deemed excessive. For instance, after the *Journal of Biomolecular Structural Dynamics*'s impact factor spiked from 1.1 to 5.0 in just one year between 2009 and 2010 (Van Noorden, 2012), Thomson Reuters asked the journal to explain its success. *JBSD*'s editor-in-chief attributed the journal's sharp rise in impact factor to their publication of a controversial paper, which generated many responses, and to a new policy encouraging authors to explain their work's connection to other articles previously published in the journal (Van Noorde, 2012). Unconvinced, Thomson Reuters excluded the journal from its *Journal Citation Reports* (the annually updated list of the impact factors

of the journals tracked in the Web of Science database), effectively denying them a ranking. But while such bans are becoming increasingly common (rising from nine in 2007 to thirty-eight in 2013), some believe they should be imposed more frequently given that even Thomson Reuters's "own statistics indicate that 140 journals have had self-citations making up more than seventy percent of total citations in the past two years" (Van Noorden, 2012).

If self-citation is easy to track, citation cartels between journals are significantly more difficult "to detect since they represent coordinated efforts among several journals to collectively self-cite."¹¹ Perhaps emboldened by this gap in the available technologies of detection, in 2009, the editors of eight Brazilian science journals decided to boost their impact by agreeing to publish "articles containing hundreds of references to papers in each others' journals" (Van Noorden, 2013). The scheme worked well for a few years, until 2013, when Thomson Reuters developed an algorithm to detect citation rings by spotting "concentrated bursts of citations from one journal to another."¹² This led them to suspend fourteen journals from the *Journal Citation Reports*, including four members of the Brazilian citation ring.¹³ One of the editors explained that:

The citation ring grew out of frustration with his country's fixation on impact factor. In Brazil, an agency in the education ministry, called CAPES, evaluates graduate programs in part by the impact factors of the journals in which students publish research. As emerging Brazilian journals are in the lowest ranks, few graduates want to publish in them. This vicious cycle, in his view, prevents local journals from improving. (Van Noorden, 2013)

Another member of the ring—the editor of the *Jornal Brasileiro de Pneumologia*—emphasized a link between the impact factor and the global politics of publication: the scheme to boost the journals' impact factor was not only self-serving but "also to show off articles in Brazilian journals, attracting better contributions and raising quality all round" (Van Noorden, 2013). Whether self-serving or patriotic, the scheme was surely incentivized and made possible by the increasing global "hegemony" of the impact factor (Barbour and Stell, this volume, chapter 11).

Some scholars who wish to bypass the constraint of having to either write papers or plagiarize them from other scholars opt to use SCIGen—an article-generating software. This is rather ironic, or worse, given that the developers of SCIGen—three graduate students at MIT—created this software as a way to expose, rather than contribute to, unethical evaluation and publication practices. Tired with the invitations to spurious conferences and journals that clogged their inboxes, they created SCIGen

to generate nonsensical (but legitimate-looking) submissions, which they then fed to “fake” conferences like the capaciously titled “World Multi-conference on Systemics, Cybernetics, and Informatics.” Their goal was to expose the fact that such conferences accepted any paper that came their way, without review, provided the presenter was willing to pay the registration fee. But, in a move that ran diametrically counter to their critical and humorous intent, the software was quickly co-opted by scientists who started to use it for real, effortlessly cranking out papers that they then humorlessly submitted to conferences—conferences that, as the SCIgen team had suspected, did accept and subsequently publish them (Antkare, this volume, chapter 14). (This is yet another example of how these new forms of manipulation seem to expand by repurposing tools to enable new opportunities, and so on.)

SCIgen-generated texts traveled far and wide. After creating software to detect such articles, Cyril Labbé (Lippman, this volume, chapter 21) identified and catalogued scores of computer-generated papers published in the proceedings of respectable conferences, not just the spam-like ones that the original SCIgen pranksters wanted to make fun of. For instance, Springer and the Institute of Electrical and Electronic Engineers (IEEE) had to retract more than 120 papers they had previously accepted and published (Van Noorden, 2014; Bohannon, 2015). Following the embarrassment, Springer enlisted Labbé and his laboratory to develop SciDetect, an open-source software to “ensure that unfair methods and quick cheats do not go unnoticed.”¹⁴ (Parenthetically, two years ago, a team at the University of Trieste introduced software to produce fake peer reviews. Like their SCIgen brethren, they mean it as a prankish tool to expose the problems of peer review, but only time will tell if it will also be used for real.¹⁵)

Peer reviews too can be manipulated in various ways. The website Retraction Watch (Oransky, this volume, chapter 10) has reported many cases—more than six hundred papers so far¹⁶—of rigged peer review. These are cases in which authors submitted email addresses of suggested reviewers that were in fact registered to the authors themselves. When the journals took up the suggested reviewers (which happened more often than one would expect), the authors received an email inviting them to review their own papers, which they typically found promising and publishable with a few revisions. In more sophisticated schemes, authors added citation rings to these rigged peer reviews (Ferguson et al., 2014). Finally, the Committee on Publication Ethics (COPE) has found that some organizations sell services “ranging from authorship of prewritten manuscripts to providing fabricated contact details for peer reviewers

during the submission process and then supplying reviews from these fabricated addresses.”¹⁷

The rising importance of the impact factor has also created a market for fake ones, which are particularly attractive to low-quality journals trying to look better than they are. Beall warned that, “in this competitive market, publishers want to stand out from the crowd and attract the author fees. One way to effectively earn these fees is to boast high journal rankings”¹⁸—rankings that one can buy pretty much off the shelf. Many of the “tailored” impact factor providers mimic legitimate scientometrics services through similar-sounding names and website domains. For instance, the shamelessly counterfeit “Thomson Reuters Institute for Scientific Information” ran the website www.isi-thomsonreuters.com (as distinct from the original www.thomsonreuters.com) and claimed to be the actual ISI (the original scientometrics company). Other equally creative citation companies supplying impact factors to so-called predatory journals include Universal Impact Factor (UIF), Global Impact Factor (GIF), and Citefactor (Jalalian, 2015). By providing impact factors for established journals such as *PLoS* and *Nature* along with less-reputable journals, fake impact factor companies contribute to the appearance of legitimacy within the ecosystem of post-production manipulations.

Finally, while universities use metrics to evaluate their faculty, they are subjected, in different contexts, to some of those same metrics (Espeland and Sauder, 2016). Several global university rankings, for instance, consider the faculty’s citation counts. In recent years, universities have sought to raise their rankings by targeting the very indexes by which they are ranked, and hiring consultants to identify exactly how to do that. Northeastern University provides one of the most successful examples of how to target the *U.S. News and World Report* rankings. Already in 1996, former Northeastern University President Richard Freeland observed “how schools ranked highly received increased visibility and prestige, stronger applicants, more alumni giving, and, most important, greater revenue potential. A low rank left a university scrambling for money. This single list... had the power to make or break a school” (Kutner, 2014). Following this insight, Freeland identified precisely what Northeastern would need to target—class size, graduation rate, admission statistics, and so on—to rise in the rankings and break into the top 100. The effort paid off, and then some. From its score of 162 in 1996, Northeastern rose to 98 in 2006, and to 44 in 2018. This is by no means an isolated case. Other universities’ tactics for swaying rankings have included hiring top-cited faculty as well-paid, part-time, affiliated, nonresident faculty,¹⁹

inflating students' SAT scores, high school GPAs, and graduation rates, and refunding poorly performing freshman students (or, in Yale's case, first-year law school students) their tuition after their first semester if they are willing to drop out.

Fake Is No Longer What It Used to Be

We find it intriguing that the results of post-production manipulations developed to meet or exceed the performance benchmarks established by institutions, funding agencies, or global university rankings cannot be easily categorized as “fake” in the same way that traditional misconduct could be said to involve fake evidence or fake authorship. For lack of a better term, we may use “spammish” to point to a tension inherent in some post-production manipulations that is not captured by terms such as “fake,” “bogus,” or “predatory.”

No doubt, there are differences between a traditional conference and a for-profit event organized largely for the purpose of having scientists pay to deliver papers and have them printed in some obscure journal (with possible ties to the conference organizers) (Grant, 2016; Straumsheim, 2016). Still, it would be inaccurate to say that the latter is simply a “fake” version of the former. Similarly, obscure journals whose editors clog our inboxes with offers to publish next week what we submit by this Friday are definitely suspect, but not merely “fake.” They surely engage in misrepresentations (as when they boast stringent peer-review standards), but they mostly withhold information from their prospective authors, creating ambiguities that play in their favor. They are still academic journals, some of them listed in the standard indexes (though often in fewer than the ones they boast). And they do indeed publish articles, some of which get cited. Occasionally, some of these journals are bought up by prime-time publishers, suggesting that they may be perceived as “emergent” rather than simply “fake.” (Unless of course you think that the big publishers buy up these journals simply because they are profitable, without worrying too much about their publishing ethics).

Similarly, while some of the scholars whose names grace their editorial boards may not be aware of being listed there (see Morgenstern, this volume, chapter 15), that does not mean that “fake” fully describes those boards. In some cases, the editorial boards are indeed made up, but, in others, advisors agree to have their names listed, perhaps because they do not understand the nature of those journals, or because they want to

support affordable open-access publishing.²⁰ In other instances, however, they may decide not to ask too many questions and simply accept the invitation so as to add another line to their vitae.

Again, while there is a certain fakeness about these journals, to say that they are plainly “fake” or “predatory” misses the complexity of the ways in which they are both fake and not fake, and the fact that such ambiguities are central to their role and business model. While it is difficult to find kind things to say about these journals, their relentless vilification as illegitimate, fraudulent, and rapacious looks like othering—an index of how the publishing professionals of the Global North use these journals as a foil to project a desirable identity and image of themselves. (As some of the chapters argue, these “predatory” journals may be in fact a blessing in disguise for the more established publishers by providing a “bad” benchmark against which they can strut their quality.) Furthermore, while the generic and random nature of the invitations we receive to publish in these journals, the wildly capacious assemblages of disciplines that are conjured in their titles, or the impressively fast publication time they boast seem as far fetched as the large sums of money that spam emails claim to be waiting for us in some remote bank account, these journals cannot be said to be truly “spam” or “predatory” either (Brunton, this volume, chapter 18). It would take a truly naïve scientist to believe that these publication venues belong in the same category and have the same credibility of the peer-reviewed journals in which they would rather publish.²¹ Equally naïve would be the belief that their submissions could be properly reviewed and published in a matter of days, or that the emails one receives from these editors (populated by strange typos and dubious academic links) are actually coming from those people and from those addresses. Given that academics are a reasonably intelligent bunch, those who choose to publish in these venues are not likely to be deceived into doing so. An attempt to deceive is surely involved here, but one too transparent to justify saying that those who accept those invitations are “preyed upon,” cheated out of the money they send to these journals for publication costs or for gold open-access fees (which, in any case, are a fraction of the going rates of more established journals).

“Spammish” may be a better term to capture the ambiguous nature of these practices that, while appearing spam-like, are to some extent collusive. It may be to the authors’ advantage to treat publications in these suspicious journals or attendance to these suspicious conferences as perfectly legitimate and worthy of inclusion in one’s vitae, only to say that

they got tricked into publishing in these journals or going to those conferences in case their university questions their choices. Neither “real” nor “fake,” these practices are effective precisely because they can be played and presented differently, depending on the circumstances (Jacob, this volume, chapter 19).

New Evidence, New Watchdogs

It should not come as a surprise that modes of misconduct detection have changed with the emergence of new metrics-based post-production manipulations, and the changing notions of fakeness that go with them. While some of these manipulations may still be detected through peer review, most of them are beyond the reach of traditional referees. In some cases, peer review is unable to function as a gatekeeper precisely because it becomes itself the target of those manipulations. (For instance, peer review and citation rings function by rigging peer review, that is, by replacing legitimate reviews with counterfeit ones.) Some of these new manipulations, therefore, can be detected not through careful reading of a manuscript, but only through extensive analysis of journal databases—of the wording of reviews, review turnaround times, citation patterns, and the mutual relationships between authors and reviewers across different publications.²²

This requires both a different kind of expertise as well as access to different levels of evidence and data. Much of this evidence, in fact, can be mined only by teams of investigators, hired by the publishers, carefully poring over information held in proprietary journal databases. And as shown by the terseness and brevity of most retraction notices, editors and publishers are often reticent to expose how their editorial processes have been gamed, as that exposes weaknesses in their systems and services (Biagioli, 2016). Less than ten years ago, a now-prominent misconduct researcher contacting a journal about an uninformative retraction notice was told that, “it’s none of your damn business”—a kind of answer that some editors still relay today (when they respond, which they do less than half of the time).²³ We are seeing, in sum, both an increase in the amount of forensic traces of misconduct as well as the decrease of the readability of those traces, which are now often beyond the reach of traditional peer review. This “privatization” of forensic evidence and the proliferation of its forms have been paralleled, however, by an opposite trend: the crowdsourcing of the discussion and analysis of evidence of potential misconduct.

The emergence and pervasiveness of new forms of misconduct exceeds the reach, resources, and conceptual framework of traditional governmental watchdog organizations like the Office of Research Integrity in the United States—agencies that are already undergoing some identity crisis (Kaiser, 2016). But these institutional bodies are no longer the sole players. Noninstitutionally affiliated watchdog organizations have emerged, like Retraction Watch, PubPeer, blogs such as Scholarly Open Access, and other sites like the now-defunct Science Fraud (Pain, 2014; Aschwandan, 2015; Blatt, 2015). This new generation of watchdogs is successfully making up for their lack of resources by mobilizing hundreds of scientists—some named, but mostly anonymous—who are willing to read texts, evaluate images, run through statistical analyses of a publication's data, and share their findings and views on websites, blogs, wikis, and social media. As Eric Raymond famously said about open-source software collaborative practices, “given enough eyeballs, all bugs are shallow” (Raymond, 1999). And though they lack legal authority, these new watchdogs can be very effective through their ability to maximize the visibility of these issues, which may force the authorities to intervene (Guaspere and Didier, this volume, chapter 12). Interestingly, all these efforts have been moving in the opposite direction from the forms of evaluation characterized by the adoption of the impact factor and other metrics. Rather than looking at metadata, these watchdogs engage in careful reading of the content of the publication—data, images, text—and sometime attempt to reproduce the claims. Their *modus operandi* is that of traditional peer review but, through the adoption of a crowdsourcing model, it operates on a scale and is able to draw expertise from a population that is an order of magnitude larger than that of traditional peer review as practiced by journals. This change of scale has a direct impact on the granularity of the review, but it also profoundly shifts the “governance” of misconduct research from governments and institutions to the practitioners themselves.

And as these developments mark a transformation from top-down to bottom-up knowledge production, they are often accompanied by an affect that is rather unusual in academia: humor. There is a clear migration (Lippman, this volume, chapter 21) away from the high seriousness and humorlessness of the discourse of university committees and governmental agencies such as the ORI, and toward the carnivalesque attitude of some of the new watchdogs. Wearing the masks of anonymity and pseudonymity, and cracking jokes (some better than others), they blur the line between “policemen” and “pranksters.”

Notes

1. In some cases, we can empirically see the shift from one to the other. In places where the number of publications is the top target, people publish much but not in particularly high-impact journals. Instead, where institutions tie rewards to publications in top-tier journals rather than to sheer quantity, authors adjust by publishing less but in higher impact journals.
2. “Goodhart’s law” is named after Charles Goodhart who formulated it in a paper from 1975. For an example of an analysis of “Goodhart’s law,” see Marilyn Strathern, “‘Improving Ratings’: Audit in the British University System,” *European Review* 5(1997):305–321. The adage is also sometimes referred to as “Campbell’s law” from Donald T. Campbell’s 1975 paper, “*Assessing the Impact of Planned Social Change*” or the “Lucas Critique,” which economist Robert Lucas articulated in “*Econometric Policy Evaluation: A Critique*” (1976). The relationship between “Goodhart’s law” and the “Lucas Critique” is discussed in Alec Chrystal and Paul Mizen, “Goodhart’s Law: Its Origins, Meaning and Implications for Monetary Policy,” in Paul Mizen, ed., *Central Banking, Monetary Theory and Practice*, Vol. I (Cheltenham: Edward Elgar Publishing, 2003), pp. 221–243.
3. The Office of Research Integrity, “Definition of Research Misconduct,” Accessed September 1, 2016, <http://ori.hhs.gov/definition-misconduct>.
4. Phil Davis, “Paying for Impact: Does the Chinese Model Make Sense?,” *Scholarly Kitchen*, April 7, 2011, Accessed August 1, 2016, <https://scholarlykitchen.sspnet.org/2011/04/07/paying-for-impact-does-the-chinese-model-make-sense>. Jufang Shao and Huiyun Shen, “The Outflow of Academic Papers from China: Why Is It Happening and Can It Be Stemmed?,” *Learned Publishing* 24(2011):95–97.
5. This analogy brings up interesting questions about the difference between printing (money) and publishing knowledge, and how the JIF is effectively narrowing that difference, if not blurring it outright.
6. Encouraging scholars to submit to high-impact, international journals also harms the development of high-quality, higher impact Chinese journals according to Wang Shuhua and Paul Weldon, “Chinese Academic Journals: Quality, Issues, and Solutions,” *Learned Publishing* 19(2006):97–105. See also Xu Jie and Matthias Wahls, “The Scholarly Publishing Industry in China: Overview and Opportunities,” *Learned Publishing* 25 (2012):63–74.
7. Alison McCook, “7 Signs a Scientific Paper’s Authorship Was Bought,” *Retraction Watch*, October 10, 2016, Accessed October 27, 2016, <http://retractionwatch.com/2016/10/24/seven-signs-a-paper-was-for-sale/>. Mara Hvistendahl, “China’s Publication Bazaar,” *Science* 342(6162):1035.
8. See, for instance, Phil Davies, “On Moose and Medians (or Why We Are Stuck with the Impact Factor),” *Scholarly Kitchen*, April 12, 2016, Accessed August 15, 2018, <https://scholarlykitchen.sspnet.org/2016/04/12/on-moose-and-medians-or-why-we-are-stuck-with-the-impact-factor>.

9. For years, Beall has maintained an online blacklist (now removed) and a widely read and sometimes-critiqued blog, Scholarly Open Access. See for instance, Monica Berger and Jill Cirasella, “Beyond Beall’s List: We Need a Better Understanding of Predatory Publishing Without Overstating Its Size and Danger,” *The Impact Blog*, March 18, 2015, Accessed June 26, 2016, <http://blogs.lse.ac.uk/impactofsocialsciences/2015/03/18/beyond-bealls-list-predatory-publishers>.
10. Mario Biagioli, “Recycling Texts or Stealing Time? Plagiarism, Authorship, and Credit in Science,” *International Journal of Cultural Property* 19(2012):463–464.
11. Phil Davis, “Citation Cartel Journals Denied 2011 Impact Factor,” *Scholarly Kitchen*, June 29, 2012, Accessed June 15, 2016, <https://scholarlykitchen.sspnet.org/2012/06/29/citation-cartel-journals-denied-2011-impact-factor>. On recent developments toward detecting citation cartels, see Iztok Fister et al., “Toward the Discovery of Citation Cartels in Citation Networks,” *Frontiers in Physics*, December 15, 2016, Accessed 8/1/2018, <http://journal.frontiersin.org/article/10.3389/fphy.2016.00049/full>.
12. *Ibid.*
13. *The Journal Citation Reports*. Accessed June 1, 2016, http://adminapps.webofknowledge.com/JCR/static_html/notices/notices.htm.
14. Renate Bayaz, “Springer and Université Joseph Fourier Release SciDetect to Discover Fake Scientific Papers,” March 23, 2015, Accessed August 15, 2018, <https://www.springer.com/gp/about-springer/media/press-releases/corporate/scidetector/54166>.
15. Jack Grove, “Robot-Written Peer Reviews,” *Inside Higher Ed*, September 22, 2016, Accessed September 23, 2016, https://www.insidehighered.com/news/2016/09/22/many-academics-are-fooled-robot-written-peer-reviews#.V-Qd_s9vpF0.gmail.
16. Allison McCook, “A Publisher Just Retracted Ten Papers Whose Peer Review Was ‘Engineered—,’” *Retraction Watch*, July 12, 2018, Accessed August 14, 2018, <https://retractionwatch.com/2018/07/12/publisher-has-known-of-problem-of-fake-reviews-for-years-so-how-did-10-papers-slip-its-notice/>.
17. “COPE Statement on Inappropriate Manipulation of Peer Review Processes,” December 19, 2014, Accessed June 20, 2016, <http://publicationethics.org/news/cope-statement-inappropriate-manipulation-peer-review-processes>. Mara Hvistendahl has also reported on “China’s Publication Bazaar” in which agencies, scientists, and editors sell papers indexed by Thomson Reuters and Elsevier in “China’s Publication Bazaar,” *Science* 342(6162):1035–1039.
18. Jeffrey Beall, “Look Out for Bogus Impact Factor Companies,” *Scholarly Open Access*, Accessed July 1, 2016, <https://scholarlyoa.com/2013/08/06/bogus-impact-factor-companies>. See also Mehrdad Jalalian, “The Story of Fake Impact Factor Companies and How We Detected Them,” *Electronic Physician* 7(2):1069–1072.

19. Lior Pachter wrote about how unknown King Abdulaziz University hired top-cited mathematicians as “distinguished adjunct professors” and rose to number 7 in USNWR’s global university ranking by subject. Accessed July 1, 2016, <https://liorpachter.wordpress.com/2014/10/31/to-some-a-citation-is-worth-3-per-year>.
20. Jeffrey Beall, “Bogus Polish Journal Has Completely Fake Editorial Board,” *Scholarly Open Access*, Accessed June 20, 2016, <https://scholarlyoa.com/2016/02/02/bogus-polish-journal-has-completely-fake-editorial-board/>. See also Piotr Sorokowski et al., “Predatory Journals Recruit Fake Editor,” *Nature*, March 23, 2017.
21. We therefore disagree both with the use of the term “predatory” and with the logic of predation that is actually mobilized in some of the literature on these practices, like Alexander M. Clark and David R. Thompson, “Five (Bad) Reasons to Publish Your Research in Predatory Journals,” *JAN* 73(2017):2499–2501.
22. The complexity of the forensic analysis and the proprietary nature of the sources is exemplified by an early case of fake peer reviews, which “sparked a 14-month investigation that came to involve about 20 people from SAGE’s editorial, legal and production departments. It showed that the Gmail addresses were each linked to accounts with Thomson Reuters’ ScholarOne, a publication-management system used by [various] publishers.... Editors were able to track every paper that the person or people behind these accounts had allegedly written or reviewed.... As they worked through the list, SAGE investigators realized that authors were both reviewing and citing each other at an anomalous rate.” Cat Ferguson, Adam Marcus, and Ivan Oransky, “Publishing: The Peer-Review Scam,” *Nature*, 515:480–482, doi:10.1038/515480a.
23. Adam Marcus, “Journal Editors Still Don’t Like Talking about Misconduct—And That’s a Problem,” *Retraction Watch*, August 2, 2018, Accessed August 14, 2018, <https://retractionwatch.com/2018/08/02/journal-editors-still-dont-like-talking-about-misconduct-and-thats-a-problem/>.

References

- Aschwandan, Christie. 2015. “Science Isn’t Broken.” *FiveThirtyEight*. <https://fivethirtyeight.com/features/science-isnt-broken/#part4>.
- Beall, Jeffrey. 2016. “Bogus Polish Journal Has Completely Fake Editorial Board.” *Scholarly Open Access*. <https://scholarlyoa.com/2016/02/02/bogus-polish-journal-has-completely-fake-editorial-board/>.
- Beall, Jeffrey. 2016. “Look Out for Bogus Impact Factor Companies.” *Scholarly Open Access*. <https://scholarlyoa.com/2013/08/06/bogus-impact-factor-companies>.
- Biagioli, Mario. 2016. “Watch Out for Cheats in Citation Game.” *Nature* 535(7611):201. doi:10.1038/535201a.
- Biagioli, Mario. 2018. “Quality to Impact, Text to Metadata: Evaluation and Publication in the Age of Metrics.” *KNOW* 2(2):1–27.

- Biagioli, Mario, Martin Kenney, Ben Martin, and John Walsh. 2019. "Academic Misconduct, Misrepresentation, and Gaming." *Research Policy* 48: 401–413.
- Bik, Elisabeth M., Arturo Casadevall, and Ferric C. Fang. 2016. "The Prevalence of Inappropriate Image Duplication in Biomedical Research Publications." *bioRxiv*; 049452v2. <http://biorxiv.org/lookup/doi/10.1101/049452>. May 17, 2016.
- Blatt, Michael R. 2015. "Vigilante Science." *Plant Physiology* 169(2):907–909.
- Bohannon, John. 2013. "Who's Afraid of Peer Review?" *Science* 342(6154):60–65.
- Bohannon, John. 2015. "Hoax-Detecting Software Spots Fake Papers" *Science* 348(6230):18–19.
- Brooks, Margaret. 2009. "Red-Flag Conferences." *The Chronicle of Higher Education*. March 26, 2009.
- Brunton, Finn. 2013. *Spam: A Shadow History of the Internet*. Cambridge, MA: MIT Press.
- Burrows, Roger. 2012. "Living with the H-Index? Metric Assemblages in the Contemporary Academy." *The Sociological Review* 60(2):355–372.
- Campbell, Donald T. 1979 (1975). "Assessing the Impact of Planned Social Change." *Evaluation and Program Planning* 2(1):67–90. doi:10.1016/0149-7189(79)90048-X.
- Clark, Alexander M., and David R. Thompson. 2016. "Five (Bad) Reasons to Publish Your Research in Predatory Journals." *Journal of Advanced Nursing*. August. doi:10.1111/jan.13090.
- Csiszar, Alex. 2018. *The Scientific Journal: Authorship and the Politics of Knowledge in the Nineteenth Century*. Chicago: University of Chicago Press.
- Ferguson, Cat, Adam Marcus, and Ivan Oransky. 2014. "Publishing: The Peer-Review Scam." *Nature* 515(7528):480–82. doi:10.1038/515480a.
- Gingras, Yves. 2016. *Bibliometrics and Research Evaluation: Uses and Misuses*. Cambridge, MA: MIT Press.
- Grant, Bob. 2016. "US Gov't Takes On Predatory Publishers." *The Scientist*. August 29.
- Grove, Jack. 2016. "Robot-Written Peer Reviews." *Inside Higher Ed.*. September 22. https://www.insidehighered.com/news/2016/09/22/many-academics-are-fooled-robot-written-peer-reviews#.V-Qd_s9vpF0.gmail.
- Hvistendahl, Mara. 2013. "China's Publication Bazaar." *Science* 342(6162):1035. doi:10.1126/science.342.6162.1035.
- Jacob, Marie-Andrée. 2014. "Misconduct Hunting: Research integrity via Law Science and Technology," in E. Cloatre and M. Pickersgill, eds, *Knowledge, Technology and Law*, 137–153. London: Routledge.
- Jalalian, Mehrdad. 2015. "The Story of Fake Impact Factor Companies and How We Detected Them." *Electronic Physician* 7(2):1069–1072. <http://doi.org/10.14661/2015.1069-1072>.
- Jie, Xu, and Matthias Wahls. 2012. "The Scholarly Publishing Industry in China: Overview and Opportunities." *Learned Publishing* 25:63–74.

Kaiser, Jocelyn. 2016. "New Leader of NIH's Research Watchdog Faces Staff Revolt." *Science*. August. doi:10.1126/science.aah7226.

Kutner, Max. 2014. "How to Game the College Rankings." *Boston Magazine*. August 14.

Lippman, Alexandra, and Christopher Kelty. 2019. "In a Race for Mentions, It's Open Season on Researchers." *Nature Index*. January 18. <https://www.natureindex.com/news-blog/in-a-race-for-mentions-its-open-season-on-researchers>.

Lock, Stephen, and Frank Wells. 2001. *Fraud and Misconduct in Biomedical Research*. Hoboken, NJ: Wiley-Blackwell.

Lucas, R. E. 1976. "Econometric Policy Evaluation: A Critique," in R. E. Lucas, ed, *Studies in Business-Cycle Theory*, 104–130. Cambridge, MA: MIT Press.

Macdonald, Stuart, and Jacqueline Kam. 2007. "Ring a Ring O? Roses: Quality Journals and Gamesmanship in Management Studies." *Journal of Management Studies* 44(4):640–655. doi:10.1111/j.1467-6486.2007.00704.x.

Merry, Sally Engle. 2016. *The Seductions of Quantification: Measuring Human Rights, Gender Violence, and Sex Trafficking*. Chicago: University of Chicago Press.

Pain, Elisabeth. 2014. "Paul Brookes: Surviving as an Outed Whistleblower." *Science*. March. doi:10.1126/science.caredit.a1400061.

Power, Michael. 1997. *Audit Society: Rituals of Verification*. Oxford: Oxford University Press.

PLoS Medicine Editors. 2006. "The Impact Factor Game. It Is Time to Find a Better Way to Assess the Scientific Literature." *PLoS Med* 3(6):e291.

Raymond, Eric R. 1999. *The Cathedral and the Bazaar: Musings on Linux and Open Source by an Accidental Revolutionary*. Sebastopol, CA: O'Reilly.

Rossner, Mike, and Kenneth M. Yamada. 2004. "What's in a Picture? The Temptation of Image Manipulation." *The Journal of Cell Biology* 166(1):11–15. doi:10.1083/jcb.200406019.

Shao, Jufang, and Huiyun Shen. 2011. "The Outflow of Academic Papers from China: Why Is It happening and Can It Be Stemmed?" *Learned Publishing* 24:95–97.

Shuhua, Wang, and Paul Weldon. 2006. "Chinese Academic Journals: Quality, Issues, and Solutions." *Learned Publishing* 19:97–105.

Sorokowski, Piotr et al. 2017. "Predatory Journals Recruit Fake Editor." *Nature*. March 23.

Steneck, Nicholas. 2004. *ORI Introduction to the Responsible Conduct of Research*. Washington, DC: Office for Research Integrity.

Strathern, Marilyn. 1997. "'Improving Ratings': Audit in the British University System." *European Review* 5:305–321.

Strathern, Marilyn, Editor. 2000. *Audit Cultures: Anthropological Studies in Accountability, Ethics, and the Academy*. London: Routledge.

Straumsheim, Carl. 2016. "Feds Target 'Predatory' Publishers." *Inside Higher Ed.* August 29. <https://www.insidehighered.com/news/2016/08/29/federal-trade-commission-begins-crack-down-predatory-publishers>.

Van Noorden, Richard. 2012. "Record Number of Journals Banned for Boosting Impact Factor with Self-Citations." *Nature*. June 29.

Van Noorden, Richard. 2013. "Brazilian Citation Scheme Outed." *Nature*. August 27.

Van Noorden, Richard. 2014. "Publishers Withdraw More Than 120 Gibberish Papers." *Nature*. February 24. <http://www.nature.com/news/publishers-withdraw-more-than-120-gibberish-papers-1.14763>.

West, Jevin D., Carl T. Bergstrom, and Ted C. Bergstrom. 2010. "The Eigenfactor Metrics: A Network Approach to Assessing Scholarly Journals." *College & Research Libraries* 71(3):236–244.

Wilhite, Allen, and Eric A. Fong. 2012. "Coercive Citation in Academic Publishing." *Science* 335(6068):542–543. doi:10.1126/science.1212540.

