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The Transformation of the Scientific Paper: From Knowledge to Accounting Unit

Yves Gingras

Since the mid-1990s, observers and actors in the scientific field—as defined in Bourdieu (1986, 2004) as a structured space of agents and institutions in competition for the accumulation of credit or "symbolic capital"—have commented on the many facets of an ongoing major transformation in the structural conditions of scientific practice: massification of research, mounting pressure to publish, relative decline of government investments, and the arrival into the research system of the ideology of "knowledge management" with its insistence on quantitative evaluation measures of productivity and "impact" of academic research (Bruneau and Savage, 2002). As discussed by many contributions to this book, these transformations of the research system led actors to respond with various strategies of "gaming" the system through manipulating the metrics to attain the required results or, as suggested by Griesemer in his contribution, to try to derail the whole enterprise of metrics, though it is not clear how one could do that. By the end of the twentieth century, the technical infrastructure of journal publishing had also started to be radically transformed through the use of internet and electronic publishing (Thompson, 2005). These new technologies of communications made it possible to skip the materiality of the scientific paper and directly produce digital papers and journals that could then circulate much faster and globally on the internet, in turn contributing to transforming the dynamic of scientific practice. Finally, the mounting concentration of scientific journals in the hands of a limited number of giant publishing firms submitted to an increasing demand of profitability on the stock exchange, engendered in the mid-1990s what has been called a "crisis in scholarly publishing" due to the mounting price of journal subscription for academic libraries (Thatcher, 1995; Tenopir and King, 1997; McGuigan and Russell, 2008; Larivière, Haustein, and Mongeon, 2015). An answer to that crisis has been the emergence of the "open-access" movement (Laakso et al., 2011).

The combination of internet technology and the profitability of academic journals, which could now be born digitally, gave rise to the multiplication of specialist journals, many considered of dubious quality, trying to capture a part of the value generated in answering the "offer" of more papers being produced by researchers answering the "publish or perish" injunction from their institutions. Finally, internet diffusion of papers in open access made results more easily accessible to new nonspecialist audiences who could in return influence choices and priorities of research.

Each of these three major processes (the internet revolution, the economic transformation of journal publishing, and the evaluation turn) have generated specific effects. Many of them are analyzed in this book, but my thesis is that it is *their mutual interactions* in a kind of "perfect storm" that made possible the radical changes observed in the scientific field, which saw the rise of deviant behavior on the part of scientists and journal editors as well as managers of academic institutions. One may think here of the multiplication of "predatory journals," faked peer review, the manipulation of citations to raise impact factors of journals and positions in academic rankings, and the rise in the number of "corrections" to and "retraction" of scientific papers (Corbyn, 2009; Van Noorden, 2013; Haug, 2015; Beal, 2016), topics discussed below in the contributions collected in sections 2 and 3 of this book.

In this chapter, I propose a global macro-structural analysis that aims at showing how these different transformations have become connected as a consequence of the transformation of the paper from a *knowledge unit* to an *accounting unit* used to evaluate researchers and research organizations (departments, laboratories, and universities). The present structure of the scientific field is thus the result of the complex interrelations of different causal series that converged to change the social function of the scientific paper, making it fit with the twin ideologies of the New Knowledge Management and the Audit Society (Power, 2000; Wilson, 2002). In order to better understand the nature of these recent transformations, I must first present the basic social dynamic that propelled scientific research for more than three centuries. I limit myself to academic research and publications in academic journals and exclude research done for profit or to obtain patents, as this dynamic—though obviously more and more connected to that of the scientific field in response to declining government investments in "basic" research—obeys a different logic, namely that of profit. As Sismondo's contribution to this book shows, the production of papers by pharmaceutical companies is not an end in itself but only a means to market their products (pills and syrups …). By contrast, scientists' final product in the scientific field is the paper, supposed to contain new knowledge, hence my term "unit of knowledge."

The Classic Cycle of Accumulation of Symbolic Capital

The classical sociology of science developed by Robert K. Merton and his school has well established that symbolic recognition (or credit) lies at the basis of the dynamic of scientific research (Hagstrom, 1965; Cole and Cole, 1973; Merton, 1973). In exchange for making known to the scientific community, through publication, a "discovery" (new fact, instrument, interpretation or theory), the scientist obtains the symbolic recognition of his original work. Such a social recognition then gives better chances to access material and economic resources and institutional positions to make further research. This simple but powerful model has been extended by Pierre Bourdieu to take more explicitly into account the links and mutual transformation between recognition, now seen as a form of *symbolic capital,* and the other kinds of capital (economic, cultural, social) that are also at play in the scientific field (Bourdieu, 1986, 2004).

Figure 2.1 shows how that cycle of credibility, to use Latour and Woolgar's reformulation of Bourdieu's ideas, works (Latour and Woolgar, 1982). To simplify the figure, I exclude the cases when papers are rejected. In fact, the model predicts that too many rejections, which means lack of recognition by the community, will lead the affected scientist to abandon research and search for a *different kind* of recognition, often in a different social field. This cycle is premised on the fundamental but implicit idea that the scientific paper is the embodiment of what we can call a "unit of knowledge," that is any original contribution that is perceived as legitimate by the members of a particular scientific field. Generally, the "unit of knowledge" comes only after being peer reviewed and published in a journal, as its value and legitimacy comes from the social recognition and is not intrinsic to the submitted article. Depending on the value (large or small) of that unit of knowledge, which in some fields could even be lines of computer codes, the author then accrues more or less symbolic capital, in particular through being cited by other researchers. The citations accrued in the scientific field thus work as a *symbolic credit allocation mechanism*. The accumulation of symbolic capital facilitates, in turn, access to new resources (including grants, research assistants, and postdocs) for research and favorable institutional positions. With better resources, one can then have better instruments and thus raise the probability of producing better papers and larger "units of knowledge."

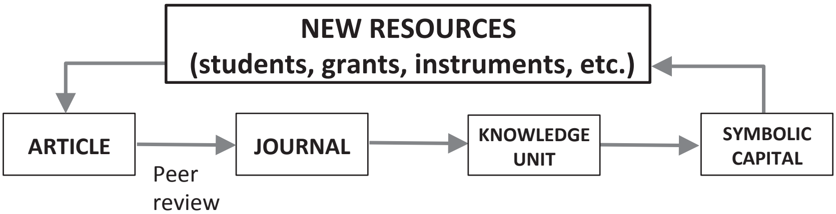

Figure 2.1
The classic cycle of knowledge production, evaluation, and accumulation of credit.

In this system, the obvious crucial step is "peer review," for it is at this very point in the process that the paper will exist or not as a socially recognized unit of knowledge circulating in the scientific field. Since its institutionalization in the middle of the seventeenth century, it has of course evolved in its detail (blind review or not, external or done by the editor), but retains its basic function of gate-keeping the entry into the scientific field (Zuckerman and Merton, 1971; Chubin and Hackett, 1990; Biagioli, 2002; Kronick, 2004; Pyenson, 2008). It could thus be expected that the mounting pressure on the research system since the mid-1970s would generate a mounting critique of peer review (Gustafson, 1975). By the end of the 1980s, the peer-review mechanism even became the object of systematic and continued analysis, particularly in the biomedical sciences, through an annual congress of peer review (Rennie, 2016). But despite all the critiques addressed to peer review, it remains central to the basic cycle of accumulation of scientific credit. When the scientific paper will have been transformed into a simple accounting unit used for the evaluation of researchers and their institutions, we will observe the emergence and growth of various more or less deviant behaviors trying to cope with the multiplications of metrics through manipulating them or even the peer-review process itself.

The Multifarious Effects of Research Evaluation

Though bibliometrics developed as a research specialty in the 1960s and 1970s studying the aggregate properties of publications and citations for the use of librarians and historians and sociologists of science, its methods became increasingly used, especially since the 1990s, to facilitate the evaluation of researchers (Gingras, 2016; Wouters's contribution to this book, chapter 4). This development was in line with the ideology of benchmarking and "knowledge management," which invaded the public sector in the 1980s and the universities in the 1990s (Bruno and Didier, 2013). As the tradition of qualitative peer review for promotion and tenure became more and more criticized in the mid-1970s for being "subjective," quantitative indicators based on counting papers started to be used as a measure of the "productivity" of researchers (Wade, 1975). Bibliometric methods thus provided the basic measures of "impact" used by promoters of "benchmarking" and "league tables" and other "rankings" supposed to evaluate researchers and journals as well as universities (Espeland and Sauder, 2016; Kehm's contribution to this book, chapter 6).

The move to digital versions of journals and papers would not in itself have transformed the traditional cycle of credit, for it just made papers accessible through the internet for those who were affiliated to institutions that subscribed to the journals. However, the pressure to publish "accounting units" for evaluation purposes triggered a series of important transformations, synthetized in figure 2.2, that gave rise to new actors and new technologies of measurement. An initial important change, though largely invisible to the scientists, is the very content of bibliometric databases. While the oldest of them, the Science Citation Index (SCI; now the Web of Science), at first coded only information on the first author of a paper (since that was sufficient to retrieve it in an information system), bibliometric databases now include the complete list of all authors of papers, because these data are now used for evaluation purposes and not only for making bibliographic searches. From the 1980s to the 2000s, the only direct measure of the "impact" of scientific papers was the number of citations by other papers as registered in the SCI database, which had a monopoly until the creation of the competing Scopus database by Elsevier in 2004.

A second effect of the new evaluation context, one visible to any active scientist, is that it offered an opportunity for companies to create new journals that could accept those papers. The multiplication of papers, related to the growth of the research system and the added competition for scarce resources, called in the creation of new journals. The creation of new journals has of course always been a part of the growth and diversification of research specialties since the emergence of the first journals in the 1660s (Peiffer and Vittu, 2008; Csiszar, 2018). But what is new is that the combined effect of internet publication and the emergence of "open-access journals," which transfer the cost from subscribing libraries to authors, has opened the door to what many scientists called "predatory journals," which, without the need of a large investment in infrastructure (as it was necessary with printed journals), could offer rapid publication to scientists struggling to find outlets for their research results (Grant, 2009a, 2009b; Beal, 2016). As for already well-established journals having a strong brand, like *Nature*, they could maximize their monetary value by creating sister journals, using their main brand as attractor. Thus, after having capitalized for more than a century off the value of a single multidisciplinary journal called *Nature*, the company understood that it could extract even more value from this brand by multiplying the titles associated with it. Since the 2000s, it has thus created more than thirty titles associated with the name *Nature*: *Nature Physics*, *Nature Chemistry*, and so on, betting on the aura effect of the original journal to attract the "best papers" from authors in search of maximum visibility for their research results. Property of the American Association for the Advancement of Science (AAAS), the journal *Science* has also adopted that strategy by creating a series of sister journals like *Science Robotics* and *Science Immunology*, for example.

Now, much as the number of "citations" had become the "objective" measure of the value of papers too numerous to be read carefully by evaluation committees, so the Journal Impact Factor has become the measure of the value of journals too numerous to be known to researchers and their evaluators. First published in 1975 as a measure of the visibility of a journal in the scientific field, it was aimed at librarians as a tool of collection management. Significantly, it only became an object of contention in the scientific field in the mid-1990s, when it had been transformed into a mechanical tool for evaluating *researchers* instead of *journals*. The main drawback of using "citations" to papers as a quantitative indicator of their "value" is that they take time to accrue, whereas "evaluation" thrives on "timeliness." Before citing a paper, one must first read it (or at least see a reference to it), then write a new paper citing it, make it pass through peer review, and then see it published. That means that, generally, it takes at least a few years to begin to be cited and about five to seven years (depending on the field) to reach a peak before observing declining citations as the knowledge becomes standard, what Merton called "obliteration by incorporation" (Merton, 1988; McCain, 2014). Faced with this problematic time delay, evaluators thus tended to replace citations by the more readily accessible Journal Impact Factor. Though it characterizes the *journal* and not the *paper*, it provides an immediate "proxy" measure of "quality" and "impact" that obviates the problem of having to wait a couple of years to know whether or not a given paper has had a scientific impact on the field.

Having become a measure of "quality," the impact factor was then used by companies to promote their journals and attract new authors. This apparently inconsequential change gave journals an added centrality in the evaluation system, as it was access to them that was now measured and valued and no more the content of the paper itself and its visibility (measured by its citations). Hence government officials and research institutions in a number of countries (Pakistan, China, South Korea, and Japan) have even established financial incentives based directly on the numerical value of a journal's impact factor, despite the obvious fact that these values cannot be compared across disciplines (Fuyuno and Cyranoski, 2006). This, in turn, put added pressure on scientists, some of whom went so far as to manipulate the peer-review system of journals to make sure their papers get published (see also Brunton's contribution in this book, chapter 18). Once uncovered, the scam led journal editors to retract the papers on the basis that they had been fraudulently peer reviewed by their authors (Barbash, 2015; Haug, 2015; see also Barbour and Stell's contribution in this book, chapter 11). Hence, these new metrics can directly influence access to the resources needed for research (figure 2.2).

The simplistic nature of metrics based on Journal Impact Factors was much denounced by scientists and bibliometricians. After more than a decade of harsh critiques, many journals, including *Nature* and other "elite journals," took some distance from an indicator they had long used to brand themselves, in order not to be associated with simplistic evaluation methods that could tarnish their reputation (Callaway, 2016). As journals supposed to publish units of knowledge, they thus seem to resist being transformed into a mere measurement tool. A similar situation happened with academic book publishers in the 1990s. Pressured by authors to publish the book they needed to justify tenure, publishers asked universities to decouple the tenure system from university presses (Waters, 2004).

The central place now accorded to quantitative indicators of productivity and impact also created a new market for companies offering their service to "evaluate" researchers, laboratories, and universities (American Association of University Professors, 2016). As papers and journals were being transformed into accounting units, journal publishing companies saw a new opportunity to market themselves as providing new evaluation indicators based on their own set of journals, while at the same time enrolling institutions to push their researchers to publish in their self-proclaimed "high-quality" journals. The best example of such a strategy is the creation in 2014 of the "Nature Index," which ranks countries and institutions on the basis of the papers they publish in only sixty-eight journals, defined as "high-quality science journals," and which include seventeen (25 percent) from the *Nature* group itself, owned by Macmillan and now merged with Springer, one of the largest publishers of scientific journals. This new ranking is said to offer "a perspective on high-quality research on the basis of published articles" and to "provide institutions with an easy means to identify and highlight some of their best scientific research."[1]

In addition to radically transforming the mode of circulation of scientific papers, internet access also affected the kind of metrics promoted to measure the "impact" (rarely clearly defined) of research. Through the internet, one can access the article and look at the screen for a minute or download a copy of the entire paper. These actions leave traces and thus create new data on the "uses" of scientific papers. This situation gave rise to "altmetrics," presented as an *alternative* to citation analysis, though they simply measure something different than citations (Priem et al., 2010; see also Lin's contribution to this book, chapter 16). Whereas citations are essentially coming from within the scientific field, measures of internet uses survey a larger spectrum of users of unknown nature in the larger social space, as the "clicks" don't tell us if the author is a simple citizen or a publishing researcher.

By broadening the spectrum of metrics, internet-based measures of the visibility of publications have also contributed to the rush to measure the "impact" of scientific research, even though the exact meaning of those "impacts" remains evanescent. The evangelists of these new web measures have been quick to point out that it takes years to be cited, whereas "views" and "downloads" are accessible in real time, day after day, even hour after hour. By adding blogs and tweets to the array of metrics, one certainly gets tools to follow the diffusion of research results among diverse communities, but that does not say much about the quality or even robustness of the published results. Moreover, these various metrics have radically different temporalities and correspond to different audiences: whereas the scientific field with its citation culture works on the scale of many years, blogs and tweets operate in the larger social space and fluctuate and vanish within days for blogs and within hours for tweets. One can thus expect that scientists will be very critical of institutions trying to use such crude and ill-defined metrics to evaluate the "impact" of their research, be it social, economic, cultural, or whatever else it could be. Pressured to constantly show the usefulness of scientific research, laboratories and universities may nonetheless succumb to the cynical use of these metrics when they suit their purpose, much as they do with the so-called "world university rankings," despite their obvious flaws (Gingras, 2016; see also Kehm's contribution to this book).

Conclusion

After having been in a relatively stable state for more than three centuries, and this despite its continuous exponential growth during that period (Price, 1963), the basic structure of the scientific field has been submitted over the last twenty years to a series of radical transformations. It is not really possible at this point in time to evaluate their long-term effects. We are still in a transition period that sees many kinds of experimentation going on with various ways of making knowledge claims circulate: preprint servers, open reviews, post-publication comments, "fast-track" peer review, and many others. But as the physicists have shown through their use of the preprint server system ArXiv since the beginning of the 1990s, not all disciplines can follow the same model. The potentially dangerous social impact of false medical discoveries announced without peer review would be far more important, for example, than the surprising announcement by physicists of their having observed a neutrino moving faster than the speed of light (Cho, 2012). Though erroneous, this spectacular news item kept the social media and the blogosphere excited for a few days in the fall of 2011, but this major mistake could not have had any serious effect on the daily life of citizens.

One thing seems certain: the pressure to get faster results—and faster evaluation of results—has given rise to a reaction among scientists and the creation of the "slow science" movement (Berg and Seeber, 2016). The manifesto proclaims that "slow science was pretty much the only science conceivable for hundreds of years." The partisans of the movement insist that "society should give scientists the time they need, but more importantly, scientists must take their time […] to think, […] to digest […] to misunderstand each other, especially when fostering lost dialogue between humanities and natural sciences."[2] And only time will tell whether scientists, by becoming more conscious of the mechanisms that triggered the transformations that now affect their research activities, will also take the time needed to make sure the evaluation methods that contributed to transforming the product of their research into mere accounting units get replaced by ones that are more consistent with the nature of research, which, as the manifesto also underlines, "develops unsteadily, with jerky

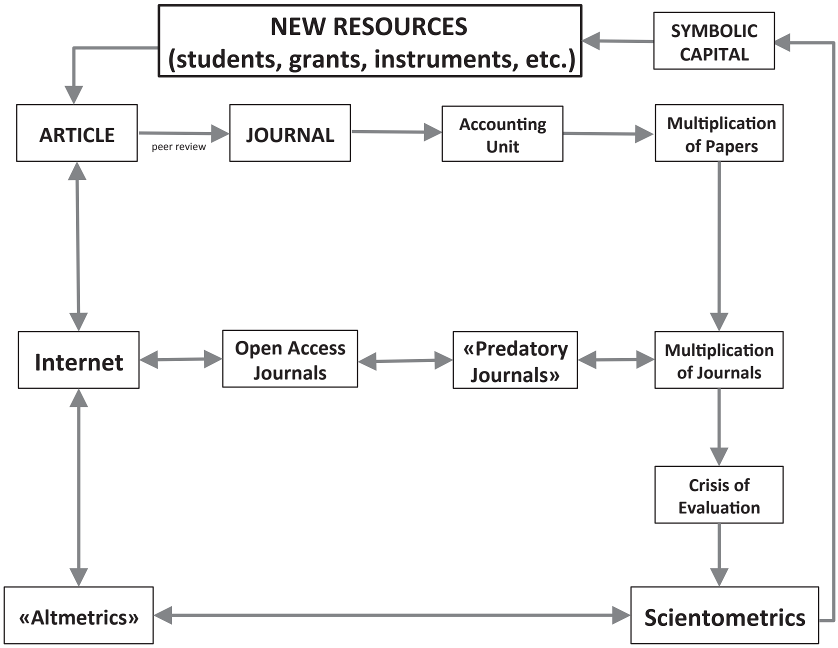

Figure 2.2
How the classic cycle of evaluation (figure 2.1) has been transformed by making the scientific paper a simple accounting unit of research activity, thus generating a series of intermediaries and new evaluation metrics.

moves and unpredictable leaps forward—at the same time, however, it creeps about on a very slow time scale, for which there must be room and to which justice must be done."

Notes

1. http://www.natureindex.com/faq#introduction2. Accessed May 2, 2017.
2. http://slow-science.org/slow-science-manifesto.pdf.

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This is a section of doi:10.7551/mitpress/11087.001.0001

Gaming the Metrics

Misconduct and Manipulation in Academic Research

Edited by: Mario Biagioli, Alexandra Lippman

Citation:

Gaming the Metrics: Misconduct and Manipulation in Academic Research
Edited by: Mario Biagioli, Alexandra Lippman
DOI: 10.7551/mitpress/11087.001.0001
ISBN (electronic): 9780262356565
Publisher: The MIT Press
Published: 2020

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The MIT Press

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This book was set in Sabon by Westchester Publishing Services.

Library of Congress Cataloging-in-Publication Data

Names: Biagioli, Mario, 1946- editor. | Lippman, Alexandra, editor.
Title: Gaming the metrics : misconduct and manipulation in academic research / edited by Mario Biagioli and Alexandra Lippman.
Description: Cambridge, MA : MIT Press, [2020] | Series: Infrastructures | Includes bibliographical references and index.
Identifiers: LCCN 2019010150 | ISBN 9780262537933 (pbk. : alk. paper)
Subjects: LCSH: Scholarly publishing—Corrupt practices. | Learning and scholarship—Corrupt practices. | Research—Corrupt practices. | Communication in learning and scholarship—Moral and ethical aspects.
Classification: LCC Z286.S37 G36 2020 | DDC 070.5—dc23
LC record available at https://lccn.loc.gov/2019010150

10 9 8 7 6 5 4 3 2 1