Scientific Misconduct and Fraud

Nicolas Chevassus-au-Louis
Fraud in the Lab: The High Stakes of Scientific Research
Harvard University Press 2019
(Book Review Essay)

Marcel Herbst
4mation
herbst@4mat.ch

November 17, 2019 (April 14, 2021)

Science is said to seek the truth, at least within the bounds that societies or individuals see as explorable from an ethical point of view. Truth may be elusive or even unattainable, tainted by paradigms and schools of thought, but the attempt to strive for factuality is generally seen as an intrinsic component of any scientific activity. Apart from the indwelling components of science, there are extrinsic reasons to engage in science and research, similar to those that form the motive to pursue other professions or avocations, and it is unclear — or at least disputed — to what extent extrinsic reasons in scientific inquiry are actually helping or hinder that occupation.

With regard to the intrinsic components of science. i.e., science's ethos and search for truth, there exists a philosophy or an epistemology which try to explain how this search does or should evolve. Ethical concerns — as they developed in the context of a reflection on Nazi science, for instance, or in the setting of today's biology and genetics — are normally nor part of those disputations, and if they are, they are dealt within the context of a sociology of science or a discussion on scientific paradigms [Fleck, 1979; Kuhn, 1962]. And yet, the more powerful science becomes, the more doors it opens in the pursuit of technological advancement, a strict distinction between positive and normative matters is not only difficult to sustain, it is perhaps also simply wrong: not everything that is technically feasible ought to be done, and not everything that is researchable ought to be studied. Science may have to reassess its ethos to encompass not only truth but also decency.

With regard to the extrinsic components of science, the old sociology of science [Merton, 1973] posited that science was part of a reward system where recognition or esteem were used as natural currencies in exchange for the ardent — lone — labor of the scientist [Schelsky, 1963]. Science was looked upon as a noble activity not tainted by mundane selfishness that was seen as common, even intrinsic and system sustaining, in the world of business. Intrinsic and extrinsic reasons to pursue science were regarded to be mutually reinforcing — or at least not viewed to be antagonistically related.

Science as an activity to pursue truth was based on a narrow concept of science, encompassing the natural sciences (including the sciences of the artificial, e.g., logic or mathematics) and perhaps also some social sciences — leaving out other forms of science or academic activities (such as engineering, law, medicine, or the humanities). But even in the case of the natural sciences, the link between the descriptive and normative, between truth-seeking and technological or political applications, has always been manifest. Indeed, looking at the history of science (in general), we may argue that the normative served as a driving force for the descriptive — or at least that technology-assisted science in turn aided technology (and vice versa). Furthermore, technology served the respective economies — leading to today's assessment, that science or higher education function as motors of economic development.

This bundling of interests — of truth-seeking, of social recognition, of civil or economic interests — has broad ramifications for today's world of science and its societal implantation. Science, in all its forms, has to be recognized and funded by governmental agencies, academies, philanthropy, private investors or students, and funds flow into higher education or science systems in accord to politically chosen funding modes [Herbst, 2007]. This affects the various sciences in the way they are fostered, promoted, practiced, developed, or changed: science and scientists adapt opportunistically to changing environments or doctrines through legal — or illegal — means [Abbott, 2014].

&

Nicolas Chevassus-au-Louis has written a disturbing — and readable — account on fraud or, more broadly, on misconduct within the scientific community. Fraud within science is not that different from fraud in general (but the sanctions within the two domains are quite distinct), and it has been around for a long time. The disturbing aspect is that fraud is an expanding phenomenon, causing harm, waste, misallocation of societal resources, and the erosion of trust (within the science community and between scientists and society); it is, in the words of Deena Weinstein [1979], "antithetical to the very aims of science". Chevassus-au-Louis cites Charles Babbage (1791-1871) to have classified fraud to encompass the forging, the tampering or trimming of data, plus the scientific hoax (the imposture, the pretense), but fraud — of course — goes further than the fabrication, falsification or "cooking" of data to include plagiarism as well. Finally, scientific misconduct covers further aspects within the scientific community, pushing the boundaries of a narrower concept of fraud.

Chevassus-au-Louis refers to a comprehensive survey by Grieneisen and Zhang [2012] on retracted articles (1928–2011) which found that "only miniscule [sic] percentages of publications for individual years, countries, journals, or disciplines have been retracted" (that is, roughly 0.5% to 2% in some fields of medical research, biology, biophysics, developmental biology, crystallography, anesthesiology, et cetera); furthermore, "[f]ifteen prolific individuals accounted for more than half of all retractions". Retractions, as such, cannot be seen as evidence for fraud or research misconduct, but the concentration of retractions on a few individuals is telling; furthermore, retractions are increasing: between 2001 and 2010, Grieneisen and Zhang claim that they increased more than ten-fold — indicating, perhaps, that there is an incipient new academic culture feeding this increase.

Friedhelm Herrmann is one of the fifteen research offenders listed by Grieneisen and Zhang. A task force scrutinized a period of eight years (1988-96) of Herrmann's work, covering 347 publications, of which 132 appeared to be unaffected by fraud. The point is, however, that while Herrmann was active (that is, before being removed from his position as principal investigator at the University of Ulm), he enjoyed the status of a star scientist, perhaps because he was so prolific: nobody appeared to take negative note of his immense output at the pace of close to one article per week (even concocting a paper takes time and may require helping hands). The actual scandal in frauds like these is not the fraud itself, but the lack of vigilance on the part of the scientific community. If colleagues and funding agencies are blind to identify aberrant scientific behavior that should be easy to spot, we can assume that detected, uncovered fraud forms only the tip of the iceberg: this is the conclusion Chevassus-au-Louis draws on the basis of data sampled by the US National Institutes of Health.

Chevassus-au-Louis lists other cases of so-called hyper-productivity. In a range of fields within medicine, one finds "a few dozen researchers who publish more than 50 articles per year, with peaks of 140, or one article every three days". Hyper-productivity is generally not what the term might indicate, namely an excessive form of productivity; rather, it is a form of name-dropping, where individual authors are listed to enhance the reputation of the team of authors; or it is an accepted form of authorship where principal investigators (PI) are listed on every publication by a member of their research team (if a research team numbers 40 members, for instance, and every such member publishes 1-2 papers per year as a first author, the PI's output is listed at roughly 60 papers per year). Neither is hyper-productivity necessarily associated with fraud (in a narrow sense); but it might be a form of malpractice (or of scientific misconduct) in that it unduly links authors with scientific "work" that cannot be theirs.

An earlier meta-analysis on fraud cited by Chevassus-au-Louis was authored by Daniele Fanelli [2009]. He found that about 2% "of scientists admitted to have fabricated, falsified or modified data or results at least once [...] and up to 33.7% admitted other questionable research practices". If scientific misconduct referred not to oneself but to colleagues, the corresponding admission rated jumped to 14% and 72%, respectively. As Fanelli indicates, it is quite "easy [...] for a scientist to publish fabricated data in the most prestigious journals" (and, by implication, peer review is not a bullet-proof barrier against fraud). He also cites empirical studies with rather large reported discrepancies between admitting to fraudulent behavior (in the past) and the willingness to "modify" research results (in the future), that is, "to select, omit or fabricate data to win a grant or publish a paper".

As noted earlier, retraction of articles cannot be seen as evidence of fraud or research misconduct. Many retractions are due to — honestly admitted — errors and not to deliberate fabrication or scientific misconduct [Steen, 2010; Hesselmann et al., 2017]. The problem lies elsewhere, namely within the set of fraudulent articles (or authors) not yet detected. Depending on how one defines "fraudulent", that set can be quite large. Current computer programs allow the detection of wide-scale of plagiarism, but this comes into play normally only in the cases of prominent people (public figures or principal investigators which warrant the effort of investigation); in most other cases, plagiarism, common as it is, remains undetected. Other forms of fraud (or lack of decency), for instance the misappropriation of ideas, models, or findings prior to publication, or ghost scholarship (offered by commercial companies), are clearly frequent. Most damaging, apart from ideological aberrations, are a priori biases (mainly fueled by financial interests) linked to corporations or industries that guide research programs [Proctor, 2011]; or, in the spirit of "entrepreneurialism" [Clark, 1998], the direct funding of academic positions by private corporations (such as implanting "chairs" which only naive observers may call "independent" or, to recall Merton's norm, "disinterested").

Fraud, it has been said, is on the rise. Fraud is clearly linked to the scientific — or academic — enterprise in general. There is a time-dependent trajectory leading from the transformation of economies (with a focus on tertiary sector economies) to mass higher education (with student participation rates six to ten times higher now than after World War II) to credentialing (and status-seeking) to globalization (and the transformation of former Third World countries) to expanded and increased scientific activities (with a broadening of the disciplinary spectrum, the new geographies — and the associated expansion of the number of academics, researchers, academic journals, publications, and citations).

The science community does not appear to know whether fraud is growing over-proportionally, that is, faster than the science enterprise in general: corresponding studies seem to be lacking. What we do know, however, is that there is a surge in scientific cultures which directly cater to fraudulent behavior, to "breaches of scientific integrity". That change became visible around 1980 with the emergence of neo-liberalism and associated forms of governing and financing academia. The growth of higher education had become a costly aspect of public funding, competing with other governmental causes worthy of support, and bringing about a reassessment of social contracts that bound together public institutions (such as higher education) and governmental agencies. Public institutional funding (also affecting the so-called private elite institutions) changed its mode: from funding needs (as assessed by inputs) to financing results (as measured by outputs). While changing funding modes may make sense, it entails dangers that require insight and control; and if such insight is lacking, funding modes may directly weaken the scientists' — or entire institutions' — reluctance to engage in fraud.

Tying funding to output indicators, or research results to potential financial remunerations, has implications. We know that species adapt opportunistically to a changing environment and, likewise, scientists and institutions make arrangements when confronted with a new ecology of science. If funding is tied to output indicators and impact factors in an attempt to guide or "nudge" the funded, care has to be taken to specify the corresponding indicators — lest the guided researcher pursues the wrong course. If funding agencies dish out their cash (or their fame) in relation to the number of publications, publications appear to count, not content; if the number of publications are replaced by the number of citations, popularity counts; if false claims are being bought, there are incentives to engage in fraud. Worse still are improper normalizations commonly adopted (by funding councils, the OECD, et cetera), such as "publications [or citations] by a professor" (as opposed to "publications [or citations] by a researcher or staff member"), fostering a tendency to strive for and assemble large research groups (as opposed to a larger number of smaller teams which are, collectively, more productive because they tend to exploit agglomeration economies rather than economies of scale) [Herbst et al., 2002].

The funding logic may not only have implications regarding the productivity of the science system as such [Herbst, 2014b], it is likely to affect the general academic culture including the scientists' reluctance to share and to cooperate, and the tendency to compete and to engage in fraud and scientific misconduct. Hence, "[t]o tackle the problem at its root", Chevassus-au-Louis suggests, "we must modify the social structures of science"; furthermore, "a deep reform of the way researchers [or research institutions] are evaluated" is required. "Faced with the malscience that so closely resembles the junk food served in fast-food restaurants", Chevassus-au-Louis continues, "we need to slow down. And take our time. The time to think".

One aspect of this "slowing down" are the low reproducibility numbers of biological (and similar kinds of empirical) research [Prinz et al., 2011; Begley and Ellis, 2012]. The vast majority of studies, it appears, cannot be replicated, leading other researchers on wrong tracts and costly backtracking. This non-replication is not due primarily to outright fraud, but to a narrow publishing culture which focuses on positive results. If publications merit attention regarding funding, and if only positive results count, there are strong incentives to present one's findings in the required form — rather than in more skeptical terms. Hence, as Begley and Lee state, "[t]here must be more opportunities to present negative data [... F]unding agencies, reviewers and journal editors must agree that negative data can be just as informative as positive data": a change in science culture is called for.

Investigations on fraud within the scientific community center on the natural sciences (including public health and medicine) with their implicit focus on data and verification. But fraud is not the only problem in science. There is also misconduct that is not a-scientific but illegal or ethically objectionable. Many experiments (in medical, psychiatric or pharmacological research) have been conducted which are not fraudulent in a narrow sense but simply unethical, and it is not clear to what extent ethical standards are discussed, upheld, or enforced in the current field of biological or genetic research.

Finally, many scientific activities, perhaps even most, pertain not to the natural sciences but to the social sciences and the humanities where data play not that dominant a role. The major problems within these sciences are, hence, not fraud or scientific misconduct but lack of stringency — or the lack of criteria by which to assess quality. Modern empirical approaches and data mining within the humanities have remained rare, in spite of recent advances of information sciences and natural language processing. While presumably most empirical studies within the social sciences can be replicated, provided the data sets are open and the major hypotheses are accepted, their findings often remain case-specific and cannot be generalized (at least outside the field of econometrics). Within the humanities, lack of stringency has been deplored for some time now [Carnap, 1931] — and even a tendency towards verbosity and non-sense [Sokal and Bricmont, 1998]. Science, with all its shades and colors, is not above reproach.

References

Andrew Abbott. The Excellence of IT: Conceptions of Quality in Academic Disciplines. In Herbst [2014a], chapter 8, pages 147–165.

C. Glenn Begley and Lee M. Ellis. Raise Standards for Preclinical Cancer Research. *Nature*, 483(7391):531–533, 2012.

Rudolf Carnap. Die Überwindung der Metaphysik durch logische Analyse. *Erkenntnis*, 2:219–241, 1931.

Burton R. Clark. *Creating Entrepreneurial Universities: Organizational Pathways of Transformation*. Pergamon, 1998.

Daniele Fanelli. How Many Scientists Fabricate and Falsify Research? A Systematic Review and Meta-Analysis of Survey Data. *PloS ONE*, 4(5):1–11, 2009.

Ludwik Fleck. *Genesis and Development of a Scientific Fact*. The University of Chicago Press, 1979.

Michael L. Grieneisen and Minghua Zhang. A Comprehensive Survey of Retracted Articles from the Scholarly Literature. *PloS ONE*, 7(10):1–15, 2012.

Marcel Herbst. *Financing Public Universities: The Case of Performance Funding*. Springer Science + Business Media, 2007.

Marcel Herbst, editor. *The Institution of Science & the Science of Institutions: The Legacy of Joseph Ben-David*, volume 302 of *Boston Studies in the Philosophy and History of Science*. Springer Science + Business Media, 2014a.

Marcel Herbst. Academic Organization and Scientific Productivity. In *The Institution of Science & the Science of Institutions: The Legacy of Joseph Ben-David* Herbst [2014a], pages 15–35.

Marcel Herbst, Urs Hugentobler, and Lydia Snover. *MIT and ETH Zürich: Structures and Cultures Juxtaposed*. Centre d'études de la science et de la technologie, CEST 2002/9, `www.swir.ch`, October 2002.

Felicitas Hesselmann, Verena Graf, Marion Schmidt, and Martin Reinhart. The Visibility of Scientific Misconduct: A Review of the Literature on Retracted Journal Articles. *Current Sociology*, 65(6):814–845, October 2017.

Thomas S. Kuhn. *The Structure of Scientific Revolutions*, volume 2 of *International Encyclopedia of Unified Science*. The University of Chicago Press, 1962.

Robert K. Merton. *The Sociology of Science: Theoretical and Empirical Investigations*. The University of Chicago Press, 1973.

Flirian Prinz, Thomas Schlange, and Khusru Adadullah. Believe it or not: How much can we Rely on Published Data on Potential Drug Targets? *Nature Reviews Drug Discovery*, 10(9):712–712, September 2011.

Robert. N. Proctor. *Golden Holocaust: Origins of the Cigarette Catastrophe and the Case for Abolition*. University of California Press, 2011.

Helmut Schelsky. *Einsamkeit und Freiheit: Idee und Gestalt der deutschen Universität und ihrer Reformen*. Rowohlt, 1963.

Alan Sokal and Jean Bricmont. *Fashionable Nonsense: Postmodern Intellectuals' Abuse of Science*. Picador, 1998.

R. Grant Steen. Retractions in the Scientific Literature: Is the Incidence of Research Fraud Increasing? *Journal of Medical Ethics*, 37(4):249–53, 2010.

Deena Weinstein. Fraud in Science. *Social Science Quarterly*, 59(4):639–652, March 1979.