HOS

Integrating research integrity into the history of science

Cyrus C. M. Mody
Maastricht University, Netherlands

H. Otto Sibum (iD)
Uppsala University, Sweden

Lissa L. Roberts (iD)
University of Twente, Netherlands

Abstract
This introductory essay frames our special issue by discussing how attention to the history of research integrity and fraud can stimulate new historical and methodological insights of broader import to historians of science.

The politics of integrity

In the companion introductory essay to this special issue, readers encounter the accusations and counter-accusations surrounding Didier Raoult.[1] Controversial figures like Raoult make compelling reading. The lurid details of his appearance, provocative language, past defiance of conventional wisdom, current climate denialism, and complicated relationship with his staff draw attention and generate doubts about the integrity of

---

1. Lissa L. Roberts, H. Otto Sibum and Cyrus C. M. Mody, "Integrating the History of Science into Broader Discussions of Research Integrity and Fraud," *History of Science* 58 (2020): 354–68.

Corresponding author:
Lissa L. Roberts, University of Twente, Warmondstraat 181, Enschede 7500 AE, Netherlands.
Email: l.l.roberts@utwente.nl

both Raoult and his accusers. Similar spectacular episodes that have entered global circulation – Diederik Stapel, Jan Hendrik Schön, Hwang Woo-suk (the latter examined by Buhm Soon Park in this issue) – motivated a special issue on scientific fraud and research integrity.[2] But our interest was also piqued by these cases' *aftermath*: commissions, inquiries, reports, legislation, and "reforms" purportedly intended to encourage research integrity. In the wake of notable scandals, organizations such as the UK House of Commons, the All European Academies, the European Science Foundation, and the Dutch Royal Academy of Science have issued diverse concordats, statements, principles, and oaths.[3] Individual "epistemic activists" such as Jeffrey Beall and Elisabeth Bik have won praise (but also concern) for naming and shaming individuals and organizations they see as scandal-worthy.[4] And legislators of various political stripes have leveraged research integrity scandals to force through new regulations, for example on the sharing of data and even the sharing of personal communications between scientists.[5]

We see here a politicized debate over research integrity, with winners and losers. Scandals and responses to scandals form moves in a conflict over resources and over competing visions for society. Naturally, reformers cast their project as above reproach, but as scholars we should subject their arguments to scrutiny. For instance, reformers such as Bik and Beall often criticize "predatory journals" and "article factories" in countries (particularly India and China) that have long been the West's other.[6] As Mahendra Shahare and Lissa Roberts argue in this issue, reformers sometimes depict a dangerous East eroding Western values.[7] Yet such journals are responding to demand – including

2. On Schön see Eugenie Samuel Reich, *Plastic Fantastic: How the Biggest Fraud in Physics Shook the Scientific World* (London: Palgrave Macmillan, 2009). On Hwang see Buhm Soon Park, "Making Matters of Fraud: Sociomaterial Technology in the Case of Hwang and Schatten," *History of Science* 58 (2020): 393–416.

3. *The European Code of Conduct for Research Integrity* (ALLEA – All European Academies, 2017); House of Commons Science and Technology Committee, *Peer Review in Scientific Publication: Government and Research Councils UK Responses to the Committee's Eighth Report of Session 2010–12. 10th Special Report of Session 2010–12* (London: Her Majesty's Stationery Office, 2012); Peter Drenth, Sonia Flacnikova, Maura Hiney and Livia Puljak, *Fostering Research Integrity in Europe* (Strasbourg: European Science Foundation, 2010); KNAW (Dutch Royal Academy of Sciences), NFU (Netherlands Federation of University Medical Centres), NWO (Netherlands Organisation for Scientific Research), VSNU (Union of Dutch Universities), TO2-federatie (Applied Research Federation), Vereneging Hogescholen (Netherlands Association of Universities of Applied Science), *Netherlands Code of Conduct for Research Integrity* (Netherlands, DANS, 2018), https://doi.org/10.17026/dans-2cj-nvwu

4. The label "epistemic activist" is taken from a conversation with Raphael Levy and Willem Halffman.

5. Stephen Lewandosky and Dorothy Bishop, "Research Integrity: Don't Let Transparency Damage Science," *Nature* 529 (2016): 459–61.

6. Jennifer A. Byrne and Jana Christopher, "Dark Magic, or the Dark Arts of the 21st Century – How Can Journals and Peer Reviewers Detect Manuscripts and Publications from Paper Mills?," *FEBS Letters* 594 (2020): 583–589; Monica Berger and Jill Cirasella, "Beyond Beall's List: Better Understanding Predatory Publishers," *C&RL News* 76 (2015): 132–5.

7. Mahendra Shahare and Lissa L. Roberts, "Historicizing the Crisis of Scientific Misconduct in Indian Science," *History of Science* 58 (2020): 485–506.

from Western scientists – for more publication outlets. That demand is stoked by globalizing tenure and promotion criteria that originated in the West. Critics also fail to note that (as Shahare and Roberts show) India hosted an indigenous scientific reform movement long before it became trendy in Europe and North America.

The politics of fraud accusations are also evidenced by the differential ease with which Western scientists avoid them. In the Hwang Woo-suk scandal, a scientist at the University of Pittsburgh, Robert Schatten, was Hwang's close collaborator and deeply implicated in results that were retracted. Yet (as Buhm Soon Park shows) Schatten avoided the media condemnation, legal problems, and career annihilation visited on Hwang. That observation does not condone Hwang's fraud; rather, it demonstrates that accusations of research integrity violations are not neutral or self-evident. Similarly, status seems to guarantee integrity. In Eugenie Samuel Reich's fascinating account of the Bell Labs fabulist Jan Hendrik Schön, Schön's senior coauthors and managers – who benefited from his fraudulent papers in *Science* and *Nature* – were rewarded with prestigious jobs and cleared by the investigations that condemned Schön himself.

Finally, the most obvious tell that research integrity is political is its appropriation by politicians, think tanks, and businesses to make it *harder* to publish research they find inconvenient. The 2009 "Climategate" email hack of four prominent climate scientists, for instance, led to numerous investigations of the climate scientists – that is, the *victims* – in the name of transparency and research integrity.[8] The transformation of infractions into scandals spreads the view that fraud is endemic and therefore that inconvenient science can be dismissed as corrupt. Moreover, as Philip Mirowski argues, the "open science" offered as a remedy for such scandals will not increase public trust in science, but will benefit corporations seeking monopoly control over open science.[9]

What history offers

The politicized back-and-forth of scandals and reforms should hold much interest for historians of science. More importantly, historians of science can contribute to the debate itself. Most reformers assume a static and universal picture of what counts as 'good' or 'bad' science. Yet as historians of science have repeatedly shown, the meanings currently attached to seemingly self-evident terms such as 'objective', 'author', and 'experiment' would be unrecognizable to yesteryear's 'scientists' (another term with an historically evolving referent).[10]

Research integrity reformers do sometimes acknowledge change over time, especially when claiming that research integrity violations are becoming more common. For

---

8. "Climatologists under Pressure," *Nature* 462 (2009): 545.
9. Philip Mirowski, "The Future(s) of Open Science," *Social Studies of Science* 48 (2018): 171–203.
10. Lorraine Daston and Peter Galison, *Objectivity* (New York: Zone Books, 2010); Mario Biagioli and Peter Galison (eds), *Scientific Authorship: Credit and Intellectual Property in Science* (Abingdon: Routledge, 2003); Peter Dear, "The Meanings of Experience," in Katharine Park and Lorraine Daston (eds) *The Cambridge History of Science, Vol. 3, Early Modern Science*, (Cambridge: Cambridge University Press, 1987), pp.106–31.

instance, the introduction of Photoshop is sometimes cited as the reason large numbers of images in scientific articles have been found to be manipulated.[11] In fact, 'touching up' micrographs and other images goes back to the beginnings of scientific photography.[12] Similarly, the growth of scientific research in China and India is, as noted above, often blamed for the supposed increase in questionable publishing practices.

Moreover, research integrity reforms are often based on a simplistic picture of how science works. For instance, efforts to combat the supposed 'replication crisis' in psychology and other fields through reforms such as open data neglect that tacit knowledge is unavoidable in science.[13] If a result is not replicable, it is not necessarily a sign that the result was sloppy or fraudulent. And even carefully crafted reforms have unintended consequences. Indeed, a theme running through some articles in this issue, especially Joseph Gabriel and Bennett Holman's, is that some practices that today's reformers cite as violations of research integrity originated as attempts to curb practices that earlier reformers regarded as sloppy or unethical.[14]

Reformers also show little tolerance for generative or positive aspects of violating research integrity. It may sound strange to say that we *need* fraud, and we should not be overly permissive. But as Jonathan Coopersmith has argued, promoters of claims that have not yet gained consensus often need to overpromote those claims (through 'froth') to get a fair hearing.[15] Other frauds, such as the Sokal hoax, constitute a form of critique (or, as Penders and Shaw label it, 'civil disobedience').[16] Michael Barany's article in this issue examines several such examples from mathematics, most famously the Bourbaki collective.[17] Some colleagues thought Bourbaki lacked integrity, while others appreciated that 'his' antics exposed assumptions built into the social organization and content of mathematics. Some of Didier Raoult's clowning could be understood in this light. We

---

11. Mario Biagioli, "Before and after Photoshop: Recursive Fraud in the Age of Digital Reproducibiliy," *Angewandte Chemie international Edition* 58 (2019): 16334–5.

12. For some examples of how such manipulations were widely accepted in cytology see Aryn Martin, "Can't Any Body Count? Counting as an Epistemic Theme in the History of Human Chromosomes," *Social Studies of Science* 34 (2004): 923–45.

13. Bart Penders, J. Britt Holbrook and Sarah de Rijcke, "Rinse and Repeat: Understanding the Value of Replication across Different Ways of Knowing," *Publications* 7 (2019): 52.

14. Joseph Gabriel and Bennett Holman, "Clinical Trials and the Origins of Pharmaceutical Fraud: Parke, Davis & Company, Virtue Epistemology, and the History of the Fundamental Antagonism," *History of Science* 58 (2020): 533–58.

15. Jonathan Coopersmith, "Fraud and Froth: Free-Riding the 3D Printing Wave," in Bibi van den Berg, Simone van der Hof and Eleni Kosta (eds) *3D Printing: Legal, Philosophical and Economic Dimensions* (Berlin: Springer, 2015), pp.37–152.

16. Bart Penders and David M. Shaw, "Civil Disobedience in Scientific Authorship: Resistance and Subordination in Science," *Accountability in Research* 27 (2020): 347–71. See also Malcolm Ashmore, "The Theatre of the Blind: Starring a Promethean Prankster, a Phoney Phenomenon, a Prism, a Pocket, and a Piece of Wood," *Social Studies of Science* 23 (1993): 67–106; Jim Schnabel, "Puck in the Laboratory: The Construction and Deconstruction of Hoaxlike Deception in Science," *Science, Technology & Human Values* 19 (1994): 459–92.

17. Michael J. Barany, "Impersonation and Personification in Mid-Twentieth Century Mathematics," *History of Science* 58 (2020): 417–36.

should not *accept* Raoult's or Bourbaki's critiques, nor ignore their unsavory aspects – but we should understand them in part *as* critique.

To explore how historians of science might contribute to the research integrity debate, the editors of this special issue organized a workshop in Uppsala, Sweden in April 2018 on the theme "Making It Up: Histories of Research Integrity and Fraud in Scientific Practice." About half the workshop papers appear in this issue. To our great satisfaction, the workshop also showed that thinking about research integrity has much to offer the history of science: unexpected connections to our field's cherished themes and new ways of applying our favored concepts, which this essay draws out. The themes surveyed here complement the special issue's other introductory essay. There, the focus was outward-looking: what does the history of research integrity mean for broader discussions of scientific fraud and misconduct? Here, we look inward: what does research integrity mean for historians of science? The two questions are obviously related, and the essays form a dialogue; but their aims are different enough that we thought readers would find it more convenient if we separated them.

Crossing the line

To start, let us split research integrity into two components: integrity *in* science; and the integrity *of* science. The former encompasses activities within the scientific community and places where science is generally considered to take place, for example, universities, laboratories, scientific societies. Examining integrity *in* science lets us ask how scientists act in good or bad faith toward each other, and how and when they test whether their peers are acting in good faith (e.g., by replicating an experiment) or accuse them of acting in bad faith. "Bad faith" has a Sartrean genealogy but we use it here in the Wittgensteinian sense of *cheating* in a language game. Science is constituted from many language games. The rules of science's language games continually evolve and cannot be fully specified, yet participants are still able to recognize and apply those rules. The existence of such rules implies the possibility of cheating. As rules evolve, what counts as cheating and how it is recognized evolves too. Thus, an historical examination of cheating tells us how the rules change.

Meanwhile, the integrity *of* science orients us to the *audience* of the language game and the ways the scientific language game is integrated into a broader world where most people do not consider themselves scientists. Here we draw an analogy with "games" in the literal sense of professional sports. An audience is existentially necessary for professional athletics. But so is that audience's belief (or suspension of disbelief) in the integrity of the game, despite anomalies that cast doubt on that integrity. Think of professional cycling, or any sport (baseball, horseracing, weightlifting, etc.) where one of the main meta-rules is a high tolerance for rules being broken through endemic doping. What constitutes cheating, how much tolerance there is for cheating, how cheating is recognized – these are fraught questions for professional athletics, but also for science. Crucially, these questions can only be answered with input from actors who make the sport possible but do not play it professionally. As cycling's colorful history attests, sponsors and fans tolerate (even encourage) some forms of cheating, and push for certain practices not to be labeled as cheating. But they do not tolerate cheating *in* cycling that is flagrant enough to call the integrity *of* cycling into question, since cycling would then

become a different kind of language game: say, a theatrical spectacle à la professional wrestling or reality television. Unfortunately, no one can say beforehand where the line between tolerable and intolerable infractions lies. It takes someone like Lance Armstrong to push so flagrantly past the line that it suddenly becomes visible. With respect to science, it is governments, universities, corporations, journals, and the wider public who routinely tolerate *some* questionable practices, but who can be called to action by episodes (e.g., Diederik Stapel or Climategate) that arguably put the integrity *of* science into question. Yet, as the articles in this special issue show, no one quite knows which practices (and under what circumstances) will be deemed to cross that line.[18]

The analogy to sport casts science as a field of contention rather than consensus where both competing interests (most players want to win) *and* competing values are at play. Some value sport for its excitement, which might be boosted by a little cheating. Others value ingenuity and victory by any means: cheat if you can get away with it. But for others, that is 'not cricket': they value sport for instilling discipline and respect for rules. Crucially, these competing values cannot be understood solely by reference to the players. Science, like sport, is interwoven with other fields of endeavor that participate in training and paying the players and building the arenas, making and changing the rules, narrating the action, and judging whether rules are followed.

Finally, the analogy to sport motivates this special issue's temporal horizon. None of the articles delves further back than the 1860s, and most focus on the twentieth century. The special issue examines science as it has been practiced since the category of professional scientists emerged alongside systems for supporting them and rules that set their conduct apart from other occupations. This is, not coincidentally, the same era when the category of professional athletes emerged, the rules of many sports were codified, and the system of leagues, arenas, cups, sports journalists, and so forth, came into being. We do not ignore earlier or parallel forms of knowledge production, nor do we accept at face value historical actors' declarations of science as a separate sphere. Indeed, we see the porosity of science as fundamental to the research integrity debate. But the emergence of a *belief* in science as a professional activity with its own rules is the historical starting point for this special issue.

Turn and turn again

We approach science's rules and rulebreakers via Wittgenstein, but for some readers 'rules' will recall one of the most influential analysts of research integrity, Robert Merton, and his 'norms'. That is understandable but inaccurate. The rules of the scientific language game vary more across time, culture, and discipline than Merton's norms. While players of a Wittgensteinian game usually feel they understand the rules, they cannot specify them completely. Merton did specify his norms, and wrote as though norm

---

18. A nice example is the 'Stanford yacht scandal', wherein Stanford thought it was bending the rules on overhead spending in a permissible manner but found to its dismay that it had crossed an invisible line. Brent D. Maher, "Technically Allowed: Federal Scrutiny of Stanford University's Indirect Cost Expenditures and the Changing Context for Research Universities in the Post-Cold War Era," *History of Education Quarterly*, 59 (2019): 97–127.

violations were easy to identify. As Jessica Wang argues, in the 1940s and 1950s, Merton was anxious to associate 'bad' science – including fraudulent and unethical science – with regimes whose ideologies were incompatible with his norms.[19] This special issue instead shows that identifying infractions is messy and contentious. This was in fact recognized within Merton's school by the early 1960s; 'counter-norms' later were offered as a workaround.[20] Yet as Wang notes, Merton's initial clarity and simplicity continues to attract research integrity reformers. The language of norms and norm violations still suffuses the research integrity debate.

In the 1970s, though, historians came to reject the view that any short list of norms could offer the recipe for good science. Kuhn's *Structure of Scientific Revolutions* fomented disenchantment with internalist stories showing science in all its rationality, and spurred interest in externalist stories showing science in its humanity.[21] That shift did not happen overnight; one of us identifies 1981 as the year this new way of writing history of science caught fire.[22] But as it took hold, historians lost interest in the Mertonian project of distinguishing 'good' and 'bad' science. Indeed, many turned to arguing that canonically 'good' science was just as questionable and unethical as canonically 'bad' science.

That latter project emerged, in part, in response to prominent scientists' defenses of the increasingly indefensible: nuclear energy, nuclear earthmoving and above-ground nuclear testing (and even nuclear war!), eugenics, counter-insurgency tactics in Southeast Asia, and the oppression of women, people of color, sexual minorities, and other subalterns.[23] Historians of science and science and technology studies (STS) scholars who disapproved of these entanglements sought to show that the scientific method did not confer moral or even technical certainty, indeed that there was not a scientific method, and that skepticism toward Establishment science was therefore legitimate. Among the claims offered in this vein were: that scientific articles are works of fiction; that science's culture-heroes doctored their results; and that the individual scientific author is an unnatural construct.[24] All these findings undermine today's research integrity discourse, which

19. Jessica Wang, "Merton's Shadow: Perspectives on Science and Democracy since 1940," *Historical Studies in the Physical and Biological Sciences* 30 (1999): 279–306.

20. Ian I. Mitroff, "Norms and Counter-Norms in a Select Group of the Apollo Moon Scientists: A Case Study of the Ambivalence of Scientists," *American Sociological Review* 39 (1974): 579–95.

21. Thomas S. Kuhn, The *Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962).

22. Cyrus C. M. Mody, "Historical Studies in Which Sciences? The Revolving Door of Engineering and Technology," *Historical Studies in the Natural Sciences* 50 (2020): 41–9.

23. E.g., Scott Kirsch, *Proving Grounds: Project Plowshare and the Unrealized Dream of Nuclear Earthmoving* (New Brunswick: Rutgers University Press, 2005); Sharon Ghamari-Tabrizi, *The Worlds of Herman Kahn: The Intuitive Science of Thermonuclear War* (Cambridge, MA: Harvard University Press, 2005); Daniel Kevles, *In the Name of Eugenics: Genetics and the Uses of Human Heredity* (New York: Knopf, 1985); Mark Solovey, "Project Camelot and the 1960s Epistemological Revolution: Rethinking the Politics-Patronage-Social Science Nexus," *Social Studies of Science* 31 (2001): 171–206; Alondra Nelson, *Body and Soul: The Black Panther Party and the Fight against Medical Discrimination* (Minneapolis: University of Minnesota Press, 2011).

24. Bruno Latour and Françoise Bastide, "Writing Science – Fact and Fiction: The Analysis of the Process of Reality Construction through the Application of Socio-Semiotic Methods to Scientific Texts," in Michel Callon, John Law and Arie Rip (eds) *Mapping the Dynamics of Science and*

imagines that it is straightforward to report procedures in a way that can be replicated and to list authors in a way that reflects who did what.

This line of work continues to the present, with a new generation of studies valorizing forms of science that research integrity reformers would view as fraudulent: parapsychology, New Age science, psychotropically enhanced sciences of expanding consciousness, and so on.[25] This work repeats earlier claims that there is no firm basis for sorting science from pseudoscience, but also goes further to argue that 'fringe' research that reformers would view as undermining the integrity of science was necessary for the very best science that we have – that is, if we apply a narrow understanding of research integrity then we risk purging those who push science in new, groundbreaking directions.[26]

Highlighting the ordinary fraudulence of science is, of course, controversial. If scientific methods are anarchy and there is no difference between science and pseudoscience, then why should we not accept Donald Trump's claims that hydroxychloroquine or even bleach can cure COVID-19? If the best science rests on doctored results, why should we care whether Hwang, Stapel, and Schön fabricated their data? If the author is dead, why does it matter whether men like Raoult get their names on every article their subordinates write, or whether women like Jocelyn Bell *do not* get credit for their discoveries? Worries like these led some to ask "should the history of science be rated X?" – that is, maybe we should keep science's disreputability to ourselves so we do not condone fraud or turn the next generation against science.[27]

When such worries about constructivism and relativism were first posed, critiques of science mostly came from the New Left and allied social movements. Today, doubts about science more often than not come from the populist right and corporations seeking liberation from regulation. For some historians and STS scholars this shift in the politics of skepticism has led to interesting contortions. Take, for instance, those STS scholars who believe that 'responsible research' is 'responsive' to the will of the people. For some in that group, if the people supposedly want right-wing populism that rejects scientific facts, then rejection of scientific facts is the only legitimate course.[28] Scholars in this

---

Technology (Berlin: Springer, 1986), pp.51–66; Gerald Holton, "Subelectrons, Presuppositions, and the Millikan-Ehrenhaft Dispute," *Historical Studies in the Physical Sciences* 9 (1978): 161–224; Gerald L. Geison, *The Private Science of Louis Pasteur* (Princeton: Princeton University Press, 1995); Biagioli and Galison, *Scientific Authorship* (note 10).

25. David Kaiser, *How the Hippies Saved Physics: Science, Counterculture, and the Quantum Revival* (New York: W. W. Norton, 2012); Michael D. Gordin, *The Pseudoscience Wars: Immanuel Velikovsky and the Birth of the Modern Fringe* (Chicago: University of Chicago Press, 2012); David Kaiser and W. Patrick McCray (eds), *Groovy Science: Knowledge, Innovation, and American Counterculture* (Chicago: University of Chicago Press, 2016).

26. H. M. Collins and T. J. Pinch, *Frames of Meaning: The Social Construction of Extraordinary Science* (London: Routledge & Kegan Paul, 1982).

27. Stephen G. Brush, "Should the History of Science Be Rated X? The Way Scientists Behave (According to Historians) Might Not Be a Good Model for Students," *Science* 183 (1974): 1164–72.

28. Steve Fuller, "Is STS All Talk and No Walk?," *EASST Review* 36 (2017), https://easst.net/article/is-sts-all-talk-and-no-walk/; Daniel Sarewitz, "Science and Innovation Policies for Donald Trump: Daniel Sarewitz Says the New President Will Need to Promote Creative Science to Benefit the Disaffected Millions Who Voted for Him," *Science* 539 (2016): 331.

vein, such as Daniel Sarewitz and Steve Fuller, are surely right that simply citing facts is not going to win over antivaxxers or creationists. But their willingness to side with those who view facts and expertise as an elite plot is, well, quite likely to end badly. Moreover, the distinction they draw between the elite's facts and the people's values is questionable. Although there are instances in this special issue of elites wielding technical expertise in antidemocratic ways, the opposition of 'experts versus the people' ignores the heterogeneity of science that figures in all our articles.

History also shows that those wielding 'experts versus the people' rhetoric are usually no friends of 'the people'. Opposition to mainstream climate science, for instance, did not arise as a grassroots movement of those whom faceless experts have marginalized. Rather, climate denialism is just as much an elite project as climate science.[29] Moreover, where populism does tap into grassroots 'values' that Sarewitz and others believe should have more sway than scientists' 'facts', the populists' values often include prejudice and xenophobia.[30] Nothing new in that: Tatjana Buklijas' article in this special issue examines a similar episode from *fin-de-siècle* Vienna where elites leveraged populist prejudice against Jews and experts in order to destroy the career of an inconvenient Jewish expert.[31]

That is, there is a long history of populist skepticism of science where neither the populism nor the skepticism are offered in good faith. Yet it is only relatively recently that historians and STS scholars have confronted efforts to manufacture and weaponize skepticism. While some, such as Sarewitz and Fuller, treat the right-wing doubt machine as a good-faith actor, other prominent STS scholars appear to have had an epiphany. Harry Collins and Bruno Latour, in particular, have vocally abjured their original positions in order to more clearly oppose climate denialism and other attacks on science – or, perhaps, they have clarified that their original positions were not as skeptical as everyone took them to be.[32] Now, they emphasize that history of science and STS can tell real experts from fake ones, and that we should *aid* science in overcoming manufactured skepticism.

29. Naomi Oreskes and Erik Conway, *Merchants of Doubt: How a Handful of Scientists Obscured the Truth on Issues from Tobacco Smoke to Global Warming* (New York: Bloomsbury Press, 2010).

30. For an example of Sarewitz's call for values to rule debate over issues such as coronavirus, see Daniel Sarewitz, "What the Pandemic is Telling Us about Science, Politics, and Values," *Slate*, 24 March 2020, https://slate.com/technology/2020/03/coronavirus-pandemic-science-politics-values-convergence.html. Our point is not that values are not important or that scientists always have the right facts. Rather, our point is that Sarewitz's argument requires a considerable naiveté (or disingenuousness); in the essay Sarewitz maintains that with coronavirus (unlike, say, climate change) "for once, we all agree" and "the facts are good enough." It should have been obvious given the history of climate science that "we" were not going to "all agree" for long once the usual denialists realized there was money and political capital to be made from manufacturing their own bad-faith 'facts'.

31. Tatjana Buklijas, "Publicity, Politics, and Professoriate in *fin-de-siècle* Vienna: The Misconduct of the Embryologist Samuel Leopold Schenk," *History of Science* 58 (2020): 458–84.

32. H. M. Collins and Robert Evans, "The Third Wave of Science Studies: Studies of Expertise and Experience," *Social Studies of Science* 32 (2002): 235–96; Bruno Latour, *Facing Gaia: Eight Lectures on the New Climatic Regime* (Cambridge: Polity, 2017).

Some readers may wonder how this relates to research integrity. Our point is that the turn away from Merton entailed a critique of scientific norms, including norms relating to research integrity. Where the Mertonians of the early Cold War deployed their norms to link Lysenkoism and other frauds to authoritarianism, Merton's critics were less sure that supposedly liberal democratic science was any more upright. Since the 1970s, therefore, our field has been ambivalent about fraud and research integrity. Scandalous frauds like Schön and Stapel have not attracted much attention from historians; when we have examined fraudsters we have often left readers in doubt whether they were any worse than their opponents.[33] Conversely, the field has attended to nonscandalous frauds committed by scientific culture-heroes such as Pasteur and Millikan, and has happily cast doubt on the integrity of research conducted in the name of eugenics, racial prejudice, and anticommunism. It is only with the growing flagrance of heavy-handed and sometimes criminal attacks on climate science, epidemiologists and pediatricians, oncologists, evolutionary biologists, and other scientists that the pendulum has partially swung back toward criticizing fraud and defending research integrity.

Two mingled literatures exemplify that newfound willingness to confront integrity in/of science. One blames the growing influence of neoliberalism and unfettered capitalism for fraudulent attacks on technical experts and the erosion of research integrity. Lee Vinsel, for instance, has shown that one of the first victories for neoliberals in the Nixon administration was to substitute proximity to the market for technical expertise in selecting regulatory officials.[34] Philip Mirowski has described extensively how neoliberal ideology underwrites assaults on any science that endangers profits.[35] Sergio Sismondo has investigated how the pharmaceutical industry makes negative results disappear while generating (possibly specious) positive results and spreading them via ghostwritten articles.[36] Probably the most influential work in this line is Naomi Oreskes and Erik Conway's *Merchants of Doubt*, which exposes, according to the subtitle, "how a handful of scientists obscured the truth on issues from tobacco smoke to global warming." Their book hardly mentions neoliberalism – its 2010 release predates the term's trendiness – but it ends with an essay attacking 'free lunch' market fundamentalism.

Yet Oreskes and Conway also bridge to a literature that traces such dubious practices to before neoliberalism held much sway. Their 'merchants' became enrolled into the neoliberal project in the 1980s, but earlier Cold War defense debates were where they learned to mislead, exaggerate, and distract.[37] Similarly, Nicolas Rasmussen has shown

33. See, for instance, Gordin, *The Pseudoscience Wars*, which makes Harlow Shapley and Carl Sagan seem quite as duplicitous and unscientific as Immanuel Velikovsky (note 24).
34. Lee Jared Vinsel, "The Crusade for Credible Energy Information and Analysis in the United States, 1973–1982," *History and Technology* 28 (2012): 149–76.
35. Philip Mirowski, *Science-Mart: Privatizing American Science* (Cambridge, MA: Harvard University Press, 2011).
36. Sergio Sismondo, "Ghosts in the Machine: Publication Planning in the Medical Sciences," *Social Studies of Science* 39 (2009): 171–98.
37. Lawrence Badash, *A Nuclear Winter's Tale: Science and Politics in the 1980s* (Cambridge, MA: MIT Press, 2009); Jacob Darwin Hamblin, *Arming Mother Nature: The Birth of Catastrophic Environmentalism* (New York: Oxford University Press, 2013); Rebecca Slayton, *Arguments That Count: Physics, Computing, and Missile Defense, 1949–2012* (Cambridge, MA: MIT Press, 2013).

that the pharmaceutical industry's campaigns of ghostwriting and erasing negative results stretched to the early twentieth century.[38] Other scholars, such as Robert Proctor, have labeled long-standing deceptions by the lead and cigarette industries as 'agnotology' – the deliberate creation of ignorance rather than knowledge.[39] Since Proctor co-developed the notion of agnotology with an early modernist, Londa Schiebinger, it is unsurprising that they extend its reach long before neoliberalism and even before capitalism itself.[40] Schiebinger also highlights how agnotology complicates research integrity: in her studies, the creation of ignorance was a means for preserving the perceived integrity *in* and *of* science by displacing holders of alternative forms of knowledge (women, especially indigenous and enslaved women) outside the bounds of science.[41] It is a point with implications for all the views summarized above: in particular, research integrity reformers should be wary of promoting exclusionary policies; while supporters of populist science should admit the existence of organized campaigns to promote ignorance.

Integrating old and new

Thus, the research integrity debate has spurred reconsideration of basic tenets in STS and history of science. Yet other foundations of our field have much to offer in understanding research integrity: in particular, the understanding of knowledge production as a collective, heterogeneous enterprise rather than individual endeavor. Distinctions between 'good' and 'bad' science therefore must go beyond individual actions, or even the internal workings of science, and must account for the system governing the collective. Proponents of this view, such as Willem Halffman, argue that reforming one piece of science without confronting all of science, or targeting individuals without targeting the system, will not solve the problem.[42]

Halffman's position points toward some concepts from STS-informed history and sociology of science that could nuance the research integrity debate. In particular, the finger-pointing and contested narratives surrounding research integrity episodes indicate that the controversy studies approach can be helpfully deployed. Admittedly, most classic controversy studies steered away from explicit questions of fraud and research integrity. The aim of works like *Leviathan and the Air-Pump* or *Inventing Accuracy* was to show that both sides in a controversy can marshal facts and logic and that the outcome of a scientific controversy is determined not by nature but by a

38. Nicolas Rasmussen, "The Moral Economy of the Drug Company–Medical Scientist Collaboration in Interwar America," *Social Studies of Science* 34 (2004): 161–85.
39. Robert N. Proctor, *Golden Holocaust: Origins of the Cigarette Catastrophe and the Case for Abolition* (Berkeley: University of California Press, 2011).
40. Robert Proctor and Londa L. Schiebinger (eds), *Agnotology: The Making and Unmaking of Ignorance* (Redwood City: Stanford University Press, 2008).
41. Londa Schiebinger, "Agnotology and Exotic Abortifacients: The Cultural Production of Ignorance in the Eighteenth-Century Atlantic World," *Proceedings of the American Philosophical Society* 149 (2005): 316–43.
42. S. P. J. M. Horbach and W. Halffman, "Promoting Virtue or Punishing Fraud: Mapping Contrasts in the Language of 'Scientific Integrity'," *Science and Engineering Ethics* 23 (2017): 1461–85.

process of social contention.[43] Episodes where scientists instead marshal fraudulent data hardly contribute to that aim. Yet closer inspection of classic controversies shows they often devolve into mutual accusations of fraud and sloppiness resembling the shoving match between Didier Raoult and his detractors. As Bruno Latour argued in *Science in Action*, controversies about facts of nature inevitably become controversies about the practices by which facts are produced; and those often become controversies about whether the disputants actually carried out the practices they claimed they did.[44] Controversies coproduce technical facts (e.g., 'high-flux gravitational radiation has not been observed') with *moral* facts (e.g., 'Joseph Weber is a crank and sloppy scientist willing to certify suspect data and unwilling to listen to reason').[45]

The moral dimension of controversies implies that another oft-used concept, the 'moral economy of science', could also aid in historicizing research integrity. As noted in this issue's other introductory essay, the importation of that concept into our field is often credited to Lorraine Daston, who defined moral economy as "a web of affect-saturated values that stand and function in well-defined relationship to one another."[46] The articles in this special issue are full of affect arising from (accusations of) breaches of research integrity: friends falling out with each other (Gabriel and Holman); professors smeared in the press (Buklijas and Mercelis); administrators of American professional societies fuming and scheming to undermine upstart French poseurs (Barany); even a scientist committing suicide to draw attention to his peers' transgressions (Shahare and Roberts)![47]

What do these affect-laden episodes tell us about when and why scientists attack their peers' integrity? After all, scientists criticize each other all the time, but criticism only occasionally erupts into outrage. Here it helps to look back to E. P. Thompson's articulation of 'moral economy'. The anthropologist Webb Keane quotes Marion Fourcade's pithy summary of Thompson. Early modern bread riots did not happen just because the price of grain went up, since sometimes prices rose without a riot. Rather, "when market forces expanded into traditional communities, 'more was at stake than grain prices: time-honored norms, customary duties and communal solidarities had come under threat, too'."[48] The same applies in science: accusations of infraction often surface when different 'economies' intrude on each other.

---

43. Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985); Donald A. MacKenzie, *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance* (Cambridge, MA: MIT Press, 1990). H. M. Collins, "The Sociology of Scientific Knowledge: Studies of Contemporary Science," *Annual Review of Sociology* 9 (1983): 265–85, explicitly says that sociologists in this tradition tackled the "hard case" of widely accepted physics and mathematics first, in order to prove their point, and that they shied from "politically-flavoured" controversies as too self-evidently social in character.

44. Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Cambridge, MA: Harvard University Press, 1987).

45. The examples are paraphrases of views expressed in Harry Collins, *Gravity's Shadow: The Search for Gravitational Waves* (Chicago: University of Chicago Press, 2010).

46. Lorraine Daston, "The Moral Economy of Science," *Osiris* 10 (1995): 2–24.

47. Joris Mercelis, "The Scientist and the Advertisement: *Reklamegutachten* in Imperial Germany," *History of Science* 58 (2020): 507–32.

48. Webb Keane, "How Everyday Ethics becomes a Moral Economy, and Vice Versa," *Economics: The Open-Access, Open-Assessment E-Journal* 13 (2019): 1–25.

Keane therefore links moral economies to the work of Luc Boltanski and Laurent Thévenot.[49] They describe various 'orders of worth' in which different kinds of things have value (or where the same thing has different values in different orders); actions that are praiseworthy in one order of worth are condemnable in another. There are probably many orders of worth, but Boltanski and Thévenot discuss six: market, industrial, civic, domestic, inspired, fame (plus a seventh, green, added later).[50] The key to applying these categories to research integrity disputes is the heterogeneity of the social contexts that encompass science. Science participates in many orders at once; thus scientists who have committed an act that would be criticized in one order of worth can switch to another to justify their actions.

The following example is taken from Kara Swanson's study of a famous biotech lawsuit: if I write a patent that says I did something that I did not actually do, you can appeal to the domestic order of worth to claim my patent is illegitimate – it does not pass the test of trustworthiness.[51] But I can reply that that is an order of worth more suited to judging knowledge, whereas a patent operates in the market and industrial orders of worth. Who cares if I did not do what the patent says I did? What matters is that I *could* do it that way in the future to obtain a reliable product (a test of worth in the industrial order) and make a profit (the ultimate test in the market order). Because biotechnologists interact with a heterogeneous set of stakeholders, they can appeal to multiple – seemingly contradictory – orders of worth. Actions that uphold integrity in one order undermine it in another. There is no recipe for research integrity that all participants can agree to.

This heterogeneity allows scientists to draw on all Boltanski and Thévenot's orders and more. Even a researcher who is committed to curiosity-driven basic science has to buy equipment and deal with vendors, and therefore participates in the market and industrial orders. Indeed, the innovations vendors develop for basic research projects are how those projects get justified to legislators.[52] Conversely, even the most applied researcher – for example, the leader of a team building an atomic bomb – must use presence, vision, grace, and other attributes of the inspired order.[53] There is hardly a science that cannot use all the orders in some way.

When times are good, science's participation in multiple orders is one of its great strengths. Whoever you are, whatever order of worth appeals to you in this moment, you

49. Luc Boltanski und Laurent Thévenot, *On Justification: Economies of Worth* (Princeton: Princeton University Press, 2006).
50. Gerardo Patriotta, Jean-Pascal Gond and Friederike Schultz, "Maintaining Legitimacy: Controversies, Orders of Worth, and Public Justifications," *Journal of Management Studies* 48 (2011): 1804–36.
51. Kara Swanson, "Biotech in Court: A Legal Lesson on the Unity of Science," *Social Studies of Science* 37 (2007): 357–84.
52. Michael Riordan, Lillian Hoddeson and Adrienne W. Kolb, *Tunnel Visions: The Rise and Fall of the Superconducting Super Collider* (Chicago: University of Chicago Press, 2015).
53. Charles Thorpe and Steven Shapin, "Who Was J. Robert Oppenheimer? Charisma and Complex Organization," *Social Studies of Science* 30 (2000): 545–90; Hugh Gusterson, "A Pedagogy of Diminishing Returns: Scientific Involution across Three Generations of Nuclear Weapons Science," in David Kaiser (ed.) *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives* (2005), pp.75–107.

will find something in science that you value highly. You may also see much of science as worthless, but usually the things you value will predominate in your conception of science. But when times are bad – the economy changes or resources become scarce – then orders of worth conflict.[54] *That* is when questions of integrity often come to the fore. In normal times science appears to integrate the orders of worth into a single functioning whole. But when the orders conflict, it becomes apparent that science is *split* among the orders – that it is not integrated and its practitioners have lost the integrity they can usually claim.

We see this dynamic – of science's integration and then splitting of the orders of worth – in all the articles in this special issue. Joseph Gabriel and Bennett Holman show the market and industrial orders initially teaming up to overthrow the civic and domestic orders: in the late nineteenth century, the efficacy and market potential of a drug became a more trusted proof of its worth than the imprimatur of traditional remedies and the collective authority of the medical profession. But at the turn of the century, proponents of the industrial and market orders fell out over how to increase the value of a pharmaceutical: by improving the drug's efficacy (the industrial order) or by improving its image (the market order). Similarly, Joris Mercelis examines debates over whether manufacturers (agents of the market order) should be allowed to trade on the good name (a token of the domestic and fame orders) of eminent professors. Otto Sibum takes us inside a group of physicists bound by solidarity and trust (typical of the domestic and civic orders), but threatened by outsiders using justifications drawn from the industrial order.[55] Buhm Soon Park looks at a case where the fame and domestic orders initially reinforced each other: Hwang Woo-suk's celebrity seemed to be built on, and affirm, an exaggerated picture of paternalistic benevolence – until, spectacularly, members of his laboratory exposed him as a bullying work 'father' who, among other things, pressured women in his lab to donate their eggs for research. Conversely, Tatjana Buklijas presents an episode where a scientist's fame was used to depict him as a threat to the patriarchal authority of the domestic order. Mahendra Shahare and Lissa Roberts survey cases where the domestic order, borrowed from colonial India but also making use of long-standing divisions in Indian society, reinforced a hierarchical form of science increasingly at odds with independent India's nascent civic and industrial orders. Finally, Michael Barany examines conflicts within mathematics between the civic order (built on rules guaranteeing equality) and the inspired order (in which the creative individual undermines rules and makes himself an exception to them). We might also see in Barany's article a new order, the ludic or hedonic, where the test of worth is pleasure and playfulness.

So Boltanski and Thévenot's orders of worth seem to apply to the history of science, particularly where conflicts between orders elicit accusations of violations of research integrity. But the orders of worth are not sufficient for historicizing research integrity: they help us understand the lines of argument that actors deploy but do not help us understand how disputes over research integrity *unfold*. For that, we need to synthesize Boltanski and Thévenot with insights from controversy studies and the moral economy of science literature. The orders of worth offer a way to understand how controversies arise from the

---

54. Reich, *Plastic Fantastic* (note 2).
55. H. Otto Sibum, "When Is Enough Enough? Accurate Measurement and the Integrity of Scientific Research," *History of Science* 58 (2020): 437–57.

heterogeneity of science's commitments, stakeholders, and participants. The concept of moral economies helps us to understand the affective heat generated when orders of worth collide. And the controversy studies approach helps us overcome simplistic post hoc explanations for the outcomes of controversies. That literature shows us that – *pace* the research integrity reformers – there is no independent or pregiven yardstick for determining which order of worth is the 'right' one to apply. Instead, participants in a research integrity controversy must fumble their way – through a fog of outraged affect – to consensus regarding the 'right' relationship among the orders of worth.

The historiographic relevance of research integrity

This discussion of orders of worth shows that science's irreducible heterogeneity guarantees there will be disputes over research integrity – and that simple prescriptions for enforcing research integrity will be at odds with one or more of the orders of worth that allow scientists to confer legitimacy and meaning on their work. That heterogeneity has, of course, been a predominant theme for historians of science since at least Kuhn's *Structure of Scientific Revolutions*. These days even historians of science who vehemently reject social constructivism nevertheless accept that they cannot write from a purely internalist perspective; the participation of science in some wider society *is* the story that our field strives to tell. Thus, attention to the integrity in/of science offers a probe of some of our field's frontline topics.

For example, authorship and credit have long been fruitful themes for historians of science. At the same time, plagiarism and authorship fraud are among the most prominent concerns of research integrity reformers. There should, therefore, be notable convergences. Indeed, one of the leading historians of early modern authorship, Mario Biagioli, has taken up writing about 'gaming' and manipulation of authorship and citation in the present.[56] As several of the articles in this special issue – Park, Mercelis, and Shahare and Roberts in particular – show, disputes (or nondisputes) over the integrity of authorship tell us much about who is or is not allowed to be credited as a creator of knowledge.

Similarly, our field has spent a great deal of effort to overcome the Eurocentrism of earlier notions of what counts as science. Much recent work in our field has shown that science is a global enterprise, but also that it is structured to advantage some parts of the world and some kinds of people more than others. One of the underappreciated ways that structural advantage is conferred is by the bar by which some people are accused of research integrity violations being lower for some than for others (and the 'reforms' applied to some people being more stringent than those applied to others).

Finally, research integrity offers a way to get beyond good and evil in our understanding of corporate and industrial science. One of the most obvious ways in which Merton's norms failed is that they seemed to prohibit any applied or commercial form of science – that is, the kind of science done by most scientists. Although applied and commercial/industrial science are core topics for historians today, for many, it is still an implicit – and sometimes

---

56. Mario Biagioli, Martin Kenney, Ben Martin and John P. Walsh, "Academic Misconduct, Misrepresentation, and Gaming: A Reassessment," *Research Policy* (forthcoming); the essay is the introduction to a special issue on the topic.

quite explicit – view that commercial science is uninteresting and possibly suspect. The interaction of scientists with commercial and industrial organizations, and with their customers and vendors, highlights the heterogeneity and porosity of science – and also the heated disputes over integrity that arise when money is on the line. Clearly, as noted, corporate interests can pervert research integrity, and our field can shine a light on that perversion. But not all corporate science is 'bad' science – it is simply science that appeals to a different order of worth than science that justifies itself solely on academic grounds.

There are many other themes in the history of science that could helpfully intersect with the topic of research integrity: scientific personae, professionalization and amateurization, countercultural science, tacit knowledge and embodiment, studies of alternative forms of science communication such as gossip and mass market science, and so forth. This special issue only scratches the surface. The wider research integrity debate is unlikely to disappear any time soon – there are too many vocally interested parties on all sides – and historians of science have important points to bring to that debate. But that debate also places in front of us an historically evolving complex of practices, disputes, and emotions that connects to all of our other work. We can critique research integrity and its reforms without remaining agnostic about scientific fraud. We do not need to become neo-Mertonians in order to say something about science's cheaters. Bad-faith science is prevalent enough now, and in the past, that we must incorporate it into science's history without giving up our understanding of science as a collective endeavor that is too complicated for simplistic labels or unalloyed heroes or villains.

Declaration of conflicting interests

Funding

ORCID iDs

H. Otto Sibum [iD] https://orcid.org/0000-0001-6438-9789

Lissa L. Roberts [iD] https://orcid.org/0000-0002-9370-8467

Author biographies

Cyrus C. M. Mody is Professor of the History of Science, Technology, and Innovation at Maastricht University. He is the author of *Instrumental Community: Probe Microscopy and the Path to Nanotechnology* (MIT, 2011) and *The Long Arm of Moore's Law: Microelectronics and American Science* (MIT, 2017). He currently leads a project funded by the Netherlands Organization for Scientific Research on the oil industry's involvement with scarcity, environmentalism, and alternative energy during the long 1970s.

H. Otto Sibum holds the Hans Rausing Chair of History of Science at Uppsala University and is a Director of the Office for History of Science. His research focuses on the social and cultural history of the physical sciences in the 18th and 19th century with an emphasis on experimental practice

and embodiment of knowledge. Recent publications: *Histoire des sciences et des savoirs, tome 2, Modernité et globalisation* (co-edited with K. Raj). Paris: Points, 2019, "Science and the Knowing Body: Making Sense of Embodied Knowledge in Scientific Experiment", in *Reconstruction, Replication and Re-enactment in the Humanities and Social Sciences*. Dupré, Sven et al., (eds) Amsterdam University Press, 2020.

Lissa L. Roberts is Emeritus Professor at University of Twente, the Netherlands, where she held a Personal Chair in Long-Term Development in Science and Technology, and Editor-in-Chief of *History of Science*. She has authored and edited numerous publications in the history of science and technology, including (co-edited with Simon Werrett) *Compound Histories: Materials, Governance and Production, 1760–1840* (Brill, 2017).