4 Research ethics I

Responsible conduct

Chapter 3 introduced the epistemic and social or behavioral norms in science as a method of knowledge production and as a social institution. These norms were described in general terms by the sociologist Robert Merton as communalism, universalism, disinterestedness, and organized skepticism (known by the acronym CUDOS). In the last quarter of the twentieth century, questions arose in society and among a new generation of social scientists about the extent to which the normative ideals of science actually govern scientific practice. To what extent are scientists really living up to the normative ideals that science seems to espouse? Chapters 4, 5, and 6 examine various realities of science that pose challenges to its ideal normative structure. The present chapter digs into the details of operationalizing the norms of science and considers some of the scandals that have occurred as a result of their breach.

There are numerous ethical issues associated with scientific research, which presents a challenge for organizing them into a logical framework. Alphabetically they range from avoiding conflicts of interest and honesty in reporting results to protecting human subjects and recognizing intellectual property. Positively good scientific practices (GSP) or the responsible conduct of research (RCR) are often summarized under the rubric of scientific integrity or responsibility. Negatively, the official US governmental definition of scientific misconduct identifies FFP (fraud, falsification, and plagiarism) as the most egregious failures. Sometimes specifics are analyzed in terms of professional responsibilities to oneself as a scientist, to the scientific community, or to society as a whole. Another common organizer considers ethical issues in relation to the three overlapping, iterative moments of planning, conducting, and reporting research. This chapter adopts a version of the last organizer and distinguishes anticipating, doing, and disseminating research. But it should be recognized that any such framework is to some extent simply a matter of convenience rather than a natural kind. What is most important is to call attention to a number of specific possible experiences in which there will be ambiguities and dilemmas, temptations to cut corners, or opportunities to exercise strength of character. Critically analyzing these experiences helps to cultivate and reinforce appropriate institutional norms in the practice of science.

Setting the stage: a cloning scandal

The South Korean biomedical scientist Dr. Hwang Woo-Suk and his research team would begin work at 6 a.m. in their laboratories at Seoul National University (SNU) and often stay until midnight. The hard work appeared to pay dividends in 1999, when Hwang announced he had created two cloned cows. Though he offered media sessions and photo-ops, he did not provide scientifically verifiable data for the achievements. Despite the lack of data, Hwang gained tremendous popularity, because his work promised both economic gain and international recognition for South Korea. In 2004, Hwang and his team reported in the prestigious journal *Science* that they had derived an embryonic stem cell line from a cloned human blastocyst by using somatic cell nuclear transfer (SCNT). In 2005, *Science* published a second Hwang paper that reported further success in human cloning. Hwang's team claimed they had created eleven embryonic stem cell lines by using SCNT to inject skin cells from patients with disease or injury into enucleated oocytes (egg cells with their nuclei removed).

The papers drew international attention, because they were the first reported successes in human SCNT cloning. The 2005 paper was especially noteworthy, because the stem cells were derived from somatic cell donors that differed from the oocyte donor. (In the 2004 experiment, the eggs and somatic cells were from the same donor.) This seminal accomplishment in "therapeutic cloning" was a major step toward genetically tailored treatments for people suffering from Alzheimer's, Parkinson's, or spinal cord injuries. Due to the enormous medical and economic implications of his research, Hwang's celebrity status skyrocketed in South Korea. The Ministry of Science and Technology selected him as the first recipient of the title Supreme Scientist, which included a $15 million award, and he was appointed to head the new World Stem Cell Foundation at SNU. *Time* magazine named Hwang one of its "People Who Mattered 2004," claiming that "Hwang has already proved that human cloning is no longer science fiction, but a fact of life."[1]

That "fact," however, was soon challenged as questions surfaced about Hwang's research. In November 2005, Dr. Roh Sung-il, a coauthor on the 2004 paper, held a press conference in which he admitted that he had paid women for their eggs despite telling *Science* that all egg donors were unpaid volunteers.[2] Roh and Hwang both claimed that Hwang was unaware of the payments. It later emerged that eggs had also been used from two junior researchers. The South Korean Ministry of Health found that no commercial interests were involved and that no laws or ethical guidelines had been breached. Yet Hwang admitted to lying about the source of the eggs in order to protect the donor's privacy, and his team lied about the number of eggs used in the experiments. Furthermore, Roh had lied to *Science* about the payments, and it is a violation of international standards for scientists to conduct research on human subjects who are in a dependent relationship under them. Hwang resigned from his position as head of the Foundation, but vowed to continue his research, claiming that the experimental results were valid.

Public opinion of Hwang remained high. He was seen by most South Koreans as a hero driven by the noble desire to find cures. An investigative television show that accused Hwang of misconduct was forced off the air. Major sponsors of the show withdrew their support in the face of public outrage at such an "unpatriotic" program and in response to charges that producers of the show had used unethical tactics in obtaining information about Hwang's research. The editors of *Science* magazine also initially dismissed the program's claims as lacking credibility. Over a thousand sympathetic female supporters gathered outside Hwang's laboratory, all offering to donate their eggs for his research.

Then in December, 2005, allegations arose through posts on an online message board that both papers contained fabricated data. The allegations were picked up by the mainstream media. This initiated an investigation by SNU, which concluded that fraud had occurred, resulting from

[1] "The 2004 Time 100," *Time*, www.time.com/time/specials/packages/article/0,28804,1970858_1970909_1971678,00.html.

[2] Holden 2005.

serious misconduct. Hwang's team did not keep proper records and did not have proper evidence to support some of its most important claims. Furthermore, a significant amount of data presented in both papers was fabricated, including manipulated photos and DNA test results. The SNU investigation concluded that in fact no cloned stem cell lines existed. In light of these findings, *Science* retracted both papers, with the consent of Hwang and his coauthors. A now disgraced Hwang was dismissed from his position as professor at SNU.

In delivering the results of the SNU investigation, the dean of academic affairs noted that Hwang and other researchers failed to uphold standards of integrity and honesty. The editor-in-chief of *Science*, Donald Kennedy, issued a statement about the scandal: "Fraudulent research is a particularly disturbing event, because it threatens an enterprise built on trust."[3] *Science* conducted its own in-house investigation of its manuscript evaluation processes, including peer review. Its report concluded that operating in an atmosphere of trust is no longer sufficient and that "substantially stricter" requirements for reporting primary data would be institutionalized. Yet skilled researchers will always be able to perpetuate deceit, at least in the short term. There is no institutional or procedural substitute for virtuous scientists. As one group of bioethicists concluded, "The lesson to be learned is that we need to do a better job of holding research institutions accountable for setting up systems and mentorship that will produce integrity in its scientists."[4]

From norms to realities

In the preceding chapter, we argued against the idea that science is independent of ethics or "value-free." Certain principles and methods (emphasizing empirical evidence and logical consistency) derive from the goal of science to extend reliable knowledge. In the first instance, science could not function without epistemic norms structuring its practices. Complimenting these epistemic norms are moral ones such as those

[3] Statement available in "Science Editor-in-Chief Donald Kennedy's Statement on Latest Hwang Findings," AAAS website, www.aaas.org/news/releases/2006/0110stemstatement.shtml.

[4] Cho et al. 2006, p. 615.

identified by Merton as well as a more general commitment to honesty, integrity, and trustworthiness.

Thus, the very practice of science and its ideal of objectivity presuppose and require ethical norms. Scientists are responsible for upholding these norms. Doing so constitutes RCR (responsible conduct of research) or GSP (good scientific practice); failing to do so constitutes scientific misconduct. Such misconduct often amounts to the negation of science, because practices that violate the norms detract from the systematic pursuit of knowledge. This was clearly evident in the Hwang case: lies and fabricated data derailed the collective scientific endeavor to pursue knowledge. At the end of the ordeal, not only were reputations shattered, but there were no trustworthy findings on which to build further research.

The norms of science constitute an exacting ideal that is not always easy to implement. Science is an activity conducted by people who are, like all humans, susceptible to certain weaknesses of pride, ambition, greed, and vanity. The pleasure of being right and pain of being wrong can contribute to arrogance and defensiveness that might sway judgment to the point of making false claims or fabricating results. For much of its modern history, society trusted that scientists – as members of a community defined by impersonal rationality – were immune to imperfections and believed that their integrity could be assumed. In the wake of misconduct cases such as the Hwang affair, this assumption became difficult to defend. As a result, the scientific community has worked to make guiding norms more explicit while seeking improved mechanisms for instilling and enforcing these norms. In addition, governments have imposed greater oversight and regulation due to the perceived failures of science to act as an effective self-governing institution that nevertheless seeks public financial support.

This rethinking of assumptions has called a naive view of science into question. According to this view, scientists who deviate from the norms are anomalies. The vast majority of scientists properly internalize the norms and the resulting social institution constitutes a self-policing community of accountability between peers. In 1981, testifying before a US congressional subcommittee investigating fraud in science, then president of the National Academy of Sciences, Philip Handler, remarked that the rare instances of deviation from the norms can only be understood "as psychopathic behavior originating in minds that have made very bad judgments."[5] Handler recognized an element of human weakness in science, but it is an element that is methodically, automatically discarded by the system.

In the wake of scandals uncovered throughout the 1970s and 1980s, many now maintain that misconduct is in fact symptomatic of, rather than contrary to, the structure of science. This alternative view sees the structure of science not in terms of idealized norms but in terms of the concrete motivations and needs of scientists. The professionalization of science means that it is a career. Careerism sets the criteria for success, including quantity of publications, funding, tenure, and esteem. A system that rewards quantity creates incentives for mass production, which may come at the cost of dishonesty. Furthermore, with its emphasis on priority of discovery, science is a highly competitive race. Under the pressures of competition, some scientists yield to the temptation to cut corners, improve on or falsify data, or plagiarize the work of others.

As the Hwang case shows, the motive of scientists is not only to seek truth but to achieve recognition of their efforts. This explains why factors that often contribute to misconduct include the pursuit of fame, money, and reputation; the quest for promotion; and pressures to produce. According to this view, addressing misconduct is not a matter of automatic self-policing. Rather, it requires fundamental changes to the professional culture of science.

Part of this culture and another reality of contemporary research is what the physicist Alvin Weinberg and the historian Derek de Solla Price termed Big Science. Large and often multinational collaborative groups tend to diffuse accountability and inhibit communication. Research environments are so fast-paced and complex that mentors often do not have the time to explain decisions or instil norms of responsible conduct. Investigators cited this as a contributing factor in the Hwang misconduct case.

Another reality is the increasing commercialization of science. Privately funded research accounts for a growing percentage of the investments in science. Even publicly funded research conducted at universities is increasingly linked to private enterprise as governments seek to stimulate the transfer of knowledge from laboratories to markets. Such developments have multiplied scenarios in which scientists stand to gain financially as a

[5] In Rollin 2006, p. 261.

result of patenting, licensing, or otherwise marketing the results of their research. This in turn increases the potential for conflicts of interest in which a scientist's personal gain may compete with his or her professional obligation to pursue the truth.

But the issue is not always profits. Humanitarian interests can also lead researchers to cut and trim data. Consider, for instance, the testing of a new drug. A clinical trial might well yield a result that fails by some very small margin to reach the level of statistical significance in the technical sense but nevertheless seems to indicate some effectiveness. The researcher, aware of the vagaries of research results and of the good such a drug might do for desperately ill patients, and confident that more tests would yield statistical significance, might well alter or exclude some data. This would help bring the drug to market to help those who would otherwise suffer or die while further testing was being done. The distinction between bold determination and misconduct is not always a bright line.

Other factors that contribute to misconduct will vary in prominence between nations, cultures, scientific disciplines, and even between individual laboratories. For example, a contributing factor in the Hwang case was a strict South Korean lab culture that leaves junior researchers with little formal power to refuse unethical demands made by their superiors. (It should nevertheless be noted that junior scientists on Hwang's team acted as whistle-blowers by reporting their concerns to an investigative television program and posting allegations on an online message board.) Furthermore, South Korea has a research system that often distributes funding based on lobbying and personal ties rather than transparent, peer-reviewed competition. This system concentrates funding in the hands of the few well-connected researchers such as Hwang. It also prevents the development of multiple groups of experts to assess claims made by their peers.

Yet as Kennedy noted in the Hwang case, there are practical limits on the ability of peer reviewers to identify both unintentional error and intentional fraud. Teams of junior researchers often work tirelessly for lab chiefs who get most of the credit even though they are not involved directly in the day-to-day work. Replication is not as commonly performed as the ideal would indicate, because there are no prizes for second place.

In sum, there are several realities of science that can conflict with and undermine the normative ideal. Misconduct is not simply the anomalous workings of deranged minds or immoral characters. It is also endemic in contemporary science because it often results from the incentives and pressures established by the system itself.

Influential cases

In 1830, English mathematician Charles Babbage identified three malpractices in science: cooking, forging, and trimming of data. There have been several contemporary attempts to formulate a more precise definition. Definitions of misconduct are important for establishing the range of activities that should be investigated and that merit sanctions if guilt is determined. Before consulting these, however, it will be helpful to survey a small sampling of important cases from a wide variety of scientific fields.

Samuel G. Morton and physical anthropology

In the mid-nineteenth century, the American physician Samuel G. Morton used a collection of over 1,000 human skulls to rank various races in terms of intelligence, putting whites on the top, blacks on the bottom, and American Indians in between. The results were presented as inevitable conclusions, compelled by objective facts. But Morton's racial dogma shaped not only his theory but also the data from which it was derived. He juggled the numbers to get the results he wanted (e.g., by excluding subgroups and individuals with small skulls when he wanted to raise the group average). This data manipulation was done openly in scientific journals, so it was apparently performed unconsciously rather than deliberately.[6]

Piltdown man and human archeology

In the early twentieth century, an entire generation of British scientists was duped by a transparent hoax. The British Empire was at its peak and it seemed self-evident that Britain must have been the cradle of civilization. Yet Paleolithic cave paintings and tools were being discovered in France and Germany but not in Britain – until, that is, an amateur geologist named

[6] See Gould 1996.

Charles Dawson discovered the "Piltdown man" in a gravel pit in England. The skull and jaw bones had been salted in the pit by hoaxters, who even fashioned a "Paleolithic" cricket bat. The hoaxters hoped that their increasingly transparent and childish tricks would expose the gullibility of the British scientific community. But under the temptation to believe, the community long continued to accept the findings as legitimate.[7]

William Summerlin and immunology

In 1971, junior researcher William Summerlin began working for the highly regarded immunologist Robert Good at the Sloan-Kettering Institute for cancer research. Two years later, Summerlin reported a breakthrough in transplantation research to journalists, claiming that he had found a method for making human skin universally transplantable without rejection. Summerlin became a celebrity, but many scientists were skeptical because they could not replicate his work. Good reassured scientists, staking his reputation on his junior colleague's research. In October, 1973, one colleague suspected fraudulent research, but later remarked that he "lacked the moral courage" to suggest a hoax was being perpetrated. By March, 1974, however, Good had become convinced of the need to publish a report announcing failure to reproduce Summerlin's results. Summerlin requested a last-minute meeting with Good to argue that the negative report was unnecessary because his latest skin transplantation experiment with mice was going well. On his way to Good's lab to make the case, Summerlin used a felt-tip pen to ink black patches on white mice, supposedly just to make the transplanted black patches of skin stand out more clearly. Once this "improvement" of the data was noticed, Summerlin was immediately suspended from duty. Summerlin argued that his only error was succumbing to pressures created by Good to publish. Good denied creating unreasonable pressure, but an investigative committee did find that Good shared in the blame for allowing Summerlin to announce results to the press before they had been confirmed. Outside of this mild rebuke, the committee exonerated Good, arguing that the usual presumptions of trustworthiness in science would have made it difficult for Good to entertain the notion of fraud.[8]

7 See Weiner 2003. 8 See Hixson 1976.

Mark Spector and oncology

In 1981, junior researcher Mark Spector and his mentor Efraim Racker at Cornell University published a paper in *Science* announcing a remarkable unified theory of cancer causation known as kinase cascade theory. The theory was so elegant and of such potential significance that it garnered headlines and attracted top researchers from around the world. Spector appeared to be both intellectually gifted for conceiving of the theory and uniquely talented as a technician, because he could often get experiments to work when everyone else failed. Indeed, rather than go through the laborious process of replicating his work, scientists from around the world would often send their samples to Spector so that he could run the analyses. They were convinced by the beauty of his theory and the cleanness of his results. But then a colleague tried to explain why his results were so erratic whereas Spector's were so clean. His work uncovered a cunning act of forgery. Spector denied any wrongdoing, claiming that someone else had spiked his test tubes.[9]

Retin A and aging research

Retin A is a cream product of Ortho Pharmaceutical, a subsidiary of the Johnson and Johnson Corporation. It had received FDA approval as an acne medication in 1971 and several older users reported a beneficial side effect: it made their skin smoother and younger looking. In the 1980s, Ortho began conducting the clinical trials necessary to market Retin A specifically as a wrinkle fighter. This promised to make an already lucrative drug even more profitable: at the time, the US cosmetic anti-aging business was roughly $3 billion. In January 1988, the *Journal of the American Medical Association* published an article claiming that Retin A could reverse the effects of aging on skin.[10] That issue also included an editorial titled "At Last! A Medical Treatment for Skin Aging." After the study was published, sales of Retin A quadrupled. While investigating this boom, a reporter for *Money Magazine* (*CNNMoney*) discovered that the author of the editorial and one of the authors of the research article received financial compensations

[9] See Wade 1981. [10] Weiss et al. 1988.

from Johnson and Johnson. The author of the editorial stated in an interview, "Yes, we have a financial relationship in this regard, and we have similar relationships with many, many groups … This is a fact of life in American medicine today."[11] A dermatologist quoted in the same story conceded this fact, but feared that "in this case, science and marketing are becoming confused." A Congressional report noted that the *Journal of the American Medical Association* study was not well designed and the safety and effectiveness of Retin A were not established.[12] This heightened suspicions that financial incentives may have biased the clinical trial and the reporting of its results.

Bjørn Lomborg and environmental science

In 2001, the Danish political scientist Bjørn Lomborg published *The Skeptical Environmentalist: Measuring the Real State of the World*. Lomborg systematically attacked what he called the "Litany," or the worldview common in most environmental scientific publications, that global environmental systems are collapsing due to resource consumption, population growth, and pollution. In effect, he argued that those espousing this view are like Morton. They have dressed an environmentalist ideology in the garb of scientific facts, while ignoring or misrepresenting data contradicting their position. Anticipating the controversy this book would stir, Cambridge University Press subjected it to an unusually rigorous peer-review process. Despite this, many scientists and environmentalists argued that Cambridge should not publish the book. Once it was published, criticism intensified. *Scientific American* ran a series of essays against the book titled "Misleading Math about the Earth." The Union of Concerned Scientists argued that Lomborg consistently misused or misrepresented data and selectively cited only the literature that would support his conclusions. Several scientists brought formal charges of scientific dishonesty against Lomborg to the Danish Committees on Scientific Dishonesty (DCSD). Their findings were mixed, but did conclude that the book was scientifically dishonest. Lomborg filed a complaint, and a higher ranking ministry annulled the decision by DCSD, clearing Lomborg of all charges.[13]

[11] Vreeland 1989, n.p. [12] Stern 1994. [13] See Pielke 2004.

Yanomami blood samples and anthropology

The Yanomami are indigenous Amerindians residing in the Amazonian rain forest on the border between Brazil and Venezuela. In the late 1960s, anthropologists collected blood samples from several Yanomami villages. The Yanomami claim they were led to believe that those blood samples would be used only briefly for medical research and then destroyed. But thousands of frozen blood samples still existed forty years later in laboratories around the world. Many of the individuals who donated the blood samples are no longer alive and Yanomami funerary practices require that all body parts and social remains of the dead be ritually annihilated. It is unacceptable to the Yanomami people to think that parts of their ancestors are still in a lab. Some claim that failure to return the samples constitutes a callous disregard for the Yanomami's beliefs, while others argue that the issue distracts attention from the more serious threats to the Yanomami of malaria and extractive industries. In 2005, a Brazilian public prosecutor sent letters to fifteen institutions that he believed held blood samples. Yet it was not until 2010 that researchers agreed to return the blood samples.

The blood sample debate is only one of multiple controversies surrounding anthropological work with the Yanomami. Journalist Patrick Tierney accused the anthropologists Napoleon Chagnon and James Neel of research misconduct. Tierney accused them of exaggerating the degree of violence among the Yanomami, of fostering violence by distributing weapons, and of fabricating data in a 1988 paper. The debate over this book continues, with some turning the tables and accusing Tierney of shoddy scholarship and even outright misconduct in leveling unfounded accusations.[14]

The psychology of torture

In 1966, the US Air Force created a training program, SERE (Survival, Evasion, Resistance, and Escape), that included mock interrogations designed to train soldiers to resist enemy interrogations. SERE techniques were modeled from Chinese interrogations used against US soldiers in the Korean War and included slaps, sleep deprivation, stress positions, wall-

[14] See Glenn and Bartlett 2009.

slamming, and waterboarding. During the 1980s, the psychologists Jim Mitchell and Bruce Jessen supervised the SERE program. Jessen eventually switched from supervising the mock interrogations to playing the role of mock enemy interrogator. After the September 11, 2001, terrorist attacks, Mitchell and Jessen wrote a proposal for the CIA (US Central Intelligence Agency) to turn the SERE techniques into an American interrogation program to be used against captured alleged Al Qaeda operatives. In 2002, with the backing of the CIA, the two psychologists first tested their techniques on Abu Zubaydah, initially described as Al Qaeda's number three, in a CIA jail in Thailand. Zubaydah was confined in a box, slammed into the wall, stripped, exposed to the cold, blasted with rock music to prevent sleep, and waterboarded eighty-three times. The techniques continued until Jessen and Mitchell decided that no more information was forthcoming. During that interrogation, the US Justice Department completed a formal legal opinion authorizing the SERE methods. Jessen and Mitchell reportedly made between $1,000 and $2,000 a day apiece and they had permanent desks in the Counterterrorist Center. Their methods were used on at least twenty-seven more prisoners until, in 2009, the Obama administration discontinued the interrogation program calling it one of the CIA's "mistakes" and defining waterboarding as torture.[15]

Plagiarism in India and physics

In 2007, Indian prime minister Manmohan Singh doubled financing for research, claiming that newly industrializing nations such as China and South Korea have "leapfrogged ahead of us by their mastery of science and technology."[16] Additionally, new rules linked the number of published papers to promotions and pay increases. The resulting pressures to publish and a lack of awareness about misconduct have been associated with a rise in instances of plagiarism. This is compounded by the lack of professional or government institutions capable of detecting scientific misconduct. To fill this lacuna, some Indian scientists established the independent ethics watchdog group, the Society for Scientific Values.[17] The most high profile case of plagiarism in India comes from the field of theoretical physics. In

[15] See Shane and Mazzetti 2009. [16] Neelakantan 2008, n.p.

[17] See the website for the Society for Scientific Values, www.scientificvalues.org/.

2002, a research group at Kumaun University headed by the university's vice-chancellor, B.S. Rajput, was accused of plagiarism.[18] The main allegation centered on a paper published by Rajput and his student S.C. Joshi that was later found to copy significant portions of another paper published six years earlier by Stanford physicist Renata Kallosh.[19] Over forty Indian physicists endorsed a website that made several other charges of plagiarism against Rajput. Dr. Kavita Pandey, head of the Physics Department at Kumaun University, claimed that she was suspended for blowing the whistle on the vice-chancellor's plagiarism.[20] Rajput resigned after a formal inquiry found him guilty of plagiarism. In the wake of the scandal, attention turned to the peer-review process. Why had reviewers not detected the multiple acts of plagiarism committed by Rajput?

A spectrum of conduct

Research misconduct results when the norms of science are violated. Norms can pertain to defining and planning research, investigating and conducting research, and reporting and disseminating research. Breaches of norms can involve such egregious behaviors as outright fraud or less clear cases of misconduct such as trimming data. Defining misconduct in some cases seems straightforward (e.g., Summerlin, Hwang, and Rajput), but in other cases is less so (e.g., Did Lomborg inappropriately manipulate data? Was there an inappropriate conflict of interest in the Retin A study?). Scientific misconduct is, then, a broad and potentially vague term.

This is to be expected for two reasons. First, scientific conduct is not simply rule-following. Rather, it depends on intuition, judgment, and tacit knowledge. This means that identifying cases of misconduct is not usually as simple as identifying when someone did not stop at a stoplight or added two cups of sugar instead of one. Efforts to produce knowledge that is empirically based, community certified, and practically reliable involve multiple complex methods and sometimes depend on the "golden hands" of embodied expertise or insightful paradigm shifts. This means that in many instances the difference between boldness, mistakes, and misconduct can be debated. Second, the norms of science are generalized ideals – putting them into practice in particular contexts often

[18] See Ramachandran 2002. [19] See Joshi and Rajput 2002. [20] Kazmi 2002.

shows how fuzzy the boundaries are between creativity and bending or violating the norms.

Rather than paint it in stark terms, then, it is better to conceive of scientific conduct along a spectrum from the ideal behavior of RCR or GSP through questionable research practices (QRP) to the worst behavior of FFP (fabrication, falsification, and plagiarism). Articulations of the ideals and what counts as their violation can be found in codes of conduct, governmental regulations, institutional policies, and informal mentoring and education processes.[21] The vagueness of the term, lack of data, and biases toward either over- or underreporting have made estimating the prevalence of misconduct difficult. One influential survey of miscounduct reported roughly 2 percent of scientists admitting to FFP and nearly 34 percent admitting to QRP. But when asked about the behavior of their colleagues, admission rates were 14 percent for FFP and 72 percent for QRP. The study notes that these are likely conservative estimates of the actual prevalence of scientific misconduct.[22]

In Europe, there is not yet a widely instituted or universally accepted definition of misconduct. The UK, Denmark, Norway, and Germany tend to employ relatively broad definitions of misconduct (or "scientific dishonesty"). For example, the UK Wellcome Trust Fund includes FFP and "deviations from accepted practices."[23] In the US, broad definitions of misconduct have also been proposed. In 1986, for example, guidelines established by the US National Institutes of Health defined scientific misconduct as FFP or other acts that deviate from commonly accepted practices within the scientific community.

Subsequent reports by other US federal agencies, however, proposed narrowing the definition to just FFP. This was motivated in part out of concern that the clause about "other serious deviations" was too vague to be enforceable. Such a broad definition also invites an overexpansive

21 For some important articulations of normative ideals in the sciences see Institute of Medicine and National Research Council 2002; National Academy of Sciences, National Academy of Engineering, and Institute of Medicine 1992; and National Academy of Sciences, National Academy of Engineering, and Institute of Medicine 2009.

22 Fanelli 2009, n.p.

23 Council of Science Editors, *White Paper on Promoting Integrity in Scientific Journal Publications*, 2012 update, n.p., Council of Science Editors website, www.councilscienceeditors.org/i4a/pages/index.cfm?pageid=3644.

use that might punish honest mistakes and stifle unorthodox approaches to research. Since 2000, the US federal-wide research misconduct policy states simply, “Research misconduct means fabrication, falsification, or plagiarism in proposing, performing, or reviewing research, or in reporting research results.”[24] Fabrication is making up data or results that are then reported. Falsification includes manipulating, omitting, or changing data or results. Plagiarism is the appropriation of another’s work without giving due credit, or the misappropriation of intellectual property. Other behaviors can also constitute misconduct that is legally sanctioned by some federal institutions, including failure to report misconduct, obstruction of investigations, abuse of confidentiality in peer review, and retaliation against whistle-blowers.

In addition to this narrow legal framing of the term, misconduct is still used more broadly to label actions that are ethically controversial or questionable. Such QRP include shingling of publications (duplicate or near-duplicate publications), self-plagiarism, citation amnesia (failing to cite important works in the field), careless data management, unacknowledged enhancing of digital images, honorary authorship, and excluding data from publication. The term could even apply to extreme instances of gullibility or credulousness as in the “Piltdown man” case. QRP and misconduct do not apply, however, to error (where there is no intent to deceive) or most differences in judgment regarding research design, statistical analyses, or data interpretation. What is important is to clearly acknowledge and explain as much as possible what procedures have been used for selecting and interpreting data. When this fails to happen, conduct can shade into unethical practices, as in the Morton case where relevant samples were excluded for no valid reason. The ideal of objectivity requires scientists to be as aware as possible of their biases and to take measures to counteract or at least disclose them.

Other instances of data management can make the determination of misconduct difficult and contentious. For example, the Lomborg case and other controversies in environmental science hinge not on intentional FFP, but the selection and interpretation of data. Depending on the methods and interpretations chosen by any given researcher, science paints a variety of

[24] “42 C.F.R. § 93.103 Research misconduct,” *Justia.com*, http://law.justia.com/cfr/title42/42-1.0.1.8.71.1.29.4.html.

often conflicting pictures of the world. This can result from inappropriate biases. Yet it is not always the case that one party has true science on its side while another is misled or distorting the truth. For example, a data set may demonstrate that forest cover is increasing, but the reason for this may be an increase in forest plantations rather than recovery of more natural systems. Thus, the fact of more forest cover leaves room for interpretation about its meaning.

The flow 1: anticipating research

The term "scientific research" connotes the activities involved in creating and clearly formulating testable hypotheses, exploring hypotheses in laboratories, field studies, or simulations, and drawing and publishing conclusions that contribute to certified knowledge. This definition suggests a flow of research from planning projects through conducting research to reviewing and reporting results. Research flows from anticipating (in the office and in proposals), to doing (in the field, laboratory, or virtual environment), to disseminating (in conferences, publications, and popular media). Throughout this flow, researchers face questions about what constitutes responsible scientific practices. The categories of anticipating, doing, and disseminating are artificial constructs with fuzzy boundaries. Science often involves feedback in which disseminating influences anticipating and doing. But as a heuristic the distinctions are useful, although the topical categories that follow should be read with flexibility in mind.

Research begins with a question, an idea, or a hypothesis. From the very beginning ethical issues arise: Which hypothesis should be pursued? Are there questions that should not be asked? How can this idea be tested responsibly? Although they pertain to the entire flow of research, three issues are particularly important at the stage of planning and anticipation: (a) mentoring; (b) conflict of interest; and (c) judging the value of the research.

Mentoring

Mentors have responsibilities not only to transmit knowledge and skills, but also to initiate the next generation into the traditions and values of the scientific enterprise. Unlike an adviser, a mentor oversees not just the conduct of research, but the personal and professional development of his or her mentees. This special responsibility is crucial at the anticipation phase, because the mentor's experience and judgment are necessary for informing the conceptualization of a research project and the distribution of responsibilities for bringing it to fruition. From the inception of a project, mentors also play a leading role in ensuring the collegial atmosphere and accountability necessary for successful collaboration.

The mentor–mentee relationship is defined by its imbalance of power, knowledge, and experience. It is a fiduciary relationship, wherein the powerful party is entrusted with protecting the interests of the vulnerable party. Exploitation in such relationships is behind many instances of QRP and misconduct. Mentors may abuse their power by overworking their students, excluding them from the planning stages of grant proposals, failing to give them proper credit, discriminating against them, or failing to advance their careers. Yet those who strive to be good mentors often face obstacles such as lack of time and few incentives for effective mentoring.

The 1967 discovery of pulsars by Jocelyn Bell, then a twenty-four-year-old graduate student, is an important example of the difficulties associated with allocating credit between mentors and mentees. Under the supervision of her thesis adviser, Anthony Hewish, Bell was in charge of operating and analyzing data from a radio telescope. One day she detected "a bit of scruff" on the chart. She remembered seeing the same signal earlier, so she measured the period of its recurrence and determined it must be coming from an extraterrestrial source. Bell and Hewish later found more examples and together with three others published a paper announcing the discovery of what came to be known as "pulsars." Hewish was awarded the Nobel Prize; Bell did not share in the award. Some claimed that her recognition of the signal was the key act of discovery. But others, including Bell herself, said that she received ample recognition and was only doing what graduate students are expected to do.

Conflict of interest

Conflicting commitments – the need to divide limited time among various responsibilities – often pose dilemmas about prioritizing time, but they do not usually threaten the integrity of science. This is not the case with conflicts of interests. One good formulation of this concept comes from Michael Davis, who argues that

> a person has a confict of interest if, and only if, that person (a) is in a relationship with another requiring the exercise of judgment in the other's behalf and (b) has a (special) interest tending to interfere with the proper exercise of such judgment.[25]

All researchers have many interests that may at times conflict with one another. Scientists are responsible for thinking through potential conflicts in the anticipatory phase, before they become real and cause harm. Once a conflict of interest is in place, it can infect the entire flow of research.

Conflicts of interest can exist at both the individual and institutional levels. Those involving financial gain and personal relationships are the most important. Researchers are entitled to benefit financially from their work. But in some cases the prospect of financial gain may inappropriately influence the design of a project, the interpretation of data, or the dissemination of results. A clear example is the biased research conducted by the Center for Indoor Air Research funded by the tobacco industry. Similarly, funding agencies often require scientists to disclose personal relationships, because they may bias judgments about the worthiness of grant proposals. Even if a conflict of interest is only apparent, it can damage reputations and undermine trust.

Conflicts of interest can undermine scientific integrity and objectivity in two ways. First, they can affect a person's thought processes (judgment, perception, decision-making, or reasoning). The conflicting interest can bias or skew these processes. Second, a conflict of interest can affect motivation and behavior. That is, a scientist may be perfectly capable of sound thinking but might fail to carry it out due to temptations. Scientists need not be aware of conflicts of interest in order for them to impact thought and behavior, and even small gifts can exert subconscious influences. Many studies of articles published in biomedical journals show that a study sponsored by a pharmaceutical company is more likely to favor the company's products than a study funded by the government.[26] Conflicts of interest, both real and apparent, can undermine the trust placed by colleagues and by society in scientific research.

[25] Davis 2005, p. 402. [26] See, for example, Ridker and Torres 2006.

Judging the value of the research

When planning a project, researchers face difficult questions that should be confronted explicitly. Is the research really worth doing? Whose interests will it serve? Are there possible negative side effects? What are the justifications: making money, gaining notoriety, advancing theoretical understanding, developing applications, for military purposes, etc.? Researchers should consider if these reasons are morally justifiable and consistent with their obligations and integrity as scientists.

Scientists must make claims to private or public benefactors about the value of their proposed work. The scarcity of funds compared with the abundance of potential scientific pursuits pressures scientists to make exaggerated claims about the import of their work. This poses questions about what constitutes ethical promising and how to distinguish justifiable claims from unjustified hype. This issue has been especially acute with embryonic stem cell research where claims about the potential health benefits have occasionally stretched any reasonable assessment of likely outcomes. Irresponsible promising can not only raise hopes that are later dashed but may even constitute a type of dishonesty on a par with other acts of misconduct.

The flow 2: doing research

Once research has begun, new ethical issues are added to those discussed above. These include: (a) objectivity, inferences, and data management; (b) bias and self-deception; (c) trust; and (d) values embedded in research design.

Objectivity, inferences, and data management

Researchers draw conclusions based on their observations. They make inferences from data that are almost always incomplete and imperfect. How these data should be treated in the process of inference lies at the core of the ethics of doing research. What counts as a valid inference?

Even before a fact or data point is collected, scientists make decisions about where and what to investigate. Interpreting data entails yet further human acts of framing and meaning-making. The data a chemist sees, for example, is a line spiked high on a graph printed out from a gas chromatograph. When she points to this line and says "that is oxygen," she is drawing from a rich theoretical framework to interpret data as meaningful.

For the ideal of objectivity to guide practice it cannot mean that human perspectives are stripped from science. Rather, it means that in making the many unavoidable decisions and interpretations, scientists are guided by scientific norms as discussed in Chapter 3, especially behavioral norms of honesty, carefulness, open-mindedness, and skepticism. This ideal is compromised by dishonesty, carelessness, bias, and self-deception. Furthermore, the ideal means that the scientific community has structures to mitigate these corrupting influences such as peer review and experiment descriptions that allow replication.

Scientists may be tempted to inappropriately alter the data in order to present a case that is stronger than the data warrant. Such dishonesty amounts to clear-cut misconduct when the alterations include the outright fabrication or falsification of data. However, the issue is not often so simple. Disagreements often exist about when certain data may legitimately be considered outliers and thus appropriately ignored in reporting findings. Statistical tests and procedures can be used in questionable ways or ways that are not fully disclosed to the reader. Researchers may manipulate digital images in a variety of ways and to various extents. This was an issue in the Hwang case, as investigators discovered that his team had doctored photos of two authentic stem cell colonies to give the false impression that they had created eleven such colonies.

Misleading data can also result from carelessness in experimental design, measurements, or record-keeping. Responsible researchers must strive to avoid negligence, haste, and inattention in their work. The standards of data collection and management vary between disciplines, but it is widely acknowledged that researchers have an obligation to create an accurate and accessible record of their work sufficiently detailed to allow for checking and replication. This requires that researchers keep orderly and secure notebooks or electronic files. Beginning researchers often receive little or no formal training on these important topics.

Many misconduct investigations have raised questions about standards of care for recording, analyzing, and storing data. For example, in 1986 Nobel Prize-winning biologist and then professor at MIT, David Baltimore, coauthored a paper in the journal *Cell* with five others, including MIT colleague Tereza Imanishi-Kari. The paper reported a novel finding in the genetics of immune systems. Margot O'Toole, a postdoctoral fellow in Imanishi-Kari's lab, reported concerns about the paper after a year of unsuccessful attempts to replicate the study and after some of her own experiments produced contradictory results. When the US Congress subpoenaed Imanishi-Kari's notebooks, she admitted that she did not really have any. She only had disorganized sheets of paper. In the days before the hearing, she quickly bound them into a notebook. The investigators claimed they found clear signs of fraud: data were overwritten in different colors of ink, findings were erased, and dates were changed. At one point, Imanishi-Kari could not even make sense of the meaning of some of her numbers. She admitted to poor record-keeping but always maintained she was innocent of misconduct.[27]

Another example of carelessness is the practice of citing articles without actually reading them. One study found 391 biomedical articles over a ten-year period that cited retracted papers (the papers had often been retracted because of misconduct).[28] Scientists may often not take the time to study the growing reservoir of scientific knowledge. Instead, they may simply copy citations from secondary sources. Responsible conduct would seem to require that researchers actually read any articles that they cite. Otherwise, their own work may be contaminated by the errors or misconduct of others.

Bias and self-deception

Processes of inference can also be distorted by biases, which are systematic or nonrandom errors. For example, Morton's unconscious bias led to invalid inferences. Biases can also stem from consciously made false assumptions, such as the assumption of craniometrists that human head sizes and shapes determine personality traits and intelligence. This indicates why biases can be difficult to identify. They require an independent source of verification outside of a community of practitioners. If an entire field of science accepts the same bias, then it will not be identified. This

[27] See Kevles 1998. [28] See Howard 2011.

means that biases are not always best considered unethical. They may be more akin to hypotheses that are later proven wrong. Craniometrists, for example, may have conducted careful, honest, and responsible research and, as is the case with the progression of science, their biases or hypotheses were eventually discarded. Nonetheless, biases can stem from racial, patriarchal, or other assumptions, which again points out the importance of scientists' skepticism about the assumptions behind their research design and interpretation of data.

Self-deception is perhaps the greatest threat to the ethical ideal of scientific objectivity. It often stems from carelessness and wishful thinking. Hoping that his or her theory is true, a researcher may fall into the trap of experimenter expectancy, or seeing only what he or she wants to see. Self-deception is not intentional fraud – the researcher truly believes that he or she has not manipulated the data to accord with a preferred outcome. Yet there may be some self-awareness involved, as is the case, for example, when a researcher omits data that give the "wrong" answer.

Expectancy contributes to self-deception, which in turn leads to credulity. The Piltdown man hoax is one example of an entire community falling prey to a common delusion. Another example is the 1903 discovery of the N-Ray by the French physicist René Blondlot. Over the next three years over 100 scientists wrote more than 300 papers on N-Rays. Even after the American physicist R.W. Wood demonstrated that N-Rays were nothing more than an "observer effect," several French physicists continued to support Blondlot's work.

So self-deception is dangerous, because it can dupe entire communities into a set of false beliefs. But there is a danger in self-deception even when it leads to beliefs that later prove correct. For example, the English physicist Robert Hooke believed strongly in the Copernican heliocentric theory of the solar system. Proving the theory required observing a stellar parallax – a perceived difference in the position of a star due to the Earth's motion around the Sun. Hooke observed a star with a parallax of almost 30 seconds of arc. Yet, as it turns out he only observed what he wanted to see. There is a stellar parallax, but it is very small (about 1 second of arc); in fact, it is too small to be detected by Hooke's relatively crude telescope.[29]

[29] See Broad and Wade 1983 for more on the N-Ray and Hooke stories.

That heliocentrism later proved correct does not justify holding that belief as a result of credulousness or wishful thinking. For the ethical ideal of objectivity, getting the right answer is not most important. How that answer is derived is the key. It cannot be the result of blind faith or obedience, of expediency, or of deception, intentional or otherwise. As Jacob Bronowski exhorted his fellow scientists, "If we silence one scruple about our means, we infect ourselves and our ends together."[30] One landmark study reported that bias in science trends toward a pervasive over-reporting and over-selection of false positive results.[31] Another study in 2012 reported that researchers were only able to confirm six of fifty-three "landmark studies" in preclinical cancer research.[32]

Trust

Trust is essential to the conduct of science because understanding the world is a task that is far too big for any single individual to undertake successfully. Even describing the particulars of a small slice of reality – say, cellular metabolism or the marriage customs of a tribe – requires the collective efforts of several researchers. As Newton remarked in a 1676 letter to Hooke, "If I have seen a little further it is by standing on the shoulders of Giants." Because scientific knowledge is built up communally, its objectivity depends on the intersubjectivity of human communication. It follows that scientists must be able to rely on one another to be truthful. As Bronowski put the point: "We OUGHT to act in such a way that what IS true can be verified to be so."[33] In short, facts about what is the case rely on the values necessary for "objectivity."

If scientific predecessors conduct careless or dishonest work, then their shoulders will not be reliable perches for seeing further. Each member of an increasingly networked scientific community that relies on more and more specialized domains of expertise must trust in the work of all the others. Scientists have neither the time nor the expertise to independently verify every finding derived from the work of others; and in an endeavor that values priority of discovery, they certainly do not have the motivation.

[30] Bronowski 1956, p. 66. [31] Ioannidis 2005. [32] Begley, Glenn, and Ellis 2012.
[33] Bronowski 1956, p. 58.

Values embedded in research design

We have argued that the ideal of objectivity does not mean the absence of values or a view of the world somehow removed from human interests and perspectives. Rather, the ideal requires a critical awareness, explicit recognition, and rational defense of the values and perspectives that are unavoidable aspects of the human quest for knowledge. When scientists make decisions about equations, models, constants, and variables, they often must make certain assumptions that amount to the embedding of values in their experimental design. The ideal of objectivity demands self-awareness and an explicit justification of such choices.

The example of integrated assessment models (IAMs) to analyze climate change management strategies will illustrate the point.[34] IAMs are models of the global climate–society system. Environmental scientists and economists use IAMs to study various social responses to climate change and identify an economically optimal trajectory of investments in reducing greenhouse gas emissions. This requires choices about the criteria to define "optimality" (the objective function of the underlying optimization equation), and these choices are necessarily value-laden.

For example, some IAMs frame the goal of climate management strategies in terms of optimizing "utility" as a measure of time-aggregated societal wealth. These models sometimes assume a definition of utility that does not distinguish between situations of evenly distributed consumption and those where the wealthy consume a lot more than the poor. In other words, the scientific model makes a typical utilitarian value calculation where total utility matters, but the distribution of utility does not. The model may further assume that utility is a sufficient proxy metric for human happiness and that this can be adequately measured in terms of money and consumption. In other words, the model assumes that aggregated global utility is the ultimate social goal. These are all value judgments that are intrinsic to the model.

IAMs entail other value judgments as well, such as the choice of a utility discount rate (used to compare the value of future utility with that of present utility). Those who design the equations that govern the model must also make choices about how to quantify climate-related damages. Some,

[34] See Schienke et al. 2010.

for example, convert various climate change impacts such as droughts and floods into units of money and utility, and this conversion of course entails further assumptions about values. There are further value judgments to be made about the reliability of the model for guiding policy decisions.

For any scientific method or model that attempts to measure costs, risks, and benefits, these terms can be defined in a variety of ways and the chosen definition creates a certain way of framing the issue. In making one decision rather than another, a researcher creates and measures one reality rather than another. What counts as a cost, a benefit, a risk? Whose interests are included? The ideal of objectivity is not to avoid or eliminate these value judgments. Rather, it is to make them transparent and explicit and to justify them rationally while remaining open to the potential merits of alternative formulations. For example, perhaps a better way to structure IAMs is not with globally aggregated utility, but with utilities disaggregated by region or nation. African utility and consumption could be optimized separately and weighted equally with North American utility and consumption. This may be a more just or fair calculation given the high probability that damage from climate change will be borne primarily by poorer populations where people benefit little from the fossil fuel consumption by the wealthy that causes the damage.

The flow 3: disseminating research

As a communal enterprise, science depends on outlets (e.g., conferences, journals, and press releases) for disseminating information. As an activity governed by norms that define acceptable practices, science institutes the gate-keeping or quality control mechanism of peer review to ensure the work is sound. As a career, science requires ways to grant recognition for contributions to communal knowledge. As a commercial enterprise, research often generates intellectual property with restrictions on its dissemination. Thus, this section takes up the issues of: (a) peer review; (b) authorship and allocation of credit; and (c) intellectual property.

Peer review

In the seventeenth century, Newton and many other natural philosophers would keep new findings secret so that others could not claim the results as their own. Henry Oldenburg, secretary of the Royal Society, solved this problem by guaranteeing authors in the society's *Philosophical Transactions* both rapid publication and the support of the society if an author's claim to priority of discovery was questioned. Oldenburg also pioneered the practice of submitting manuscripts for review by experts prior to publication. These innovations evolved into the modern scientific journal and the practice of peer review. Together, they create a system that rapidly disseminates high-quality information and rewards authors with recognition through the practice of citation.

Since only scientific peers have the requisite knowledge, many important decisions about publication, hiring, promotion, tenure, the awarding of degrees, and funding depend on the peer-review system. Ideally, in terms of the dissemination of research results, this system eliminates errors, deceptions, and biases and prevents the publication of substandard research. In reality, however, many flaws slip through the system. This is partly because scientists rarely have sufficient time, incentive, or resources to approximate the ideal. The breakdown of peer review can also result from unethical behavior on the part of editors and reviewers. Most importantly, they may possess conflicts of interest that bias their findings, and they may violate the confidentiality of the work under review by stealing ideas, theories, or hypotheses.

New media technology has made peer review a contested domain in many professions. Mainstream journalism, with its fact-checking and gate-keeping practices, is under onslaught by blogs and other forms of new media. Under the influence of WebMD and other websites, physicians have lost their monopoly on the dissemination of health information to their patients. Similarly in science, not all pathways of scientific dissemination pass through the gate of peer review. New and emerging techniques for publishing research online often increase speed and access but at the cost of bypassing the quality control of peer review, although it could be argued that peer review happens in such cases as a postpublication process. One curious case study is the journal *Rejecta Mathematica*, which publishes papers that have been rejected by peer reviewers at other journals. The journal's website lists several ways in which previously rejected papers can be of value to the scientific community.

Furthermore, researchers have occasionally made premature announcements of their work to the press, as in the 1989 press conference about cold fusion at the University of Utah. In that case, scientists and a university eager to capitalize on a revolutionary development by staking priority of discovery disseminated their results at a press conference prior to publishing them in a peer-reviewed journal. The lesson from this case and others is that researchers should be very cautious in bypassing the peer-review system. If the preliminary results that are released to the public are later proven to be incorrect, then the effort of other researchers is wasted and public trust in science is undermined. Of course, given the flawed nature of peer review, these outcomes can never be avoided for certain, and in some cases urgency may rightfully preempt the peer-review process. In the 2009 swine flu pandemic, for example, scientists faced the difficult task of being responsive to the public's demand for knowledge while remaining honest about the knowledge gaps and uncertainties of a rapidly evolving situation.

Peer review is both epistemological *and* political – it is a matter of deciding who has the relevant knowledge *and* the power to make decisions. Accordingly, we will return to issues of peer review in Chapters 9 and 10, especially as we consider ways in which scientific peers are being asked to assess not only the intellectual quality of research but also its ethical and social implications. The "peer" category is currently a site of important flux and contestation. Who should count as a peer, that is, who ought to have the power to determine the value of research?

Authorship and allocation of credit

The practice of citing others' work reflects a core ethical principle of fairness and accountability in research: to give credit where credit is due. Failure to uphold this principle can amount to plagiarism. The principle also has a flip side: to not give credit where credit is not due. The practice of honorary authorship, listing undeserving authors (often because they provided financial support), violates this aspect of the principle. Responsibility and credit are closely related in this dual principle, because it states that a person should only receive credit to the extent that he or she can take responsibility (deserves praise for new knowledge or blame for errors) for the work.

Deciding the appropriate allocation of credit and responsibility is often made difficult by the communal nature of science. Mentors, for example, may insist on being listed as first author for any research issuing from their laboratory even if they have not been actively involved in that particular study. Is this fair because, without the mentor's support, the study would not have existed? Or is this unfair because only direct intellectual contributions should count in the determination of authorship? But what counts as an "intellectual contribution"?

Some scientific disciplines publish papers with dozens of authors because the study entailed the collaboration of multiple forms of expertise and even multiple laboratories. Does this mean that each author can legitimately be held responsible for the entirety of the publication? If a mistake or an instance of misconduct is discovered in just one element of the study, it is not obvious that all of the authors would be equally blameworthy. Some have proposed modifying the practice of authorship by assigning credit to individual authors only for the piece of the study that resulted from their direct contribution. This would clarify questions about responsibility but it would do so at the cost of further Balkanizing the research system. It would also introduce novel questions about how to weigh partial credit or credit for part of a publication. How would this compare with a single-author or a traditional multiauthor article?

A fundamentally important question bearing both on peer review and the allocation of credit is: What metrics should be used to determine a scientist's excellence, influence on the field, and value to society? The easiest metric is simply to count his or her number of publications, but this does not measure the quality of the work or the impact factor of the journals that print it. Incentivized by metrics that reward sheer quantity of publications, authors are often tempted to shingle their work or to divide it into the "least publishable unit" in order to inflate their numbers. Another strategy is to use a citation index that ranks scientists according to how often their work is cited. This may be a better metric, but it does not capture the reasons why the scientist is being cited or what qualitative influence their work has had on their field or beyond. Furthermore, there are biases built into the system such that established researchers and standard approaches are more likely to be cited than newer researchers or novel approaches. And citation search engines do not always cover all scholarly journals. Another metric is to count the patents that result from a scientist's work. But not all fields of science aim for applications, at least not in the short term.

The peer-review system and the allocation of credit revolve around the same vital questions: What constitutes excellent research? Who deserves praise and why? The metrics used to evaluate scientists are proxy answers to these questions and thus deserve careful ethical scrutiny.

Intellectual property

When scientific discoveries have profit potentials, oftentimes dissemination is restricted through the mechanisms of intellectual property. Intellectual property is a legal monopoly over creations of the mind that grant their owners certain exclusive rights to a variety of intangible assets such as discoveries, designs, and processes. Examples include patents, trademarks, copyright, and trade secrets.

Intellectual property rights create financial incentives to pursue new creations by granting their owners the right to profit from ideas, and they provide the wherewithal to recoup the costs of R&D. Indeed, this is the primary moral justification for intellectual property: it rewards the parties responsible for taking financial risks to create something of social value, by allowing them to profit from their work. Defenders of the practice argue that if the protections of intellectual property rights are not in place, then entrepreneurs will not invest in research and society will suffer. This is why the US Constitution permits federal patents intended to "promote the Progress of Science and useful Arts" (art. I, §8, clause 8).

Nonetheless, the role of intellectual property in science is controversial. The profit motive arguably inhibits the free flow of information and influences the kinds of research that are undertaken. For example, pharmaceutical research, which is overwhelmingly profit driven, is not surprisingly geared in the main toward the diseases that afflict wealthy nations. Might intellectual property skew science away from helping those who need it the most? Should scientists seek monetary gain through patents for their work? Or does this contradict the norms of science, which call on scientists to only seek the progress of knowledge and the recognition of their peers? Furthermore, controversies exist about what should be considered "intellectual property." Is a gene or an organism something that can or should be patented? Should indigenous or traditional forms of knowledge be considered intellectual property? Another contemporary issue with intellectual property in science is the enforcement challenges posed by digital technologies that can cheaply and quickly copy and distribute information.

Consider briefly just one of these controversial issues, namely, the patenting of human genes. The Human Genome Project (1989–2003) created a database with enormous medical and commercial potentials, which quickly raised the question of intellectual property: Who, if anyone, would own specific genes or even the entire human genome? Patents are exclusive rights granted by a government to an inventor that exclude others from making, using, and selling the invention. A gene patent is a patent on a specific gene sequence, its chemical composition, or the process of obtaining it. It is a subset of biological patents, which have existed since the 1906 US ruling that purified natural substances can be patented because they are more useful than their nonextracted, natural states (adrenaline was the first such substance to be patented). The 1970 US Supreme Court case of *Diamond v. Chakrabarty* clarified that an organism can be patented as long as it is truly "man-made," such as through genetic engineering – although this finding remains in flux as science and law coevolve.

By 2010, roughly 40,000 US patents existed relating to about 2,000 human genes. This does not mean that anyone owns genes that exist in any human body, as all genes in the human body (to date) are natural products (although genetic therapy muddies the issue). Rather, the patents cover isolated genes, methods of using them to manufacture products (e.g., drugs or consumer goods), and methods to use the genes to diagnose diseases.

In 2009, the American Civil Liberties Union together with several scientific and patient advocacy organizations filed a case against Myriad Genetics seeking to invalidate and discontinue all patents for naturally occurring genes.[35] A US District Court Judge ruled in favor of the American Civil Liberties Union, arguing that several of Myriad's patents, including some on the breast cancer genes *BRCA1* and *BRCA2*, had been improperly granted. The ruling hinged on the argument that DNA is fundamentally different than adrenaline or other chemicals found in the body. The ordering of nucleotides defines the construction of the human body, so merely isolating it in an unaltered state is not enough to change its status as a natural product. A later brief from the US Department of Justice upheld

35 See Begley 2010; Schwartz and Pollack 2010.

the district court ruling that “genomic DNA that has merely been isolated from the human body, without further alteration or manipulation, is not patent-eligible.”[36] This ongoing controversy about when a gene is “natural” (thus discovered) and when it is “man-made” (thus invented) has far-reaching implications for the biotechnology industry and the ethics of scientific research.

Research ethics in a global context

For much of the twentieth century, scientific research was concentrated in a small set of countries. Since the last decades of the twentieth century, science has become increasingly and genuinely global. In addition to South Korea, China and India are now often cited as emblematic of this new geography of science. China is in the midst of the most ambitious science-funding program since the US undertook its race to the moon in the 1960s. The Chinese government has set a target for investing 87 billion euros in research by the year 2020. Over a twenty-year period from the late 1980s to the early 2000s, India’s investment in biotechnology quadrupled. The breakneck pace of globalizing science and the overwhelming emphasis on economic growth pose challenges for ensuring that emerging scientific powers conduct responsible research. Consider the following reports from India and China.

In *The Hindu*, a national Indian newspaper with a circulation approaching 1.5 million, the science journalist N. Gopal Raj began a 2002 opinion piece on “Scientific Misconduct” by noting how the practice of science depends on trust. Yet “reports keep surfacing from various countries about work being plagiarized, results which were doctored and data fabricated.”[37] Raj mentioned four cases from India: fraudulent fossil discoveries (by V.J. Gupta of Punjab University in the 1980s), plagiarism (by R. Vijayaraghaven of the Tata Institute of Fundamental Research, 1995), questions of fraud (by B.K. Parida of the National Aerospace Laboratories, 2001), and charges of multiple plagiarism (as noted above, against B.S. Rajput, vice-chancellor of Kumaun University, 2002). Raj goes on to quote from an editorial in

[36] *Association for Molecular Pathology v. Myriad Genetics* (11–725, October 29, 2010), US Court of Appeals for the Federal Circuit, p. 10, *New York Times* website, http://graphics8.nytimes.com/packages/pdf/business/genepatents-USamicusbrief.pdf.

[37] Raj 2002, n.p.

Current Science (Bangalore, India), complaining that "scientific misconduct cases are rarely pursued and publicized in India." Precisely because as "the world leader in scientific research, the US has done the most to come to grips with scientific misconduct," Raj argues the need for India to better define scientific misconduct and establish clear procedures for addressing cases when they arise.

In 2010, sociologist Cong Cao made a related argument with regard to China. As he wrote, "China's path toward becoming an innovation-oriented nation by 2020 … will be significantly derailed if the nation does not make serious effort to eradicate misconduct in science."[38] After citing one case of charges that scientists from Jinggangshan University had fabricated as many as seventy papers, Cao went on to maintain that scientific misconduct was widespread and "likely more serious than any observer of Chinese science could imagine." Indeed, he noted that some estimates suggest that as much as "one-third of Chinese researchers have engaged in some sort of problematic practice." At the same time,

> The institutional watchdog responsible for investigating, exposing and punishing deviance exists on paper only, largely because of the lack of autonomy in the scientific community. And it is extremely difficult, if not impossible, to sanction high-profile scientists because of the interference from both the persons who commit the fraud and the political leadership who made them preeminent in the first place.

Cao suggests that "China's research community has adapted to an environment in which the influence of commercialism has been powerful and the bureaucracy has become seriously corrupt."

A few years later, a related report in *Science* quoted Xu Liangying, a Chinese historian of science, to the effect that the root cause of lack of both integrity and creativity in science is the declaration of Deng Xiaoping, in his post-Mao rehabilitation of science and technology as productive forces. As the reporter noted, "the Chinese words for "science" and "technology" have become fused into "scitech" … In China, science is expected to contribute directly to economic development and not to the pursuit of truth and knowledge."[39]

In China, a kind of crude pragmatism may thus aid and abet scientific misconduct. At the same time, and paradoxically, former Chinese Academy

[38] Cao 2010, n.p. [39] Hao 2008, p. 666.

of Sciences president Zhou Guangzhao, who criticizes the contemporary situation in which "success is often scored by quantity rather than quality," remembers with nostalgia the period during the 1950s and 1960s when scientists working on the Chinese atomic and hydrogen bombs worked collectively and creatively in pursuit of strengthening national military capabilities.

Such tales illustrate the cultural and values differences that can affect both formal governance frameworks and informal scientific practices. At an extreme, developing countries may serve as "ethics-free zones" where ethical oversight is simply nonexistent. In attempting to avoid such scenarios, the question arises as to whether scientific research must be governed by a universal ethical code.

In July, 2010, the Second World Conference on Research Integrity met in Singapore in order to craft such a code. The preamble of the "Singapore Statement on Research Integrity" asserts that there are "principles and professional responsibilities that are fundamental to the integrity of research wherever it is undertaken." Scientific research must adhere to a universal set of ethical principles. Yet even universal codes such as the UN Universal Declaration of Human Rights can be enacted and interpreted differently in different local contexts. So, as science globalizes, does it need a universal ethical code, what would such a code look like, and how much interpretive flexibility can be permitted for diverse local approaches?

Summary

Careerism, commercialization, Big Science, and human weaknesses are some of the major realities of science that can conflict with its ideal norms. When the norms are undermined, the result is misconduct and questionable research practices. Misconduct can be defined narrowly as fabrication, falsification, and plagiarism or more broadly to include other practices that seriously deviate from widely accepted standards. A broader definition may help to identify inappropriate behaviors, but it also may stifle novel approaches to science.

Throughout the flow of research – anticipating, doing, and disseminating – scientists face difficult choices about applying the norms, and powerful temptations to stray from the norms. Anticipating research raises issues about the appropriate roles of mentors, identifying potential conflicts of interest, and asking substantive questions about the value of the proposed work. Doing research involves upholding the standards of objectivity. This means upholding norms of honesty, carefulness, and open-mindedness in the interpretation and management of data and critically evaluating assumptions that might be built into research designs. Objectivity is threatened by bias and self-deception and is dependent on trust. Disseminating research poses questions about peer review and quality control, authorship and allocation of credit, and the appropriate role for intellectual property rights in scientific research. As a globalizing enterprise, scientific research is engaged in a dynamic tension between diverse local practices and standard, universal principles.

Extending reflection: Victor Ninov and Jan Hendrik Schön

It often seems like scientific misconduct is associated more with the socio- and biomedical sciences than with the physical sciences. In the 1988 Nova program "Do Scientists Cheat?" all eight cases of scientific misconduct involved anthropology, psychology, or medicine.[40] Since the post-World War II rise of concern for the ethics of science, the physical sciences largely escaped scrutiny. Any scrutiny was directed more toward physics–society relationships (think the atom bomb) than toward physics–physics relationships. When a physical science case did burst on the scene – such as that of the "cold fusion" claims of Fleischmann and Pons in 1989 – it was nipped in the bud by failures to replicate. Because of their theoretical and experimental rigor, the physical sciences seemed immune to the need for RCR education, which after all had originated in the National Institutes of Health.

Shortly after the turn of the millennium, however, the apparent purity of physics – as queen of the physical sciences – was subject to dramatic challenge. In 2001 and 2002 on both the west and east coasts of the US, it was revealed that physicists from two prestigious laboratories had practiced years of fraudulent research. As *Scientific American* reported at the time, "The physics community's collective jaw dropped this past summer when allegations of fraud were raised against two of their own [creating] a

[40] *Nova*, Public Broadcasting Service, Arlington, VA, October 25.

wake-up call for a field that has considered fraud within its ranks a freak occurrence."[41]

At Lawrence Berkeley, a world-class, federally funded US national laboratory in California, during 2001 it became clear that the nuclear chemist Victor Ninov had since the mid-1990s been fabricating evidence to support discoveries of new elements. Initial exposure centered on fabricated evidence for claims for a discovery of element 118. Although an internal investigation led to his being fired by the laboratory in 2001, Ninov maintained his innocence. Problems in replication were, he argued, caused by lack of skill on the part of other scientists or by faulty equipment. They were at most errors, not misconduct. Indeed, in many cases, scientific advances often appear to be dependent on the "golden hands" of experimentalists. Yet subsequent independent investigations also called into question earlier Ninov results from 1994 and 1996 related to the discoveries of elements 111 and 112 at the Centre for Heavy Ion Research in Darmstadt, Germany. By early 2002 the physics community concluded that Ninov had in reality perpetrated scientific fraud. The questions became why and how – for so long?

At Bell Labs, a world-class industrial laboratory in New Jersey, the case of Jan Hendrik Schön was equally if not more dramatic. Schön was an experimentalist wunderkind who, after earning his PhD in physics from the University of Konstanz in 1997, took a position at Bell Labs to continue work in condensed matter physics. In short order in 1999 he began publishing a series of papers with numerous coauthors in *Nature* and *Science*, the two most prominent scientific journals in the world. He published papers on plastic conductors and nano-scale transistors that promised to revolutionize electronics. Papers continued to multiply through 2000 and 2001, when in December his work was recognized in *Science* as the "Breakthrough of the Year."

Then in 2002 questions began to be raised by some physicists outside Schön's working group. The result was the formation of a special investigation committee from outside Bell Labs, leading, by December, to his work being described as "Breakdown of the Year." The journals *Nature* and *Science* withdrew a total of fifteen of the publications on which Schön had been first author. What was equally remarkable, however, was the quick

41 Minkel 2002, p. 20.

exoneration of all nineteen coauthors (including his manager at Bell Labs), and his dissertation adviser – although two years later even Schön's doctorate was revoked on grounds that his dissertation exhibited fraudulent data.[42]

Questions for research and discussion

1. Does all the blame for misconduct lie with Ninov and Schön? For example, was it not possible that coauthors, mentors, and journal editors had been too ready to be associated with exciting if not flashy results? Would such an attitude contradict the norms of science? Why had peer review not detected weaknesses in the original submissions, with only outsiders having brought organized skepticism to bear? Why, in the middle of the Schön investigation, was the editor of *Science* so quick to reject the allegation that there was anything wrong with the peer-review process and to maintain "there is little journals can do about detecting research misconduct"?[43]
2. Is physics, as the paragon of the "hard" sciences, less susceptible to misconduct than other branches of science? Or are the same challenges to ideal conduct equally present in all disciplines?
3. Can you identify a principle for distinguishing legitimate from illegitimate instances of data omission? That is, when is it acceptable to ignore a specific observation?
4. Is there ever a legitimate role for the inclusion of nonexperts in peer-review processes? Should scientific peers evaluate the broader social impacts of an article or grant proposal in addition to its intellectual merit?[44]

Further reading about the cases

Altman, Lawrence, and William Broad (2005) "Global Trend: More Science, More Fraud," *New York Times*, December 20.
Goodstein, David (2010) *On Fact and Fraud: Cautionary Tales from the Front Lines of Science*. Princeton University Press.

[42] See Reich 2009. [43] Kennedy 2002, p. 13.
[44] For more on this topic see Chubin and Hackett 1990.

Reich, Eugenie Samuel (2009) *Plastic Fantastic: How the Biggest Fraud in Physics Shook the Scientific World*. New York: Palgrave Macmillan.
Vogel, Gretchen (2006) "Picking Up the Pieces after Hwang," *Science*, vol. 312, no. 5773, pp. 516–517.
Wade, Nicholas, and Choe Sang-Hun (2006) "Researcher Faked Evidence of Human Cloning, Koreans Report," *New York Times*, January 10.

Video and online resources for teaching about research misconduct

"Do Scientists Cheat?"
A Nova television program that originally aired October 25, 1988. Approximately 50 minutes run time. Through historical case studies and interviews with practicing scientists, this video examines cases of misconduct and various explanations for why it occurs. Available in seven segments on YouTube.

"The Lab"
A National Institutes of Health online interactive video that portrays a research misconduct case. It allows students to adopt the role of four different actors and make decisions about research integrity, mentoring responsibility, responsible authorship, handling of data, and questionable research practices. Includes a facilitator's guide. Available for free at http://ori.hhs.gov/TheLab/.