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University Responsibility for the Adjudication of Research Misconduct

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Foreword

Several years ago, we made a terrific discovery in our university laboratory – not a world-changing discovery, not a cure for cancer or a new way to harness the energy of the sun, but a bright spot in our small corner of the universe of science that would have formed the basis of an attractive publication. After our initial exuberance subsided, and after discussing how to organize our findings, a conscientious doctoral student recognized that our "discovery" was merely an artifact of a miscalibrated instrument, a consequence of a "+1" in a computer program that should have been a "−1." The student entered my office cautiously to tell me what he had determined, concerned as the bearer of bad news; he thought that I would be disappointed by the apparently unfortunate turn of events. Instead, I was thrilled that a student in my charge deliberately tried to rain on our parade. There was no better evidence that I was doing my job, a judgment that was made plain by his extra detective work, a double check of all systems to make sure that our presumed good fortune really was good fortune. It was not.

The process of experimental science includes a design strategy, the collection of data, the interpretation of the results, and last but not least, an aggressive effort to roll back any uplifting news that should follow from the first three steps. An uncritical presumption that good fortune really is good fortune is not proper conduct for a professional scientist. It is adolescent self-deception. That is why the fourth step, an effort to reject a noteworthy result for credible reasons, is essential. There are few professions that operate in this witheringly self-critical manner. A banker does not tell his or her client the reasons why a recommended investment may be a bad risk. A defense attorney does not argue in front of a jury that a new piece of evidence may implicate rather than exonerate a client. In other fields, we expect some actors to sell, not discredit, good news. This distinction is what makes Science special. This is why scientists have historically earned great public confidence. Scientists expect that other scientists tell the truth, as best as they are able, all the time, no matter what. Nevertheless, cynicism may be growing in the public mind as reports of corporate intrusions into research, among other conflicts of interest, are increasingly analyzed in the media. Scientists should be concerned about the erosion of public support, since taxpayers are unwittingly the recipients of many of the bills that scientists accumulate in expensive university and government laboratories.

Sometimes, our capacity as scientists for self-criticism reaches its limit and we deceive ourselves. Scientists are human and wishful self-deception is a human quality. Discoveries and apparent discoveries are like drugs because they create feelings of exuberance. We want these feelings to last forever. Therefore, it is essential that in today's highly collaborative scientific society there are critical participants or witnesses who work against what our brain's pleasure centers crave, good news. At some point, every scientist needs someone else's help to sober up. We stay vigilant for one another and, in fact, we spend many hours a week in this kind of work, *gratis,* through the peer review of grant proposals to federal funding agencies and manuscripts submitted to academic journals for publication, and in meetings with students and colleagues about the progress of research projects. However, when an error of yours has been revealed, the singular way forward is to first stop, absorb the impact of the mistake, tell it, and fix it. Failure to rigorously implement these steps marks the moment when self-deception may shade into misconduct. The misguided scientist may prefer to keep selling the original story because mistakes are embarrassing and the consequences of being wrong may be personally and professionally inconvenient. No one *likes* to admit being wrong.

What if the happy science story you are selling, as in persuading, is also *selling*, being exchanged for money? What if patents and intellectual property are at stake if your story can no longer be supported in light of new evidence? How much harder will it become to do the right thing? A lot. If telling the truth will cost money, scientists may try to wrap unambiguous mistakes in the cloth of equivocality. The expectation that scientists are truthful has been, in part, a consequence of the fact that science at universities has been long free from commercial interests. But this changed. Financial entanglements of companies and intellectual property are now the norms at research universities and these entanglements have coincided with a disturbing rise in scientific misconduct [1]. The correlation between academic finance and academic fraud is sufficiently strong that Stefan Franzen asks in *The Science Bubble* whether science can survive the continued intrusion of commercial interests in academic work.

If forced to mark when the modern American university became beholden to outside financial interests, many would cite the passage of the 1980 Bayh-Dole Act, legislation sponsored by United States Senators Birch Bayh (D-Indiana) and Bob Dole of (R-Kansas) that was intended to bolster American competitiveness in an increasingly globalized economy. Prior to Bayh and Dole, universities and their employees could not directly profit from discoveries based on federally funded research that turned out to have a value in the marketplace. After all, if the research was purchased by taxpayers at universities that have already been exempted from taxes, selling discoveries back to the public while drawing profits for professors and their employers might seem to some like charging for the same thing in three different ways. In former times, it was presumed that benefits of publicly funded science research should serve the public welfare and that these benefits should freely accrue to those who supported the work, taxpayers. However, Congress recognized that practical benefits derived from basic research might never come to fruition unless the commercialization of technologies was incentivized. Creating a saleable technology requires more than a good idea somehow aligned with industrial interests. Making a product is hard, creative work. Bayh and Dole therefore sought to unleash the profit motive inside American colleges and universities. The promise of royalty riches for faculty as well as institutions of higher education, they surmised, would draw out insular professors pursuing curiosity-driven lines of inquiry with little concern for practical application, encouraging researchers to drive abstract discoveries to market, a job formerly conceded to industry. Bayh and Dole baited the hook. Researchers as well as university administrations bit hard. Some have argued that the commercialization of the university was well underway before Congress became involved [2], but Bayh-Dole certainly gave a legal justification and congressional imprimatur to those keen on a transformation of the social organization of American science.

Now that the train has left the station, so to speak, every research university has joined an "intellectual property arms race," patenting all manner of discoveries while hoping for purchase or license by a major corporation. This does happen, but so infrequently it is fair to characterize such activities as sanctioned gambling that is costly for institutions, and unconscionable for public institutions, in my view. High-salaried lawyers are required; patenting fees are steep. Most inventions, whether aggressively marketed or not, produce nothing in return. Nevertheless, a rising university administrator recently announced to her faculty that her chief ambition was to put "commercialization and entrepreneurship on steroids" [3]. The journalist Jennifer Washburn called sentiments of this kind part of a "foul wind" blowing across campuses [4]. In former generations, such a statement would have sounded discordant, even anti-academic. Today, it is met with applause.

Athletes take steroids because they are shortcuts to victory. Unfortunately, the creation of science cannot survive shortcuts. For science to work as it should – and we have some 350 years of science history to convince ourselves that it has been working very effectively – scientists must be as truthful as they are able. This is the *sine qua non* of science, but it is a stiffer standard if the business of science becomes entangled with business. As Ravetz presciently forecast in 1971, "the goals of a successful career in science can change from being a series of successful research projects made possible by a parallel series of adequate contracts, to being a series of successful research contracts made possible by a parallel series of adequate projects." Not only can this change happen, it has happened. University leaders, those who should be arguing for the preservation of academic values of honesty, transparency, and common welfare, are driving this rush to the marketplace. Laws simultaneously encourage universities to commercialize technology from academic research while simultaneously charging those institutions with the responsibility for monitoring the ethics of their own faculty. How can a university be objective if ethical issues arise in its own financial portfolio?

What has been the consequence of commercialization on steroids? When public universities decided to enter the common marketplace, state support for higher education began to plummet. By 2050, most states will likely be out of the business of funding universities altogether. The great American public universities may become nothing more than profit centers on state ledgers.

Author Stefan Franzen has experienced the consequences of the conflicts of interest at a contemporary research university at first hand. He coined the term "science bubble" to make an analogy with the real estate bubble that led to the worldwide financial crisis of 2008–2009. Junk science, invented to earn big grants, venture capital, and royalties, is akin junk mortgages. It has the capacity to poison the enterprise of science that is centuries old and international in scope. Junk science has been engineered by an academic culture that has adapted "perverse incentives" that are not only dollar-centric but based on superficial metrics of output rather than the richness of intellectual and educational outcomes [5]. For example, when Franzen and I were students, publications were sent aloft, so to speak, into the breeze; we were unsure of how or where they would land. Perhaps someone read them. Perhaps we would discover their fate/impact in the fullness of time. They were gifts to a community – even if they were gifts that nobody may have wanted. Whatever could be expected in return would be, at best, gratifications much delayed. Today, publications instantly begin to accumulate scores (#s of citations) on Google Scholar, checked in a moment. Gifts have been transformed into commodities – if not junk, publications have become junkifed. Selling junk science for money and creating science for the purpose of performance metrics has the potential to devalue all science. The science bubble will burst when it becomes widely recognized that scientific assets are grossly overvalued, and the public will no longer consider the support of science a virtue. Should we lose confidence in science when the future of humanity depends on the forecasts of scientists – climate change scientists and epidemiologists today are the most obvious examples – we do so at our peril.

One might presume that "grown-ups" are somewhere in charge, predisposed to prevent catastrophe. However, we all discovered during the financial crisis that regulators in the ratings industry worked hand in hand with the most corrupt bankers thereby making a bad situation worse. The tragi-comic narratives of many scientific misconduct investigations reveal that in science regulation, grown-ups are likewise few and far between. The Director of the Office of Research Integrity (ORI) for the Department of Health and Human Services (HHS), charged with integrity of ca. $30 billion in annual research expenditures, resigned in disgust, describing his office as "remarkably dysfunctional" and hamstrung by "secretive, autocratic, and unaccountable" supervisors [6].

The most consequential case of scientific misconduct or alleged scientific misconduct, a case that birthed the ORI in its present incarnation, is known as The Baltimore Affair. This name does not refer to site of the malfeasance, but rather David Baltimore, a prodigy of molecular biology celebrated for discoveries that will last for ages, who moreover served as the president of *two* leading American research universities. Baltimore collaborated with another senior scientist who was accused of misrepresentation of research data by a young coworker, Margot O'Toole. At the time that the allegation was made there was no formal procedure for reporting improper research practices; there was no "research misconduct system" such as it is called today. Frequently, the associated universities held perfunctory, *ad hoc* inquiries and declared that the rules had been obeyed. However, in the Baltimore case, evidence was strong enough that others became involved including the ORI and eventually even the Secret Service. The case was brought to the attention of a powerful critic, Michigan's John Dingell (D), the all-time longest-serving member of the U.S. House of Representatives. Battle lines were drawn, explosive hearings were held on the floor of Congress pitting two giants in their respective fields, forensic investigations were launched, the Department of Justice was asked to bring criminal charges (it didn't), and lives were upturned, none more so than O'Toole, the researcher who first came forward with concerns. For speaking out, she was driven from academic science altogether.

Howard Temin, a molecular biologist who shared the Nobel Prize with Baltimore (and Renato Delbucco), was asked by the historian Horace Freeland Judson to comment on the Baltimore Affair after the dust had settled. Temin said of the Baltimore failure:

> "When an experiment is challenged no matter who it is challenged by, it's your responsibility to check. That is an ironclad rule of science that, when you publish something, you are responsible for it. And one of the great strengths of American science...is that even the most senior professor if challenged by the lowliest technician or graduate student, is required to treat them seriously and to consider their criticisms." [7]

Baltimore did not check. If any member of a research team has a credible concern regarding a piece of research, then every member of the team must stop, take stock, and work either to affirm or refute the concerns raised. Scientists must not only tell the truth; they must be aggressive about testing the veracity of what they report and about what others report.

The Baltimore mistakes were nevertheless equivocal. The misdeeds of history's greatest science frauds have been incandescent by comparison [8, 9]. Why then was The Baltimore Affair "most consequential"? As Franzen describes herein, in the aftermath of Congressional hearings, federal science and regulatory agencies recognized that rules and procedures for adjudicating allegations of scientific misconduct were piecemeal and inadequate. The persecutions and exonerations of Baltimore highlighted a federal system that lacked guidelines and systematic procedures. The case was conducted over a 10-year period that coincided with writing of research misconduct regulations by various federal agencies under the guidance of the Office of Science and Technology Policy. The rules that were conceived and refined in the 1990s were compromises between government authorities keen on oversight, and scientists who feared the intrusion of ill-informed science police mucking up research. Confidentiality was the foremost consideration of those who were invested in protecting the interests of scientists fearful of false accusations. Secrecy, the anathema of good science, rules all those responsible for investigating improper science at universities and at funding agencies. Federal Offices of Inspectors General have conducted multiyear investigations without ever questioning those who brought concerns about complex matters to their attention in the first place. Regulators are required to operate in shadow, even if in ignorance, anonymizing public records that often can only be obtained – and only be obtained at *public* universities subject to state open records laws – by those with the means to sue for them. Investigators can never be questioned because there is no occasion for conversation. Communication is fractured in time and unidirectional.

Worse still, regulations for dealing with scientific misconduct investigations that developed in the wake of the Baltimore case presume that universities are intrinsically disinterested parties in such inquiries. This is absurd in the post Bayh-Dole world with universities trying to strike it rich through the exploitation of commercialized science. Regulations have encouraged universities to invent any process they should choose to adjudicate misconduct, processes that will naturally protect the considerable financial interests of the very institutions who created the rules of the game. If a university is monetarily invested in the outcome of a misconduct inquiry, it can declare that all has been fully and fairly adjudicated and seal its records. External funding agencies are obligated to accept university judgments and dismiss cases even when there is ample concerning evidence. Such eye-popping conflicts of interests would be scandalous in business or law. A journalist wrote of an account of *The Baltimore Case*: [10] "You read with a rising sense of despair and outrage, and you finish as if awakening from a nightmare only Kafka could have conceived." This characterization captures essential features of many scientific misconduct investigations, then and now.

In former times, it was assumed that scientists and engineers had assimilated the basic principles of the responsible conduct of research, including the concept of competing interests. Now ethics is taught. Universities require that investigators watch and answer questions about on-line case studies of questionable research practices. The presumption in this training is that only scientists are subject to unethical practices. In contrast, it is presumed that those who administer science do their work in a value-free environment. A young scientist is never told in the face of incessant exhortations to be self-aware and vigilant with respect to misconduct in the laboratory that should she bring forward concerns about questionable science, science that is best unquestioned because it is profitable, she stands a strong chance of being betrayed by her employer, and there is nothing that anyone in government can do to help. Few scientists recognize that the absurd accommodation of competing interests by universities is *required* by federal regulation because these regulations are never revealed in the course of ethics training. Meanwhile, individual investigators must declare their own potential conflicts of interest to federal agencies and publishers many times a year when submitting and reviewing grant proposals and journal articles. Annual conflicts of interest disclosures must be signed and filed by university employees. However, many academic institutions and their representatives are free to carry and exercise massive and systemic conflicts [11]. Such hypocrisy is unsustainable.

An investigator will only learn about our system of scientific misconduct regulation if she is unfortunate enough to discover how truly ineffective they are. Franzen learned these rules the hard way by merely insisting in the course of doing his job that certain facts claimed by his collaborators were mistaken, facts that any of his investigators might easily have checked. But the facts were inconvenient and potentially costly. University leaders, stewards of history, law, and sociology, should not have to be reminded that money makes human beings and the institutions they create less righteous than they might be otherwise.

And, they should not have to be reminded by a chemistry professor like Stefan Franzen. Franzen's expertise is in biophysics, studies at the interface of biology and physics, and as such he was welcomed in a fascinating collaborative project spearheaded by two colleagues who discovered that RNA molecules, principally DNA's helpers in the chemical realization of genetic information, could be "trained," so to speak, to build metal (palladium) particles with catalytic capabilities. This work was a surprise, joining two important yet disparate areas of chemical inquiry. The work generated a high-profile paper in the *Science,* the leading American general science journal. Data therein led to patents that in turn led to major support from entrepreneurs and the administration of another public university. In 2004, professors with a hot result were keenly aware of how to generate capital and interest in the post Bayh-Dole marketplace. Yet, as occurs often in a university, the two inventors were encouraged to team with Franzen to increase their perceived competitiveness in the funding competition announced by a private foundation of an external funding agency. Franzen joined the project after the foundational publications appeared, but for him, it was new territory that he greeted with enthusiasm familiar to most scientists embarking on a quest to understand a new, yet unexplained, phenomenon.

The future was bright until Franzen was notified by a concerned PhD student that the palladium particles did not seem to have much palladium in them. In fact, they were 95% carbon. As a newcomer invited to join the project in order to help gain private funding, Franzen was not privy to the original data. While trying to understand the inconsistent results he noticed another loose thread. He pulled it. For the RNA-mediated metal synthesis to work, it had to work in water, RNA's natural milieu in which it can take on structures encoded in its sequence of building blocks. An aqueous solution was specified in the groundbreaking *Science* publication. But the source of the palladium metal was a compound that did not dissolve in water. If the palladium source did not dissolve in water, the palladium atoms could not be accessed by the RNA, the potentially lucrative process could not work, and the company based on the process would have little value. It seemed that no metal particles had been produced at all. It was the responsibility of his collaborators to check, just as Temin said. Perhaps they did. However, they nevertheless continued to insist that they had invented an amazing transformation that took place in water and aggressively tried to dissuade Franzen from saying otherwise. One obvious issue in dispute was a simple fact – the solubility of a chemical compound – that any high school science student easily could have checked. This difference of opinion about a fact morphed into the longest scientific misconduct investigation in the history of the National Science Foundation (NSF), a sponsor of the questionable research that took 9 years to resolve in Franzen's favor. The length of the process undoubtedly had to do with the fact that according to the Code of Federal Regulations that governs the NSF "Awardee institutions bear primary responsibility for prevention and detection of research misconduct and for the inquiry, investigation, and adjudication of alleged research misconduct." [12] Franzen's one-time collaborators were employed at two public universities, one of which was threatened with legal reprisals by a company in which the other held an equity state. It was in the best financial interests of both institutions to allow Franzen to twist in the wind by disavowing his concerns. As long as universities are able to disingenuously argue that there is legitimate scientific debate surrounding an unambiguous, easily demonstrable error, it can continue to collect money from its financial entanglements or avoid reprisals from a rigorous and transparent examination of the facts. In this case, any administrator or inquiry panel participant could have tried to dissolve the palladium source in water and thus saved many from a painful and protracted struggle. Facts can be constraining if the outcome of an investigation is predetermined.

The Science Bubble is based on Franzen's experience with colleagues who created a fantasy based on easily falsified chemistry, but the book goes far beyond one case – the so-called hexagon case – to examine the general failings of the research misconduct system to properly adjudicate falsification and fabrication of research. The narrative is documented with first-hand evidence as well as that from other cases in the scientific literature. The point of this book is not merely to tell a story about falsification, but to use the evidence to illustrate the ways in which universities have failed to live up to their responsibility, and to their promise that many in society continue rely on.

The characters that are described in the case study are neither heroes nor villains. They are archetypes. There are scientists who were not proactive in the face of legitimate concerns, Baltimore types. And, as the social psychologist Jennifer Crocker makes clear, we all run the risk of not being sufficiently vigorous self-critics. However, once there is a hesitant suggestion that something clearly wrong could be right, then it becomes easier and easier to support subsequent insistences, even if the next step in the defense of the indefensible has become more incredible:

> "Every minor transgression – dropping an inconvenient data point, or failing to give credit where it is due [or in this case refusing to acknowledge a mistake] – creates a threat to self-image. The perpetrators are forced to ask themselves: am I really that sort of person? Then, to avoid the discomfort of this threat, they rationalize and justify their way out, until their behaviour feels comfortable and right. This makes the next transgression seem not only easier, but even morally correct." [13]

The perpetrators in the hexagon case were human and burdened with the psychological limitations that are the birthright of us all.

What kind of character is Stefan Franzen? Franzen recognized that a matter of scientific fact was wrong and said so. In this way, he crossed a line that he did not even know that he was crossing. By suggesting that, *hey, something is not quite right*, by applying the critical outlook that was part of his training as a scientist, by challenging something that looked fishy, a rebuff no different from hundreds of others spread across a career in science, he was, in this instance, unwittingly crossing the line that transformed him from discerning critic into whistleblower. A whistleblower must live with the occupational hazard of counter-accusations.

Franzen was subsequently subject to numerous internal, university-supported inquires and investigations, awash with retaliatory allegations. Despite confidentiality regulations that were supposed to protect whistleblowers, Franzen's case had become gossip in university chemistry departments from coast to coast. None of this makes it easy for a professor to hew to his foremost responsibilities of teaching, research and university service. Nonetheless, Franzen continued to pursue his externally funded scientific interests, disclosed in peer-review publications. His commitment to education led him to create new international programs devoted to science and technology education. This was presumably a necessary distraction from the onslaught of misinformation and hearsay. Opposition to Franzen reached its zenith when he was accused of being a "science bully" for his insistence that demonstrable falsehoods had to be corrected despite the implications for colleagues, journals, companies, and universities administrators. Franzen's unwillingness to concede inspired a short-lived website directed at him called "Stand up 2 Science Bullies," that invited readers to tell their own tales of alleged persecution by colleagues. The website did not create the expected outrage from sympathetic souls and was taken down after more than a year. Nevertheless, a record of it is preserved on the website *Retractionwatch.com* [14]. In addition to the construction of a website targeting a single individual, the informant in an investigation, there were many other behind-the-scenes efforts to discredit Franzen, a few of which are disclosed in this book. The epithet and creative neologism "science bully" falls within the bounds of the so-called "nuts and sluts" characterization of whistleblowers [15].

We can look to history to find others asked to live with knowingly false facts about the world because it was in someone's best interest to accommodate a phony reality. Giordano Bruno was burned at the stake in 1600 for refusing to disavow the wisdom of Copernicus, an opinion that was inconvenient for Roman clerics. Fortunately, Franzen is still with us. Still, he was consumed, if not by fire. He endured a disinformation campaign that was promoted in scientific circles while waiting *for 9 years* until the Inspector General's Office of the NSF finally confirmed that publications were falsified – a justice so torturously slow that anyone should have lost his or her mind. This was not planned. Professor Franzen was merely doing his job as an experimental scientist and in a blink was suddenly in opposition to big organizations. His effort to correct the scientific record in the face of institutional resistance, an effort eventually vindicated, was Herculean. Thousands of hours of correspondence, study, and trips to Washington D.C. were required to produce a judgment that ended up fixing the scientific record but at great personal cost. This is time that Franzen could have used in service to his own science and his doctoral students, but he stepped away from his own interests in order to advance the collective. Franzen's insistence that wrong facts must be removed from the scientific record was a communal act intended to leave the edifice that is science in better shape for all who will come along and carry research forward. That said, Franzen's civic spirit and indefatigability are not sufficient to explain why he undertook the titanic journey described herein. It is possible that had Franzen looked away just once, of the many times he had been asked to do so, he would have accepted that the interests of universities to which he had devoted his career can be at odds with science and truth telling itself. Were Franzen to concede that the university was just like every other corporation, his identity as a university scientist and teacher would suffer a severe attack jeopardizing his every small and daily act in the laboratory and classroom. This was a bargain he could not make. This was a world he did not want to know.

Arguably, the most valuable part of *The Science Bubble* is Franzen's optimistic appraisal of what we can do to ensure that science endures despite the intrusion of financial interests. I am not going to reveal his prescriptions here. These creative and sensible remedies are best discovered as the concluding chapters of the *Bubble* unfold and we would be well advised to consider them in earnest. It may sound like hyperbole to suggest that if we don't fix the system for adjudicating scientific misconduct and restoring public confidence, science will cease. History nevertheless tells of dark ages. There is nothing in the human experience that guarantees that the network of human relationships that is contemporary science must endure. It is the obligation of every scientist to ensure that it does; the program of science is hard, it is multigenerational. Unfortunately, this obligation, in my experience, is not generally recognized or respected by federal regulators or university officers. Ultimately, only scientists can ensure that science endures. Science is the gift that is passed along from teacher to student, carefully, like the penguin's egg in that great movie of endurance [16]. For this reason, we have a right to know when actions of colleagues tear at the fabric of science and threaten our obligation to ensure the survival of a precious practice. When righteous concerns about the veracity of scientific work are investigated, we all have a right to know who said what to whom. We have a right to examine the facts. We have a right to ask questions. Whenever a community is threatened, its members are obliged to clearly understand the threat and its potential consequences. In words made famous by the great Rachael Carson in a different context, wisdom my words have played on here, "The obligation to endure," dear research integrity officers, university administrators, and inspectors general, "gives us the right to know" [17].

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Chapter 1
Evolution in a Test Tube

Science is a search for the truth, that is the effort to understand the world: it involves the rejection of bias, of dogma, of revelation, but not the rejection of morality.

–Linus Pauling

Scientists have high expectations of themselves. Society has come to expect a great deal from scientists because of the role played by technology in the modern world. Science relies on the honor system consistent with Pauling's description of moral behavior in science. Each scientist is expected to tell the whole truth about his or her work as faithfully as possible, whether welcome news of a great discovery or the misery of failed experiments. Despite years of training that reinforces among scientists the use appropriate control experiments and the necessity of examining data critically, the honor system may come undone. There are checks and balances in the system of peer-reviewed research that encourage scientists to lean in the direction of honesty, but there are other forces at work encouraging them to lean the other way. A recent epidemic lack of scientific reproducibility and the rising rate of journal article retractions for malfeasance suggest that the honor system is not up to the task.

The culture of science has evolved dramatically since the end of World War II. Researchers once cosseted in ivory-tower universities have been encouraged to become entrepreneurs and challenged to make science relevant to society. Today's successful scientist must be skilled at marketing research proposals in an increasingly tight science funding climate, savvy about navigating the increasing influence of corporations, and swift to patent inventions. These additional mandates are transforming the honor system into a business ethic in which the risk falls to the buyer, i.e. the funding agency, investor, or journal reader. Since the majority of scientists have dedicated their lives to an evidence-based investigation of a narrowly defined topic, few are equipped to step back and ask whether science as a whole is in danger of succumbing to the financial pressures of the modern university. The pressure to obtain funding combined with the complexity of modern science can lead both mentors and students to push their scientific interpretations and methods into areas where it is possible to either make a mistake or purposefully misrepresent research.

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What does a misrepresentation look like? Has an appropriate system been put in place to ensure the integrity of scientific publications? These are questions I should have asked myself years ago, but my training, like that of many of my peers, focused on excellence with the expectation that everyone would be scrupulously honest and careful. It was considered obvious that any mistake or self-deception would be quickly spotted by the watchful eyes of the research team. It was taboo to ask whether someone could ever try to fool the group or somehow create a result that looked correct, but actually was a fraud. Because of the assumptions of honesty as the norm, academic journals have been slow to implement enforceable ethical guidelines. Universities typically do even less to implement general procedures that research groups must follow to ensure research ethics.

My training was strong with regard to spotting inconsistencies and logical errors, but it did not in any way prepare me for the possibility that a collaborator would be so sloppy that they could follow a completely erroneous path all the way to publication in a high-profile journal, submit patent applications and obtain legal protection for a fraudulent idea. Like many scientists, I assumed that the scientific community has mechanisms in place to adjudicate and correct scientific fraud. Instead, I learned that the federal regulations place trust universities to adjudicate allegations against their own faculty. The central hypothesis of this book is that federal research misconduct regulations have failed to appropriately consider university conflict of interest in giving them the responsibility to investigate allegations made against their own faculty. I will present facts from research misconduct investigations as evidence in support of this hypothesis. Some of the facts result from personal experience, other examples come from personal communications (with permission) and the third set of examples come from the literature. My hope is that this book will serve as a text for students, a guide for faculty and blueprint for policy makers.

Anyone can make a mistake. Consequently, scientific research has many levels of checks and balances that are supposed to identify mistakes before they make it into print. Published research articles are part of community discussion. Thus, one would imagine that any mistakes that make their way into journal articles would be found out and then retracted. Graduate students and post-doctoral researchers (post-docs) are taught to carefully check their work and examine the work of others with a critical eye to find any small telltale signs that there is a problem in the research. Like many scientists, I believed that the training of the scientific community was sufficient to prevent problems in reproducibility, or worse yet fraud, from becoming widespread in science. I made a hard landing when I discovered that I was wrong. Once I witnessed an ethical problem and began to discern a breakdown in the mechanisms that were supposed to respond to ethical transgressions, I began to investigate the issues. As my own case unfolded with its complicated set of failures to properly adjudicate (what should have been) a simple case of falsification, I came to understand that there is a record retraction rate, thousands of examples of fake peer review, a pervasive lack of reproducibility and sufficient evidence of falsified research results in every field to cause alarm. Beneath the surface of these indicators of a systemic failure in science lies a much greater problem that is masked by confidentiality. The confidentiality clause in federal regulations and university rules is