Recent years have witnessed increasing attention to misconduct and fraud in academic scholarship, particularly in scientific research. As shown in Figure 7.1, retraction rates among publications in scientific journals have increased astronomically – roughly ten-fold since 2000 (Steen, Casadevall, and Fang 2013; Van Noorden 2011).

That trend seems both stunning and perplexing if one considers the typical narrative provided to explain these events: Some individual scientist, succumbing to avarice, insecurity, or incompetence, whether knowingly or unwittingly, publishes erroneous findings. His peers nevertheless discover this deviance and ensure its correction. In essence, errant research, whether intentional or not, comes from a set of “bad apples.” But given the slow rate at which turnover occurs in the population of scientists, this explanation would appear to have little purchase in explaining the dramatic rise in retractions over the past decade. Other putative causes – such as the increased competition for scarce funding and positions and improvements in the ability to detect errors and fraud – have also changed far more gradually and slowly than the rate of retractions. Therefore, they would appear incomplete, at best, as explanations for this trend.

However, this trend does closely resemble the rate at which the popular press has called attention to misconduct and fraud in another setting: the corporate world. Figure 7.2 documents that trend, with a pattern and magnitude of increase strongly resembling Figure 7.1, save for two anomalous years: 1996 (fallout from the price-fixing

The authors gratefully acknowledge generous research support from the Yale School of Management, especially through its Initiative on Leadership and Organization. Donald Palmer offered invaluable comments on an earlier version of this chapter.
scandal involving Archer Daniels Midland) and 2002 (notable for the collapse of Enron and Andersen).¹

¹ Figure 7.2 reports results from a search on August 14, 2014 of all publications tracked by Dow Jones’ Factiva Online database, using the following query: mention of the word (company OR companies OR corporation OR corporations OR corporate OR business OR businesses OR organizational OR organization OR organizations OR firm OR firms) within three words of a mention of the term (corrupt OR corruption OR crime OR criminal OR fraud OR fraudulent OR unethical OR wrongdoing OR misconduct OR misbehavior).
Far from being a coincidence, we suspect that the correspondence between these plots reflects the fact that both stem from common causes of organizational wrongdoing, a point to which we return in our conclusion. Below, we draw on our ongoing work examining misconduct in scientific research to identify factors at the interpersonal, organizational, and institutional levels that encourage and inhibit organizational wrongdoing.

Beyond simply connecting these trends, we also argue that the study of scientific misconduct can enrich our understanding of organizational wrongdoing in other contexts. Scientific research provides an unusually fruitful domain within which to broaden and deepen this understanding for at least two reasons. First, relative to the corporate world and many voluntary associations, academic science takes place within relatively “weak” organizations, with highly autonomous and mobile actors. Documenting the embeddedness of academic science (and scientific misconduct) within its social and organizational contexts should therefore provide something of a lower bound on the extent and consequences of such embeddedness in other settings. Second, academic publications offer the equivalent of a fossil record through which one can infer the probable occurrence of misconduct over time and across settings. Recently developed statistical techniques, for example, allow one to estimate the probabilities that particular investigators have engaged in questionable practices, such as “p-hacking” and use of the “file drawer” – which misrepresent the nature of empirical relationships through selective reporting – even when no questions have been raised about the quality or validity of their articles (Benford 1938; Goldfarb and King 2013; Ioannidis and Trikalinos 2007; Simonsohn, Nelson, and Simmons 2014). By contrast, studies of scientific misconduct that have relied only on retractions suffer from the same obvious limitation as studies of corporate wrongdoing that have analyzed earnings restatements and other irregularities: selection on the dependent variable – they only include instances that have already been discovered as erroneous or fraudulent.

Whereas unintentional errors have generally been considered random or the result of incompetence, the dominant approach to scientific fraud has portrayed it as a rational calculation, balancing the benefits of publication against the probability and costs of detection. In our view, such rational cost-benefit perspectives give inadequate weight to the context (in our case, academic science) within which action is embedded.
Varieties of scientific wrongdoing

The trend in retractions documented in Figure 7.1 subsumes a wide variety of types of scientific misconduct. We define scientific misconduct as behavior that violates professional or institutional norms and standards governing scientific inquiry. Scientific inquiry, in turn, includes both the planning and execution of scientific projects as well as their dissemination. Misconduct can occur not simply in a given scientific investigation but also in the communications and claims of credit associated with its findings (Meyers 2012).

We find it useful to distinguish between two dimensions of scientific conduct: cognizance and agency. Cognizance captures whether individuals involved understand their misconduct. Agency captures the proactivity involved: the degree to which misconduct reflects the commission of specific acts versus the failure to engage in some specific actions. Combining the two dimensions yields the typology shown in Table 7.1.

We include negligence, error, and incompetence under the rubric of scientific wrongdoing because, like fraud and plagiarism, they contravene the norms and standards of scientific inquiry (even if by accident), serving thereby to erode trust in the scientific enterprise.

Characterizing specific instances of misconduct along the dimensions of cognizance and agency can prove difficult, even with detailed information about an episode and its context. Furthermore, wrongdoing can evolve over time, so that behavior that began as unintentional becomes deliberate. For instance, a poorly designed or ineptly executed study might (unwittingly) produce a pattern of (bogus) results in which the investigator becomes invested, to the point that, even after learning of the mistakes, he or she continues to represent the findings or the methods as legitimate.

Table 7.1 A typology of scientific misconduct

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<th>Commission</th>
<th>Omission</th>
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<tr>
<td>Intentional</td>
<td>Fraud, Plagiarism</td>
<td>Negligence</td>
</tr>
<tr>
<td>Unintentional</td>
<td>Error (or Incompetence)</td>
<td>Incompetence</td>
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2 We thank Donald Palmer for bringing this point to our attention.
Case studies: not-so-great moments from the annals of science

To motivate our discussion and illustrate the importance of some contextual factors in scientific wrongdoing, we first summarize three prominent cases of scientific misconduct, which vary markedly in the strength of incentives for fraud and the probability of the protagonists being detected.

Suspicious social psychology: Diederick A. Stapel (weak incentives, low probability of detection)

Diederik Stapel had been seen as a future star even from his early days of graduate study in psychology at the University of Amsterdam. He had developed a reputation for being a talented experimentalist who almost always got his results. Following the completion of his dissertation in 1997, his fame and fortune grew rapidly as he published nearly ten papers per year in prominent journals. Only three years after beginning his academic career at Tilburg University, he received a full professorship at the University of Groningen. Six years later, he returned to Tilburg as the head of a research institute and, in short order, became the dean of the social and behavioral science faculty.

In 2011, it all came crashing down on Stapel. Three graduate students alerted the Rector to irregularities in Stapel’s research practices and findings (Levalt, Drenth, and Noort 2012). Those allegations led to the formation of three commissions – one each at Tilburg University, the University of Groningen, and the University of Amsterdam – that investigated nearly every one of his published papers and every dissertation that he had supervised. When the investigators delivered their verdicts, they declared that sixty-two of his publications reported fraudulent research (Levalt et al. 2012), resulting in fifty-four retractions to date. The extent of his deception and misconduct seems all the more striking given that the rewards appear weak relative to the risks, at least once Stapel had achieved prominence.

Stapel’s frauds generally took one of two forms (Levalt et al. 2012). Sometimes he would claim to have data from an earlier set of experiments that could answer important questions raised by another scholar. In other cases, together with coauthors, Stapel would plan a set of clever and careful experiments to examine the next logical, incremental
step in some line of research, often to the point of writing up all of the experimental materials (e.g., survey instruments). Stapel would then volunteer to run these experiments himself, supposedly drawing on contacts at schools across the Netherlands. But in either version of the fraud, instead of actually conducting the experiments, he would simply fabricate a dataset on his computer, which he would then pass on to a graduate student or a coauthor for analysis.

Although the commissions investigating the fraud concluded that no one beyond Stapel himself could be held accountable for these crimes, they also noted that a number of factors contributed to the long time that it took for his deception to be discovered. By claiming absolute and sole control over the process of collecting and entering the data, Stapel provided himself the opportunity to fabricate the data without accomplices. He also maintained control over those post-docs and graduate students working with him by being unusually friendly but simultaneously threatening anyone who questioned him. His stellar reputation also helped to protect him against earlier allegations when some suggested that his results seemed too good to be true.

But blame also resides in the culture of the departments in which he worked and, indeed, in the entire field of social psychology (Levalt et al. 2012). Despite the fact that many of his papers included statistical issues that should have raised red flags – if not about the veracity of the data at least about the quality of the analysis – numerous colleagues and dozens of editors and peer reviewers failed to notice these problems as they authorized promotions and accepted papers for publication.

The commissions attributed these issues to a culture of sloppy science where even those not engaged in fraud would nonetheless see no issues with questionable practices: failing to report the results or even the existence of some experimental conditions; removing subjects without reporting either the reason for their removal or the fact that they had been removed; merging cases from more than one experiment; measuring multiple potential dependent variables but only reporting those that yielded significant results; and so on. In such a culture, where much “research” offered little more than an exercise in confirmation bias, the fabrication of the data itself may have seemed a small step to Stapel.
Phony physics: Jan Hendrik Schön (strong incentives, low probability of detection)

It was arguable that the scientific misconduct was Schön’s alone, and not an indictment of Bell Labs. But it was difficult to believe such an incident could have occurred years before. “What does the Schön scandal mean?” an interviewer from the New York Times asked a young physicist named Paul Ginsparg. “The demise of Bell Labs by becoming corporate,” Ginsparg replied.

(Gertner 2012: 337)

In late 2002, the revelation that Jan Hendrik Schön, of the renowned Bell Labs, had fabricated numerous seminal findings in condensed matter physics and nanotechnology shook the physics community. After completing a lackluster dissertation at the University of Konstanz in 1997, Schön moved to Bell Labs and began publishing pioneering results at an astounding rate. Schön claimed to have created an organic superconductor, an electrically driven organic laser, and a single-molecule transistor that outperformed silicon-based devices while being magnitudes of order smaller. Simply put, if Schön’s findings stood, they could have laid the foundation for a new era in computing and electronics. Based on these discoveries and others, Schön averaged a publication every eight days between 2000 and 2001 (Agin 2007); in that period, Schön and his coauthors published seven papers in Nature, nine in Science, and five in Physical Review. All of these papers, however, ended up being retracted after an investigation found that they contained fabricated and duplicated figures, as well as results inconsistent with accepted physics.

Suspicion of Schön’s fraud surfaced in the summer of 2001 (Reich 2009). Numerous groups found it impossible to replicate his results. Moreover, no one, including his own coauthors, had ever witnessed any of the experiments Schön reported. Then, in October of that year, a chi-square test revealed that a figure in one of his papers reported a distribution unrealistically similar to a Gaussian. But Schön’s detractors remained largely at bay because the materials he supposedly worked with had been notorious for their sample-specific properties, and the methods he claimed to have used would have been difficult to master. Although the mounting concerns led to informal questioning at Bell Labs, a formal investigation did not begin until coworkers serendipitously discovered that two papers – one published in Science,
a second in *Nature*—contained the exact same figure, even though they supposedly analyzed two very different materials (see Reich 2009 for a detailed account).

The external investigative committee convened by Bell Labs found Schön guilty of scientific misconduct in sixteen cases and his conduct “troubling” in six of the remaining eight cases investigated (Beasley et al. 2002). In their report, the committee concluded that Schön alone bore responsibility for the fraudulent findings; none of his more than twenty coauthors appeared guilty. According to Dr Malcom Beasley, who headed the committee, “He committed scientific fraud ... Nobody else did” (quoted in Chang 2002). Typical of historical accounts of scientific misconduct, Schön has been portrayed as a lone, rogue perpetrator—a bad apple.

Institutional changes at Bell Labs may nevertheless have allowed fraudulent behavior—which historically would have been detected quickly—to go unnoticed. Henrik Schön had been the first case of fraud discovered at Bell Labs. When asked whether duplicity similar to that committed by Schön would have been possible in the past, a former Bell Labs chemist remarked, “I honestly don’t believe it would have ... As soon as we tried to release [our own ground-breaking findings] for publication, we had a director and two department heads coming down and demanding to see this experiment work” (Haddón in Chang 2002). For much of the twentieth century, Bell Labs held the distinction of being the most innovative research institution in the world: “This was a company that literally dumped technology on our country. I don’t think we’ll see an organization with that kind of record ever again” (Michael Riordan in Gertner 2012). During this period, Bell Labs maintained a strict internal review system. Prior to publication, scientists would circulate an internal memorandum describing their findings. Managers had to approve these memoranda, which they would typically subject to peer review by Bell scientists outside the department of origin, before they would allow authors to submit their research to journals. But when the breakup of AT&T led to Bell Labs being transferred to Lucent Technologies, oversight began to weaken. Subsequent to the revelations of Schön’s fraud, managers admitted that the system had fallen into disuse in the 1990s, perhaps as a response to the pressures associated with the parent company’s poor financial performance (Reich 2009). But Bell Labs’ reputation for research integrity persisted; Schön’s affiliation with it may therefore have helped protect him from earlier scrutiny.
Faux fusion: Martin Fleischmann and B. Stanley Pons (strong incentives, high probability of detection)

On March 23, 1989, Martin Fleischmann and B. Stanley Pons of the University of Utah announced the sustained production of “cold fusion,” using the most rudimentary of materials and equipment. Over the next year, millions of dollars and countless person-hours went toward assessing their claim. Aside from a few persistent believers, the scientific community soon concluded that the research had been flawed, sloppy, rushed, and inadequately reviewed prior to the announcement.

At a critical point, Fleischmann and Pons learned of seemingly related work being done at nearby Brigham Young University. Pressured by University of Utah officials and patent attorneys, the researchers became secretive and hastily convened a press conference to announce their discovery, seeking to secure subsequent intellectual property rights. But the work had not been thoroughly vetted. Fleischmann and Pons’ research cut across scientific boundaries, including electrochemistry, nuclear physics, materials science and metallurgy, and radiation measurement (Close 1990: 105). With such expertise absent from their team, this lack of review, combined with the frenetic rush to get “results,” led to numerous errors and oversights.

The chemists’ announcement made for a fascinating and exciting story line, promising a future in which the power of the Sun could be harnessed inexpensively and safely. Overnight, Fleischmann and Pons literally became household names, with cold fusion stories gracing the covers of *Newsweek*, *Time*, and *Business Week*. The media wanted the claims to be true, and Fleischmann and Pons wanted the media to want the claims to be true, fueling the cold fusion fire. Compelling conspiracy theories, such as the oil companies and military establishment being behind the naysayers, helped to dismiss the detractors.

Certain features of the organization and division of labor seem to have exacerbated the shortcomings of the science. In 1988 alone, Fleischman and Pons collaborated on sixteen papers, and Pons’ name appeared on another twenty publications, all unrelated to test-tube fusion (Close 1990: 74). But Fleischmann, a British citizen, spent much of his time consulting a major atomic energy laboratory in England; Pons, meanwhile, chaired Utah’s chemistry department. Neither of them seems to have had much bandwidth to oversee research. Indeed, responsibility for carrying out the
experiments fell mainly to an inexperienced graduate student and to a radiation safety officer (RSO) at the University of Utah, who “was used to measuring … [radiation] … in rather different circumstances and with different instruments” (Close 1990: 280–281). Interviewed by the New York Times months after completing the measurements, the RSO said, “the cold-fusion researchers used the data ‘any way they liked’ without consulting [me]” (Broad 1991: 30); months later, he added that he had found the measuring instrument used to be faulty.

Status considerations also shaped early reactions to the announcement. Fleischmann’s pre-eminence led some initially to give him the benefit of the doubt. One British fusion expert stated, “it is hard to see how a competent electrochemist could be mistaken on this scale” (Close 1990: 236). Investigators reporting replications at BYU, Texas A&M, and other institutions expressed glee at having (at least momentarily) outflanked America’s East and West Coast scientific elite (Close 1990: 244). Under pressure from the White House, the Department of Energy urgently convened a meeting of the directors of its major research laboratories, the “collective Goliath whom David, in the guise of the [Utah] chemists, seemed to have felled” (Close 1990: 143). Fleishmann and Pons’ reported discoveries threatened not only the existing status hierarchy and funding priorities within energy research but also called into question the prevailing “big science” model for funding research, which supported premier institutions with large staffs, massive physical plants, and cutting-edge technology.

Locally, institutional rivalry and resource competition also clearly came into play. With only the most preliminary of findings, University of Utah leaders decided to pursue public announcement, even at substantial cost and reputational risk. The University, for example, secretly gave a $500,000 grant to the off-campus National Cold Fusion Institute established by the Utah legislature, falsely representing the transfer as an anonymous gift from an outside donor. After documenting that no fusion had occurred, a Utah physics professor, asked by the University to do measurements in Pons’ lab, received a letter from an attorney representing Pons, demanding that he retract a manuscript reporting his negative findings. Some of the funds paid to this attorney came from the University’s own Office of Technology Transfer. Amid these revelations and facing a loss of faculty confidence, Utah President Chase Peterson resigned.
Beyond bad apples and good cops: the social organization of scientific wrongdoing

These vignettes underscore several important points. First, the anecdotes vary in terms of agency and cognizance – for example, more intentional errors of commission in the case of Stapel versus more (probably) unintentional errors of omission in the case of Fleischmann and Pons. As elaborated below, intentional and unintentional errors, in particular, likely differ in their determinants as well as their consequences, meaning that researchers should distinguish between them in examining organizational wrongdoing.

Second, they highlight the limitations of prevailing accounts of misconduct. As noted by Palmer (2012), one approach has characterized these events as the consequence of “bad apples” – the moral or intellectual shortcomings of the individuals involved, or perhaps the ethical inferiority of the culture of an entire organization. Another common approach grossly oversimplifies the phenomenon as rational responses to incentives, policing, and sanctions. In this narrative, most of us have the potential for misconduct; it just depends on the potential costs and benefits that each actor faces. In a two-by-two table with strong versus weak incentives on one axis and high versus low probability of detection on the other, one would expect fraud primarily in the quadrant with strong incentives and a low probability of being caught. The cases above, however, do not fit neatly into either of these narratives (note that we have roughly assigned these cases to their risk-reward quadrants). Diederick Stapel, for example, would appear to have had little to gain from his fraud; Martin Fleischmann and Stanley Pons, meanwhile, had little hope of going undetected.

Third, some similar interpersonal, organizational, and institutional factors appear to underlie these three episodes. Incentives, monitoring, and sanctions do not operate in a vacuum; rather, they interact with and are shaped by features of the interpersonal, organizational, and institutional contexts (Greve, Palmer, and Pozner 2010). Inspired by these three cases, we draw on economic and organizational sociology to identify four sets of contextual influences likely to influence the dynamics of wrongdoing: network embeddedness; organizational design, demography, and the division of labor; institutional fields; and status processes. Below we discuss how, when, and where each of these factors might influence scientific misconduct, and we conclude by
pointing to commonalities with respect to the four sets of factors in
science and commerce.

**Network embeddedness**

Social structures importantly shape the propensity for intentional mis-
conduct by restricting the transmission of information. Simmel (1906)
brought early attention to this issue, arguing that secrets could only
remain safe in dyads. But subsequent theory and empirical research has
expanded these ideas beyond the dyad. Granovetter (1985: 490), for
example, noted, “The embeddedness argument stresses instead the role
of concrete personal relations and structures (or “networks”) of such
relations in generating trust and discouraging malfeasance.” In essence,
both Granovetter and Simmel argue that denser levels of connectedness
dissuade individuals from engaging in aberrant behavior because their
peers quickly become aware of it and sanctioned them.

What network structures then would allow conspiracies and fraud to
occur? Networks with a high degree of centralization – think of a star
(or hub-and-spoke) structure, with sparse and asymmetric communi-
cation, moving information from the center outward. These structures
prevent knowledge of illicit activities from diffusing widely into the
community, compartmentalizing awareness of wrongdoing and pre-
venting defection by raising fears of becoming the fall guy. Baker and
Faulkner (1993) found just such patterns of interaction among the
organizations involved in price fixing scandals. And Aven (2012) calls
attention to parallel patterns among the employees at Enron involved
in the creation and maintenance of that firm’s illegal accounting
entities.

We might expect similar patterns of communication and connection
among teams of scientists to enable intentional misconduct. More
precisely, scientists occupying boundary-spanning positions, sitting
between otherwise unconnected sets of collaborators, would appear
to have the greatest capacity for engaging in fraud. One would expect
this capacity to become most pronounced in situations in which the
boundary spanner also holds higher status than most of his collabora-
tors (Brass, Butterfield, and Skaggs 1998). Those two conditions, in
essence, allow the individual at the center to control flows of informa-
tion and therefore quickly to squelch questioning of dubious practices.
Stapel appears to have been just such an example, both in structure and
in relative status: for instance, compared to his legitimate publications, a higher proportion of his fraudulent articles had been co-authored with junior and/or female colleagues. The other two cases exhibit some similarities, with the protagonists interacting primarily alone or in dyads, rather than promoting multilateral exchange among all other members of their teams.

Another interesting manner in which network structures might shape misconduct, particularly intentional misconduct, emerges from their role in transmitting and enforcing norms. Since Sutherland (1947), scholars have argued that individuals learn criminal and delinquent behavior in intimate groups. The timing, frequency, and intensity of exposure to norms supporting misconduct shape an individual’s propensity to engage in such behavior. Thus, the extent to which scientists consider various forms of behavior illegitimate may depend both on their training—the practices they observe in their advisors or mentors—and on their peers. Both have been shown to be important in determining who engages in deviant behavior. Stuart and Ding (2006), for example, found that the commercial activities of advisors, mentors, and senior members of departments significantly influenced the commercial behavior of scientists—whether they would license their inventions, whether they would sit on advisory boards, and whether they would found their own companies. Although such activity now seems normal, only a couple of decades ago most in academia would have considered it deviant. Along similar lines, Pierce and Snyder (2008) reported a strong tendency for individuals to follow the ethical norms of their organizations and colleagues; when employees moved from an automotive smog testing facility with average pass rates to one with suspiciously high rates, the frequency with which those same individuals passed vehicles subsequently rose dramatically.

Organizational design, demography, and division of labor

The organizations literature has also frequently called attention to the ways in which organizational design, operating routines, and the division of labor can lead to unintended errors. Consider, for example, Perrow’s (1984) study of Three Mile Island, which ascribed the meltdown to “normal accidents”—almost inevitable events given the
complexity of the system, the degree of interdependence among its subsystems, and the limited understanding of these connections among those involved in operating the system. Vaughan (1997) points to similar problems that led to the demise of the space shuttle Challenger. Organizations can solve complex problems efficiently by dividing problems and production into simpler pieces that individuals or subgroups of people can perform more effectively. But doing so does not come without cost. Responsibility for the end product can become diffuse and the potential for miscommunication rife. One therefore often witnesses problems “falling between the cracks” (i.e. between domains of responsibility) and people believing that someone else has taken (or will take) care of it.

In the cross-section, one would expect these errors to emerge from larger, more complex, and more dispersed research teams. Individuals on the team not only bear distinct responsibilities but also, to the extent that they do not overlap in their abilities and training, may not even have the capacity to screen for and correct potential errors made by their colleagues. We observe this in the case of Schön. Because his research crossed many different domains within physics, each requiring specialized technical skill, his team and outsiders had limited ability to replicate his work, allowing his fraud to go undetected far longer than it otherwise might have.

Consistent with the trend in Figure 7.1, we also expect that these coordination challenges have become more pronounced over time. Jones (2009) described the problems associated with the “burden of knowledge” in innovation, both in science and in industry. As the community accumulates knowledge, any research project must incorporate more and more information to produce ideas at the frontier. Indeed, the current stock of knowledge in most fields appears to have exceeded the ability of any individual to learn it. As a consequence, scientific research increasingly occurs in teams, with differentiated expertise and an extended division of labor. These teams, moreover, have grown ever larger as the role of each member has necessarily become increasingly narrow (Wuchty, Jones, and Uzzi 2007).

Beyond the simple size of the team, its demography might also influence the probability of scientific misconduct. Interestingly, here, the predictions would appear to run in opposite directions for witting versus unwitting wrongdoing. Diversity – of expertise or along demographic, geographic, or linguistic dimensions – should generally
increase the difficulty of coordination and consequently also the probability of errors. By contrast, sorting on similarity may facilitate the formation of teams in which all of the members feel comfortable engaging in fraud. Whereas a diverse team might keep in check someone prone to questionable research practices, a group of like-minded scientists could fall prey to believing such behaviors normal and thus that nothing should preclude them from participating in them. We would therefore expect the odds of unintentional errors to rise, but the probability of intentional malfeasance to fall with team heterogeneity.

Another interesting structural dimension of scientific inquiry concerns an analog of the organizational boundary. Studies in petrochemicals, aircraft maintenance, and other settings have raised serious questions about the relationship between the outsourcing of maintenance and subsequent safety or accident records (e.g., Kochan et al. 2006). A common refrain from such studies has been that outsourcing erodes internal understanding of the outsourced activities and results in poor communication between the employees of the primary firm and those of the contractor. Although outsourcing per se has generally not been prevalent in science, it seems another natural response to the burden of knowledge noted above. In some cases, teams may choose to pay for expertise, such as specialized statistical advice, rather than including the source as a coauthor. One sees this choice at work increasingly in clinical trials, where clinics and contract research organizations receive contracts to provide a certain number of subjects (Bodenheimer 2001). Not only does such outsourcing increase the odds of unintentional errors due to miscommunication, but it may also raise the probability of fraud to the extent that it reduces the ability of the primary investigators to monitor and ensure the quality of the inputs from their subcontractors.

Institutional field

Organizational scholars have documented quite convincingly how an organization’s institutional context shapes its structures, processes, routines, and outcomes. Just as some neighborhoods are more dangerous than others, institutional fields likely vary in their propensities to produce errors and outright fraud.
Scientists typically operate simultaneously within multiple institutions—laboratories, departments, universities, scientific societies, and fields and disciplines. One dimension on which these institutions vary is the intensity of competition for resources and status. Competition should increase the rate of both intentional and unintentional wrongdoing. Resource scarcity may mean that scholars cannot afford to use sufficiently large samples or to replicate their own results before attempting to publish them, thereby reducing the probability of internal error detection. Moreover, the race for priority, to publish first, may lead even well-funded teams to go forward with results before they have been properly vetted, as occurred in the cold fusion case. But competition can also promote intentional wrongdoing to the extent that it creates a culture of results and publications “at any cost.” Growing fields, with an abundance of resources, therefore, should witness fewer problems than declining ones, where scientists may have begun fretting about finding funding.

Uncertainty over the availability of resources can arise not only from competition but also from changes in the political environment (e.g., Pfeffer and Salancik 1978). This uncertainty typically leads actors to engage in defensive maneuvers designed to solidify their positions in a rapidly changing landscape. In the context of scientific research, changes in political administrations and agency directorships may represent such occasions, due to shifting priorities in the federal funding of research and to changing budgets and administrative rules regarding research grants. Individuals threatened by such institutional changes may cut corners to strengthen their position amidst increasing competition and/or diminished resources, leading to an increase in intentional misconduct. Or, threatened researchers might attempt to shift fields, funders, and journals, in the pursuit of “hotter” or less rigorous arenas, potentially then leading to an increase in unintended errors.

Fields vary not only in the incentives to engage in wrongdoing but also in their ability to detect it. For example, we expect to see dubious science occurring more frequently in those fields that have the weakest gatekeeping and monitoring institutions. The more rigorous and involved the review processes to obtain funding and to achieve publication, the less likely research with intentional or unintentional errors is to receive support and publicity. Of course, such review processes have their own costs and associated risks (Meyer 2012), including the
possibility that they motivate scientists to migrate toward less rigorous fields and journals.

One might also expect the propensity for errors and malfeasance to vary as fields and disciplines mature. Studies of competitive environments across many sectors suggest a pattern of increasing legitimation when fields or industries emerge, with an accumulating critical mass enhancing the prospects for most actors (Carroll and Hannan 2000). As industries mature, however, competitive forces come to the fore, with each entrant in an already crowded field pulling resources away from those already there. The returns to policing others also decline as fields mature and research shifts toward increasingly incremental results (Lacetera and Zirulia 2011). Given the increasing difficulty of innovating in and contributing to established fields, combined with the lower incentives of peers to monitor incremental research, one might expect errors and fraud in a field to increase over time.

But other forces act in the opposite direction as areas of study mature. Norms, standards, and values emerge over time as the scientific community develops a shared understanding of problems and accepts certain approaches to addressing them. Without these norms and agreed-upon practices, peers likely find it difficult to detect scientific wrongdoing, inadvertent or otherwise, in nascent fields compared to mature ones. Consistent with this idea, in the case of Diederich Stapel, his fraudulent work appeared primarily in the newly emerging fields of moods and of embodied cognition, not in his contributions (often with eminent co-authors) to the more established psychology subfields of stereotyping and power and status.

**Status processes**

Status processes undoubtedly interact with these factors in a variety of ways. One way concerns the allocation of attention. The community pays disproportionate attention to the opinions of and the research done by those of high status (Merton 1968; Simcoe and Waguespack 2011). One might therefore expect articles authored by high-status scientists to receive a higher level of scrutiny and attempted replication. Conditional on having produced problematic research, one would expect the scientific community to uncover with greater probability both the accidental errors of high-status individuals and any research manipulation or fraud in which they might have engaged. Given that
the community holds them in such high esteem, moreover, both errors of omission and commission by high-status investigators might result in more severe punishments when discovered. Baker and Faulkner (1993), for instance, reported that juries more commonly found higher-status actors guilty in price-fixing scandals and judges punished them with more severe fines and sentences. Having discovered misconduct, the community might therefore sanction high-status scientists more strongly. Consistent with these ideas, Furman and his colleagues (2012) reported that the author(s) of a scientific article being associated with a premier research university was the strongest predictor of the future retraction of that publication. And, indeed, we saw this pattern illustrated poignantly in the case of Diederich Stapel.

But might status also influence the probability of producing problematic research? At first blush, one might expect it to decrease. Recognizing themselves as being subject to greater scrutiny and potentially as having more to lose, both in terms of their positions and in terms of punishment, high-status scientists should exert extra care in the reporting of their results (as one saw during the halcyon days of Bell Labs) and consider the cost of fraud simply too high relative to any potential gains.

Even without this extra effort, one might still expect high-status scientists to produce fewer unintended errors. Consider the relationship between status and quality. Audiences often view status as a proxy for quality and, indeed, achieving high status generally requires producing at least some high-quality output (Gould 2002; Merton 1968; Podolny 1993). High-status scientists have therefore typically achieved this prominence, at least in part, by producing better than average research (Merton 1968). If status remains tightly connected to quality even once high status has been achieved, then one would simply expect fewer errors from high-status scientists. Status, however, has a strong tendency to become decoupled from quality, particularly in contexts where people find that quality difficult to assess. As Merton (1968: 2) noted, “eminent scientists get disproportionately great credit for their contributions to science while relatively unknown scientists tend to get disproportionately little credit for comparable contributions.” To the extent that such decoupling occurs, one might find few differences in the error rates between those of higher versus lower status.

Status processes also influence other aspects of research production. Merton (1968) coined the term the Matthew Effect – named after the
passage in the Gospel of St. Matthew about the rich getting richer – to refer to the disproportionate rewards and resources received by high-status scientists. Those of high status accrue more and larger grants and attract larger numbers of students, for example. High-status individuals in modern science, therefore, run larger organizations, with more post-docs, more graduate students, and more researchers. They also tend to collaborate at higher rates (Moody 2004). As we noted above, this expanded scale, diversity, and division of labor increase the odds of unintentional errors for a variety of reasons. On this dimension, one might therefore expect the laboratories of high-status scientists to produce errors at a higher rate than those of lower status peers, particularly as these scientists progress in their careers and manage larger and larger operations.

Another strand of literature, meanwhile, suggests that those of high (and low) status might engage in intentional misconduct more frequently than those of intermediate status. The organizational literature on middle-status conformity argues that adherence to community norms should operate most intensely in the middle of the status distribution (e.g., Phillips and Zuckerman 2001). Those of high status, believing themselves less likely to be punished for deviance (Jin et al. 2013), flout convention. Those of low status, meanwhile, have little to lose by risking deviance and therefore do not bother to conform. In the middle of the status hierarchy, individuals perceive themselves both as having a lot to lose and a high probability of being punished, therefore refraining from behavior that might threaten their positions. This theory would then point to an inverted-U relationship between the status of scientists and their rates of intentional misconduct.

Another likely effect of status processes on wrongdoing in science relates to our previous discussion of institutional fields. Fields and sectors vary a great deal, both cross-sectionally and over time, in the permeability and visibility of their status hierarchies. Similarly, they vary in the frequency and accuracy of the ongoing assessments of key stakeholders. We would expect status structures to be less permeable in fields characterized by slow and small changes in content and methodology; longer cycle times for developing, implementing, and publishing results; higher barriers to entry; longer-term funding cycles; and less frequent formal assessments by external evaluators (e.g., the magazines that rate hospitals and business schools on an annual basis). Under such circumstances, wrongdoers find that it is more difficult or time
consuming to appropriate the benefits of their deceit in the form of greater rewards. In other words, one potential consequence of the increasing frequency and prominence of ranking exercises within fields and sectors may be to increase behaviors, including forms of wrongdoing, that would enhance the status of a given individual, team, or organization.

**Summary and conclusions**

Prior research within organizational theory leads to several clear—but sometimes competing—hypotheses about factors that are likely to increase the rates of scientific misconduct. Table 7.2 briefly summarizes our predictions about how the prevalence of witting and unwitting wrongdoing vary as a function of network embeddedness, organizational design and division of labor, institutional factors, and status processes. Adjudicating between these propositions will provide evidence as to the mechanisms and processes underlying unintentional versus intentional scientific errors.

We have now reached a point where we can return to the similarity noted at the outset of this chapter between two graphs: one (Figure 7.1) showing trends in detected scientific misconduct; the other (Figure 7.2) showing trends over the same time frame in media references to corporate wrongdoing. A substantial body of research has been devoted to understanding the increased prevalence of corporate wrongdoing. Increased complexity (Boisot et al. 2011; Jelinek 2013; Weinschenk 2012), resource scarcity, intensified competition, an emphasis on meeting performance expectations (Kaplan 2010), and perverse managerial incentives (Economist 2009) have all been blamed for contributing to corporate fraud. Moral “credentialing” (Monin and Miller 2001)—the idea that professing responsibility in one domain might lead actors to feel authorized to act less morally in another—has also been suggested as an unintended consequence of Sarbanes–Oxley and other regulations (Megan’s Law Journal 2012). Others suggest that a paucity of inside ownership, long-standing organizational or professional tenure, inadequate internal controls, and an unquestioning culture of trust combine to create a toxic cocktail that undermines organizational barriers to wrongdoing (e.g., Finotti 2011; Weinschenk 2012).

All of these processes have analogs in the world of science. In the cold fusion case, for instance, we see increases in the scale, number, scope,
| Hypothesized determinants of intentional and unintentional errors in scientific inquiry |
|----------------------------------|----------------------------------|
| **Network embeddedness**          | **Intentional errors**            |
|                                  | Higher in hub and spoke-shaped networks, by higher-status boundary spanners |
|                                  | Evidence over time of assortative mating by error rates (through mentorship and collaborator relationships) |
| **Organizational design, demography, and division of labor** | **Unintentional errors** |
|                                  | Evidence over time of assortative mating by error rates (through mentorship and collaborator relationships) |
|                                  | Increase with complexity, hierarchy, and division of labor |
|                                  | Possible increase with “outsourcing” of key facets of research process |
|                                  | Increase with “outsourcing” of key facets of research process |
|                                  | Increase with diffusion of PI effort across multiple projects and/or roles |
| **Institutional field**           | **Intentional errors**            |
|                                  | Decrease with scope and quality of internal review processes at sponsoring institution |
|                                  | Decrease with scope and quality of internal review processes at sponsoring institution |
|                                  | Increase with team diversity (demographic, functional, geographic, etc.) |
|                                  | Decrease with maturity and competitiveness of research field |

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Table 7.2 (cont.)

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<thead>
<tr>
<th>Intentional errors</th>
<th>Unintentional errors</th>
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<tr>
<td>Higher in fields with permeable status hierarchies</td>
<td>Lower in fields with rigorous, lengthy journal review processes</td>
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<tr>
<td>Lower in fields with rigorous, lengthy journal review processes</td>
<td>Higher in “hot” fields with many new entrants</td>
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<tr>
<td>Higher during periods of economic or political upheaval affecting the research field</td>
<td>Higher on projects with corporate funding</td>
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<tr>
<td>Lower on projects with corporate funding (more cautious vetting given financial stakes)</td>
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<tr>
<td>Status processes</td>
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<tr>
<td>U-shaped relationship (middle-status conformity)</td>
<td>Inverse relationship (status as proxy for quality); net of experience and training, positive relationship due to competing demands of multiple responsibilities among those with high status</td>
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<tr>
<td>Increase with status inequality on research team</td>
<td>Possible increase with proximity to promotion and research-funding decisions</td>
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<td>Increase with proximity to promotion and research-funding decisions</td>
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complexity, skill diversity, and geographic scope of the projects under management within investigators’ portfolios. In the Schön case, we see heightened performance expectations, including from corporate sponsors, along with an erosion of internal controls, possibly driven by increased cost pressures. In the Stapel case, we see hierarchical control supplanting norms of collegiality and shared responsibility. We also
find it intriguing that the network structures observed in these cases mirror those that distinguished between corrupt and non-corrupt projects at Enron (Aven 2012). Academic researchers have also begun to face increasing human subjects and clinical trials requirements, conflicts of interests, and other ethical mine fields, which may have fostered moral credentialing effects similar to those that observers of corporate wrongdoing attribute to Sarbanes–Oxley. More generally, numerous scholars have noted that higher education and academic science have become increasingly “corporatized” (e.g., Soley 1995; Bok 2004; Mills 2012), implying that academic science has become increasingly similar to big business along many of the dimensions claimed to have heightened corporate wrongdoing.

Indeed, by virtue of these similarities, we believe that studies of wrongdoing in the scientific setting hold the promise of forwarding our understanding of intentional and unintentional misconduct in the broader organizational world. As noted above, insofar as scientific collaborations represent relatively “weak” organizations, documenting the importance of social structure to wrongdoing in science should provide a lower bound on the importance of similar processes in other organizational settings.

In developing hypotheses about likely correlates of scientific wrongdoing, we have found numerous parallels between known cases of corporate and scientific wrongdoing. To develop a true understanding of what increases the likelihood of corporate or scientific wrongdoing, however, we must move beyond examining only cases where misconduct has occurred. Here, the study of wrongdoing in science has an unusually attractive feature. The public and digitized nature of the outputs of contemporary scientific research makes detecting errors and fraud considerably more feasible than is the case for most organizational behavior. Academic scientists almost inevitably leave a wider trail of publicly available forensic evidence than do corporate actors, enabling outsiders to assess the veracity of the scholarship. Furthermore, a variety of methodological tools allows one to assess the likelihood of misconduct even in situations where it has not (yet) been identified or alleged, thereby circumventing the selection bias that perennially plagues research on organizational wrongdoing. Hence, we believe that studying wrongdoing in academic science – and building increasing bridges between the sociology of science and organizational and economic sociology – will greatly enhance our understanding of
the social structure of intentional and unintentional wrongdoing within contemporary workplaces.

References


