A Social Control Perspective on Scientific Misconduct

Scientific misconduct, in recent years, has drawn intense press coverage and substantial policy attention. Various explanations have been advanced to account for its occurrence, ranging from the individual deviance of scientists to the collective transformation of contemporary science, and though each explanation illuminates a facet of the phenomenon, all seem incomplete in some crucial respect and suffer from a dearth of empirical evidence. In this article I briefly review definitions of misconduct and available evidence about its prevalence and critically examine three sorts of explanations: individual psychopathology, anomie, and alienation. Drawing on recent theories of social control, I then suggest a perspective that focuses on the social response to misconduct, not the causes of misconduct itself. I close by outlining some of the implications of this perspective for research and policy.

Definitions of Scientific Misconduct Are Vague and Unsettled

Before a phenomenon can be measured or explained it must first be defined, yet definitions of scientific misconduct are vague, changeable, and disputed [7, 20, 26, 34]. It is difficult to settle on a definition of
tion, these two authors offer policy recommendations to buttress the deterrence, detection, and sanctioning of scientific impropriety.

The articles of this special issue should be useful to scholars interested in the study of various facets of scientific misconduct, because they contain much heuristic research value; several of them include directions for further research. In addition, they serve to circumscribe the possibilities and limitations for the exercise of "collective responsibility" for addressing scientific impropriety. Therefore, individual faculty, department chairpersons, laboratory directors, chief academic officers, university presidents, journal editors and reviewers, and public policy makers should find these articles useful to the development of policies, procedures, and programs designed to deter, detect, and sanction scientific malfeasance.

If "collective responsibility" for addressing misconduct is not effectively exercised, then scientists and practitioners alike will be misled by research begotten by wrongdoing. Moreover, public trust and support for science will erode to such a degree that the resulting lack of trust could affect the autonomy of the scientific community to conduct research and hamper future contributions to knowledge. Thus, this issue is devoted to the exercise of responsible self-regulation through increased efforts to deter, detect, and sanction scientific improprieties. The obligation for such efforts rests with the "collectivity of responsibility" delineated in these articles.

References

scientific misconduct because research practices are not well codified or understood, the gradations between proper (even exemplary) technique and misconduct are subtle, and the process of settling on a definition is muddled by considerations of the politics and public image of science [10, chap. 5]. Behaviors that scientists may be willing to accept among themselves (concerning, for example, the selection of data for publication or the proper recording and storage of laboratory notes) may be judged unacceptable by laypersons. Even within science, the acceptability of certain behaviors has varied over time, from field to field, and by the state of knowledge within a field [40].

At present the quandary of definition has taken its most pointed form in a dispute between proponents of a quite limited definition of misconduct (as put forth in a recent National Academy of Sciences report [34]) and those who favor a more inclusive definition (as employed by the Public Health Service [12] and the National Science Foundation [7, 8]). The Academy report argues that the definition of misconduct in science should be limited to “fabrication, falsification or plagiarism in proposing, performing, or reporting research”[34, p. 27]. Questionable research practices, including failure to retain good records, unwillingness to disclose or share data, improper allocation of credit, and the “misrepresentation of speculations as facts” are relegated to a category of lesser severity because such behaviors “do not directly damage the integrity of the research process”[34, p. 28]. Harassment, misuse of funds, vandalism, gross negligence, and similar misbehaviors are placed by the Academy panel in a third category because other remedies exist for these generic forms of misconduct.

For the NSF, by comparison, scientific misconduct includes fabrication, falsification, plagiarism and “other serious deviation from accepted practices”[7, p. 647]. This more comprehensive definition is intended to reflect the ethics of the scientific community and to provide a forum for adjudicating a broad range of complaints that actually arise in the research process. For example, of the 124 cases considered by NSF during a three-year period, only 10 had to do with data fabrication or misrepresentation, whereas 70 concerned intellectual property rights ranging from plagiarism to “theft of research ideas [and] failure to give credit”[7, p. 584].

The inclusive definition has the advantages of clarity and simplicity — there is only one type of misconduct, not three. By addressing a greater range of behaviors that palpably harm the research enterprise, the inclusive definition may better serve the scientific community’s need to air and resolve most complaints on its own, thus reinforcing the professional autonomy and self-regulation of scientists. By casting a broad
net, however, the inclusive definition risks expending much energy, emotion, and legitimacy on cases that may be relatively unimportant to science, scientists, and society, whereas the limited definition may have the virtue of concentrating attention on fewer but arguably more serious sorts of misconduct.

Counts and Rates of Misconduct are Probably Inaccurate

Setting aside ambiguities in the definition of misconduct, what is known about the number of cases and their prevalence in the population of U.S. scientists? Hyperbolic and unfounded claims for the purity [28] or pollution [1, 13, 38, 39] of science had been the norm for years. Recent regulations governing the investigation and reporting of alleged misconduct have brought many cases to light and dispelled some of the confusion. Even so, reporting is neither comparable across agencies nor complete and it is impossible to know what proportion of all occurrences are reported [see 34, chap. 4 for a summary].

There are two sources of data about scientific misconduct, official counts of cases processed by oversight offices and survey-based self-reports of scientists’ experiences. The two sources suggest rather different rates.

In the three-year period ending July 1992, NSF resolved 67 allegations of misconduct, finding 3 cases of plagiarism and 1 case of fabrication (57 more cases remained open). In roughly two years’ time, the Office of Scientific Integrity of the Public Health Service (PHS) closed 110 alleged cases of misconduct in science, completing full investigations of 21 cases, and finding misconduct in 15 of the cases [34, pp. 84–85]. Drawing upon various sources, Patricia Woolf counted 26 instances of misconduct for the 1980–87 period [49]. Taken together, these amount to 45 cases in a twelve-year period, or about 4 cases per year.¹

In contrast, a survey of universities that belong to the Council of Graduate Schools found that about 40 percent (118) of the graduate school deans received allegations of possible misconduct during the period 1983–88, and that institutions receiving external research support in excess of $50 million per year were far more likely than others (69 percent to 19 percent) to hear such allegations [44]. Similarly, a survey of AAAS members, conducted in late 1991, found that 27 percent of the 469 respondents had “personally encountered or witnessed scientific research that [they] suspected was fabricated, falsified, or plagiarized” during the past ten years”[46, p. 187, emphasis in original]. But as Teich notes, possible nonresponse bias, inconsistent definitions across respondents, and vagueness in the meaning of “personally encountered” make
it difficult to evaluate this result [186–87]. Finally, a survey of 2,000 doctoral students and 2,000 faculty from the largest graduate departments in four fields (chemistry, civil engineering, microbiology, and sociology) found that about 7 percent of respondents "observed or had direct evidence" of data falsification or plagiarism by faculty. Much larger fractions reported exposure to questionable research practices (such as misallocation of authorship credit or inappropriate use of research funds) and other forms of misconduct (such as harassment, discrimination, or ignoring research policies) [45]. Although this survey did not directly estimate the prevalence of misconduct and it is difficult to make sound inferences from the sample to a population of researchers, the study does clearly demonstrate that scientists have substantial exposure to various forms of misconduct.

What can we conclude from these various sorts of data? Is it true, as Paul Friedman has suggested, that the "prevalence of such fraudulent acts [as deliberate data fabrication, falsification, or plagiary] is known to within an order of magnitude" [20, p. 154], or are known instances merely the tip of an iceberg [13]? I certainly share Friedman's exasperation with misconduct and his judgment that sloppiness and minor ethical infractions may damage science far more than the notorious cases of egregious misconduct, but I doubt that the prevalence of misconduct is known within two orders of magnitude.

Thus it is difficult to know if there is much or little misconduct in science, if the rate has risen or remained constant, or if variation in the number of reported cases indicates change in behavior, ethical standards, reporting systems, publicity, or any combination in unknown proportion. Even if a rate could be calculated, by what standard can we judge it to be large or small, acceptable or reprehensible? In all, knowledge of the number and prevalence of cases of scientific misconduct is poor but improving. Yet against this backdrop of disputed definitions and meager data several reasonable explanations have been offered to account for misconduct.

Three Explanations of Misconduct

Following the pattern of Harriet Zuckerman's reviews [51, 52] major explanations of scientific misconduct are conveniently considered under three broad headings: individual psychopathology, anomie, and alienation. I will briefly review and evaluate each explanation in turn, then offer a more comprehensive perspective on misconduct that focuses on the exercise of social control, not the event of deviant behavior [41, pp. 685–88].
Individual Psychopathology

Individual psychopathology is the simplest, most intuitive, and least satisfying explanation for scientific misconduct. Social scientists may have little regard for this explanation, but it seems widely held among scientists who comment on misconduct. Proponents of this view tend to believe that misconduct is rare, that its occurrence reflects defects of personality or upbringing (significantly, defects acquired prior to the misconduct's arrival at their institution), and that systemic intervention would be useless or damaging. They would "medicalize" the offense, ostracize the offender from the scientific community, and continue with research as usual. In this view, William Summerlin inked dark patches onto a white mouse (to suggest that skin grafts had taken) not because of performance pressure or faulty supervision but because of a medical condition that warranted a year's leave of absence and appropriate treatment [24].

Efraim Racker, of Cornell University, offers the most persuasive statement of the individual psychopathology explanation. Racker was the graduate advisor of Mark Spector, a "brilliant young man [who] wrote poetry, painted and was a superb experimenter," but who also seeded autoradiograms with a contaminating isotope to fabricate data in support of his hypothesized "kinase cascade" [35, p. 91]. Drawing upon personal experience with this case and three other instances of fraud, Racker distinguishes between "professional" perpetrators of fraud who are intelligent, informed, skilled experimenters who know exactly which research problems matter most for their field and then convincingly falsify and fabricate experiments to solve those problems, and "amateurs" whose fabrications are clumsy, perhaps trivial, and easily detected [35, p. 91]. Professional fraud, Racker argues, is the product of "mentally unbalanced, . . . emotionally and mentally ill" persons who "subconsciously want to be caught" because they must know that the possibility of successfully falsifying such significant scientific work is very small [35, p. 91]. The fraudulent acts of such persons, arising as they do from illness, could be neither detected nor deterred by increased oversight and regulation.

Individual psychopathology is an inviting explanation for the misconduct of otherwise brilliant persons who have a tragic flaw. Yet for most social scientists it is an unsatisfying explanation. First, the attribution of personality disorder, though convenient, is vague and unmeasured. No one has stated precisely what is wrong in such personalities and then measured those qualities. Second, many of the characteristics that seem to underlie putatively defective scientific personalities also
seem to characterize effective, even eminent scientists: imagination, boldness, persistence, self-assurance, single-mindedness, and disregard for orthodoxy, to name a few [33]. Thus, a useful explanation must indicate which other factors determine whether such qualities lead to the best or the worst of science and how such factors operate. Third, many of the same personality traits attributed to those accused of scientific fraud have been applied to other scientists whose actions or ideas are controversial or inconvenient, including whistleblowers such as Margot O’Toole (whose complaints about irreproducible data set in motion the Baltimore case), iconoclastic skeptics such as Peter Duesberg (who remains vocally unconvincing that HIV causes AIDS), and such zealous inquirers into the soundness of research as Walter Stewart (who, along with Ned Feder, has been a fraudbuster without portfolio at NIH). Innovators and critics are often labeled “disordered personalities” as a tactic of control. Finally, the individualistic explanation is too convenient and too self-serving of the interests of established scientists to be accepted on faith and assertion without evidence. The psychology of scientists remains a promising but underdeveloped topic of research [20; see 17 for a review]. Psychological factors probably do play a part in scientific misconduct, but the arguments and evidence linking personality to misconduct in science are too thin and unsystematic to warrant much credence.

Anomie

In its contemporary presentation, anomie is connected to deviance through Robert K. Merton’s theory of anomie and social structure [30]. According to this view, deviance may arise when great cultural value is placed on achieving an end, but the means for its achievement are unavailable to persons in certain positions. Under such circumstances, some may reject the culturally prescribed goals, the socially endorsed means, or both, resulting in various sorts of deviant behavior. The extremely high value placed upon originality in science, combined with differential access to the intellect, training, assistance, research resources, and good fortune necessary to produce such results, constitute just such anomie circumstances [30, 51, 52]. In addition to Merton’s formulation, there are two further theories of anomie and deviance that may be applied to science.

By comparison, anomie as originally used by Durkheim [14] is a state of moral deregulation resulting from a period of rapid social change during which social arrangements outstrip principles and mechanisms of social control. There is great potential for anomie of this sort in contemporary U.S. science, caused by specialization, technical innovation and
the attendant obsolescence of skills, the changing organizational culture of academic science, new goals and bases for legitimating scientific research, and a changing relationship between performance and reward [22, 23]. Under anomic circumstances behavior is only weakly guided by shared rules, and thus deviance results.

Anomie may also be seen as an extreme form of sociological ambivalence [31], for when there is great uncertainty about which of two or more values to honor in a particular social situation, the resulting behavior may be unpredictable, if not chaotic. The presence of “counternorms” [33] and new norms of science [50], as well as the new entrepreneurial spirit on campus [16] are precisely the circumstances that might give rise to anomie.

The anomic explanation of misconduct has more sociological appeal than the individualistic explanation, but it is very difficult to substantiate empirically. In Merton’s formulation, where anomie results from a gap between cultural goals and structurally determined opportunities and resources, one might predict greater anomie and greater prevalence of misconduct among scientists in positions of lesser prestige and resources. Yet there is no evidence that this is so; to the contrary, a large fraction of known cases involve senior scientists employed by prestigious universities with substantial research programs [44, 49]. But the data are so poor that mere counts of incidents are suspect and rates are unfoathomable, so the proposition remains untested.

The main difficulty with the two more general versions of anomie theory is that anomie is so profound, pervasive, and multifaceted that one would expect almost every scientist to be deviant in some respect. But this pervasive disregard for principle does not seem to plague academic science. According to self-report data, academic scientists claim close adherence to the norms of disinterestedness, universalism, and communality, with substantial deviation only from the norm of organized skepticism [4, table 2, p. 220].

Alienation

Alienation is the separation of a worker from the work, the self, or other workers. Specialized, segmented work roles cause alienation by restricting the application of talent and creativity in the workplace, thus limiting the worker’s opportunities for self-expression and engagement with the work. Accompanying the objective circumstance of being alienated from work are subjective feelings of estrangement, malaise, and disengagement.

Science, long regarded as a calling or vocation that evokes profound
personal engagement, would seem the antithesis of an alienating career. But in his 1917 lecture, "Science as a Vocation," Max Weber observed that the "large institutes of medicine or natural science are 'state capitalist' enterprises" where one encountered "the same condition that is found everywhere capitalist enterprise comes into operation: the 'separation of the worker from his means of production.' The worker, that is, the assistant, is dependent upon the implements that the state puts at his disposal; hence he is just as dependent upon the head of the institute as is the employee in a factory dependent upon management. . . . Thus, the assistant's position is often as precarious as is that of any 'quasi-proletarian' existence" [47, p, 131].

Weber, of course, was arguing that the social organization of academic science in Germany alienated junior scientists. This view of German academic science in the early years of the twentieth century is echoed in the social organization and culture of contemporary U.S. academic science [16, 22, 36, 43]. Further, there is some survey-based evidence that U.S. scientists [11, 29] and university faculty report feelings of alienation that are expected to accompany the structural condition [4, table 2, p, 220].

Alienation can contribute to misconduct by eroding the social circumstances necessary for effective social control [48]. When research becomes so specialized or so strongly mediated by instruments that a scientist cannot appreciate the connection between daily tasks and the larger purposes of the research, then expediency and wishful thinking may supplant the requisite diligence and skepticism. When the products of research become esoteric in character and collective in production, then the scientist can hardly believe, in Weber's poignant phrase, that "the fate of his soul depends upon whether or not he makes the correct conjecture at this passage of this manuscript" [47, p, 135]. When the interests of research group members diverge because of gross differences in autonomy, financial reward, job security, career prospects, or recognition for performance, then alienation may rise along with the likelihood of personal animosity and scientific misconduct [22]. When the rewards and satisfactions of science are overshadowed by organizational demands, economic calculations, and career strategies, then science is no longer a vocation.

Although the proposed process is different, alienation theory shares many of the same difficulties as anomic theory. The structural circumstances that might give rise to alienation are pervasive, but the level of alienation among scientists has not been measured (and is probably lower than that of other occupations). One would need to decide whether
the structural fact or the subjective sentiment of alienation has priority in an explanation, then one would need a sound measure of misconduct in order to determine whether alienation has had an effect. Available evidence suggests that structural alienation in science may be common and increasing, so misconduct should also be quite common. To explain its nonoccurrence would require a helper theory of contingencies that trigger (or suppress) the behavior or facilitate detection and investigation. Given the state of knowledge about misconduct and difficulties in studying deviance, especially among the relatively powerful, such explanations may long remain speculative.

I do not wish to be despairing or disparaging of research into the causes of misconduct in science, yet the preceding overview suggests that explanations cannot be developed or tested without a leap in the quality of data, clarity of definitions, and precision of hypotheses. Such research must go on, not only because the proposed explanations are plausible but also to produce incremental gains in understanding and to sustain pressure for better data and research resources. But I fear those approaches are unlikely to yield much new insight in the near term. Indeed, arguably more important features of the rising concern about scientific misconduct may be overlooked if research is too narrowly focused on the causes of scientists' putative misbehavior. For the balance of this article, I wish to use recent ideas in the theory of social control to call attention to the societal reaction to scientific misconduct and to develop this new, more comprehensive perspective on the phenomenon.

**Scientific Misconduct and the Exercise of Social Control**

Though we do not know much about the prevalence and causes of scientific misconduct, we do know that the amount of public attention and policy concern devoted to scientific misconduct has grown dramatically over the past two decades, spawning federal guidelines [12], investigative offices at PHS and NSF, congressional inquiries, blue-ribbon panels [34], and uncounted scholarly papers, edited volumes, conferences, journalistic articles, and sessions at professional meetings. Not only the volume of interest in scientific misconduct but also the character of that interest has changed. In 1976 Donald Black observed that the system for penalizing misconduct in science was "informal and decentralized, rarely involving litigation or formal action of any kind" [2, p. 80]. Similarly, in her 1977 review essay, Zuckerman proposed that systematic records of scientific misconduct were not kept because "scientists are themselves the primary consumers of one another's products and services [and] typ-
ically have ‘clients’ who are qualified to appraise the products and services they receive’ [51, p. 107]. But in recent years the informal, decentralized, and private process they describe has given way to one that is formal, centralized, and public.

It may be valuable to ask why such changes in the amount and character of concern have occurred and what consequences they may have for understanding the changing relationship between science and society. Let me first develop a perspective for thinking about efforts to increase external control of science, then turn to some of its implications for research and policy.

The level of social concern about any form of deviance reveals much about the society that disapproves of the behavior. On one side of the coin, social standards are formed and deviance is defined in the act of deciding whether or not certain behaviors are acceptable, whereas on the other side, “much of the social control apparatus is consciously fashioned through the visible hand of definable organizations, groups, and classes, rather than being ‘naturally’ produced by the invisible hand of society” [41, p. 686]. This means that changes in the exercise of social control may reflect the distribution of power, lines of conflict, and persuasive ideologies of a society.

Thus we may ask of misconduct in science, as Donald Black has asked about law and social control in general: how and why are certain behaviors defined as misconduct and placed under increased scrutiny and regulation? [2, 3, 41] That is, why has scientific misconduct recently received so much public attention and opprobrium? Why have there been calls for increased oversight and regulation? To raise such questions does not deny, excuse, or justify scientific misconduct, but calls into question the changing exercise of social control rather than accepting it as arising naturally from a sea of increasing misconduct in science.

When one thinks of how prestigious, successful, powerful, and independent U.S. science has been during the past forty years, it may seem odd to be concerned about increasing external oversight and control. Yet the notion of a relatively autonomous science is new and fragile, grounded in a constellation of philosophical, political, and technical arguments, abetted by the unique economic and social conditions of the postwar era. As historians remind us, “scientific activity, the scientist’s role, and the scientific community have always been dependent: they exist, are valued, and supported insofar as the state or its various agencies see point in them” [42, p. 339]. What is it about contemporary science that may attract the attention of powerful social interests and invite their efforts to assert greater control over the enterprise?

First, science has become increasingly expensive and visible as per-
formance expectations have risen, budgets have swelled, mega-projects have emerged (such as the superconducting supercollider and the human genome initiative), science journalism has gained prominence, and specialists in public relations have touted the latest scientific triumphs. In all, science has become a more public enterprise, winning fame and consuming resources while inviting increased accountability and scrutiny through the mere fact of its success.

Second, science is a resource for power, offering the possibility of control over aspects of nature and the attendant ability to alter the world. Atomic physics, information processing, molecular biology, and the development of various antibiotics and vaccines are among the notable accomplishments of twentieth-century science. Such products and processes are hardly neutral, but instead serve the interests of various powerful groups in society.

Third, science is a source of intellectual authority, legitimacy, and prestige capable of justifying actions that might not otherwise win acceptance. Invocations of science in matters of policy or law lend a logical, objective, and definitive tone to the deliberation. But scientific arguments may also be used to disguise political preferences and the exercise of power, and therefore the ability to influence and invoke scientific research conveys the ability to develop or restrict such arguments. Conversely, elites may wish to control science because independent inquiry may threaten to reveal the exercises of preference and power that might otherwise be concealed in the guise of objective necessity.

Fourth, science is held in such high esteem that some accord it the status and purity normally reserved for a religious order. (Hence the enduring image of science as a vocation or calling to religious office.) Others employ the religious metaphor as a rhetorical device to justify or provoke moral outrage. But in either instance, if science is considered a sacred institution, then misconduct would be sacrilege and would evoke moral outrage. Books about scientific misconduct bearing such titles as *Betrayals of the Truth* [5], *Impure Science* [1], and *False Prophets* [27] convey this sense of sacrilege in their images of betrayal, purity, and revelation.

Fifth, the knowledge-based elitism of science may provoke leveling urges, and such urges may be especially intense in times of growing social inequality. In his study of deviance among New England Puritans, Erikson suggested that in those times of increasing social differences the pursuit of social deviants gave rise to a moral solidarity that bridged the social distance between people [15, p. 9]. Perhaps the recent rising concern for scientific misconduct represents a similar effort to direct hostility toward an intellectual elite and away from political and social elites.
Last and most important, the past decade or so has seen an intensification of the postwar tendency to forge a more speedy and certain connection between scientific research and such putative national purposes as high-technology medicine, innovative weaponry, policy formulation, international prestige, economic competitiveness, and the resolution of legal disputes [36]. To this end,

Congress and the executive branch have sought to transform the very nature of science. Setting research agendas, intervening in peer-reviewed grant decisions, micromanaging federal laboratories, and refusing to fund research that is not politically or morally “correct” are but a few manifestations of the government’s effort to gain a greater control over U.S. research. Nowhere, however, is the heavy hand of the government more apparent than in the area of ethics [9, p. 76].

Less grandly stated, science has acquired increased importance within organizations (such as universities and businesses), requiring scientists to engage in more intense interactions with lawyers, accountants, public relations specialists, and professional administrators. For example, as university budgets become more dependent upon research funds, commercial contracts, and indirect cost recovery [21], scientists may experience organizational pressures to become more entrepreneurial, to undertake more funded research, and to perform it according to businesslike practices of accountability, efficiency, secrecy, and the like. In the aggregate, this might increase competition among scientists for research resources, as more scientists are urged to do more research and prepare more proposals. However, this intensified competition threatens to compromise scientists’ standards of cooperation, communication, quality, and problem choice [22]. Similarly, if new results are pushed more rapidly to application [36], then the time available for error correction within science, including detection of misconduct, will be reduced while the consequences of error become more immediate and serious.

In itself this tighter coupling of science to other social and organizational purposes would be expected to increase scrutiny and the likelihood of intervention. With more riding on the outcome, those who stand to gain through the efforts of scientists will be motivated to ensure that the promised goods are delivered on time and on budget. Also, it is in the very nature of these professions to manage, regulate, and oversee activities; that is what managers, lawyers, and accountants do for a living. But beyond such obvious effects, tight coupling might also affect the social organization of science, scientists’ roles and career opportunities, and even the principles that guide their work [22, 36, 43].
As scientists are brought into more frequent, intense, and consequential contact with other professions, competition for dominance between professions becomes more likely. In this regard, Sheila Jasanoff has asked "why has science — historically so robust in contests for power, prestige, and patronage — proven so weak and defenseless when called to account in legal proceedings?" [25, p. 345]. While she may have overstated the case — science has hardly seemed "weak and defenseless" — the very language used to describe the interaction ("science called to account") reveals much about the relative power of these two professions, the nature of their encounter, and the turf on which it takes place. Within the university, scientists' work falls increasingly under the purview of professional managers, accountants, technology transfer agents, public relations specialists, and development officers. Such interactions also occasion tests of strength between professions wishing to impose their standards on common but contested turf.

**Implications for Research and Policy**

For all these reasons scientific misconduct and science itself have received increased attention during the past decade. Importantly, this increased attention and its secondary consequences for the social organization of science would be expected even if the (unknowable) rate of misconduct did not change at all. (Indeed, evidence that scientific judgments may have changed more than scientific behaviors can be found in the "epidemic" of misconduct among scientists long dead, including Freud, Galileo, Kekulé, Mendel, Newton, and Pasteur.) But what are the implications for research and policy that follow from viewing public concern about misconduct as a change in the science-society relationship?

**Some new research ideas.** The ideas sketched above suggest new avenues of research and emphasize the pertinence of enduring themes in science studies that have to do with external efforts to control science and the changes in social organization that may result. One theme that merits more attention is the nature of interactions between science and other professions, including law, engineering, and business. In the case of science and law, which has received much recent attention, we need to know more about the standards of argument and evidence that prevail in each domain, and how those standards might be reconciled when they disagree [37]. We also should examine interaction between members of the two professions — say, expert witnesses' testimony in court, judges' decisions about the admissibility of scientific evidence, or lawyers' behavior as participants in research or patent applications — to learn more about the interplay of knowledge and power that occurs in such exchanges. At a more macrosocial level, the demands each profes-
sion might place on the other and the consequences of such demands for professional autonomy and practice are worthy topics of research.

A second theme is how the social organization of science changes in response to new organizational conditions, funding contingencies, and career constraints. Does increased contact between science and industry, for example, alter scientists' research agendas, standards of scholarship, research ethics and practices, professional roles, or definitions of research? The newly-acquired "scientific" status of such fields as materials research, decision science, and manufacturing suggest that what is termed "science" may change significantly in response to such connections. Does the scientist's role change as well, "hybridizing" to incorporate elements of the entrepreneur or business person? What of the scientists actions and values?

Third, is there evidence that science is now controlled by other professions, and if so, how and with what consequences was this accomplished? Study of regulations governing misconduct and their application in a broad range of cases may reveal new power arrangements. For example, there has been a neutral, technical tone to much that has been written about the procedures followed by universities and government agencies in the investigation of allegations of scientific misconduct. The impression is that these are procedures devised in the best interests of all, with little attention to the professional values served by process. Yet, one may also view these as tests of strength, pitting scientific standards of investigation, evidence, proof, and procedure against those of law or other professions. In that light the procedures are explicitly seen as value-laden, hardly the neutral, technical protocols they might first appear to be.

Finally, cross-national studies of scientific misconduct and the social response it elicits would be valuable occasions for examining the role of government agencies, economic interests, university organizations and ideologies on the exercise of social control over science.

A re-examination of policies. Using instances of misconduct to reexamine the science-society relationship also raises questions about recently proposed policies for science. Rising concern about scientific misconduct has elicited calls for tightening the connection between science and other professions — policies purportedly designed to wring out misconduct, indifference, and shoddy work — and such calls have met with grudging acquiescence from some scientists. For example, an observer closed his remarks on the increasing external control of science with these words:

Such new procedures would also mean still more academic bureaucracy, still more paperwork, still more committees and powerful chairmen, and further
erosion of academic freedom and the attractiveness of a career in research. Is this additional oversight necessary or desirable? Probably not. However, the question is, Do we have a choice? [18, pp. 1937–38].

A second observer contended that, as preceptors become responsible for increasing numbers of young scientists, the preceptors find themselves overburdened, unable to transmit the norms, values, standards, and practices of good science at the laboratory bench as they once had done. Into this vacuum, she argued, the universities must inject greater formal controls, more oversight and regulation [32, pp. 1932–36].

The perspective on social control sketched above would not accept such policy conclusions at face value but would raise the possibility that the exercise of social control was motivated by reasons other than the misconduct of scientists. It would demand that we ask whose interests were served and whose were set aside by the proposed actions. Further, the social control perspective would suggest, in conjunction with the alienation and anomie explanations of scientific misconduct, that such policies may be counterproductive because more scrutiny may create more interprofessional conflict, weaken and confuse internal standards of conduct, further alienate scientists from their work and from one another, resulting in more questionable behavior and more demands to control science.

Instead, policies might be devised to renegotiate the relationship between science and society in a way that might alleviate pathogenic pressures. Some specific suggestions include:

1. Loosen the connection between science and other social purposes by reinforcing (rather than supplanting) scientific standards of evidence and argument, ensuring that adequate review and replication stand between research and product, and by establishing more realistic hopes about which sorts of problems are amenable to scientific solutions.

2. Reduce and redistribute the financial rewards immediately available to scientists, their companies, and their universities by installing a form of “escrow” account to hold profits for a fixed period of time [19] and by replacing indirect cost allocations with more stable infrastructural support.

3. Lessen the extremes of reward and status that increasingly characterize science, as they do other activities ranging from publishing to sports, thus relieving some of its unhealthy competitive pressure and debilitating inequality of career prospects and working conditions.
4. Do not allow scientists to take on unreasonable responsibilities for graduate and postdoctoral education but ensure that they take more seriously their duties as role models and exemplars of ethical conduct.

5. Decouple graduate student and postdoctoral support from research grants, reversing a trend of the late 1970s and 1980s, to allow students the freedom to leave unsuitable arrangements without jeopardizing their careers.

6. Counteract the tendency to hire more adjunct faculty, doctoral research associates, and others whose standing within the university is insecure. By the very nature of their positions such persons are more susceptible than regular faculty to inappropriate influence by others and more easily displaced with changes in the resource environment. (Unfortunately, for precisely those reasons some institutions find them attractive to hire.)

7. Resist the trend to replace intrinsic rewards and controls with extrinsic ones, as such measures will weaken science’s capacity for self-regulation. Instead take steps to restore and fortify the intrinsic satisfactions of science and its internal mechanisms of social control.

8. Improve public understanding of the practice of scientific research, thus developing a public literate in both the substance and practice of science. Of particular concern are the overly idealized perceptions of scientific knowledge and research practices held by some judges, public officials, and business executives.

If scientific misconduct and efforts to control it are viewed within the framework of a whole society, not treated as an individual pathology or institutional failing, then it may be possible to understand the origins, significance, and potential remedies for such behavior and to invent new patterns of interaction that may reduce misconduct, alleviate its dangers to others, and avoid harmful remedies.

Notes

1In the summer of 1993 NIH published the names of 15 persons found to have committed scientific misconduct in the NIH Guide to Grants and Contracts (25 June 1993 and 30 July 1993).

2The recent Supreme Court decision in the case of Daubert v. Merrell-Dow Pharmaceuticals will demand greater scientific discernment on the part of judges and, by bringing science and law into more intimate contact with one another, will alter the relationship between science and law in ways that are difficult to predict.
References

22. Hackett, E. J. “Science as a Vocation in the 1990s: The Changing Organizational


