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# Truth and Trustworthiness in Research

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## Description

This paper develops an overview of the subject of trustworthiness among researchers. Beginning with the breaches of trust that constitute major wrongdoing in research misconduct, Whitbeck argues that these are more often examples of lesser betrayals and defections that undermine trust, than they are of outright fraud. Whitbeck also considers trust and trustworthiness among collaborating researchers and a range of intentional and unintentional behaviors that influence the character of these trust relationships, particularly between a supervisor and a supervisee.

## Abstract

We have recently reached a watershed in the research community's consideration of the ethics of research. The way is now open for a more nuanced discussion than the one of the last decade which was dominated by attention to legal and quasi-legal procedures for handling misconduct.<sup>[1](#)</sup> The new discussion of ethical issues focused on trustworthiness takes us beyond consideration of conduct that is straightforwardly permitted, forbidden or required, to consideration of criteria for the responsible as contrasted with negligent or reckless behavior.

This paper develops an overview of the subject of trustworthiness among researchers. It illustrates and discusses various types of betrayal and defections in research conduct, and locates these in relation to many of the situations discussed elsewhere in this issue.

Beginning with the breeches of trust that constitute major wrongdoing in research (research misconduct), I argue that these are more often examples of negligence or recklessness than they are of "fraud." Acts of negligence and recklessness figure not only in misconduct, narrowly defined, but in many lesser betrayals and defections that undermine trust. The presence or absence of an intentional deception is not a sure indicator of the seriousness of some moral lapse. Such a lapse, where it does occur, may be simply a particularly poor response to perennially difficult research responsibility. Finally, I consider trust and trustworthiness among collaborating researchers and a range of intentional and unintentional behaviors that influence the character of these trust relationships. The supervisor-supervisee relationship is of particular significance because it is both a difficult area of responsibility for the supervisor and because this relationship is formative for a new researcher's subsequent expectations and behavior.

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# **Introduction**

The level of trust that has characterized science and its relationship with society has contributed to a period of unparalleled scientific productivity. But this trust will endure only if the scientific community devotes itself to exemplifying and transmitting the values associated with ethical scientific conduct.[2](#)

The scientific research enterprise is built on a foundation of trust: trust that the results reported by others are valid and trust that the source of novel ideas will be appropriately acknowledged in the scientific literature. To maintain this trust in the current climate of research, we believe that more attention must be given by the scientific community to the mechanisms that sustain and transmit the values that are associated with ethical scientific conduct.[3](#)

These quotations mark a watershed in the discussion of the ethics of research within the research community. The first is from the new edition of [\*On Being a Scientist\*](#) published early in 1995 and the second is from a recent article in *Science* magazine by National Academy of Sciences President, Bruce Alberts, and Institute of Medicine President, Kenneth Shine. The quotations lend an authoritative voice to the growing recognition that the research community must do more than develop quasi-legal mechanisms for handling charges of falsification, fabrication and plagiarism. They call for sustained ethical reflection on a range of questions of research responsibility. Central to this recognition is an emphasis on trustworthiness and not merely on trusting. Alberts and Shine concur with Harold Varmus in recognizing that it is a mistake simply to trust that science is self-correcting and ignore wrongdoing in research.[4](#) (*How are Scientific Corrections Made?* by Nelson Kiang and the commentary on that paper by Robert Guertin in this issue richly illustrate the point that such trust is naive and mistaken. As they argue, it is often very difficult to remove mistaken or even fabricated results from the literature.) The bulk of both documents from which these two quotations are taken concern, not the acts that are generally agreed to constitute "research misconduct," [5](#) but a host of subtler, and more common violations of standards of ethical conduct in research, violations that nonetheless erode the trust required for research to flourish.

Discussion of trust and trustworthiness in research takes us farther than the discussion of even general rules governing research practice, such as "Do not fabricate or falsify data" or "Only those who have contributed substantially to the research reported in an article should be listed as authors." Important as moral rules are as components of ethical standards, trustworthy behavior often requires the responsible exercise of discretion which is a much more complex matter than simple

rule-following.

Furthermore, consideration of trust and trustworthiness requires attention to the multiplicity of perspectives on an enterprise like research: every party to research trusts and is trusted in some way. Consideration of trustworthy behavior and the integrity of the research enterprise fosters the examination of that enterprise from the perspective of every party to it, rather than from the perspective of the rule makers alone.<sup>6</sup> Such an expanded perspective on research practice may be especially important in fields where it is the most junior and least prestigious members of research teams who actually make the observations.

As the philosopher Bernard Williams argued <sup>7</sup>, building the trust required for a complex cooperative enterprise requires understanding that situation. As he says, "there is no one problem of cooperation: the problem is always how a given set of people cooperates." Of the greatest interest for the conduct of research is the trust and cooperation among researchers:

- among collaborators, both among peer collaborators and between senior researchers and their trainees;
- among research groups who build on each other's work;
- among researchers and the institutions that are the primary sites of research and research education;
- among the individuals and groups who review, publish and disseminate research findings

Trustworthiness among researchers is necessary to retain the public trust, although it is not sufficient.

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## **Ethics & Competence in Trustworthy Behavior**

Consider the trust required for one member of a research team to use materials such as reagents, devices or computer programs, prepared by another member of the team, or the trust required for a researcher to base the design of a new project on results obtained by another laboratory. From the trusted party the truster needs

attention, concern, fairness, and competence as well as honesty. Emphasizing all of these factors is necessary because trustworthiness has too often been treated as the absence of deception.[8](#)

Writers on trust frequently suggest that trust is necessary because the trusting party cannot control or monitor the trusted party's performance. It is certainly true that the inability to control or monitor behavior is an element in the need for trust. Laboratory heads frequently candidly admit that the volume of data collected in their laboratories makes it impossible for them to personally check even their own graduate students research results; and would attempt to do so only if some reason were presented to doubt a result. Therefore, the circumstances that, at least according to some,[9](#) set the stage for misconduct, are now increasingly common. However, trust is also required in many situations in which one party could not evaluate another's behavior even if the first could monitor the behavior of the second.

Limits on the efficacy of monitoring is especially clear where research collaborators come from different disciplines. Two researchers from different disciplines engage in a collaboration would not benefit from full prescience of the other's actions, or even the ability to guide the other's behavior. Although one collaborator might be able to recognize some acts of gross incompetence or malfeasance on the part of the other, neither collaborator would fully understand the implications of all that she saw the other do and might have little idea of how to improve the other's performance. (The point that monitoring may be futile is also well illustrated in the situation of data collection that Stephanie Bird discusses in her contribution to this issue.) In the many circumstances of collaboration, responsible conduct has no adequate substitute. In particular, although audits of research behavior [10](#) can document untrustworthy behavior, they cannot eliminate it.

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## **Is *Fraud* a Common Form of Research Misconduct?**

To understand what makes for trustworthy conduct we need an understanding of the character of the defections and betrayals to which researchers are actually tempted.

The most serious types of research wrongdoing, commonly called "research misconduct" (or, less aptly, "scientific misconduct") are sometimes called "fraud." Is that term an accurate one for most of the major departures from trustworthiness in research?

Once one strips the legal notion of fraud of its requirement that there be a party who has been injured by the fraud,[11](#) there remain three elements:

1. The perpetrator makes a false representation
2. The perpetrator knows the representation is false or recklessly disregards whether it is true or false, and
3. The perpetrator intends to deceive others into believing the representation.

The third condition has turned out to be particularly hard to prove in recent misconduct proceedings - for example the Gallo/Popovic case [12](#) - but it should be noted that the second condition also fails to be met in the majority of agreed upon cases of fabrication and falsification. (Plagiarism, the misappropriation of another's work or ideas, differs from cases of fabrication and falsification in two significant respects: first, it leads others to believe a false attribution of authorship or invention rather than a false conclusion about natural phenomena, and second, it immediately injures the person plagiarized. Intention to deceive others into believing another's words or ideas are one's own is essential to plagiarism.[13](#) In these two respects plagiarism better fits the definition of fraud than do most cases of falsification and fabrication. Plagiarism is usually looked upon as more akin to theft than fraud, however. Plagiarism and other questions of publication are ably addressed in Rose and Fischer's paper for this issue, "Policies and Perspectives on Authorship." Fabrication and falsification will be discussed here.)

I do not take issue with those who prefer the term "fraud" to "misconduct" on the grounds that "fraud" has a nice ring of moral outrage. Rather my point is that we need more precise descriptions of the moral failings involved in cases of fabrication and falsification if we are to understand the causes of these defections and betrayals.

One case that meets the strict definition of fraud is the notorious case of William Summerlin.[14](#) Summerlin was a transplantation immunologist whose hypothesis that culturing before transplant would lessen rejection failed to be born out. The misrepresentations Summerlin made were so gross that they gave rise to the

expression "painting the mice" as a term for research fraud. A second is the case of Mark Spector who went to great lengths to concoct evidence to support Efraim Racker's new theory of cancer causation, a case that Racker has described in detail. [15](#) A third is the case of the falsification by the desperate graduate student that Nelson Kiang describes in his paper for this issue. However, the total number of fraud cases that I have been able to discover out of many cases of fabrication or falsification is well under ten.

Both the cases of fraud in the strict sense, and more common cases of fabrication or falsification of data or experiments to support a conclusion in which the researcher firmly believes, teach the important lesson that reliance on the self-correcting mechanisms in science is naive. It is, therefore, important to include both in discussions of trustworthiness in research. However, the relatively rare fraud cases have captured a disproportionate amount of attention. More typical cases give better insight into the moral lapses to which researchers are commonly tempted.

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## **Negligence and Recklessness as Departures from Trustworthiness**

Among the many cases of misconduct that I have gleaned from published reports or in discussion with colleagues who review such cases for their own institutions, much more common than fraud is what I call instances of "reckless research." The concepts of recklessness and of negligence give a better characterization than does "fraud" of the wrongdoing that is common in cases of falsification and fabrication.

In the recent past, when discussion of research ethics was polarized and dominated by legal and quasi-legal considerations [16](#), the role of intention in research misconduct was exaggerated, and a simplistic contrast between honest mistakes on the one hand and intentional acts of wrongdoing on the other, identified wrongdoing with intention to deceive. Some mistakes are honest mistakes-even a careful and conscientious person might make them. Others are sloppy or careless-they show insufficient care to meet some standards. However, not all standards are ethical standards. Therefore, *some, but not all, careless mistakes are ethically blameworthy.*

It is negligent (or reckless) to be careless about matters for which one bears a moral responsibility. If a surgeon sews up the patient with instruments inside, the surgeon is guilty of negligence. This act is morally blameworthy even though the surgeon does not intend to harm. If someone dribbles his soup down the front of his sweater, or designs an experiment that is logically flawed, that is sloppy and careless. However, since the norms violated are not ethical norms, the carelessness is not ethically blameworthy; it is not negligent or reckless behavior.

A dereliction of responsibility more serious than negligence is recklessness (or "gross negligence" as it is called in the law). To leave one's small children alone for days at a time would be not merely negligent but reckless. Reckless behavior is behavior likely to result in serious injury or damage even if it was not intended to cause that harm.

Much more common than the deliberate misrepresentation of a conclusion as true when the perpetrator knows it to be false (or disregards whether it is true or false) is the exaggeration of what he has done or of the strength of the evidence for some conclusion in which the researcher firmly believes. Such exaggeration may be disregard of counter-evidence rather than fabrication or falsification of data or experiments. However, when the researcher engages in such acts as fabrication and falsification (or failure to properly credit sources) he recklessly endangers the integrity of research whatever his beliefs in the conclusion.

The term "cutting corners" is commonly used by researchers to describe negligent or reckless distortion of the evidence, but "cutting corners" fails to indicate what is morally objectionable in this behavior. In other contexts "cutting corners" means taking some short cut. Short cuts are often desirable simplifications and may be responsibly undertaken. There is a saying among design engineers, "The best part is no part at all." To use "cutting corners" to refer to practices that, unlike honest mistakes or even many careless mistakes, violate standards of responsible research, encourages self-deception.

Eleanor Shore, in *Effectiveness of Research Guidelines in Prevention of Scientific Misconduct*, in this issue describes negligent or reckless acts motivated by what she identifies as "expediency." "Expediency" aptly connotes putting short term personal interests ahead of concerns for research integrity. The terms that I use, "negligence" and "recklessness" name the moral failing in the action, rather than its motivation. The terms "expediency" and "recklessness" jointly correct the misperception that



intention is crucial for the commission of misconduct. Those who recklessly endanger research integrity, motivated by expediency need not intend to put bogus conclusions into the literature (that is, commit "fraud"). It is enough that (as they know or should know) their actions risk placing corrupt results into the literature.[17](#)

Not only does "reckless research" rather than "fraud" best describe the actions at the heart of most cases of fabrication or falsification, but recklessness and negligence underlie other untrustworthy research practices.[18](#) They include, along with clear acts of fabrication, falsification and plagiarism, such things as taking unfair advantage of one's position as a reviewer of manuscripts or grants, making one's data appear more conclusive than they are without outright falsification of them, and giving less credit to one's sources than they deserve, although stopping short of outright plagiarism.

In the interest of brevity I will describe only one of the cases that I find to be typical of actual fabrication or falsification. This is the case of James Urban.

James Urban was a post-doctoral fellow at Caltech who was found to have fabricated data in a manuscript he submitted to the journal, *Cell*. He claimed that the data reported in the published version of the paper were genuine. They certainly were different from those in the manuscript that was originally submitted to *Cell*. (Some of Urban's lab books were missing and so could not be examined. He said that they were lost in a subsequent move across the country.)

Urban did not deny the charge of fabrication but he did deny any intent to deceive. Of course, he did intend to lead the reviewers for *Cell* to think that he had obtained experimental results which he had not in fact obtained; that much intent to deceive is implied by the term "fabrication." (One official close to the case said that Urban believed he knew how the experiment would turn out and, because of the pressure to publish, tried to "speed" the review process by fabricating the data in the original manuscript. But the official was convinced that Urban would not have published without having first inserted data he had actually obtained experimentally.[19](#)) So the point of Urban's denying an intent to deceive was that he did not intend to deceive others about the phenomenon he was studying, that is, he did not commit fraud in the strict sense. Apparently Caltech also understood Urban's actions in this way because they found him guilty of "serious misconduct" but not of "fraud," which Caltech distinguished from "serious misconduct" and regarded as a graver charge. The *Cell* article was retracted.

Supposing Caltech to have been correct in its assessment of Urban's case, Urban's action certainly ran the danger of putting false results into the literature even if he did not intend to do so. What if the experiments had not turned out as he had expected? How would he explain his retraction of an accepted article, especially to the editors of a journal like *Cell*, who are widely reputed to be very demanding of their authors? Had he died or been incapacitated before completing the actual experiments, the fabricated results probably would have appeared. Like driving recklessly but without an intention to harm others, reckless research behavior endangers the integrity of research results even without an intention to do so. Reckless action is a dereliction of responsibility even if, in a given instance, no serious harm is done. (In addition *reckless* does have the connotation of moral indignation.)[20](#)

Clearly distinguishing recklessness or negligence from fraud prepares the way to clarify the circumstances that contribute to wrongdoing in research. Although unusual pressure is only occasionally a factor in the few documented cases of fraud in the natural sciences - Nelson Kiang's example of research fraud by a desperate graduate student being the exception where it was - pressure is often a factor in documented cases of reckless research, a point that is nicely reflected in Eleanor Shore's term "expediency."

John Edsall, in his paper for this issue, draws attention to some of the pressures that have become part of the life of young investigators in the life sciences. These pressures, brought on by shrinking funding and reduced career opportunities, certainly contribute to the likelihood of reckless "corner cutting." Funding is only part of the story, however. The moral character of trust relationships is another, to which I shall return later. A third element in the pressures on researchers, young or old is whether in facing moral problems they can draw on community experience, or whether they have to entirely improvise their response when a problem comes along. The case of Robert Millikan's misrepresentation of his data selection provides an instructive example of a moral lapse by a brilliant and generally honest researcher when faced with changing standards.

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# Understanding Robert Millikan's Data Selection

In addition to illustrating the ethical challenges brought about by changing standards in research, the example of Robert Millikan's data selection practices and an odd lie he told about them illustrate two other important points that help to fill out the picture of criteria for trustworthiness in research:

- The unresolvable tension in scientific research between reliance on intuition, and a demand for peer evaluation of experimental results.[21](#)
- Intentional deceptions in the form of lies about data may be less serious breaches of ethical standards than some acts of recklessness or negligence.

With his water drop experiment and subsequent oil drop experiment Millikan established that the electron carries a discrete rather than a variable amount of charge. For this discovery Millikan was awarded the Nobel Prize for Physics in 1923. The full story of Millikan's research makes fascinating reading. It has been thoroughly researched and engagingly written by Gerald Holton in two articles.[22](#) Holton makes a convincing case that Millikan's intuition was part of what made him a better researcher than his rival, Felix Ehrenhaft. In particular Millikan's ability to recognize and select which of his data were the most trustworthy put him ahead of Ehrenhaft who indiscriminately used all of his data and therefore came to the wrong conclusion.

Describing some of Millikan's data selection as the operation of his "intuition" rather than as "reasoning" implies that Millikan could not articulate all of what he recognized when he recognized that something was amiss with some of his experimental observations. He was often able to think of reasonable explanations of why "things went wrong" with the experimental situation, however. For example, among the hypotheses that he offered were that "two drops stuck together" or "dust" interfered. Some of these hypotheses helped him improve conditions in subsequent experimental preparations.[23](#)

Present-day scientific research has a much more difficult time with intuition than do practical areas of scientific work such as clinical medicine or engineering design in which the ability to recognize underlying phenomena is valued even if one cannot

justify one's assessments. This difference is due in large part to the role of outside evaluation, peer review of research results. In evaluating a report of experimental findings, a researcher's reasoning, and not just the outcome of her judgments, is subject to scrutiny. Saying "I discarded all of the data that were taken when there was something funny going on in the experiment" may reflect the operation of true insight, but it is not likely to be very convincing to others. There is the added problem that relying heavily on intuition leaves a researcher particularly vulnerable to self-deception, something that the researcher continually guards against. The tension between reliance on intuition and the reliance on peer review of written accounts of research is one element in the story of Millikan's lie that research method and research ethics will have to negotiate for the foreseeable future.

The present-day evaluation of Millikan's data selection is complicated by the fact that present-day standards for data selection were just developing. For example, Holton quotes passages from the paper that Millikan published in *Science* in 1910, in which Millikan states views, methods and attitudes toward data handling that sound outlandish by contemporary standards.

[I]n the section entitled "Results," Millikan frankly begins by confessing to having eliminated all observations on seven of the water drops. A typical comment of his, on three of the drops, was: 'Although all of these observations gave values of  $e$  within 2 percent of the final mean, the uncertainties of the observations were such that I would have discarded them had they not agreed with the results of the other observations, and consequently I felt obliged to discard them as it was.' Today one would not treat data thus, and one would surely not speak about such a curious procedure so openly.[24](#)

That Millikan was so open about the methods he used demonstrates how far he was from attempting to deceive anyone in this paper. That his paper including these comments was published in a very prestigious journal shows that his description of his data handling did not strike most of his peers as very odd. However, present-day standards were developing and three years later when Millikan published a major paper on the character of electronic charge based on his oil drop experiment he seems to have become self-conscious about the issue of selecting data. In this 1913 paper he writes, in italics, *It is to be remarked, too that this is not a selected group of drops but represents all of the drops experimented on during 60 consecutive days.*[25](#)

As Gerald Holton has pointed out, there were many more drops that were experimented on in the 60-day period to which Millikan refers. Holton himself interprets this statement as just one more expression of Millikan's view that only the observations that passed his intuitive criteria for an uncorrupted run qualified as "data." Although I agree entirely with Holton's assessment of Millikan's data selection, I cannot interpret Millikan's italicized statement as anything other than a lie. Notice first that Millikan does not speak here of "data collected " but "drops experimented on", so Millikan's statement is difficult to justify even assuming that only non-suspicious readings qualified as data for Millikan. Furthermore, even if one were willing to go so far as assuming that when Millikan said "drops" what he meant was "non-suspicious data," as Holton suggests [26](#), the sentence becomes "it is to be remarked, too that this is not a selected group of non-suspicious data but represents all of the non-suspicious data from 60 consecutive days."

What would be the point of saying such a thing, let alone putting the statement in italics? Millikan's statement makes sense only as a denial that he has dropped data points. Feeling a need to explain his data selection (which had served him so well), but being unable to fully explain the operation of intuition, Millikan lied.

One of the earliest and most influential interpretations of Millikan's lie as research misconduct is found in Sigma Xi's *Honor in Science*. *Honor in Science* calls Millikan's act "cooking the data." However, assuming the accuracy of Holton's characterization of Millikan's data selection as based on intuition, then Millikan used criteria other than the value of  $e$  obtained from a particular experimental run as the basis for accepting or rejecting the data from that run. Therefore, he did not reject data simply because it did not accord with his hypothesis. He did not "cook" the data. It was not his data handling that was wrong, it was his misrepresentation of it. Millikan's lie appears gratuitous although we do not know whether someone, say a reviewer of the article, had pressed him for an explicit statement about his data selection. Given the emerging character of research standards in 1913, Millikan's paper with his italicized statement removed would stand as an unblemished contribution to knowledge. There is no evidence that Millikan lied about other matters. Indeed, the passage quoted by Holton from Millikan's 1910 paper shows a praiseworthy openness about his methods (even if the methods themselves look peculiar by today's standards).

The example of Millikan's lie about his data selection illustrates that an intentional deception even about data or experiments is not necessarily a grave betrayal and

need not be research misconduct. To know whether a lie is a major betrayal or minor defection, one must know its significance. Another example may make this clearer: Today if someone were to lie and say that he had personally taken all of the data, when in fact some of the data had been taken by another fully qualified observer and one who was properly credited as a collaborator in the research, that lie about an experiment would be regarded as a petty one. The significance of a particular matter may change over time.

A larger lesson of Millikan's lie is that, important as specific ethical [27](#) and methodological standards are, they too change and researchers need to think through responsible ways of coping with periods of change. Part of trustworthiness is facing up to changes in standards and embracing the enduring standard of honesty about one's own behavior when it turns out that one's previous actions come to be regarded as inadequate or even unethical.

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## **The Moral Soundness of Trust Relationships in Research; The Relationship Between Thesis Supervisor and Supervisee**

The trust fails to be well-founded, if it is founded on expectations that will not be fulfilled. Trust is naive if it is not well-founded. The distinguished philosopher, Annette Baier, has gone beyond the question of the well-foundedness of a trust relationship and asked under what conditions a trust relationship is morally sound or decent. The criterion for the moral soundness of trust relationships that Baier offers is that morally sound relationships flourish when the basis for their trust is disclosed. [28](#) Baier's criterion may be usefully applied to many of the trust relationships necessary to carry out research.

Although trust and cooperation among researchers at different laboratories as manifest in such practices as the sharing of research materials and data it important and has drawn attention of many [29](#), trust and cooperation among collaborators is

also crucially important and the ethics of research collaboration has also come in for more examination.[30](#) Among trust relationships the one between a research (or thesis) supervisor and supervisee is especially important because of the formative character of this relationship in setting attitudes and expectations for the new researcher and because poor mentoring in a laboratory is a factor that is observed to be a precursor to outright misconduct in a laboratory.[31](#)

What plausible trust relationships between supervisor and supervisee will fail by Baier's test? Suppose, for example, that the basis for a senior researcher's trust that a graduate student will faithfully follow a certain experimental procedure is the senior researcher's conviction that the subordinate is too intimidated to do otherwise. Disclosure of this conviction will tend to insult the subordinate and give her an incentive to prove the senior researcher wrong. Similarly, if the subordinate does carry out the procedure only because she fears detection and punishment and not because she has a concern for the integrity of science, knowing the truth about the basis of her reliability will lead the senior person to suspect that she would betray the trust, given an opportunity to do so. The trust of the graduate student by the senior researcher is not morally sound in either of these examples.

In science, engineering and medicine, supervisors commonly rely on their thesis students to carry out research which is part of a larger effort upon which the faculty member's own reputation and ability to get future funding depends. If someone undertakes graduate study with the expectation of acquiring the knowledge, skills and judgment of a researcher and of being initiated into a life-long career, but the supervisor sees graduate students primarily as skilled labor for whose maturation as a researcher the supervisor bears no responsibility, then that student will experience disappointment and even betrayal. This disappointment may be followed by a more realistic appraisal of the supervisor. Any new trust between the supervisor and supervisee, even if well-founded, is not likely to be morally sound, since the student recognizes the exploitative character of the relationship, and is more likely to respond in kind.

Among the ethical abuses that are widely alleged to be more prevalent today than they were twenty years ago [32](#) are various forms of exploitation of graduate students and post-docs by their research supervisors. (In a letter to *Science* [33](#), I raised the issue of the disturbing phenomenon of some supervisors' policies of pitting their graduate students or post-docs against one another. At the extreme this includes assigning them thesis topic that overlap to the extent that the acceptance

of one thesis precludes the acceptance of the second.)

Graduate students and post-docs are in an especially vulnerable position in that their supervisor is often both their principal source of information about their rights, responsibilities and criteria of fair treatment in research, and, at the same time, the person whose action they most need to accurately assess for fairness. The relative absence of general discussion of research ethics in recent decades has led to an inarticulateness on the subject by many fair and decent researchers. This has retarded the development of group norms and informal sanctions to cope with wrongdoing by a much smaller number. What authoritative statements there are on the fair treatment of supervisees, do not give specific guidance. For example, despite the clarity of guidance that the latest (1995) edition of [\*On Being a Scientist\*](#) gives about other matters, on the subject of the apportionment of credit between junior and senior authors says "Senior scientists are expected to give junior researchers credit where credit is warranted. In such cases, junior researchers may be listed as co-authors or even senior authors, depending on the work, traditions within the field, and arrangements within the team."[34](#)

True as this statement may be, it is of little help to graduate students in assessing their own experience with their supervisor. Graduate students (and post-doctoral fellows in fields that have them) are very dependent upon their research supervisors for much of their early career advancement and therefore especially in need of low-risk ways of understanding and assessing many matters of research ethics including matters of their own treatment. Ruth Fischbach and Diane Gilbert in their contribution to this issues develop their proposal for an institutional resource to help individuals, especially vulnerable individuals, assess their experiences of research conduct. In addition school and departmental norms for research conduct need to be strengthened.

Strengthening what Alberts and Shine call the "mechanisms that transmit the values that are associated with ethical scientific conduct" (see [3](#)) needs to include strengthening of the norms for the treatment of graduate students and post-docs so that they may not only be treated fairly but may have a secure basis for their assessment that their own treatment was fair, and carry high standards of fairness into their own careers.

To develop both specific knowledge of norms and confidence in the discussibility of these issues graduate students need a safe way to assess their own treatment, learn



about the differences among the policies of individual supervisors', policies, and inquire about a potential supervisor's policies before becoming that person's supervisee. Discussions among graduate students and faculty of the scope and limits of what a faculty member can reasonably ask of a thesis student also foster such a well-founded and morally sound trust. The problem-solving mode of discussion rather than debate helps to build trust and respect for differences. [35](#) Research communities need to openly discuss the ethical acceptability of the changing conditions of research to identify the practices that undermine the decency of research relationships.

Trust relationships that are well-founded and can stand up to scrutiny of the basis on which they rest are crucial to the flourishing of research. The preservation and restoration of such well-founded and morally sound trust requires not only good controls of outright misconduct, but that researchers develop a greater awareness of the circumstances that may tempt the unwary to behavior that is irresponsible and untrustworthy. In addition, the research community needs to identify where trust is misplaced or rests on an uncertain foundation. These are not easy tasks. However, public trust of researchers to discover deeper truths about nature, requires that the research community demonstrate its ability to develop and maintain ethical standards for scientific research and to work out the ethical implications of diverse research practices.

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## Footnotes

- [1.](#) I review the history of the research community's discussion of research ethics in the editorial for this issue
- [2.](#) Committee on Science, Engineering and Public Policy of the National Academy of Sciences, National Academy of Engineering, and Institute of Medicine (1995) [On Being a Scientist](#), second edition. Washington, DC: National Academy Press, preface (unnumbered page).
- [3](#) Alberts, Bruce and Kenneth Shine (1994) "Scientists and the Integrity of Research." *Science*, 266 (December 9) 1660. Emphasis added.
- [4.](#) Alberts, Bruce and Kenneth Shine. op. cit., 1661.
- [5.](#) "Research misconduct" is generally agreed to cover three sorts of acts: fabrication - making up data or experiments; falsification - changing or

misrepresenting data to accord with the researcher's expectations; and plagiarism - representing the work or ideas of another person as one's own. The question of whether "research misconduct" covers other deviations from accepted research practices is hotly contested. The National Science Foundation defines "misconduct in science and engineering" as follows: "Fabrication, falsification, plagiarism, or other serious deviation from accepted practices in proposing, carrying out, or reporting results from activities funded by NSF; or retaliation of any kind against a person who reported or provided information about suspected or alleged misconduct and who has not acted in bad faith." (Office of the Inspector General, National Science Foundation. (1992) *Semiannual Report to the Congress*. Number 7: April 1, 1992-September 30, 1992, 22.) It is a testimony to the strength of influence of legal and quasi-legal concerns on current terminology that research misconduct does not cover such acts as abuse of human or animal experimental subjects or the endangerment of co-workers or the public at large through unsafe laboratory practices. Although these are instances of serious wrongdoing in research, they have not been included in definitions of research misconduct because they were already the subject of regulation.

- [6.](#) Addelson, Kathryn (1994) *Moral Passages*. New York: Routledge, 13-18.
- [7.](#) Williams, Bernard (1988) "Formal Structures and Social Reality." *In Trust: Making and Breaking Cooperative Relations*, 3-13. Edited by Diego Gambetta. Oxford: Basil Blackwell, 13.
- [8.](#) The sociologist and authority on trust, Bernard Barber, discusses both aspects at length in *The Logic and Limits of Trust* (New Brunswick, New Jersey: Rutgers University Press, 1983). See also Whitbeck, C. "Trust" in the *Encyclopedia of Bioethics* 2nd edition. New York: Macmillan, 2499-2504.
- [9.](#) See John Edsall's discussion of Byron Lane's view of the Harold Bates case in "On the Hazards of Whistleblowers and Some Problems of Young Biomedical Scientists in Our Time," in this issue.
- [10.](#) Rennie, Drummond. *How much fraud? Let's do an experimental audit*. *The AAAS Observer*. 6 January 1989. p.4.
- [11.](#) One common objection to use of the term "fraud" for research misconduct is that research misconduct does not require a victim who experienced damages. For example, the National Academy of Sciences Panel on Scientific Responsibility and the Conduct of Research said, "[M]ost legal interpretations of the term fraud require evidence not only of intentional deception but also of injury or damage to victims. Proof of fraud in common law requires

documentation of damage incurred by victims who relied on fabricated or falsified research studies." (National Academy of Sciences Panel on Scientific Responsibility and the Conduct of Research (1992) *Responsible Science: Ensuring the Integrity of the Research Process*, Volume I, Washington, DC: National Academy Press. 25.)

- [12.](#) The most famous example in which a finding of research misconduct was overturned because of the failure to prove intent is the case against Mikuls Popovic and Robert Gallo, whom the Office of Research Integrity (ORI) of the Public Health Service (PHS) claimed had misappropriated a sample of AIDS virus sent to them by Luc Montaigner of the Pasteur Institute in France. When the case against Popovic was overturned on appeal, the ORI decided not to pursue its case against the head of the same research team, Robert Gallo. (Cohen, Jon (1994) "U.S.-French Patent Dispute Heads for a Showdown." *Science* 265 (July 1, 1994) 23-25.)
- [13.](#) The applicability of the notion of recklessness even to questions of plagiarism is illustrated by a university's recent finding of misconduct against a researcher accused of plagiarism. The accusation was made against a researcher for using, without attribution, text from published articles in his grant proposal to NSF. He had not done this as a deliberate act of plagiarism, however. What the researcher had done was to copy the work of others verbatim into his notes without quotation marks or attribution. As a result he could not distinguish his own work from that of others when he came to use his notes. Although this was not deliberate plagiarism, it was not simply carelessness either. Dropping some quotation marks in transcribing some notes would have been a careless, perhaps even negligent mistake. In this case, however, there were no quotation marks to lose. The failing was one of recklessness, not of deliberate theft. In its finding of misconduct (but *not* plagiarism) against the subject, his university said that the subject had displayed "a reckless disregard for appropriate procedures of scholarship" and had "knowingly and repeatedly [engaged] in a pattern of research note taking that given enough time, was inevitably going to produce precisely the situation that arose with his NSF grant proposals." (Office of the Inspector General, National Science Foundation (1993) *Semiannual Report to the Congress* No. 9, April 1, 1993-September 30, 1993, 37.)
- [14.](#) The Committee on the Conduct of Science of the National Academy of Sciences' original (1989) edition of [On Being a Scientist](#) used the Summerlin case to introduce their discussion of what they called "fraud in science." See

also Broad, William, and Nicholas Wade, 1982. *Betrayers of the truth*. New York: Simon and Schuster. 153-157.

- [15](#). Racker, Efraim. (1989) "A View of Misconduct in Science." *Nature*. vol. 339 (May 1989) 91-93.
- [16](#). See the editorial introduction to this issue, which has been reproduced [here](#)  
..
- [17](#). In 1993 the NSF OIG sought to classify negligence and recklessness as "levels of intent," but this usage blurs the customary ethical distinctions. See Office of the Inspector General, National Science Foundation (1993) *Semiannual Report to the Congress* No. 9, April 1, 1993-September 30, 1993, 37.
- [18](#). Nelson Kiang's paper and that by Rose and Fischer for this issue provide many examples. Other examples are given in the stories presented as cases in the Association of Medical Colleges 1994 collection, *Teaching the Responsible Conduct of Research Through a Case Study Approach, a Handbook for Instructors*. (Korenman, Stanley G. and Shipp, Allan C. with Association of American Medical Colleges ad hoc Committee on Misconduct and Conflict of Interest, (AAMC) 1994. [New York: Association of American Medical Colleges.])
- [19](#). Roberts, Leslie (1991) "Misconduct: Caltech's Trial by Fire." *Science*, vol. 253, pp. 1344-1347 (September 20 1991) p. 1346.
- [20](#). The foregoing arguments that recklessness as well as intentional wrongdoing are a basis for moral blame were presented to the Health and Human Services (HHS) Commission on Research Integrity in April 1995. They have just (August 1995) issued their draft definitions of research misconduct in which they explicitly include reckless behavior along with intentional behavior as research misconduct. (HHS Commission on Research Integrity, (1995) "Professional Misconduct Involving Research," *Professional Ethics Report*, vol. VIII, no. 3, [Summer '95]).
- [21](#). I discuss the tension between intuition and peer evaluation in my book, *Understanding Ethical Problems in Engineering Practice and Research*. Cambridge University Press, 1996.
- [22](#). See Holton's "Subelectrons, presuppositions, and the Millikan-Ehrenhaft dispute," in *Historical Studies in the Physical Sciences*, 11, 166-224, reprinted in the collection of Holton's essays, *Scientific Imagination*, Cambridge: Cambridge University Press, 1978 25-83; and his "On Doing One's Damndest: the Evolution of Trust in Scientific Findings." chapter 7 in Holton's *Einstein, History, and Other Passions*. New York: American Institute of Physics.

- [23](#). Holton, Gerald. 1978. "Subelectrons, presuppositions, and the Millikan-Ehrenhaft dispute," in *Historical Studies in the Physical Sciences*, 11, 166-224, reprinted in the collection of Holton's essays, *Scientific Imagination*, Cambridge: Cambridge University Press, 1978 25-83.
- [24](#). Holton, 1994.
- [25](#). Holton, 1978, 63 quoted from Robert A. Millikan, "On The Elementary Electrical Charge and the Avogadro Constant," *Physical Review*, 2, 1913, pp. 109-143.
- [26](#). Gerald Holton, personal communication February, 1995.
- [27](#). Among the ethical standards that have changed in the last fifty years are those for the use of human subjects.
- [28](#). Baier, Annette. 1986. "Trust and Antitrust." *Ethics* 96: 232-260. Reprinted in *Moral Prejudices*, pp. 95-129. Cambridge, Mass.: Harvard University Press.
- [29](#). For example the editors of some journals such as *Cell* now require such sharing as the condition of publication, the writers of the latest edition of [2](#) *On Being a Scientist* discuss the subject. There is widespread concern that sharp or stingy practice tends to drive out magnanimous or collegial practice.
- [30](#). A colloquium in which I participated recently drafted a statement on the ethics of collaboration for the American Academy of Microbiology. (Macrina, Francis L. (1995) *Dynamic Issues in Scientific Integrity: Collaborative Research, a report from the American Academy of Microbiology*. Washington D.C.: American Academy of Microbiology.)
- [31](#). Committee on Academic Responsibility Appointed by the President and Provost of MIT, 1992. *Fostering Academic Integrity*. Massachusetts Institute of Technology.
- [32](#). For examples, see many of the "cases" in the Association of American Medical Colleges' (AAMC) recent collection. (Korenman, Stanley G. and Shipp, Allan C. with Association of American Medical Colleges ad hoc Committee on Misconduct and Conflict of Interest,' (AAMC) 1994. *Teaching the Responsible Conduct of Research through a Case Study Approach*. New York: Association of American Medical Colleges.) They include, along with clear acts of falsification and plagiarism, such things as taking unfair advantage of one's position as a reviewer of manuscripts or grants, making one's data look better than it is although without clearly falsifying it, and giving less credit to one's sources than they deserve. The AAMC cases were assembled to reflect common situations. These abuses are widely held to be more prevalent in the biological and medical fields although laboratories in other fields differ widely in the

scrupulousness of their research practices.

- [33.](#) C. Whitbeck (1994) letter to the editor on Overlapping Dissertation Topics, *Science* 263 (Aug. 19, 1994) 1020.
- [34.](#) Committee on Science, Engineering and Public Policy of the National Academy of Sciences, National Academy of Engineering, and Institute of Medicine, 1995. op. cit., 14.
- [35.](#) Such research ethics scenarios for student-faculty discussions are included in the latest edition of [On Being a Scientist](#). Albert Meyer, the head of graduate studies for computer science at MIT, and I have used such scenarios for four years with the graduate students in the Computer Science "area" within the MIT Electrical Engineering and Computer Science Department (EECS) for problem-oriented discussions of credit, or responsibility for mistakes, and of the supervisor-supervisee relationship, together with an assignment to interview a potential advisor on the subject of credit. A summary of those activities with scenarios, reading questions, interview questions may be found on the [Online Ethics Center for Engineering and Science](#) under the heading [Group Mentoring in Responsible Research Conduct](#) in the Research Ethics Section. This material may be used (with attribution) for teaching.

## Notes

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