Introduction

In November, 1993 the National Institutes of Health (NIH) ended a four year investigation and dropped charges of scientific misconduct against AIDS researcher Robert Gallo. This action came within days after the Health and Human Services Departmental Appeals Board rejected charges against his former associate, Mikulas Popovic, and a few days before the Appeals Board was to hear Gallo's arguments. NIH's reversal did not extinguish controversy. "Like an underground fire that won't go out," (Greenberg, 1994) resentment continues to seethe over the official exoneration of the renowned Gallo. Fanning that fire was the perception, by many, that NIH dropped the charges because the exoneration of Popovic made it clear that Gallo would also be exonerated by the Appeals Board.

The relevance of the Gallo-Popovic story, for our purposes, is that it draws attention to the activities of researchers within their research groups. It brings to the foreground features of the actual daily conduct of research that matter a great deal to members of research groups but rarely become public.

The case began with the charge that Gallo had misappropriated from a French research group the AIDS virus he is credited with co-discovering (C. Anderson, 1994b). Initially, Gallo, a laboratory chief at the National Cancer Institute, claimed that he had discovered the virus. Later, Gallo conceded that there may have been accidental contamination from the virus supplied to him by the Pasteur Institute in France. Controversy continues about whether Popovic, Gallo's subordinate in the laboratory, deliberately used the French virus to grow the culture for identifying the virus, and if he did, whether Gallo knew it.
Controversy also surrounds a report of their work on the AIDS virus that Gallo and Popovic published. A French researcher from the Pasteur Institute, Francoise Barre-Sinoussi, alleges that Popovic told her in 1992 that he was not responsible for deletions of references to the French virus from their May 1984 paper in Science. According to Barre-Sinoussi, Popovic claims to have retained drafts of the paper that show he tried to give credit to the French researchers and that Gallo was responsible for the deletions.

NIH's Office of Scientific Integrity (OSI) first found in 1992 that Popovic was guilty of scientific misconduct. At the same time, OSI censured Gallo but found that his behavior did not rank as misconduct. However, in a report to NIH's Director evaluating OSI's findings, Dr. Frederick M. Richards, a chemist from Yale who was a scientific consultant in the investigation, charged that Gallo's lab deliberately hid its dependence on the French virus. He also criticized the OSI findings for placing blame on the subordinate rather than the lab chief, "who had the duty to monitor the performance of all personnel in the Laboratory and to pay particular attention to the accuracy of major publications which bear his name as author" (Greenberg, 1994, p. 4). The case was reopened, with the result, a year later, that NIH issued two findings of misconduct against Gallo. Those were the findings that NIH dropped in anticipation of the Appeals Board hearings.

This case throws light on relationships between the laboratory director and subordinates, especially the director's duties to supervise subordinates, the preparation of reports of research for publication, the standards for and responsibilities of authorship, the basis for giving credit and recognition, and the handling of data and materials from other research groups.

Some hold the mistaken view that what goes on in the "privacy" of the research group is strictly that group's business. The mistake of thinking that the management of the group is not of ethical significance may not be evident from looking at instances of clear-cut misconduct. Observers are inclined to describe those instances as the misdeeds of isolated individuals, "bad apples" somehow not weeded out in time. Instances such as the Gallo case, which leave serious allegations unresolved, resist that treatment. These instances leave insistent questions about the character of the research environment and the management of the research.

All research groups have an interest in fostering a climate of trust, an atmosphere that supports responsible conduct and conduct that meets justified standards of the scientific community. This takes in more than the avoidance of falsification, fabrication, and plagiarism—the usual examples of misconduct. It includes ethical standards that are presumed to apply across research communities, as well as standards specific to the local research community. Each research group must clarify ethical standards for itself, must identify practices that support those standards, and find ways of checking on how well it is doing. Groups should make explicit underlying assumptions and rationales for their standards, while recognizing that standards evolve over time, as circumstances change and new problems come to light. Part of a group's business is to ensure that new researchers who join the group, including graduate students and post-doctoral trainees (post-docs), understand the prevailing standards. To engage in these efforts effectively, groups have to be willing to sit down and carry on open discussion about how they are to operate.

The situations that are examined here are not the kinds of cases that spill out into the pages of Science or Nature. Rather, they are born in the lab or research group and usually die there. When an advisor fails to acknowledge a graduate student's refinement of a technique in a paper utilizing that refinement, the student may express his consternation to no one beyond fellow students or friends. In the absence of guidelines or standards regarding recognition, he may be unsure about how to react. Yet, giving credit for research and taking responsibility for reported research are essential elements of the research enterprise. Failure to articulate standards, as well as what count as breaches of ethical standards, can significantly damage the research process, its products, the researchers, and the research environment.

In this essay, we first discuss certain general features of the environment in which research groups operate and features which characterize all research groups in engineering and science. A survey of ethical implications of these features follows. Next we present and analyze some vignettes, drawn from the experiences of scientists, post-docs, and students. The vignettes illustrate the complexities and ambiguities of situations and bring out issues of research ethics. The final section provides a general discussion of issues brought out in the vignettes, variations on the situations in the vignettes, and some additional observations with illustrations.
Features of the Research Environment

We often hear that a set of shared values including "honesty, integrity, objectivity, and collegiality" bind scientists into a wide, international community (NAS, 1992). Many have an image of a single comprehensive community of scientists. But the research enterprise is highly decentralized. In their university and company departments and laboratories, scientists operate under local assumptions about how research should be managed and how members of research groups should relate to one another. New apprentices, graduate students, and post-docs learn how to conduct research from local practices.

Local control allows for "diversity, flexibility, and creativity" (NAS, 1992). However, the relative autonomy of research groups that permits them to establish their own procedures and standards, without having to answer for their practices, also imposes upon them the burden of figuring out how to incorporate shared values and live up to accepted standards.

Contrary to popular notions, we do not find an abstract, universal "scientific method" that guides practice in all situations. Research groups and laboratories are differentiated by scientific discipline, by fields and research programs, by institution, by department, as well as by the personality of the director of the research group. Personal characteristics and styles of behavior, especially of research directors, significantly affect the research atmosphere. The interplay of personalities and the entanglements of human relationships cannot be excluded from research groups. And research groups are vulnerable to the kinds of misunderstandings and conflicts (e.g., rifts arising from jealousy) that can mar any cooperative enterprise. Moreover, in the view of many, the competitive environment of science in the United States imposes additional stresses on the human relationships and practices within research groups. Comments from insiders occasionally provide glimpses of the competitive atmosphere in research groups. Barbara Kingsolver, a former graduate student in the field of population biology and now the author of best-selling novels, explained that she gave up graduate study after "growing tired of the grinding lab work and tired of the academic back stabbing" (Lyall, 1993). An NSF official in a position to hear allegations remarked at conferences that "unhappy" labs out there.

The potential for the breakdown of trust underscores the need to look more closely at relationships in research and laboratory groups and at the ways in which research groups are managed. A research environment where relationships are distant, frayed, or fractured—an "unhappy" lab—may well not sustain responsible research conduct.

Some amplification of what the terms 'bench level science' and 'individual laboratory and research groups' refer to should make clear what will be under scrutiny. Groups that consist of a principal investigator or project director, an additional investigator or more, with at least one trainee, will be encompassed. Included are groups with and without laboratories, and groups at field sites outside the university.

Three major goals characterize research groups and provide a framework for examining them. These goals are: to get research done; to get students trained; and to acquire the funding needed to achieve the first two goals. The first two are generally regarded as intrinsically valuable goals; they are valuable in themselves. The third goal is instrumentally valuable for achieving the first two goals. Whether explicitly announced or not, these goals drive the group's activities and command the director's attention. The emphasis within this mix of objectives varies from one research group to research group and helps to create the characteristic "flavor" of research groups. Some make training more central, some put a priority on research, and some pursue funding with special ardor.

Research groups are cooperative enterprises, in order to accomplish their goals, they depend on members each doing their part. And members depend upon one another. Participants must be able to accommodate personal needs to the enterprises and objectives of the group. However, research groups also value independence in each of the members; it is a trait that the training aims to foster in students.

Moreover, competition pervades the broader structures and systems of science and research groups. In the view of some philosophers and historians of science, competition for recognition has fueled the scientific enterprise since the seventeenth century and continues to do so (Hull, 1988). Principal investigators or directors must always be attentive to competition with other research groups in seeking support from funding organizations. Investigators must compete to achieve recognition through publication. Graduate students and post-docs compete for admittance to research groups. Those groups compete for promising trainees. Researchers, including graduate students and post-docs, compete within the group and externally for recognition for their findings. Many who are influential in science in the United States believe that competitive features...
that have been incorporated into the conduct of research are responsible for the strength of American science (Goodstein, 1993).

Science at the bench level is also characterized by disparities of power. The principal investigator or project director acquires the resources for research; control of those resources constitutes the basis of that individual's power. Other members of the group, especially junior investigators, post-docs, and graduate students, are dependent on the director for resources and for career advancement. For the way they wield power, research group directors are relatively free of accountability.

Post-docs are particularly vulnerable since their connection with a single faculty member is usually the basis for their status in the research group; post-docs do not have their own standing in the university as graduate students do. Some observers liken the post-doc's position to that of a contractor. Though post-docs may make important contributions such as bringing a new technique to the lab or giving guidance to graduate students, generally speaking, they have little leverage of their own. However, there are variations, from the situation of the post-doc who is barely beyond graduate student status to that of the advanced post-doc who has become more independent.

There are, of course, legal restrictions, government funding agency requirements, and university rules that circumscribe the power of research directors and senior investigators. Nevertheless, a very significant power differential remains, and its management is a matter for local determination.

Overview of Ethics Issues in Research Environments

All research groups require some set of standards or ground rules for the way they operate in proposing, conducting, and reporting on research. These standards or rules may be explicit, even expressed as formal rules, but often they are not spelled out. One laboratory director will announce "There are two rules regarding openness in my lab: 1) No one should keep data to himself in the lab and, 2) Within the lab, everyone's opinion has to be open to criticism and discussion." Another will say, "In my lab, we take those rules for granted without ever announcing them." For still another, these rules will not even be consciously recognized.

Ground rules and standards of a research group interpret standards of ordinary morality and professional ethics. One helpful description of our ordinary morality is that it consists of "those standards everyone wants everyone else to follow even if everyone else's following them" means having to follow them oneself (Davis, 1994; Gert, 1988). Don't kill, Don't harm, Don't deprive of freedom, opportunity, or pleasure, Don't cheat, Don't steal, Don't lie and Keep your promises are some of the agreed-upon moral standards.

The term 'ethics' has a number of uses. Some use it interchangeably with the term 'morality.' Others use the term 'ethics' to refer to systematic, theoretical inquiry into morality. The term is used also to refer to the special standards of professions, e.g. legal ethics or medical ethics. Ethics focuses on actions that affect human welfare. In the view of many, actions that affect the welfare of animals and the environment should also be encompassed, either in their own right or on the grounds of their importance for human welfare. Thus, ethical issues or problems have to do with the welfare of humans, animals, or the environment with respect to agreed-upon standards of conduct.

Here we are concerned with the special standards of conduct "every one wants everyone else to follow" in scientific research communities. The first step to acting in accord with a standard of conduct is having clear and consistent ground rules or standards articulated. These can range from informal policies (hopefully explicit) to highly codified statements. Then comes the question of how well the standards cover the range of situations that characteristically arise and require consistent responses for coordinated behavior. Further questions concern whether the standards reflect the proclaimed values of the scientific enterprise and whether they are acceptable from the wider perspective of ordinary morality.

The process of articulating standards in research groups can be read as marking a transition from a stage in which unstated, informal standards presumably operated to one in which the more formal standards of a profession are called for. For example, the professional associations of some scientific disciplines, such as the American Chemical Society, the American Physical Society, and the American Psychological Association have adopted codes of ethics. In this respect, these scientific societies are like the associations of major professions such as law, medicine, engineering, and accounting.
Collaboration and Competition

Conventions that encourage open communication facilitate collaboration in an atmosphere of competition. Research groups have leeway in determining the conventions that govern collaboration. Local choices also shape competitive arrangements and practices within the research group. There are, for example, project directors who ask more than one student or post-doc to work on the same research problem, more as competitors than collaborators. This practice is controversial, especially where competition within a research group mirrors the "winner take all" feature of competition in the organization of science outside the research group.

The centrality of competition in science may seem a "natural" feature of science as a human activity, especially to those already accustomed to competition in our system of education. Hence, the degree to which competition results from conscious decisions may escape notice. Certain forms of competition flow from certain policies—for instance policies in the funding of science. Government grants for essentially the same research projects are sometimes awarded to more than one investigator—to many if the research area is important. We could have a competitive funding system that does not operate this way.

Ethical questions arise concerning the terms of collaboration: How open is information within the group and how easily is it shared? Is credit determined by clear criteria that apply to everyone? What are the expectations of reciprocity, loyalty, and collegiality? Though generally positive, these relationships can generate expectations of conduct that violate ethical standards. Such a situation arises when expected collegial support would mean doing nothing in the face of the sudden appearance of conclusive evidence of plagiarism. We need clarification about the ethical limits on loyalty and collegiality.

Likewise, ethical questions arise concerning competitive arrangements and practices. Recent research shows a connection between a competitive climate within a research group and increased likelihood that a student will observe misconduct over time (M. Anderson et al., 1994). The research suggests that a highly competitive atmosphere within a research group can be linked with erosion of trust.

It is not hard to see how demands in the name of competition can come into conflict with demands in the name of collaboration. For example, a post-doc may be reluctant to share recognition with a graduate student with whom she collaborates when she has made her own interesting findings in connection with the graduate student's project. Conflict is not inevitable, but there is an inherent tension that arises from the value placed on both collaboration and competition.

Power

Disparities of power in relationships and transactions pose risks of exploitation or abuse of those with little power, and they frequently precipitate questions about fair treatment. In scientific research, this asymmetry of power is located within a web of relationships that includes those between supervisors and their graduate students and post-docs, between post-docs and graduate students, between graduate students, between senior researchers who are peers and between junior researchers who are peers, and between senior and junior researchers. Technicians also can be important members of this web. The power differential causes concern because the parties with less bargaining power or information may consent to arrangements that with more information or a stronger bargaining position they would be rational to reject. It can be wrong to impose such arrangements even though the less powerful consent to them.

Among graduate students and post-docs, it is not unusual to hear the term "slave" for graduate student and "indentured servant" for post-doc. There are abundant opportunities for taking unfair advantage, and it can occur in subtle forms or in ways long taken for granted, that well-meaning people may not notice. In view of expectations of collegial support, even senior members of a group who notice abuses of power may have difficulty doing anything about them. The fear of jeopardizing career prospects makes it especially difficult for junior people to call attention to perceived abuse of power.

Even where project directors are sensitive to the vulnerability of the less powerful members of the group, they may fail to identify situations in which weaker members are taken advantage of and are afraid to speak up because of their relative lack of power. A case in point might be the assigning of heavy teaching responsibilities to a post-doc, thus benefiting the graduate students and relieving the post-doc's advisor and other senior researchers of certain burdens, but hindering the progress of the post-doc. At field sites outside the university, the risks of abuse of power or of inadequate supervision are increased. Similarly, at these locations,
failures by advisors or trainees to meet standards can have amplified impact.

Because much of science teaching is one-on-one, the relationship between mentor and student becomes critically important in educating students and transmitting ethical standards. A mentor is not necessarily the project director or the student’s thesis advisor. Rather, the mentor is a more senior, experienced person in the research setting who, over time, provides the student knowledge and nurture with respect to technical matters, professional values, and ethical standards, and with respect to building a career in science. Mentoring is identified as taking a sustained, active part in fostering the careers of post-docs and graduate students, not merely with functioning in the role of advisor or instructor.

The term “mentoring” refers to an interactive process that evolves over time. Because it is an interactive process, the role of the mentored person is not a passive one. That person has a responsibility to seek information and guidance and be ready to make use of it (Swazey, 1993). Graduate students and post-docs benefit from having a number of mentoring relationships, some probably short-term and others of longer duration. Advanced post-docs and graduate students can mentor more junior colleagues. Some recent research suggests that the quantity of mentors may be more important than the quality of mentoring relationships (Henderson & Welch, 1993). The necessity of mentoring is undisputed.

However, it is difficult to assure that all graduate students and post-docs are adequately mentored, because mentoring involves a personal relationship involving a commitment to a trainee above and beyond what is required by the formal role of senior colleague, advisor or teacher. The success of the mentoring relationship depends upon particular personal qualities of the parties and their degree of commitment. The mentor role, accordingly, is an informal role and cannot be mandated.

Recognizing that individualized mentoring cannot be guaranteed, some educators recommend “bulk” or “wholesale” mentoring. These are programs that support graduate students asking the questions they need answered, and workshops, short-courses, and seminars that transmit ethical standards and help students develop professional skills.

Relationships within research groups can go sour in a great number of ways, including: when lines of supervision are not clear; when research problems are not clearly demarcated and allocated; and when well-established lines of and regular occasions for communication are lacking.

We can collect these ways of going astray under broader ethical questions about how to wield power responsibly and how to behave responsibly as one dependent on the power of others. As we proceed to point out the kinds of standards and practices that are needed, we thereby delineate role responsibilities in research groups. To fail to fulfill these role responsibilities would be to behave irresponsibly, i.e., unethically.

Analysis of Vignette

The vignettes that follow highlight aspects of the organization and management of research groups and focus on certain relationships within them. The vignettes are “sanitized” accounts deriving from actual occurrences involving people whose true names are not disclosed. Based as they are on actual occurrences, these accounts depict situations that are ambiguous, open to alternative readings, even as they continue to unfold. They concern conduct that is troubling to members of research groups, but that has not reached the point of attracting the attention of outsiders. The commentary identifies ethically defensible, responsible courses of action, with focus on individuals and how they should respond in such problematic situations. The analysis also highlights practices and procedures that labs and research groups might adopt to support ethically responsible conduct or reduce the likelihood of such problems occurring. For guidance about what should count as responsible conduct, the analysis will rely on codes of ethics (where they exist), laws, regulations, legal codes, and agreed upon moral standards.

Vignette 1: The Lab or Last Resorts

In the aftermath of exposure of cases of misconduct in science, members of research groups in a number of universities have begun to discuss ethically problematic situations arising in the conduct of research. Many came to see the advantage of thinking about such situations in advance as a way of preventing them from arising or being prepared to deal with them if they should arise. As academic groups generated vignettes and considered ways of responding, they often noticed that they had not developed explicit policies for their groups as a basis for dealing with ethically problematic situations. If there were relevant operating assumptions or standards, they were usually tacit. In some groups, and among
some research directors, the idea of articulating standards and expectations has begun to take hold. To make explicit what had formerly been taken for granted, one group produced the following memo:

To: New & Used Graduate Students in the Laboratory of Last Resorts
From: Director Drake
Subject: General Rules

Welcome to our laboratory. As you know, research in this laboratory is funded by grants from NIH, NSF, and other agencies. The projects so funded have specific aims and a detailed research plan stated in the grant applications. Departure from these aims and plans requires reapplication for the grant funds. We would only do this if the original ideas prove early to be without merit.

Therefore, students in this laboratory are not free to pursue ideas and activities of their own design, unless these fit the aims and research plan of the project which supports them. In accepting this fact you are surrendering a significant amount of intellectual freedom. I agree to provide, as long as grant funds are available:

1. Your tuition.
2. Your stipend to live on.
3. Excellent laboratory facilities, including all necessary computers, instruments, equipment, tools, supplies, and desk space.
4. Superior research training.
5. Thesis ideas and guidance.
6. A long term commitment to your career goals.

You agree that, since the Laboratory’s highest priority is continued funding, I may:

1. Set your daily work schedule.
2. Determine your research.
3. Personally present your work whenever and wherever I deem it appropriate.
4. Decide what and when to publish.

Of course, those who produced this statement were not entirely straight-faced; they were somewhat self-mocking. Other scientists have commented, however, on how closely this statement corresponds to research life in many groups. In any case, the very fact that these ground rules are written down counts ethically to the credit of this research group, because everyone is now on notice about certain important expectations. It would be even more to a research group’s credit if it made such a statement available to prospective students before they accepted offers of admission. Prospective students would be less at the mercy of unrealistic expectations about what lies in store. They might even be in a position to compare different options for graduate study with respect to the stated terms under which graduate training is managed, as well as with respect to the intellectual opportunities. Once a graduate student has signed on to a research group, it is usually difficult to switch to another group. Advance familiarity with the ground rules would help to keep students from becoming trapped and feeling they were “lured by false advertising.”

The director of this laboratory appears to make the instrumental goal of acquiring resources to do research the highest aim of the laboratory. That is objectionable because the pursuit of funding borrows its value from the worthy goals of extending knowledge and educating students. Yet the reward system that operates seems to put a higher value on the goal of maintaining funding. By itself that goal does not justify assent to the stated terms. One might argue that what is referred to as “the highest priority” to obtain funding— is not a goal at all, but a side constraint, something you have to do in order to pursue your goal of doing research.
Once the pursuit of funding is elevated to a goal, it is easy to forget that
it is merely instrumental. To see the importance of the unstated goals to
this scheme, consider whether the ten prerogatives that the director
claims would seem as persuasive if these further goals of increasing
scientific knowledge and training researchers were not assumed. Would
it seem appropriate or reasonable to give up that much autonomy in the
name of maintaining funding?

There is plenty of room for alternative lists of ground rules. Some
argue that even the pursuit of the two intrinsically valuable goals does not
justify such a surrender of freedom by students. They see this research
group director as excessively controlling and paternalistic in the use of
power, unwilling to allow students enough freedom to learn to make
choices for themselves toward achievement of the educational goal. If
the terms for students are too restrictive, it remains to be considered what
is a fairer bargain, what the standards of fairness are and how the lines
circumscribing students' freedom should be drawn.

Often, the basis for control over a student's time and effort in the lab
is that the student is, in effect, an employee. When that is the case, then
the control should be restricted to hours in which the student is paid to
perform as an employee. "Every Waking Hour," the half-humorous title
that one research group put on its statement of ground rules, states an
excessive demand, if the student's employee status is what justifies con-
trolling the student's activities. Admittedly, there is often a blending of
work and learning, but that does not justify counting all the time in the
lab as work time. This issue brings out complexity in the role of gradu-
ate student. Some graduate students receive support from funds in grants
to be paid to research associates (RAs). For some, support consists of
stipends from traineeships. Some graduate students are hired as teaching
assistants (TAs) rather than RAs; the TA position, with its teaching
responsibilities, generally poses more problems than the RA position for
getting thesis research done. Depending on status, length of time in the
program, and other variables, the graduate assistant can be described as
student, junior colleague, employee, slave, or some mixture of these.

Determining and assessing the rationale for each of the prerogatives
claimed by Director Drake of the Lab of Last Resorts would help to
decide if they are overly restrictive. It is worth noting, however, that
students in humanities disciplines, where they are left largely to their own
devices, might be willing to exchange some freedom for some of the pa-
ternalistic support offered by the Lab of Last Resorts.

It has to be acknowledged that making explicit what was formerly
implicit can change the atmosphere. Some will regret the loss of sponta-
enity and informality. And some practices formerly taken for granted
will not survive (whether for good or ill) when they are examined and
discussed. However, the link between clear expectations and responsible
conduct is so strong that it justifies the risks associated with formulating
explicit ground rules. Two caveats should be kept in mind. First, the
ground rules have to be general; many details of life in research groups
must be managed by extrapolating from the ground rules. Second, the
ground rules are not without exceptions. For example, the project di-
rector may have an explicit policy of allowing post-docs to take all their
data with them when they depart, but with the understanding that on
occasion that policy may have to be overridden.

Vignette 2: Whose Thesis Problem?

The issue of exercise of power is linked with the issue of "ownership"
of ideas in the following situation that concerns a graduate student.
Focusing on the advisor/graduate student relationship at an early stage,
this vignette continues exploration of the vulnerable situation of graduate
students.

George Alvarez, a graduate student and a lively, imaginative fel-
low, comes up with an idea that is worthy of a thesis and appropri-
ately related to a funded project of the laboratory. However, since
Alvarez is only a third of the way through the first year of the
program, the Lab Director, Helen Jonas, judges that he is not yet
ready to pursue the idea. He has not acquired the background and
skills to carry through the research. Jonas therefore assigns Alvar-
alez' idea as a thesis problem to another student, Charlene Wright,
who is ready to pursue it. Jonas assures Alvarez that there will be
an interesting problem for him when he is ready to work on his
dissertation research. Alvarez is disappointed.

It appears that between Jonas and Alvarez there was no discussion,
persuasion, or consent, that is, none of the kinds of interactions that build
trust and forestall misunderstandings. Alvarez' disappointment might
lead to a dampening of his enthusiasm and inclination to put forth new ideas. Jonas’ action might also discourage others from putting forth ideas. To the extent these results occur, both Alvarez and the research group might lose out. More important is the question of the fairness (on some plausible criterion of fairness) of Jonas’ apparently failing to see to it that Alvarez gets proper credit for the idea he contributed. He should at least be assured that for proposing the idea he will receive readily visible acknowledgment in any report or publication that issues from the research. Recognition is the coin of the realm in science. To be denied credit for a contribution important enough to become a research problem for a member of the group is to be denied what is owed.

Some go further and argue that to be fair to Alvarez, Jonas should see to it that he is involved in the research with Wright. This might be an appropriate way for Alvarez to acquire the background and skills that he lacks. However that may be, it is indefensible to fail to assure Alvarez adequate recognition for his contribution. This is especially likely if Wright is simply handed the research topic with no need to participate in formulating the research problem. Perceptions of favoritism can grow, causing unnecessary pain and interfering with students’ progress in research settings lacking ground rules to cover such situations.

Assuming that Jonas is not an arbitrary dictator, the act of taking Alvarez’ idea from him and handing it to Wright implies that a communal ownership of ideas prevails. Should those terms apply to everyone in the lab, senior and junior people alike? If so, would Jonas give away her own ideas? If the terms of sharing are different for junior investigators, what is the basis? Can that rationale be justified? That is, is it compatible with agreed-upon ethical standards and are the consequences of the arrangement acceptable?

Vignette 3: Getting Recognition*

In the previous vignette, there was no question about whether the beginning graduate student had made a valuable contribution. Sometimes, as in the next vignette, it is not as obvious. Uncertainty about the importance of their accomplishments can undermine students’ confidence in themselves and can put strains on their relationships with others. Those who have graduate students in their charge need to take pains to communicate about these matters and remain attentive to the needs of their students. In failing to make such efforts, they can bring about potentially undermining worries, jealousies, and suspicions in students.

In his first year of graduate school, Pu-ning Lee is supported by Professor Ludwig von Bampus’ research funds in an electro-optics laboratory on a project dubbed Laserkill. By the end of the first year, Lee has not only become proficient at many of the more routine tasks of the project, but he has made a small, perhaps noteworthy, refinement to the approach to the segment assigned to him. At the end of Lee’s first year, von Bampus goes on sabbatical leave, and Lee starts working with Professor Alain Redon in the same lab but on a very different project. Von Bampus returns after a year and takes up Laserkill, among other projects. At the beginning of his third year Lee learns from another student who has been working on Laserkill, that von Bampus is publishing, with that student only, a paper on some aspects of Laserkill. The paper utilizes Lee’s methodological refinement, but he is given no acknowledgment. Lee is disturbed.

This narrative is ambiguous. It is not clear whether Lee has not been recognized for a noteworthy accomplishment or whether he has exaggerated the importance of a more routine contribution. His own uncertainty about his accomplishment may be a source of his discomfort. Lee appears to be moving through the program with little feedback about his progress or status. Of course, a student’s commitment to particular projects is often loose in the first year of graduate study; it is not uncommon for a student to be unsettled at that stage. And it is not clear that Lee has formed a unique relationship with von Bampus. We do not know whether criteria for recognition have been discussed in the lab. Whether or not Lee rightly believes that von Bampus has violated a duty to give him credit, von Bampus’ failure to discuss the matter with Lee appears to have hurt Lee. It also seems, however, that Lee has taken little or no initiative to keep in touch with von Bampus regarding Laserkill and the status of his contribution to it. As a graduate student progresses, he or she should become an increasingly active participant in research. It is altogether appropriate for the student, looking out for his own interests, to contact a professor on whose funded project he has worked in the past, in order to ascertain where things stand. If Lee had reminded von Bampus about his refinement, Lee might have prevented
an inadvertent omission of recognition. If von Bampus did not deem the refinement important enough to recognize in some form, he might have informed Lee and explained why. While supervisors have the responsibility to create an atmosphere that encourages trainees to ask questions and to see to it that graduate students make their way through the program in good time with adequate recognition, graduate students should actively seek clarification about where things stand. Of course, they also need to be informed early on about acceptable forms and lines of communication in order to express concerns and raise questions without giving offense.

Nevertheless, von Bampus' apparent failure to communicate with Lee about the status of Lee's work on Lasergill cannot be defended, considering that von Bampus apparently missed two obvious opportunities, one at his departure and the other upon his return. There is no evidence that von Bampus and Redon coordinated their supervision of Lee although they are presumably in the same research group. Since both have had Lee working under their supervision, they have a responsibility to communicate with one another regarding Lee's progress; they should not proceed independently with the same graduate student. The vignette suggests that there should be some structure or, at least, check points for marking the progress of graduate students and channels for regular communication about the progress of research. Graduate students have a right to be properly advised. They also have responsibilities as advisees.

Vignette 4: Stanton's Statistics

This vignette concerns the relationship between a supervisor and a post-doc. It presents questions about the exercise of power with respect to the ownership of data and authorship. The vignette makes clear how authorship entails responsibility and risks. It also shows how doing good scientific work and meeting appropriate ethical standards are connected.

One day, the head of the department runs into his colleague, Professor Patrick Franklin, and learns that Franklin has finally solved his "Stanton Problem." Franklin reports that when Betsy Stanton completed her post-doctoral study at the university the preceding year, she left behind in the lab raw data from a line of work she had started but never written up or published. Franklin, in whose lab Stanton was working, came across the results, believed they were important, and wanted to publish them. This work was funded by a federal agency and Franklin was feeling some pressure to produce more publications from that funding. He tried unsuccessfully to contact Stanton by letter and telephone. When Franklin heard from a colleague that Stanton might be out of the country, Franklin analyzed and compiled the data and wrote them up in an article. Still unable to reach Stanton, Franklin submitted the article with himself as first author and Stanton as second author. It is with satisfaction that Franklin tells this story to the head of his department. He thinks he has improved his prospects for renewal of his grant and expects that his department head will be pleased also.

Franklin is so satisfied with the prospect of improving his funding situation that he manages not to notice problems with his use of Stanton's data. Some questions should cross the department chairperson's mind. 1) Were there clear understandings in Franklin's research group about the status of the post-doc's data at the close of the post-doc's tenure? (Some project directors allow post-docs to take all their data with them, unless the project director is under unusually severe pressure for funding and needs to retain the data.) 2) Is it likely that Stanton would have left good, reliable data behind without reporting the results? 3) What should be the post-doc's reasonable expectations with respect to the lab director's appropriating data left behind? 4) Does Stanton's research group have any ground rules regarding the assigning of authorship? 5) What provision has the lab director made for staying in touch with former post-docs and graduate students?

Taking the last question first, it does not appear that Franklin provided for keeping in touch with the former post-doc after her departure. It is necessary to make such a provision in view of the likelihood that a scientist will have frequent changes of address in the early years of a career. (It is especially necessary when foreign post-docs return to politically unstable areas.) Why is it so important to be able to reach the post-doc? First, as question 2 suggests, it may be necessary to confirm the reliability of the data. If Franklin is not in a position to be confident of the reliability of the data, he is not practicing good science to use it without checking with Stanton. In addition he may cause injury to Stanton by using and crediting her with data she gathered, the reliability
of which is unconfirmed. (He is, of course, putting himself at risk as well.) Data that is left behind, whether by requirement or for other reasons, may subsequently acquire value for the lab director. That is why some lab directors require data to be left behind. To capture the value responsibly, however, the lab director may need to consult with the person who generated the data.

Second, even if Franklin knows the data are reliable, he needs Stanton’s consent to put her name on the paper. He must not exercise his power to confer authorship unless he reaches Stanton and receives her consent. More than that, authorship implies some form of participation and entails assuming some risks. If errors or other problems show up, all the authors are accountable. Consent alone is, therefore, not sufficient for putting Stanton’s name on the paper as an author. Has Stanton seen the paper? Is she fully aware of the interpretation Franklin has advanced in the paper and does she agree with it? Does Stanton meet local standards for the level of kind of participation required? Are there such standards? Unless Franklin can answer all those questions affirmatively, he is not justified in taking the prerogative of naming Stanton as an author. Stanton would be justified in expecting all of these conditions to be met if she were named as co-author.

Lacking affirmative answers to these questions, Franklin would not overstep his authority if he put a full and accurate statement about Stanton’s contribution in an acknowledgment or footnote, rather than naming her as an author. Yet it appears that Stanton initiated the line of work and gathered the data. Therefore, more visible recognition would be appropriate for her contribution and would carry greater value for the advancement of her career. Failing more concerted, extended efforts to reach Stanton, Franklin’s best option for giving Stanton more visible recognition might be to add a footnote explaining the circumstances relating to Stanton’s role.

Reflection on Franklin’s situation and others might produce a set of ground rules for assigning authorship, such as question 4 above suggests, as follows:

1. No one who has not been informed and given consent should be included among the authors;

2. Anyone whose name appears on a paper shares responsibility for the content, including for any errors, witting or unwitting, unless otherwise indicated;

3. The practice of including names of people who have not directly contributed to the paper is problematic, for it misleads and interferes with properly attributing responsibility; and

4. In a situation in which not every author listed actually shared in the writing, the contribution of each should be specified in the paper or in a footnote.10

Vignette 5: To Share or Not To Share 11

This vignette presents a situation in which competition could play havoc with relationships inside a laboratory. In a setting in which the director of research holds regular meetings for discussion of research projects, a post-doc, nevertheless, sees a need to keep findings secret. The post-doc’s concern for recognition threatens the collaboration she has begun with a graduate student.

Professor Edward Chang supervises post-doc Eileen Noonan and a graduate student, Richard Puchalski. Chang approaches Noonan after a laboratory group meeting at which he had expected her to report results she had obtained from her last fractionation. Chang had even tried to find Noonan in the morning before the meeting to go over the data with her, but, to his surprise, had not found her at her bench. When Chang inquires whether something is wrong, Noonan replies that nothing is wrong. She accounts for her reticence at the meeting and absence from her bench saying she had stayed up late the night before reading the gels and had overslept. Noonan says she has to leave but will come in to see Chang the next day.

When three days pass without Noonan’s appearing, Chang asks Puchalski if he has seen her. Puchalski reports that she has not been around much in the daytime but he knows that she has been working at night. He is puzzled, however, because on the day before the meeting at which Noonan had remained silent, she had told Puchalski that she had an idea that might help him find the co-activator for his DNA binding protein. He had asked her
about it at the meeting, but she had told him she had been wrong and to forget about it.

Puchalski admits to Chang that he has been so frustrated the last few weeks he has not been coming back to the lab after dinner. Chang tries to buoy Puchalski's spirits, pointing out that Puchalski has found the DNA binding protein and just has to find the co-activator to get it all to work. He suggests that some changes discussed at the meeting might just “do the trick” for Puchalski. Chang asks Puchalski to leave Noonan a message conveying Chang's request that Noonan get in touch with Chang. Puchalski agrees and promises to get back to Chang himself in three days.

On the third day following, Noonan comes in to see Chang. After he mentions his frustration from trying to reach Noonan, she hands him two papers she has drafted. One describes the work they had planned to talk about at the meeting. Noonan explains that when she read the gels the night before the meeting, she realized that she had accidentally found the answer to Puchalski's problem. She understood that she had come across an entirely new class of DNA binding proteins and their partner co-activators. She felt that she needed to do one more experiment to confirm her results.

Chang reads the two papers and is very impressed and excited by Noonan's discovery. But, he says, Noonan should have told Puchalski that she had found the answer to his thesis problem. Further, Chang says, Noonan should have done the last experiment together with Puchalski and included him in the final paper. Noonan replies that she has thought a lot about that and disagrees. She explains that she included Puchalski's name on the first paper because she started with his technique for isolating the DNA binding activity. However, she claims that the second paper on the co-activator and its implications for all regulation should be her and wants it to stand out, when published, with just her and Chang's names. Chang says that he cannot force Noonan to include Puchalski's name as an author, but, appealing to their solidarity in the lab, he suggests that she reconsider. To Chang's question whether she has shown the papers to Puchalski, Noonan answers that she has not, but has thought of presenting them at the group meeting the next day. She asks Chang what he thinks of that.

Noonan is concerned about getting proper recognition, but she is apparently not well-prepared for dealing with promising results obtained in close collaboration with the graduate student under her. Puchalski seems to need Noonan's help to move forward; Chang is leaving further progress up to them. Although Chang holds meetings concerning the progress of the research and is available for one-on-one meetings with Noonan and Puchalski, he is remote enough to miss tensions developing within this small group.

In the handling of her exciting findings, Noonan is subject to criticism for secretiveness, deception, and failing to keep her word. Her secretiveness is a violation of role responsibility, a failure to fulfill expectations associated with her role as a post-doc. It is hard to justify Noonan's failure to share any information about her findings with Chang on the day he expected her to present them, if, as we assume, he had good reason to expect them and he had made that expectation clear to her.

She has an opportunity after the meeting to explain to Chang that she believes she has discovered something important but has to recheck before saying anything to the group or to Puchalski. She can also bring up the matter of receiving credit for her breakthrough (if it is one) before agreeing to include Puchalski in the effort to confirm the findings. Instead, she waits almost a week and confronts Chang with drafts of papers and her own allocation of authorship to give herself adequate recognition.

On the day of the original meeting, Noonan lies to Puchalski and misrepresented her situation to Chang. Can those actions be justified? Researchers are entitled to hold back exciting results from public exposure until they have confirmed them. However, this situation does not involve public disclosure, and Noonan has not merely held back results. She has deceived Puchalski and misled Chang. In her excitement, Noonan may need a little time to get her bearings, but she can get the time without using deception. She can put off Puchalski, without lying, until she has talked with Chang. Sooner or later both will have to know about Noonan's findings. However, by her deception, she risks undermining trust, causing those who are misled (and made to wait) to feel resentful, and leaving herself open to the coarsening that comes from deceiving.12

Noonan is also subject to criticism for failing to appear on the day following the meeting since she had told Chang that she would return. Puchalski has relied on her, and he grows more discouraged as almost a week passes with no word from Noonan. It seems that she would not
have had to trouble herself much to make telephone contact with Chang
or Puchalski.

Noonan’s conduct, however, should not be isolated from the conduct
of the others, especially Chang, for Noonan’s ethical missteps occur
within a web of research relationships. Her conduct should also be
considered in light of practices in the lab: Where there are clear policies
covering these matters, Noonan’s conduct is more open to criticism than
it is in a scene in which the paths to credit and recognition for a post-doc
are ill-defined. In apparent uncertainty about getting adequate recogni-
tion, she acts to protect her own interests, losing sight of the interests of
the others.

Chang may be criticized for failing to respond to danger signals
promptly and directly and, it appears, for deficiencies in managing his
lab and this research collaboration in particular. When Noonan fails to
show up to talk with Chang, after having led him to expect her, Chang
lets matters ride for three days. He then does not contact her directly but
asks Puchalski, whose schedule he knows does not coincide with Noo-
nan’s, to leave her a message. The matter with Noonan is on his mind,
and he apparently senses that something is amiss. He has good reason,
therefore, to take care to contact her promptly himself. He seems not to
realize that he is in a delicate situation, and that he should not use the
graduate student as a go-between with his post-doc.

Puchalski seems to be caught in the middle. It appears that he suffers
because of deficiencies in the conduct of the other two. Even if he is not
mystified by Noonan’s conduct and has suspicions about what she is up
to, he would not be justified, as far as we can tell, in doing more than he
does to alert Chang.

The weekly exchange about research in this lab is a good medium for
communication. In order to prevent destructive competition between a
post-doc and a graduate student, Chang needs to take more care about
communication, making sure that there are policies about credit and
authorship and that they are clearly conveyed. As the director of the lab,
Chang has the right to parcel out problems to students and post-docs
(with their consent) and a duty to demarcate clearly each trainee’s
problem area. There has to be open discussion in the lab about expecta-
tions and procedures for collaboration, with the recognition that people
will develop proprietary attitudes toward their investigative results. Ex-

cplicit attention has to be paid to accommodating the sometimes divergent

interests of graduate students and post-docs. Chang should have had
some discussion with Noonan about where opportunities for her lie.

The ethics of authorship is such an important concern that it should
be discussed in the lab. The fact that Chang does not appeal to any pol-
icy about authorship suggests that there are no general guidelines beyond
the understanding that the supervisor will be named as an author.
(However, it would be premature for Chang to be named a co-author
before verifying the data.) Noonan may be uninformmed or misinform-
about the relative weight of two as against three co-authors. However
that may be, Chang should not have allowed things to develop to the
point that he has to appeal to Noonan’s sense of solidarity to secure her
agreement on authorship. He is responsible for setting a tone and
establishing ground rules for determining authorship that would preclude
his having to make such an appeal.

In summary, Chang should have responded to signs of something
amiss earlier and more decisively. As we have interpreted this ambigu-
ous situation, Noonan’s conduct is subject to ethical criticism. However,
Chang also failed to meet his obligation to establish clear policies about
what deserves credit and who determines to whom it is given.

Vignette 6: Submitting An Abstract

In this vignette, the broader competitive environment in which scientists
operate makes itself felt in the relationship between a supervisor and a
post-doc. This is a situation in which the post-doc is learning the ropes
with regard to participating in conferences at which investigators present
new research results. By his behavior, the advisor sends a message about
how things are done, suggesting that tampering with data to submit an
abstract is condoned if it seems necessary to stay in the competition.
In addition to presenting a problem in the relationship between an advisor
and a post-doc, the situation poses the more general question of what
ethical constraints there should be on competition in science.

The deadline is fast approaching for submitting abstracts for a
specialized conference in the neurosciences. Post-doc Jay Patel has
collected data for a project that theoretically promises very exciting
results. So far, however, the data from an initial study are not
very interesting. In view of the importance of the conference and
the promise of the project, Patel’s adviser, Helmut Braun, decides
that they must submit an abstract. With that aim in mind, Braun looks over the data, fills in missing data points, and eliminates others, explaining that they are noisy. A statistical recalculation using the altered data supports Patel's hypothesis.

However, the unaltered data look satisfactory to Patel. He, therefore, tries to persuade Braun that the data are sound and should not be altered (even if that means they have to miss this deadline), but Braun refuses to budge. Braun insists that Patel send in the abstract for their co-authored paper and include the altered data. He points out that they can always withdraw from the conference if more definitive experiments do not pan out. Braun says he knows of other occasions when papers have been withdrawn from conferences after abstracts were accepted. Of course, the result was a hole in the program.

Is there anything to be said in defense of Braun’s conduct? The best that can be said is that he believes that the data points he has filled in will eventually be derived from Patel’s further investigations and the data points he has eliminated as noise will prove to have been just that. It is to Patel’s credit that he does not lose his ethical bearings; he is troubled by the dishonesty of submitting the abstract with the altered data. Patel, however, is under intense pressure from Braun, who is apparently anxious about his position in a highly competitive research environment.

Should Patel be persuaded by Braun’s observation that they can withdraw from the conference if further experiments fail to yield the expected results? After all, Braun seems to suggest, they do no harm so long as they do not present or try to publish results based on altered data. (Of course, if the abstract is to be published in a program book, this rationalization is not available.) Patel may wonder whether this anticipation of research results is common practice in science. He might even ask himself whether it could be a convention in this field of science. In light of such questions, Patel may be embarrassed to inquire about the propriety of Braun’s conduct.

The reasons against having such a convention or accepting Braun’s rationale for going along with submitting the altered data will sound familiar to anyone who has thought about why we have an agreed-upon moral standard against lying. Social enterprises depend on trust. To the extent that we cannot rely on people to be truthful, we have to be wary and may even have to take defensive measures. When people find that such measures are necessary, they have to engage in costly efforts and social enterprises are impeded. (Economists would say transaction costs are increased.) In this vignette, the social enterprise is a scientific conference presenting cutting edge research. Consider how difficult it would be to plan and conduct such projects if those who submit abstracts cannot be counted on to be truthful. A conference could be undermined by the withdrawal of papers. Braun is counting on general honesty and making an exception of himself and Patel; he is prepared to take advantage of the honesty of others.

Braun has created a very difficult situation for Patel. He is setting an example of dishonestly cutting corners; he is leaning on Patel to behave dishonestly; and perhaps he is leading him to wonder what other unethical behavior Braun will require as Patel continues to work with him. Braun is also exposing Patel to added risk. Braun cannot be certain that the only untoward consequence they might have to face is the necessity of withdrawing the paper. This is a situation in which the ethical problem for Patel becomes one of figuring out how to avoid involvement in submitting an abstract containing "fudged" data without alienating his advisor.

Patel needs to find someone to talk to for advice on how to extricate himself from this claustrophobic situation. Someone within the research group who has won Patel’s trust (if there is such a person) would be the first candidate. If Patel lacks such options, he will have to turn to the department head or another experienced person with authority (inside or outside the department) who has inspired his trust. In telling his story, Patel should stick to a narrow factual account.

Vignette 7: Perils of Peer Review

This final vignette also deals with the issue of ethical constraints on competition. The problem in this situation comes from the operation of the peer review system, a part of the structure of science. This time the relationship of interest is between a senior investigator and a junior investigator—not a graduate student or post-doc—in her research group.

In a large lab of over 15 investigators, Elena Garcia is a senior investigator working on the mathematics of signal processing. Her own small group is using spectral analysis to describe a speech
signal. Right now they are stymied by problems they have
encountered with this approach. In her role as a reviewer for the
National Institutes of Health (NIH), an agency in the Department
of Health and Human Services (DHHS), Garcia receives grant
applications for primary and secondary review. The most recent
batch included an application that suggests that the speech signal
can be described by a new method of auto-regressive analysis,
eliminating the problems encountered with spectral analysis.
Garcia realizes how welcome this suggestion would be to a junior
member of her group, Geoff Bernstein, who has felt particularly
frustrated.

With regard to such grant applications as the one under re-
view by Garcia, a Memorandum from the Executive Secretary of
the Surgery & Bioengineering Study Section of NIH to members
of the Study Section states:

You may consult with colleagues in your institution re-
garding a particular application, but please do not contact
persons outside your institution; there may be some rea-
son unknown to you that would make it inappropriate to
request their evaluation. Each application should be
treated as confidential and under no circumstances should
you discuss the proposal or any aspect of the review with
the applicant; any inquiries regarding an application
should be referred to this office.

Garcia is in a quandary. Reviewers are permitted to consult with
colleagues about applications so that the best local advice is incorporated
in the review. She herself cannot now eliminate from her understanding
the insight she gained from the proposal about using the new method of
auto-regressive analysis. On the face of it, it seems that making use of
this insight would not hold back scientific advance; it might contribute to
it. Yet, she has misgivings about showing the application to Bernstein,
a junior colleague whose progress she is presumably committed to fostering.
Giving help to colleagues, especially junior colleagues, seems a
good thing; could it be ethically wrong in this instance?

Garcia’s misgivings are well founded. Ostensibly seeking advice for
her review, she would knowingly be transmitting critical information, the
fruit of another investigator’s efforts, to a competitor who is certain to
make use of the new insight. She would be doing this in circumstances
in which the originator of the information cannot defend his or her
interests in receiving early recognition for or acknowledgment of his or
her innovation. Peer review is intended to support fair competition.
Grant applicants enter competitions for funding, counting on others,
including reviewers who are competitors, to refrain from appropriating
material from their proposals. In so far as sharing the information with
Bernstein violates the rules of competition they have voluntarily accepted,
it would be taking unfair advantage.

In this situation, the NIH Memorandum offers guidance. It asks the
reviewer to keep the application confidential. The purpose of permitting
consultation with colleagues is to get the grant properly reviewed. It is
not intended for mining, i.e., for facilitating the transfer of knowledge
from the grant application to the reviewer’s research group. Since
Garcia’s research group is small, she can presumably furnish the ex-
pertise of the group without consulting members about this proposal. In
these circumstances, by simply sharing the proposal with Bernstein, who
can hardly avoid making immediate use of it, Garcia would be breaching
confidentiality. Garcia’s own acquisition of information from reading the
proposal is a consequence of peer review that has to be accepted.
However, she would be acting unethically to facilitate Bernstein’s making
use of ideas from the proposal without acknowledgment. In the context
of peer review, acknowledgment is problematic because of the common
requirement that reviewers remain anonymous. The NIH Memorandum
specifically excludes discussion with the applicant. Yet some situations
seem to call for mechanisms for breaching anonymity and confidentiality
to allow for acknowledgment or even collaboration.

There is an additional issue of conflict of interest. The question is
whether Garcia is in a position to provide to NIH the independent judg-
ment that is expected. Her own research group is competing in precisely
the same area of research; her identification with their project may com-
promise her judgment. We can note here, however, that Garcia can deal
with this problem by disclosure, i.e., informing the Executive Secretary
of the Study Section that her research group’s area of activity coincides
with the area of research in this proposal. The Study Section would be
on notice that Garcia has an interest that might distort her judgment.
This does not end the matter, however. It is common practice for the
Executive Secretary to expect a review, nevertheless, and to put the onus
of avoiding a biased negative judgment on the reviewer. In actual
practice, study sections take the view that they get expert advice from those who are doing closely similar work and the latter must take care to render reliable judgment. Disclosure by itself is a "cop-out."

**General Discussion of Issues from Vignettes**

These vignettes provide only a small sample of difficulties that arise in research groups. Nevertheless, they highlight issues that must be addressed in managing research groups, and suggest the following standards that are needed to maintain an ethical research environment.

1. Articulate ground rules.
2. Clearly assign and demarcate research problems for graduate students and post-docs.
3. Devise regular channels for communicating and sharing information concerning techniques, data, analysis, and interpretation.
4. Formulate clear policies about the basis of and opportunities for recognition.
5. State clearly a policy about criteria for credit and authorship, with the rationale for each.
6. Devise clear policies concerning control and ownership of data.
7. Formulate standards for competent advising that address the following concerns: to provide graduate students and post-docs clear benchmarks and regular feedback on their progress; to give students early notice of evidence that they cannot meet the standards of the graduate program; and to take steps to prevent students from excessively delaying completion of their graduate work.
8. See to it that all trainees receive adequate mentoring.
9. Encourage trainees to regard themselves as active participants in the research life of the group with correlative duties to seek information and technical and ethical guidance.
10. Make the use of ethical language and discussion of ethical issues an ordinary feature of the ongoing life of the group.

The situations presented in the vignettes occur with almost endless variations. It is instructive to compare variations. We noted in connection with Vignette 1 that research groups in universities have latitude for devising alternative sets of ground rules, for example, rules about retention and ownership of data. For research groups in companies, constraints specific to the world of commerce may narrow the range of alternatives. Even in companies, however, deliberation within research groups about alternative ground rules can be beneficial, producing at least a clearer sense of what is at stake from an ethical perspective.

Vignette 2, about a graduate student's surrender of a thesis idea, is similar to the story that a senior investigator in his late fifties tells about an incident from the early part of his career as a graduate student.

A student (call him Stanley) had obtained some very unusual and interesting findings about mechanical properties of material he had tested, using a machine he himself had built. Stanley had secured the material in consultation with his advisor, Dr. P., and was happy when Dr. P. indicated that Stanley's findings would furnish him a good thesis topic. He was pleased to comply with Dr. P.'s request that he write up the test results so that the data could be published as soon as possible. Upon receiving Stanley's write-up, Dr. P. compiled the data into a paper with his own observations and very acute speculations and submitted it for publication, with his name as first author. The publication of that paper denied Stanley this thesis topic. According to Stanley, "His [Dr. P's] claim later was that he didn't know that [according to local standards] this act [of publishing] would prevent me from using these data in a Ph.D. thesis."

In this story, the advisor appears to lose the student from his field of vision as he takes over the data, fashions the paper, and gets it published. The upshot, as in Vignette 2, is that the student loses a thesis problem. With hindsight, the mature Stanley wonders how it was that Dr. P.'s colleagues, chairman, and dean tolerated Dr. P.'s treating Stanley and other trainees in this way, taking over their results and blocking their progress.

Dr. P. was either uninformed or he overlooked rules laying out conditions for getting thesis topics approved. As an advisor, Dr. P. should have been aware of these conditions and kept them in mind. In his treatment of Stanley he seems to rank as a "toxic mentor." Scholarly discussion about mentors has distinguished four toxic types: 1) "avoiders,"
2) "dumpers," 3) "blockers," and 4) "destroyers" (Darling, 1986). By making themselves unavailable to students, by dropping students into new roles to "sink or swim," by obstructing with delays or other means, and by tending down with criticism, advisors can cause unnecessary pain and impede students' progress. Dr. P.'s action in taking over the data and moving full-speed ahead toward publication, heedless of Stanley's interests, marks him as a "blocker."

Unfortunately, this classification of four toxic types is not exhaustive. We can add other types: advisors interested in clones of themselves, those who overprotect, connivers or manipulators, and those who continually change the playing field so that students do not know what the rules are. As Vignette 6 shows, another category includes advisors who communicate messages that cutting corners, misrepresenting, and free riding on the ethical behavior of others is acceptable. The story of Stanley and Dr. P. and Vignette 2 show two of the many ways in which an advisor can do damage in launching a student with a problem suitable for a dissertation. The damage can be severe enough to drive very talented students out of graduate study. The story of Stanley and Dr. P. is instructive in another respect. It presents a clear instance of unjustifiable conduct that can be used as a reference point for making judgments about less-clear cases when they arise in discussion or in the course of research.

Vignette 3 pictures a graduate student who works in succession under two different senior investigators but seems to have no significant connection with either, even after the beginning of his third year of graduate study. Missing from the scene is a structure of expectations and regular channels for communication about students' progress. In this research group, relationships seem distant and communication appears to be uncertain. The advisors remain in the background, setting the stage for hurt feelings, jealousy, animosity, and loss of trust.

In another story, a post-doc reaches the point of entertaining suspicions about another post-doc, and it crosses her mind to make an accusation.

The assignment to Melanie Wong, a new post-doc in Professor Henry Busch's laboratory, is to pick up a project begun four years earlier by another Busch post-doc, Tony Cortona. After Cortona had published one paper from this research in a relatively obscure journal, he had gone on to a different problem and had considerable success. When Wong, has trouble repeating Cortona's experiment, as Cortona had warned, Busch urges her to contact Cortona. Since, in the afterglow of his research success, Cortona leaves town for job interviews, Wong is frustrated in her efforts to consult him. She begins to suspect that Cortona is holding back something, perhaps he has faked data. She is not persuaded when a mutual friend, who has looked over Cortona's procedure and her notebooks, suggests that there may be a technical problem and Wong may be missing something obvious. Wong thinks about reporting her suspicions to Busch and realizes that, if she is correct, this could bring an end to Cortona's career.

Again, regular channels of communication and a structure of expectations seem to be lacking; the advisor seems distant. Indeed, Wong finds Busch so remote that she can imagine his previous post-doc faking data. In her frustration and distress, she turns to a friend in the group who tries to help her reign in her suspicions and deal more calmly with her failure to repeat the experiment. Perhaps Wong is highly excitable; or it may even be that Cortona is holding back information or has something to hide.

Busch has behaved as a "dumper" in relation to Wong. This advisor, it appears, ought to have given more attention to getting Wong started on a project that had not borne fruit for a previous, successful post-doc. If he had done so, he might have headed off the escalation of her suspicions. There is some empirical evidence to support this interpretation of this situation. In a large-scale study of the socialization of graduate students to the life of academic research, the investigators found that "only one structure variable, the extent to which the student feels that his or her advisor and others provide useful feedback and evaluation, affects levels of observed misconduct in research" (M. Anderson, 1994). As things stand, Wong can behave responsibly by persisting until she contacts Cortona for advice on dealing with her replication problems and by detailing these problems in discussion with Busch.

There are so many common variants of the problems associated with authorship, the issue in Vignette 4, that another ambiguous instance is worth examining. The following story concerns a graduate student,
Michael Breen, whose advisor, Randall Plotner has been solicited to write a chapter for an upcoming book.

Breen accepts Plotner's invitation to "take a stab" at writing the chapter. After several drafts that take advantage of Plotner's suggestions for revisions and additions, Breen produces a chapter that he believes pleases Plotner. However, Breen learns indirectly that Plotner has submitted the chapter without including Breen's name as co-author. When he goes to see Plotner about getting credit as co-author, Plotner says "Oh, don't worry about that. This was a learning exercise. You'll get to co-author things later." Breen feels unfairly deprived of credit for his work, but is anxious to avoid trouble with his advisor.\textsuperscript{17}

On one interpretation of this case, the situation highlights the importance of having clear understandings in advance about the terms of authorship. Some advisors spell out what level of contribution is associated with a particular level of authorship. Graduate students or post-docs who voluntarily accept the terms have no cause for complaint of unfairness so long as the terms are reasonable. Breen seems to have assumed that he would be credited as a co-author if he produced an acceptable chapter. In view of the fact that the editor solicited his advisor for the chapter, Breen might have considered that, as a matter of prudence, he needed an up-front understanding of the terms. However, the advisor has the responsibility to make and announce a policy about the basis of authorship. Plotner appears to be ethically at fault for explaining only afterward that this assignment was merely a "learning exercise." Beyond that, he has a duty, if he makes use of Breen's work, to give him clear credit for his contribution. Otherwise he misrepresents his work and uses his power to deny Breen what is due him according to common standards in science. It is hard to see how he can adequately acknowledge Breen's role and deny him co-authorship.

On an alternative reading of what has transpired, Plotner has given Breen an opportunity for co-authorship, and Breen, after repeated drafts and revisions, has failed to produce material good enough for Plotner to use. Plotner is either too busy or too uneasy to explain to Breen the deficiencies of his final draft. Accordingly, he makes the comment, "This was a learning exercise," in a lame effort to soften the blow. On this reading of the situation, Plotner is at fault for not making the deficiencies of the final draft clear to Breen. Doing that would make it a genuine learning exercise. It is, of course, possible that he has tried and Breen stubbornly refuses to believe he has not produced acceptable material for the chapter.

Vignettes 5 and 6 bring out problematic aspects of the position of the post-doc. One of the positive features of the post-doc position is that it forms a bridge between graduate students and full-fledged investigators and thereby has the potential to enhance the research milieu. In that bridging position, the post-doc can stimulate or inspire graduate students and contribute to the thinking and work of more senior people in the research group. That bridging feature can, of course, benefit the post-doc, tying more firmly to the group the trainee in the most tenuous position in the research group.

In view of the complexities of the post-doc's situation, it takes considerable thought and care on the part of the supervisor to work out an arrangement for realizing the potential of the post-doc, and ensuring that the post-doc is neither neglected nor exploited. Realizing the benefits is not just a matter of luck; it requires planning and care. An example of a mechanism that has worked is the creation of a small seminar, led by the post-doc, that concentrates on the post-doc's specialty and includes graduate students and more senior investigators who have an interest in that specialty. When the advisor takes the trouble and arrangements work out, the benefits for everyone can be striking.

At the heart of the post-doc's difficulties in Vignette 5 is her concern for adequate recognition. Vignette 6 deals with a post-doc under pressure from his advisor to fudge data and raise broader questions about modifying data and "anticipating" results (to be discussed below). At the Sigma Xi Forum on "Ethics, Values, and the Promise of Science" in February, 1993, in San Francisco, a panel of post-docs poignantly conveyed the conflicts, pains, and precariousness of the post-doc position (Singer et al., 1993). Trainees in that position can labor under excessive demands costly to them but of benefit to their advisors or graduate students. Because of the precariousness of their positions, post-docs have no recourse when arrangements for them do not work out. They are entirely dependent on the support of their individual advisors, except in unusual cases. Vignette 5 illustrates how things can begin to go wrong when a post-doc becomes anxious about recognition.
Vignette 6 shows a post-doc trapped with an advisor who seems to emand unethical conduct. To prevent such problems from arising, a post-doc panelist at the Sigma Xi Forum, Tara Meyer, proposed the following list of obligations for advisors in their relations with post-docs (Singer et al., 1993). She acknowledged that there are mutual obligations or post-docs but felt she was not in a position to do them justice.

1. **Discrimination, Sexual harassment, etc.** These behaviors are not permissible in any job situation.

2. **The Project.** A prospective post-doc should have an accurate picture of what the project will entail, within reason. It is extremely disheartening to a post-doc to discover upon arrival that the advisor has changed the project substantially. The post-doc might not have accepted the position knowing about the change.

3. **Support for the Project.** Sufficient resources should be provided for the project. There is also a time factor involved. A post-doc should not be asked to wait eight months for the arrival of a piece of equipment which is absolutely essential to a project. Clearly, there are always unforeseen delays, but the advisor should have an infrared spectrometer (IR) in the lab before asking the post-doc to do IR studies.

4. **Group Duties.** The level of group responsibilities varies greatly from group to group. The extent of these duties should be explained clearly, and their execution should leave sufficient time so post-docs can do their own research.

5. **Project Success.** If a project is failing miserably, do not simply assume that the post-doc is at fault. Discuss options with the post-doc for modifying the project such that the probability of producing results increases. Do not force a post-doc to spend two years working on a bad project in order to avoid admitting that the original idea was at fault.

6. **Hiring/Termination Flexibility.** Both incoming and exiting post-docs need flexibility. Neither finishing a thesis nor getting a job can always be planned accurately. Also, if your financial situation precludes flexibility, inform the post-doc as soon as possible.

7. **Work Hours.** While it is reasonable to expect that a career scientist will be willing to work more than the arbitrary 40 hours per week, it is not reasonable to expect them to work every evening and weekend.

Discuss work expectations with your post-docs, but be realistic. Also, flexibility in work hours is a valuable fringe benefit which can be offered at no cost to you.

8. **Be a Mentor.** Give encouragement and praise where it is appropriate. Help them get jobs. Teach them to be good researchers. Your students and post-docs are your "descendants" and through them your contributions to science and your reputation will continue long past the end of your career.

9. **Project Direction.** As post-docs become familiar with the project, give them more control. They are Ph.D. chemists [or whatever] and you will probably achieve better results if you collaborate with them rather than using them as technicians.

10. **Acknowledgement of Work.** Have a clear policy about how contributions are acknowledged and discuss it with your post-docs. If a post-doc contributes a truly original idea or suggests an original project, consider allowing them to publish independently from you. Do not simply adopt their ideas and write them into future grant proposals and papers.

Meyer believes these guidelines are generally "understood" in academia but "are not universally practiced and are rarely if ever discussed." She suggests that individual departments issue such guidelines and offer "mediation/counseling" when post-docs and advisors develop problems.

Returning to the issue of modifying data, the situation in Vignette 6 is relatively unambiguous insofar as the advisor's alteration of the data includes filling in data points—that is, inventing data. If Braun merely dropped data points, it would not be as clear that he is doing something dishonest. In some contexts, dropping data due to experimental artifacts, a "machine glitch," can count uncontroversially as acceptable practice. Trainees encounter other instances that are more ambiguous; indeed, one could set out a spectrum of cases of increasing ambiguity. There is considerable debate about what constitutes acceptable practice with regard to modifying data. The particularities of investigation in different fields generate ambiguities specific to the modes and instruments of investigation in those fields, with local circumstances at a particular time also playing a role in amplifying or reducing ambiguity.
The question of the conditions in which modifying data and "anticipating" results are ethically objectionable deserves more thorough investigation by scientists, science studies scholars, ethics specialists, and groups engaged in research. The investigation should include examination of historical instances and the controversies they engender. For example, the instance of Millikan's oil-drop experiment, involving discrepancies between Millikan's published results and his laboratory notebooks, precipitates questions about "the extent to which such procedures are accepted in the scientific community" (Thomsen, 1995). In some contexts, it looks as if the reward for being first and "right" cannot be withheld even if the research process was "quick and dirty." Scientists and science studies scholars should confront the question that a science studies scholar was led to ask after studying the Millikan oil-drop experiment and the treatment of the story by various writers: "Is the ethical accountability now increasingly required from science in principle attainable in a system which is so constructed that one gets rewarded for being a quick-and-dirty first; rather than a conscientious second?" (Thomsen, 1995; Segerstrale, 1995).

Research group directors should not count on trainees learning what is acceptable practice and what is unacceptable practice in modifying data, and the reasons, without explicit, thorough discussion and examination of instances. This training also offers the opportunity to subject practices to scrutiny and to modify practices when their deficiencies or limitations are revealed. It is a useful strategy to look at a range of situations in an area of research in order to identify relatively clear cases of acceptable and unacceptable modification of data. These can serve as reference points in determining where to draw the line in borderline cases.

Vignette 7 presents a situation in which the leader of a small research group must decide whether a proposal she has been asked to review is one that she can ethically share with a junior member of her group who could hardly avoid profiting from new information in the proposal. Since the prospective reviewer is the leader of a small group, it is reasonable to suppose (though not necessarily the case) that she individually has the expertise to provide a good review without input from her colleagues. In that light, we judged that she would unjustifiably breach confidentiality to share the proposal with her colleague under the guise of needing his input for a good review. A review from the leader alone would be good enough to outweigh the risks of passing on the proposal to a colleague ready to make use of it.

A situation more difficult to decide arises if the scientist, Elena Garcia, were the leader of a large group and the expertise is dispersed in the group. In that situation, Garcia cannot escape sharing the proposal with members of the group if she is to fulfill her responsibilities as a reviewer. As noted in the commentary, disclosure to the Study Section does not resolve the problem, for the Study Section is already on notice that this group is doing work closely similar to that described in the proposal. That is precisely why it has sent the proposal to this investigator. The need to furnish an expert review seems to justify sharing the proposal with the group. In these circumstances, when Garcia undertakes the review, she needs to have a conversation with Bernstein and her other colleagues about how they will proceed so as to avoid the traps of "mining" the proposal and writing a biased, injurious review of a competitor's work. They should be sensitive to the gradual slide from learning from the proposal to stealing from it and from making apt and useful criticisms to producing a destructively biased review.

There are many variations on this situation of transferring critical information (or materials) from one research group to another, some dealing with the reverse situation. Consider the scientist on a visit in someone else's lab in another institution facing a quandary about whether to impart or hold back critical information that would facilitate the progress of researchers in this other lab. In another variant, the Gallo case presented at the outset of this paper highlighted questions about a research group's responsibilities for handling materials it has received from another research group. These are situations that call for ethical forethought with respect to colleagues outside the research group and at the same time to colleagues, especially subordinates, within the research group. The Garcia case also illustrates how responsibilities of one role, that of the reviewer, can require accommodation with those of another role, that of mentor or research group leader.

An important issue that we have not illustrated as such is the problem of the treatment of women and members of other groups heretofore under-represented in science. In view of the very low representation of these groups at every level, but especially in positions of power, this is a very pressing issue. Since it is closely associated with issues arising from the disparity of power, the treatment of groups previously excluded,
such as women and African-Americans, can be accommodated in the discussion of many of the vignettes.

There are, of course, grosser manifestations of discriminatory treatment, such as sexual harassment. The need to take measures to avoid advisors with reputations for making the going difficult for women and minority students adds to the strain of graduate study. One woman scientist now in a position of power likens her experience as a graduate student to that of a person going through the program with a broken leg. An African-American, a senior member on the faculty of a leading medical school, says that he does not recall any strain in doing the intellectual work of graduate study. What he remembers are the severe strains of being one of the few African-Americans. Even an environment where the main problem is invisibility or exaggerated visibility can be very stressful and harmful.

Recommendations for change in the management of research groups that have emerged from our analysis, with their emphasis on openness, explicitness, and back-and-forth communication, favor more democratic policies. At the same time, the examination of problematic situations underscores the need for leadership by principal investigators and research group directors who take responsibility for exercising authority and for setting forth policies that govern the conduct of research. These are not the only institutions facing the imperative to devise less autocratic practices while maintaining a structure of authority. Researchers, especially research directors, should accept an ongoing responsibility to scrutinize practices in the light of this imperative.

Because of the almost endless variety of concrete situations such as those we have examined and because of changes in circumstances, the analysis and discussion of problematic instances should be an ongoing activity in research communities. That exercise of examining particular instances seems to sharpen the skills for thinking through and resolving problems well. One test of a good resolution is whether it would make a good policy. Indeed, persistence in examining problematic situations should help to generate useful policies.

Many observers have commented that they are less concerned with out-and-out misconduct in science than with sloppiness and negligence. There is reason to think that negligence and sloppiness are more common and more insidious than outright misconduct. Because they are less rare and affect cooperation and trust, negligence and sloppiness are thought to pose a more serious threat to scientific enterprises than misconduct (Schmued, 1983; Culliton, 1988).

Negligence is the failure to meet an appropriate standard of care. Articulating and publicizing standards of conduct is an essential part of the effort to ensure that professionals meet an appropriate standard of care. Accordingly, we have been concerned throughout this essay with the need to make relevant standards and expectations explicit. We have also emphasized the need for structure and for communication that is not one-sided. We have suggested that lack of structure and communication invites problems and creates an underlying substrate for unnecessary pain.

Ready-made models for organizing and managing research groups are of value, but with limitations. They have to be custom-made to fit personalities and circumstances. Some like to think of the family as the model for research groups, but, of course, families do not conform to a single pattern. So it remains for research groups to find their own way to arrangements that nourish trust and build social relationships that support responsible conduct in research.

Notes

1. A remark in a personal conversation between one of the authors and Don Buzzelli, a federal government official in a National Science Foundation office that deals with allegations of misconduct in science.

2. For a vivid description of effects of the power disparity, a post-doc offered the following account "... several important issues that I think are common to all post-docs. The first is there are very few options available to post-docs when there is a true conflict. It seems like the only two that are really feasible are suicide or luck. That's pretty much it. You can have either social suicide by completely rebelling against your lab or you can have professional suicide. It's not only the post-doc who is right by any stretch of the imagination, but it seems like the suicide always works the same way. It's very rare that a lab is destroyed by a post-doc where there are many post-docs who are destroyed by labs." (Backus, 1993)

3. See Whitbeck (1994, p. 1020) for a description of competition between graduate students within research groups. Sheila Jasanoff (1993) comments on the "winner take all" feature of the organization of science in the United States.
4. Gerald Seltzer of the National Science Foundation in e-mail communication to the authors.

5. This point is drawn from an analysis by Alan Wertheimer presented at the annual meeting of the Association for Practical and Professional Ethics, Arlington, VA, March 2, 1995. For probing discussion of notions related to exploitation and the moral implications, see Wertheimer (1994, 1997).

6. This case was produced in 1993 by a research group at the Illinois Institute of Technology in the course of considering its own ground rules.

7. This vignette was written by Weil and Arzbaecher and is based on an actual incident discussed by the IIT Research Ethics Sack Lunch Group.

8. This case is adapted from a case contributed by Caroline Whitbeck of MIT to an interchange on e-mail on November 2, 1992.

9. This case is adapted from a set of cases compiled by C. K. Gunnsalus, Vice Chancellor for Research, University of Illinois at Champaign-Urbana.

10. This set of ground rules was produced by the IIT Scientific Research Ethics Sack Lunch Group at one of its monthly meetings. The group did not make explicit the requirement that anyone who is named as an author should have read and understood the paper and concurred with the interpretation. It is worth considering whether this requirement should be stated as a rule.

11. This case is adapted from one of a group from the Whitehead Institute at MIT. Eve Nichols wrote the original dialogue version of this case.

12. Bok (1978) emphasizes that lying has bad effects on the liar. That person becomes more susceptible to lying and less sensitive to the damage it can cause.

13. This case is adapted from a case in a collection of cases circulated by Leslie Rothenberg, Univ. of California—Los Angeles.

14. This paragraph is taken verbatim from a National Institutes of Health Memorandum of April 11, 1990, on the subject, "Assignment of Grant Applications." It was addressed to members of the Surgery & Bioengineering Study Section. The vignette, written by Weil and Arzbaecher, was suggested by one of the variations of a situation described by Michael Zigmond of the Univ. of Pittsburgh in his talk at a Satellite Symposium of the annual meeting of the American Association for the Advancement of Science in Boston, February, 1993.

15. The story of Stanley is an actual experience reported orally at an IIT Scientific Research Ethics Sack Lunch meeting. This narrative was drawn from a written report of that experience.

16. This is an adaptation of a case from the Whitehead Institute at MIT.

17. This story is part of a case in a collection drawn from actual incidents and compiled by C. K. Gunnsalus of the University of Illinois.