Honor in Science

Sigma Xi, The Scientific Research Society

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Research Triangle Park, North Carolina
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Preface

This booklet is intended as practical advice to those entering careers in scientific research. As an honor society of scientists, Sigma Xi recognizes that integrity in scientific research is vital to the advancement of knowledge and to the maintenance of public confidence in science.

In preparing this booklet, I have been assisted by many people, including several of the Society's officers and members. Discussions and publications arranged by other bodies such as the American Association for the Advancement of Science have also been helpful. It will be evident that the views expressed are my own; I hope that the general thrust of the booklet will be endorsed by other Sigma Xi members and scientists in general, but there is room for disagreement on detail and on priorities. Integrity in scientific research is very much a matter for individual conscience and commitment. I hope that future editions may be improved by comments from readers, especially from younger scientists to whom it is primarily directed.

C. Ian Jackson
Executive Director
1981–1987

1

Why honesty matters

> The reason I stop at a traffic light is not because I have a commitment to social justice, but because there may be a cop at the light and if I don't he'll nail me.[1]

This remark was made by the president of a major hospital, during a discussion of whistleblowing by scientists. It is a good place to begin, if only because there are several reasons why we stop at traffic lights:

(a) because obeying traffic lights is an effective solution to the problem of how to cross busy intersections;

(b) because the cop may be there to nail us if we do not stop;

(c) because we may get killed or injured by someone who is (legitimately) crossing the intersection in the other direction. Or we may kill or injure others, including pedestrians.

For most of us, the risk of getting caught may not be the main reason that we do not run red lights, nor is it the primary incentive that keeps us honest in our scientific research.

But how much has honesty in science got to do with such mundane matters as traffic lights? Are we comparing apples and oranges? Since the hospital president used the traffic light analogy in a discussion of integrity in science, he probably takes the view that the principles guiding a scientist in research are not significantly different from those affecting behavior in other facets of life. That is the position taken in this booklet, but it is not a universally-held view. For example, some would argue that science requires higher standards of ethical behavior than can be expected in the world at large. Others prefer to believe that the nature of science is such that ethical questions are less important than in the rest of life: how we deal with traffic lights, or with our friends and enemies, involves moral judgments and ethical standards, but the structure of DNA and the origin of submarine canyons are not affected by the character of the scientists who study them.

The latter view misses the point. Scientific problems such as the structure of DNA or the origin of submarine canyons are investigated by scientists, who may be all-too-human in their capacity to make mistakes, to miss or misinterpret critical pieces of evidence and, on occasion, deliberately to fake research results. Science may be morally neutral, but so is a traffic light; car drivers and scientists are not.

This does not mean that mistakes and omissions are frequent in science, still less that fraud and dishonesty are commonplace. Most of us follow the rules most of the time, in our daily lives as in our scientific activities. We make occasional scientific mistakes, and on deserted streets at four in the morning we may occasionally be tempted to run a red light. But accuracy and responsible behavior are much more common than their opposites.

There are, nevertheless, many scientists who believe that to stress the fact that scientists are fallible human beings does imply that mistakes, omissions and unethical behavior are common in science. They feel that this is not merely bad for the image of science but is simply not true. Few of them, probably, believe that research scientists can somehow avoid the temptations and frailties that affect humanity in general, but they would argue that the scientific method has, over the centuries, come to incorporate so many checks and balances that the mistakes and misinterpretations which do occur are inevitably detected and corrected. Scientists may be fallible, but science is self-correcting.

Such contrasting attitudes are evident in the responses of different scientists to the instances of scientific fraud that have been exposed from time to time. To many people, such spectacular cases are probably the visible tip of an iceberg of unknown but substantial dimensions. However, to those who believe that the scientific method is effective in identifying mistakes and fraud, such exposures are proof that the system is working as it should. Deliberate dishonesty is rare and quickly recognized; accidental errors are similarly corrected by subsequent research, and "there is no iceberg."

For the purposes of this booklet it is not necessary to resolve this question here. In the last analysis, there is no means of knowing how much scientific research is inaccurate or fraudulent. Intuitively, it may be wise to assume that the iceberg is rather larger than some would like to think. Error and even unethical behavior may not be much less prevalent in science than in other aspects of human life, and detection of error may not be inevitable. Most of the best-known exposures of fraud have tended to be in areas of scientific research where there is vigorous activity — cancer research, for example — and where replication of experiments and critical reviews of earlier work are therefore more likely to happen. Most scientists work in fields where there is much less interest or competition; and the specialized character of most research is such that it may be a very long time before your errors are noticed.

Before going further, a word is necessary about the distinction between fraud and error. We all make mistakes from time to time, despite our best efforts to be accurate. In our daily lives, for example, practically all of us have driven through a red light unintentionally, simply because we did not see it. Surely this is very different from deliberate fraud or law-breaking? Is this booklet concerned only with the latter, or with *both* fraud *and* errors?

> The only ethical principle which has made science possible is that the truth shall be told all the time. If we do not penalise false statements made in error, we open up the way, don't you see, for false statements by intention. And of course a false statement of fact, made deliberately, is the most serious crime a scientist can commit.

C. P. Snow, The Search, *Charles Scribner's Sons, New York, revised edition, 1959*

Mainly, of course, it is concerned with unethical behavior, rather than honest mistakes, but the distinction between them is not a simple one. As the cop who stops you is liable to point out, you are as likely to be involved in an accident if you did not see the red light as if you deliberately decided to ignore it, and the scientific paper that includes an accidental error may be as unreliable as one that is based on deliberate fraud.

It is not sufficient for the scientist to admit that all human activity, including research, is liable to involve errors; he or she has a moral obligation to minimize the possibility of error by checking and rechecking the validity of the data and the conclusions that are drawn from the data.

Some would go further and argue that mistakes should be punished as severely as outright fraud, if only because it may be impossible for anyone but the scientist involved to know whether the error was accidental or deliberate. Not seeing the red light is no defense.

Some scientists may agree that carelessness deserves to be punished, but believe that to be equally severe on all types of error is to ignore one of the most important characteristics of science: that it is very difficult to know what is truth and what is not. Much research takes the form of questioning previous assumptions or "facts," and the results often show that these assumptions are invalid or are limited to certain situations. If, as Popper has suggested, we can only disprove theories, never prove them, surely science is full of uncertainties? If this is so, is it reasonable that scientists should be blamed for unintentional error?

It is, of course, precisely because of these uncertainties that accuracy in research and in reporting research results becomes so important. The attempt to draw general conclusions from limited data is basic to science: we cannot put every specimen under the microscope nor can we put major weather systems into a test-tube. If subsequent work, by ourselves or others, shows that our conclusions are not so general as we had hoped, that is no discredit, provided that the conclusions were not inherently unlikely and that the data on which they were based had been obtained and reported accurately. If our original investigation was flawed, however, that is another matter.

One objection to this booklet may be that it is likely to be read only by those who have no need of the advice it contains: those who are honest and accurate by nature and whose scientific research will be therefore reliable. Those who are unscrupulous are unlikely to be deterred by anything short of discovery and punishment.

Certainly, neither codes of behavior nor statements of principles can prevent unethical behavior. They may even be endorsed enthusiastically by individuals who ignore them in practice, if only because many people are capable of rationalizing their own actions as justifiable exceptions. "Of course there needs to be a red light at that intersection, but in this particular situation, I was not in danger of harming myself or anyone else." Galileo was reputed to be better at devising scientific truths in his mind than performing the tedious experiments that verified them, and since his time there have been many scientists with less ability who have followed his example.

But such statements of principle need not be useless, either. When the Founding Fathers of the American Republic held "these truths to be self-evident," they did not mean that there was no point in including the truths in the Declaration of Independence, only that the statements did not need to be argued or proved. This booklet is written for those who are honest and responsible; it is intended to give them practical advice, as well as reassurance that ethical issues are of vital importance.

Another type of objection is that advice on scientific research ethics ought to be unnecessary, simply because science is *not* different from the rest of human life. There may be rules of behavior to be learned to meet specific situations (e.g. "always quote exactly, even if you spot a misprint or an apparent minor error in the passage you are quoting"), but the basic principles are a matter of human experience and individual conscience.

This may be true, but there are also many situations where ethical issues are not clear-cut, and may not even be perceived by everyone. The following problems have not received much attention — or solution — since they were stated twenty years ago, yet they affect many scientists. Note that the author is not concerned with individuals who misuse their positions, but with how the position is liable to subvert the individual.

> What is most alarming about the workings of the referee system is not the occasional overt lapse of honesty on the part of some referee who suppresses prompt publication of a rival's work while he harvests the fruit by quickly repeating it — perhaps even extending it — and rushing into publication with his own account. What is far more dangerous, I believe, because it is far more insidious and widespread, is the inevitable subconscious germination in the mind of any referee of the ideas he has obtained from the unpublished work of another person. If we are frank with ourselves, none of us can really state where most of the seminal ideas

that lead us to a particular theory or line of investigation have been derived . . . .

What has been said about referees applies with even greater force to the scientists who sit on panels that judge the merit of research proposals made to government agencies or to foundations. The amount of confidential information directly applicable to a man's own line of work in the course of several years staggers the imagination . . . . This information consists not only of reports of what has been done in the recent past, but of what is still unpublished. It includes also the plans and protocols of work still to be performed, the truly germinal ideas that may occupy a scientist for years to come . . . . One simply cannot any longer distinguish between what one properly knows, on the basis of published scientific information, and what one has gleaned from privileged documents. The end of this road is self-deception on the one hand, or conscious deception on the other, since in time scientists who must make research proposals learn that it is better not to reveal what they really intend to do, or to set down in plain language their choicest formulations of experimental planning, but instead write up as the program of their future work what they have in fact already performed. Again, the integrity of science is seriously compromised.[7]

If it is likely to be several years before you are invited to act as a referee or as a research award panel member, think instead about the situation that frequently arises in which you intend to publish a paper jointly with an author from another discipline. Say that the paper is in mathematical biology and that you as a biologist have worked with a mathematician. You have done your work conscientiously, and you believe that your colleague is equally reliable, but you do not have the necessary knowledge to verify that the mathematical analysis is fair and accurate. Nor does the mathematician know much biology. Are your respective responsibilities for the paper limited to your specific contributions, so that it is the job of the journal editor, referees and, ultimately, the readers to assess the validity of the paper as a whole? Many would say so, would behave that way in their professional scientific careers, and would have no doubt that they have been honorable and responsible scientists. Others would say that if you cannot understand every word and symbol in a paper of which you are the coauthor, it is your responsibility to have those sections read critically by someone who is not an author, and that whether you do this or not, you remain responsible for the entire paper, as do all the other coauthors.

Why does it all matter so much? Science may build on what others have already discovered, but surely an inaccurate or even forged piece of research can only delay other work: it will eventually be recognized as spurious, and science itself has not been harmed? Similarly, if I do my research and "shade" my experimental results just a trifle towards the result that seems to be obvious and logical, who suffers? Even if we agree that shading or carelessness are wrong, are they any worse than the similar lapses that we observe continually in other aspects of life? After all, most of us are irritated by the car that runs a red light but few of us are inclined to take the license number and report it to the police, unless some child or elderly person was endangered by the incident.

There are many valid answers to such questions. First, however, it should be said that there are few situations, if any, in which there is no "victim." In some situations — medical research, for example — the victims may be very obvious: those who remained ill or died because fraud or carelessness diverted research away from the problems that should have been investigated. In any, field, however, fraudulent or careless research is likely to benefit the perpetrator at the expense of others. Take, as an extreme case, the example of the "scientist" whose extensive list of publications consisted almost entirely of articles by others that he copied word-for-word from obscure biomedical journals and then published in his own name in other obscure journals.[3] It could be argued that the original authors had gained the credit due to them when the articles were first published, and that scientific knowledge benefited through the wider dissemination of these research reports in other journals. Who suffered? The answer should be obvious: those scientists who did not get the academic appointments that the plagiarist obtained on the strength of his spurious list of publications. This example is an extension of the situation in which someone obtains a job by claiming a degree or other qualifications that he or she does not possess: it is unfair both on those who do not have the qualifications and are honest about it, and on those who earned those qualifications the hard and honorable way.

More fundamentally, however, scientific honesty is vital because there is no cop at the scientific research traffic light. Nor can there be, for scientific accuracy and honesty cannot normally be reduced to something as simple as whether the light was red or green. The referee of a scientific journal, for example, is not a cop and should not be expected to determine whether a research report has been honestly produced. A referee is appointed to advise whether the results that are reported are sufficiently important to merit publication. Some errors are detected by referees, and others by readers, but neither referee nor reader can verify the critical elements of much scientific research except by doing the work over again.

It is because we cannot police scientific research as we do our highway intersections that thesis-writing is such a fundamental part of the work required for Ph.D. and other research degrees. Those of us who have been through the experience, even if it was many years ago, can usually recall that frustrating and time-consuming period, after the research was done and the thesis had been drafted, when we had to go back and check on the accuracy of quotations, page references and other details, so that the thesis could not be faulted or sent back on such grounds. The university was saying, in effect, "We put you through these hoops at this time with everyone watching your performance very carefully, because in the research that you are likely to do in the future we and other scientists need to be able to trust you to jump through the same hoops without being watched." Graduate school is also the place to learn that one does not publish research results and conclusions until one is certain of their accuracy and that this is why it is necessary to define one's problem sufficiently narrowly that one can gain the comprehensive knowledge and understanding that are essential. Inevitably, therefore, individual scientists tend to become fairly narrow specialists. Yet the progress of science as a whole depends on communication and integration of these individual specialized results: the loneliness of the individual scientist exists simultaneously with interdependence among all scientists. In Bronowski's words:

> All this knowledge, all our knowledge, has been built up communally; there would be no astrophysics, there would be no history, there would not even be language, if man were a solitary animal. What follows? It follows that we must be able to rely on other people; we must be able to trust their word. That is, it follows that there is a principle which binds society together, because without it the individual would be helpless to tell the truth from the false. This principle is truthfulness.[4]

2

"Everybody does it"

A few years ago, a liberal arts college in the Midwest, that has a high reputation and attracts well-qualified and dedicated students, circulated a confidential questionnaire among its freshmen. The object was to discover the extent to which those freshmen had recognized examination cheating and similar forms of academic dishonesty in their pre-college years, and the extent to which they themselves had been part of such activity. Since confidentiality was guaranteed, and the objective was fact-finding, rather than censure or punishment, the response was large and the results were unmistakable. As many as 48 percent of the group had been dishonest in their studies before reaching college, e.g. by copying or cheating in examinations. Virtually everyone had observed others cheating, but had done nothing about it.

Not merely can it seem that "everybody does it," it may also appear that fraudulent behavior at the pre-college and college levels is not discouraged as actively as it used to be, and is not adequately penalized when it is discovered. The "honor system" has been around for a long time, but it seems to have changed its character over the years. In the past the rules were clear, if arbitrary, and though the honor system implied that students should not be continually policed into obeying them, those who were discovered breaking the rules were punished swiftly and sometimes severely. Nowadays there is often less emphasis on enforcement, with the implicit (and sometimes explicit) belief that those who act irresponsibly or even dishonestly will suffer most in the long run.

Encouraging individuals to work out for themselves the principles of reasonable behavior is one thing; to expect that everyone will follow such principles is quite another. Despite the results of the college survey just quoted, most students, like most people, are probably honest most of the time. What matters is how we behave in these moments when the task is proving more tedious or complicated than we expected, when the reasons for those difficulties appear to be as trivial as they are persistent, when so much hangs on the result, and when nobody is watching. Such pressures are encountered well before we reach college; in the absence of firm pressures towards honesty and responsible behavior the incentives towards dishonesty are usually intensified at the undergraduate stage, especially if this is itself seen as a stepping stone to

Letter to Sigma Xi from Ralph Mason Dreger, Baton Rouge, La., 5 January 1984

I have a wry view of the signs one sees on the highway, "Keep right except to pass," for I realize that many people do just that in life; they do not succumb to temptation to be dishonest as long as there is no temptation.

greater things. If, for example, your objective is medical school after college, and you are one of a pre-med class numbering 125 of whom, on previous averages, only about four or five are likely to be accepted by a medical school, the temptation to cheat, and even to damage the chances of others, may be very intense. The undergraduate degree becomes less a basic pre-medical education than a fierce competition in which winning is the only thing, and if some corners are cut, and the brighter students find their experiments — and their grades — spoiled by a little sugar added to their test tubes while their backs were turned, well "everybody does it."

Everybody does not do it, and even if most did this would be no justification. Nevertheless, cheating and other unscrupulous behavior are probably more common than most of us would like to admit. Despite their natural reluctance, teachers of school and university have to accept that they should do more to encourage and to enforce honesty than many of them have been willing to do in recent years. To develop an honor code, for example, without providing for its interpretation, implementation and enforcement is like installing traffic lights and expecting other drivers to call in — or even to punish — those who ignore the lights: it won't happen. Nor is it adequate to say that, in the long run, the person who cheats is the one who suffers most. This is doubtful in fact (and there are certainly no adequate data to support such a contention); in any case those who behave honorably should be protected. If you are excluded from medical school because an unscrupulous person deliberately damaged your experiments and affected your grades, it is little consolation to think that he or she will ultimately have a guilty conscience.

What is needed is not a return to the old authoritarianism, but rather a greater readiness on the part of everyone — students and teachers — to assert and defend the principles of honest behavior. It is only when there is a reluctance to assert such principles that the unscrupulous are encouraged to take chances and to claim that "everybody does it."

At the risk of sounding callous, it also needs to be said that one should beware of excuses based on "extenuating circumstances." At all levels from grade school to advanced postgraduate research, there is often heard the excuse that careless or even dishonest work was caused by exceptional pressure on the individual concerned. "I had too much to do, too little time to do it in, and was greatly fatigued mentally and almost childlike emotionally. I had not taken a vacation, sick day, or even a day off from work for six years."* Such individuals may be entitled to sympathy, but not to exoneration. Special pleading, whether based on fatigue, family difficulties or other factors, is only special pleading. Many other scientists work under similar pressures, experience similar temptations to fraud, and remain honest. They deserve protection rather more than the unscrupulous deserve sympathy.

*Darsee to Braunwald, December 1981, quoted in *Harvard Magazine*, July-August 1983. The effect of this special pleading is rather diminished as Darsee continues: "I had put myself on a track that I hoped would allow me to have a wonderful academic job and I knew I had to work very hard for it."

3

"Trimming, Cooking, and Forging"

Charles Babbage (1792-1871) is generally remembered as the prophet of the electronic computer, because of his "difference engine" and the uncompleted "analytical engine." But he had a much more extensive influence on scientific development. As professor of mathematics at Cambridge University, he published a book entitled *Reflections on the Decline of Science in England.*[5] Since the year was 1830, the same year that Charles Lyell began to publish his *Principles of Geology* and shortly before Charles Darwin set sail on the "Beagle," the title may seem as premature as his calculating devices. Babbage's book, however, is generally given credit as a catalyst in the creation of the British Association for the Advancement of Science, and indirectly of similar associations in the U.S.A., Australia and elsewhere.

Babbage, the "irascible genius," was also concerned with how science should be done, and the same book describes the forms of scientific dishonesty that give this chapter its title. The definitions used here are phrased in contemporary English; otherwise not much seems to have changed in 150 years.[6]

Trimming: the smoothing of irregularities to make the data look extremely accurate and precise.

Cooking: retaining only those results that fit the theory and discarding others.

Forging: inventing some or all of the research data that are reported, and even reporting experiments to obtain those data that were never performed.

All of us, whatever our scientific specialization, can recognize situations in which trimming would be very easy. The temptation to trim certainly arises long before we begin real research: it appears in the earliest experiments in a school science class. For example, at the same time as we begin to understand the concept of specific heat, we learn that our laboratory equipment is inadequate to give us exact measurements. We learn very early that physical truths often tend to fit smooth curves, but our experimental curves are by no means smooth. Both teacher and student know that a high level of accuracy is usually impossible in the conditions of a high school lab, yet both teacher and student want to get as close an approximation as possible. Is it surprising, therefore, that the teacher may be inclined to give the highest marks to the individual or group with the smoothest curve?

It is usually also at this elementary stage that the unscrupulous student learns that trimming can be overdone. Since perfection is inherently impossible, a perfect result is immediately suspect.

This chapter, a summary of the various types of unacceptable behavior that may occur in scientific research, is relatively short. The reason is simple. Although the individual circumstances are always unique and may be extremely involved, practically all of them involve carelessness, the three forms of dishonesty described by Babbage, or one other: plagiarism.

Trimming, for instance, is a temptation that extends well beyond high school physics experiments. In the investigation of what appeared to be mainly a case of plagiarism at Yale University in the late 1970s, an audit of the research involved was conducted by an external expert, Jeffrey S. Flier. It soon became evident that more than plagiarism was involved:

> In hindsight, Flier realized that he and his colleagues had wondered at the beauty of Soman's published data. Yet even though Flier and friends had not been able to achieve such clean results, they never suspected that the elegant data were the result of deliberate fraud.[7]

The answer, as the culprit admitted a week later, was that the data had been trimmed. "I smoothed out the data. I took the curves and smoothed them out."[8]

One of the best-known cases of cooking is that of the physicist Robert A. Millikan. In the second decade of this century a vigorous controversy, with strong personalities and conflicting experimental results, erupted between Millikan and the Viennese physicist Felix Ehrenhaft. After several years of inconclusive experiments, including disagreement on basic physical principles as well as experimental accuracy, Millikan published in 1913 a major paper on the electronic charge, based on a series of experiments on liquid droplets. This paper contained the explicit statement that "this is not a selected group of drops but represents all of the drops experimented upon during 60 consecutive days."[9] Ten years later, in 1923, he received the Nobel prize, partly for his work on the electronic charge. More than half a century later, however, an investigation of Millikan's own laboratory notebooks showed that

> The 58 observations presented in his 1913 article were in fact selected from a total of 140. Even if observations are counted only after February 13, 1912, the date that the first published observation was taken, there are still 49 drops that had been excluded.[10]

In his review of these notebooks, Holton points out that Millikan's research "would ultimately lead to results of great importance not only in physics but also in chemistry, astronomy and engineering,"[11] whereas his opponent's results "would give rise to nothing useful at all."[12]

Nor does Holton criticize Millikan's selectivity for its own sake. There were several reasons, related to equipment limitations and similar factors, to justify rejection of anomalous values, and "Millikan had quite enough observational material left — 58 drops out of about 140 — to make a sound case."[13] What cannot be justified, however, is the statement that the published data were based on all the observations, not on a selected number. That is cooking the data.

Apart from its use as an example of "cooking," the Millikan case is an embarrassment to those who would like to assume that mistakes or fraud are quickly discovered or that the chief sufferer from scientific dishonesty is the fraud himself. Millikan lived a successful life as a physics professor and then as Chairman of California Institute of Technology. "At the height of his career he was perhaps the most renowned and influential scientist in the United States: physicist, administrator, educator and policymaker."[14] He died in 1953, well before the re-examination of the 1913 data by Holton and Franklin. Other scientists may have had their doubts before then, but this did not materially affect Millikan's career or reputation.

As an example of forgery, there are too many to choose from. Some are as deliberate as the examples of trimming and cooking; others appear to have been largely self-deception. Some have to be classified as deliberate hoaxes, since it seems improbable that those whose scientific reputations were built on such "discoveries" could have been the ones who planted the false evidence.

What is more important than the details of such frauds is that they are so similar to the types of fraud that are possible at any stage of scientific training or research. Often, unfortunately, the frauds might have been exposed much more quickly if other scientists had maintained a healthy skepticism rather than been very willing to believe, or if those who were skeptical had done something about it. Sometimes the signs of possible fraud are very clear, as when Burt's reports on inherited intelligence mentioned three different sample sizes — 21 in 1955, over 30 in 1958, and 66 in 1966, yet key correlation coefficients remained the same (to three decimal places!) in all three sample sizes.[15] Or, as admitted by a Nobel prizewinner when shown a living rabbit that had allegedly received corneal grafts in both eyes:

> I could not believe that this rabbit had received a graft of any kind, not so much because of the perfect transparency of the cornea as because the pattern of blood vessels in the ring around the cornea was in no way disturbed. Nevertheless, I simply lacked the moral courage to say at the time that I thought we were the victims of a hoax or confidence trick.[16]

The difficulty with this line of argument, however, is that too much skepticism can be self-defeating, and even evidence that the skepticism is justified may fail to convince. One of the fascinating aspects of a recent book on scientific fraud — *Betrayers of the Truth* — is that the authors contradict themselves on this point within a few pages. Introducing the "kinase cascade" cancer research fraud at Cornell University in the early 1980s, Broad and Wade claim that "A single attempt at replication would have stopped the fraud dead in its tracks."[17] Seven pages later they change their minds, if not their metaphor:

> Why did none of the many biologists caught up in his theory try first to replicate some of the basic results? The answer is: they did. Their failure to get the same answers as Spector should have stopped the theory dead in its tracks. It didn't.[18]

One understandable reason why these replication attempts did not lead directly to exposure is that replication is seldom easy; maybe the replicators make mistakes. Equally understandable is the paradox that skepticism and trust have to be simultaneous in research since, quoting Bronowski again, "we must be able to rely on other people; we must be able to trust their word."[19] To the extent that science is competitive, attempts to investigate possible fraud can easily be made to appear as jealousy rather than as honest doubt.

Although smoothing experimental data without saying so may seem more forgivable than reporting experiments that were never undertaken, such distinctions are irrelevant to the basic need for honesty in scientific research. The scientist who yields to the temptation to smooth the data in one minor respect and is not found out may well be inclined to do so again, until it becomes a habit.

Rationalization of the action to oneself is not far behind: "Look, I may be working in much better conditions than I was at school, but this lab isn't perfect either. The odds have to be at least nine to one that these irregularities are due to uncontrollable factors that have nothing to do with the problem. It therefore makes sense to get rid of them in the published research report, so that the reader doesn't get confused." Get used to that type of rationalization and it becomes relatively easy to convince oneself in another research report that some entire samples must have been contaminated, and therefore do not need to be mentioned as divergent cases.

Whether or not you agree that trimming and cooking are likely to lead on to downright forgery, there is little to support the argument that trimming and cooking are less reprehensible and more forgivable. Whatever the rationalization is, in the last analysis one can no more be a little bit dishonest than one can be a little bit pregnant. Commit any of these three sins and your scientific research career is in jeopardy and deserves to be.

Plagiarism is equally dishonest. It is perhaps unfortunate that many scientists tend to be more concerned about plagiarism than other forms of fraud. This is not because plagiarism is worse, nor even because such scientists believe it to be. Straightforward plagiarism is, however, more easily proved. To be sure that someone trimmed or cooked results, or even faked them completely, is usually a time-consuming and very messy task that scientists would prefer to avoid. If, however, you are clearly quoting someone else's work as

Not all questions of professional ethics, honor and morality reside solely on the authors' side of the author/editor-referee interface. . . .

Editors have been guilty of the following actions against me:

- Failure to respond to inquiries.
- Excessive delay (i.e., more than 3 months) in review without explanation.
- Losing a manuscript without advising me.
- Deletion of significant portions of the manuscript without coordination.
- Abetting a reviewer in obtaining secondary citations.
- Making concurrent publication without authority.

Reviewers (referees) have been guilty of the following actions against me:

- Obtaining a secondary citation for his own publication in the guise of "improving" the manuscript.
- Pirating the topic of my manuscript for an article of his own.
- Providing reversed criticism on a second review.
- Quibbling excessively over minor points.

Letter to Sigma Xi from Robert Irving, Northridge, California, 6 January 1984

your own, there is much less investigation to be done.

Plagiarism, however, can take many forms. Here is part of a "Definition of Plagiarism" by Harold C. Martin, Richard M. Ohmann, and James H. Wheatly, contained in Wesleyan University's *Blue Book.*

> The spectrum is a wide one. At one end there is a word-for-word copying of another's writing without enclosing the copied passage in quotation marks and identifying it in a footnote, both of which are necessary . . . . It hardly seems possible that anyone of college age or more could do that without clear intent to deceive. At the other end there is the almost casual slipping in of a particularly apt term which one has come across in reading and which so admirably expresses one's opinion that one is tempted to make it personal property. Between these poles there are degrees and degrees, but they may be roughly placed in two groups. Close to outright and blatant deceit — but more the result, perhaps, of laziness than of bad intent — is the patching together of random jottings made in the course of reading, generally without careful identification of their source, and then woven into the text, so that the result is a mosaic of other people's ideas and words, the writer's sole contribution being the cement to hold the pieces together. Indicative of more effort and, for that reason, somewhat closer to honesty, though still dishonest, is the paraphrase, an abbreviated (and often skillfully prepared) restatement of someone else's analysis or conclusions without acknowledgement that another person's text has been the basis for the recapitulation.[20]

The spectrum of plagiarism in science is even broader than Martin *et al* suggest. It includes the use of knowledge picked up — as described in the quotation in Chapter 1 — when acting as a journal referee or as a grant award advisor. It includes the theft of a research idea mentioned by a colleague who had no idea that you would appropriate the thought and who is not given credit by you for having originated it. It includes the credit taken by heads of laboratories or others in authority for research work in which they had no real part, through their insistence that they be included as coauthors of the research reports.

It is indeed regrettable that the willingness of many scientists to act against plagiarism is often limited to instances of flagrant copying where, in the above definition, it is difficult to believe that there is no "clear intent to deceive." Proving the subtler forms of plagiarism is as difficult and messy as with other forms of scientific fraud. Stealing words is more obvious a sin than stealing someone's ideas. Unfortunately, it is usually also less important.

Plagiarism, unlike some other dishonest practices, may affect an individual scientist both actively and passively. You may steal someone else's ideas or words, or someone may steal your own. Chapter 4 is primarily concerned with what you are entitled to expect as reasonable behavior from your scientific colleagues, including those for whom you work or who provide supervision of graduate study.

Avoiding plagiarism and other dishonest behavior is normally straightforward enough. Most of us know when we are tempted to trim, cook, fake or steal, and we resist — or yield — consciously. If we yield, we may attempt to rationalize our actions, but if we are honest with ourselves, we can usually recognize rationalization for what it is: a defense of the indefensible.

But how do we avoid the more subtle forms of plagiarism: the almost unconscious "borrowing" of an idea that is properly someone else's, especially when our use of the idea may be weeks or months after it was heard or read? The best answer is to attempt to develop the habit of detached criticism of one's own work that is one of the most difficult and yet most essential characteristics of a true scholar. Oliver Cromwell's famous request to the General Assembly of the Church of Scotland: "I beseech you, . . . think it possible you may be mistaken" ought to be a constant reminder to every research scientist. It is up to you, before you ask for advice from others, to analyze your research as critically as you know how, to ensure that it is accurate and that it gives appropriate recognition to anyone who has helped you or on whose own research you have drawn. "Appropriate recognition" means what it says. We are not ordinarily required to acknowledge in print the services provided by our typists, lab assistants or equipment suppliers, but those whose careers and reputations depend, like our own, on intellectual qualities and scientific ability deserve recognition. Not to give it is dishonest.

One area where carelessness or dishonesty is particularly likely to occur is in the misuse of statistical techniques. No scientist can avoid the use of such techniques, and all scientists have an obligation to be aware of the limitations of the techniques they use, just as they are expected to know how to protect sam-

I believe that way too often, invalid research does result from misuse of computers through neglect, incompetence and sometimes even fraud. The application of computers does provide an easy opportunity to excuse error by blaming the computer. It also offers the temptation to compromise experimental design or to avoid developing proper research plans in order to take advantage of some readily available software. . . . The trend away from using software produced by researchers for their own use means that researchers cannot be as certain of the accuracy and correctness of their computations. Also, they cannot as easily modify the software they use. They sometimes tend to feel that they are not as accountable for error in supplied software. . . .

. . . also, I am sad to say, there are situations where the researcher relies upon computer results without even understanding what it is the computer is supposed to be computing. . . . Associated hypotheses and assumptions may not be understood at all, but the computations proceed, results are printed out, some kind of inference is made, graduate degrees are granted, and publications follow. . . . I am convinced that a lot of nonsense and unfounded claims are made under the guise of research which has been based upon misunderstood computation. . . .

. . . our best researchers know better than to trust "others' software". They are skeptical of it, and they insist that they create their own software themselves. . . .

. . . when using computing equipment, researchers certainly should personally be able to carry out in concept the computations that the computer produces or be working with a colleague who can. They should really understand the algorithms they use. It would be healthy for all researchers to develop automatic skepticism that a software package actually performs the algorithms as the researcher understands them. They should really understand what computations are carried out inside the machines that do their work.

Letter to Sigma Xi from Gordon R. Sherman, Knoxville, Tenn., 28 February 1984 ples from contamination or to recognize inadequacies in their equipment.

Some of this may be self-evident: we should not use an average if the median or mode is more appropriate to the data being analyzed; everyone ought to be aware of the perils involved in extrapolation. But nowadays the task of calculating statistical indices has become so routine and swift that many scientists are liable to use improper techniques without being aware of it. Our desk computer or our programmable calculator will give us a "solution," however inadequate or non-homogenous are the data that we feed into it. In statistics, more than in some other fields, "a little learning is a dangerous thing." We cannot all become expert in statistics as well as in our field of specialization. If we develop that faculty for self-criticism, however, we can learn to recognize when the significance of a piece of research appears to depend primarily on a particular statistical test, and we can seek confirmation that the result is a valid one.

4

Research as a cooperative activity

At a discussion on honesty in science that took place during a Sigma Xi Annual Meeting, one delegate told how, as a graduate or postdoctoral student, he had been working with others in a laboratory with a leading scientist at its head. A new postdoctoral student joined the group. When she arrived, she was told not merely what topic she should work on but also what results she was expected to get. She set to work, did her research conscientiously, and arrived at quite different conclusions from those that had been expected. As the delegate told it, "Her work was never published; she left the lab within a month . . . and the rest of us drew the obvious conclusions."

It is difficult to think of any situation farther from what scientific research ought to be about, yet there are many other "horror stories" of a similar kind. However honest and conscientious one's own approach to scientific research, others may not be, and their actions can have a damaging, even permanent, effect on your research, career in science, and perhaps life as a whole.

To admit this is not to suggest that we should go through life, or even through graduate and postdoctoral research, with a general suspicion of our colleagues and supervisors. The process of becoming a research scientist, by studying for a research degree and following this with postdoctoral research working with others on the fringes of knowledge, ought to be one of the most stimulating, satisfying and rewarding periods of one's life. Most of us will never forget that experience, nor the help and friendship that we received from our research advisors and from those who had similar hopes for the future as ourselves. To accept that there are some bad apples in every crop should not lead us to avoid eating fruit.

Bad apples there are, however, and bad research situations as well. If you find yourself in an unsatisfactory situation that seems beyond your capacity to change, you may be faced with little alternative but to leave and look for better things elsewhere.

Before that step, what can you reasonably hope to find among those with whom you work? If there is one phrase that sums it up, it is probably *esprit de corps,* defined in Webster as "the common spirit existing in the members of a group and inspiring enthusiasm, devotion, and strong regard for the honor of the group."[21] That may seem to be aiming rather high, but it is very evident in the best graduate schools and in many other places as well. Perhaps you wanted to come to this department in this university because it had a reputation for outstanding work. You knew the scientific reputations of the leading figures associated with the research done here, and you wanted to be part of it. Or, another familiar situation: here we are, just a few of us, at what some might regard as an obscure university away from the mainstream of research in my discipline. But there is at least one professor here who knows how to bring out the best in me and the other graduate students, and you can see it happening. It might have happened also at that major research university I could have gone to, but then again it might not. We are a small group that can help one another, and I wouldn't want to be anywhere else right now.

Esprit de corps is therefore not dependent on the size or prestige of the institution, though these can help. Nor can it be imposed, though the right sort of leadership for the group is crucial. The spirit — *esprit* — is recognizable in many different research settings. It may in fact be more normal than elusive: scientific research — the discovery of what was previously unknown — is inherently exciting compared to many other activities.

What is apt to destroy or prevent such a general feeling within a group of research scientists is the canker of excessive competition. Competition is part of research and part of the excitement: at any stage in the development of a science there are several problems that seem ripe for solution and it is good to feel that our group may make a significant contribution. At the individual level, competition is inevitable and healthy: "If she can put in those extra hours in the lab in order to get her thesis finished by October, maybe I should be doing the same."

Competition is one thing, but excessive competition between research groups or among individuals within a group is something else. Worse still is when one group or individual steals an advantage by . . . stealing. You are entitled to expect that research data you have collected will be used by you alone, unless you have explicitly agreed to collect the data for someone else, or unless you specifically give someone else the right to use it, in which cases you are entitled to appropriate credit. If you exchange ideas with other scientists, including other students, it should be possible to distinguish between such different actions as the sharing of ideas that can be followed up by anyone, the offering of advice, and discussion of your current work and what you are planning to do next. Stealing research ideas does happen; when it does, the thief is apt to claim that it was not clear that this was something you were actively working on yourself or that it was a definite part of your future research program. Maybe such an assumption was understandable; if it was not, there is probably not much that can be done, except to make known to others that they should be careful in sharing their ideas with the individual or group concerned. But do not assume from the outset that theft of ideas or data is likely to happen: that way lies a view of the world as a conspiracy, implying that one of the

The indifference of many senior people to what their junior colleagues do in the laboratory is more serious . . . .

During the rapid growth of the research enterprise in the past three decades, research institutions, universities especially, have slipped into the sloppy habit of substituting for their own judgement of their own achievements the judgement of external assessors as delivered by the appropriate sub-net of the peer-review system . . . .

. . . a research laboratory jealous of its reputation has to develop less formal, more intimate ways of forming a corporate judgement of the work its people do. The best laboratories and university departments are well-known for their searching mutual questioning.

Editorial in Nature, *303, 2 June 1983.*

greatest benefits of graduate and postdoctoral work — friendly cooperation with other students and senior colleagues — is impossible.

From those who guide your research or for whom you are working as a student or junior in the research lab or other setting, you also have some reasonable expectations. If these expectations are not fulfilled, there may be mechanisms within the university or research institute to improve matters — or at least provide a change of supervisor — but they may well be insufficient to deal with the situation adequately.

You are, for example, entitled to assume that a graduate advisor is there to assist his or her graduate students, not *vice versa*. How much assistance, formal or informal, you will actually get will vary from one advisor to the next, and you need to remember that a graduate advisor wears other hats as well. But it should be easy to recognize the "advisor" who either regards his graduate students as cheap labor for his own research or who expects unreasonable recompense from the student in return for the assistance that has been provided. One hears, for instance, of students whose graduate work is essentially completed, but who are held back from the next stage of their careers by the need to do more work in the supervisor's lab, so that the research reports can carry the supervisor's name as well as the student's. This is enforced by the need for a good recommendation from the supervisor before the student can move on. The letter of recommendation becomes an "exit visa." It is disgusting behavior on the part of the supervisor, but it occasionally happens.

The graduate student is also entitled to the same treatment in respect of written work from laboratory heads or supervisors that the latter would expect from journal editors and referees: the work should not be unduly delayed nor misappropriated. What to the student may seem undue delay may legitimately be seen by the supervisor as a refusal to accept work of an inadequate standard. The supervisor should be prepared to send back substandard work for as long as he or she is prepared to have the student remain in the department. However, if that work is taken and "improved" by the supervisor and published without the student's knowledge or permission, that is a different matter: call it plagiarism or plain theft.

More generally, and like it or not, any leader of research — head of a laboratory, graduate advisor or whoever — inevitably becomes a role model for those who are beginning scientific research. If such leaders act as though the quantity of papers published is more important than their quality; or if they demand their names on every paper published by the laboratory as some sort of "rent" owed to them for making the facilities available, then others are likely to acquire a view of scientific research which for the conscientious is dispiriting and debilitating and for some seems a license to be equally unscrupulous.

For the student, the best way to avoid such situations is not to get into them in the first place: go to some other graduate school, or change your advisor. Unfortunately this is much easier said than done. The traditional measures of a good graduate school — reputation of the faculty, facilities, published research — may tell you little about whether the school encourages and inspires its graduate students, or whether it uses them as cheap labor and leaves them with a negative view of the whole research enterprise. Nor is it easy to find out from those already there whether the atmosphere is one of *esprit de corps* or dog-eat-dog. If you find yourself in the latter situation, however, you will detect the symptoms very quickly. If you are in any doubt, some discreet sharing of experience with students in other departments should enable you to decide whether the problem is in your research environment or is in yourself. If the former, then you have probably only three alternatives:

(a) accept the situation and go along with it;
(b) decide that you are too committed to the degree program or the specific research to be able to move; endure the situation and leave as soon as you can;
(c) decide that the situation is unlivable and that, despite the difficulties, you have to go elsewhere.

If it is feasible, (c) is probably the best solution. There are, after all, far more departments and graduate schools where your introduction to scientific research can be exhilarating than those where the attitude is oppressive, unfair or dishonest. Find one.

5

"Things are very different in my field"

In the previous chapters it has been taken for granted that ethical behavior in one branch of scientific research is true of every other and, for that matter, that plagiarism and other forms of dishonesty in research and publication are as unacceptable in the humanities and social sciences as in the natural sciences.

That *is* the belief underlying this booklet and it is probably held by the vast majority of scientists. The principles governing the way that research is carried out and reported are the same in geography as in physics, in medicine as in archaeology. Anyone who argues otherwise invites a very critical hearing.

Nevertheless, habits and conventions do vary from one major field to another, sometimes for reasons that seem to be closely linked to the character of the research problems, at other times for no apparent reason other than "this is the way we tend to do things." Two problem areas are discussed in this chapter, because they are of considerable importance in current scientific research in North America: irresponsible authorship and alternative sets of values in biomedical research.

Irresponsible authorship

The tradition of publication in science is similar to that in other branches of knowledge. An individual scientist reports his or her findings and conclusions, whether in the Latin of Newton's *Principia,* in Darwin's easily read and easily misinterpreted *Origin of Species* or in the nine thousand words in the *Annalen de Physik* in 1905 by Einstein that "overturned man's accepted ideas of time and space." The literature of science does have a long tradition of cooperation among two or three authors, who come together because each can contribute specialized knowledge, or because research is often more exciting and rewarding if it is not done entirely alone. What is comparatively new is the practice, in some disciplines, of publishing research reports in which five or even fifty individual scientists claim "authorship" of the same paper. It is particularly evident in some forms of biomedical research, in high-energy physics and in some branches of geophysics, and is usually explained in terms of the complexity of the research, demanding that many skills are brought together in a carefully-planned program. If the research requires such cooperation, it is argued that those who contributed should be credited with authorship of the report.

What, it might be asked, has this to do with scientific honesty? How is multiple authorship related to our taxonomy of trimming, cooking, forging and plagiarism? Nothing in principle, perhaps, but it seems evident that multiple authorship increases the opportunity for each of these to occur, if only because the responsibilities of authorship are diffused or diminished when they are widely shared.

Irresponsible authorship, rather than multiple authorship, is in fact the real problem in such situations. In principle, it is possible for fifteen or fifty scientists to coauthor a single research report, using the term "author" in the full sense of that word. More usually, however, multiple authorship indicates a claim for credit rather than an acceptance of responsibility. Multiple authorship, in other words, can easily become irresponsible authorship simply because it tends to debase the notion of what authorship really means. Too often, someone is named as an author less because of the need to accord appropriate recognition than because a publication list is regarded as the index of a scientist's worth, and the more the better. How much the "author" actually contributed to the writing of the paper, or even to the actual research on which the paper is based, comes to matter less than the fact that the scientist is listed as an author, preferably as close to the head of the list as possible.

The end of this particular road, as suggested in the opening chapter, may be the Alsabti case in which a publication list was created largely by republishing, under Alsabti's name, scientific articles by others that had already appeared in other journals. Since Alsabti plagiarized alone, this may seem to be irrelevant to the notion of multiple authorship as irresponsible authorship, but there are several way-stations along the road. They include what Broad and Wade describe as "the gratuitous addition of coauthors by a researcher trying to curry favor."[22] For example, a former graduate student may send his supervisor an article several years later, based on research done long after the graduate studies have been completed, in which the supervisor is surprised — and should be outraged — to find that he is named as a coauthor. Or, as Broad and Wade report,

> An editor at one journal, *Blood,* received a call one day from an irate researcher who asked that his name be removed from a manuscript that he had just seen and with whose conclusions he did not agree. His sole contribution had been a few seconds of conversation with the lead author in an elevator.[23]

It may seem paradoxical that authors are multiplied in this way by scientists who are well aware — perhaps to the point of paranoia — that the length of one's own publication list may be measured against those of others. That paradox is neatly, and even more dishonestly, resolved by those scientists who agree with one another that each will add the other's name to a paper, with or without any contribution to the work, in the knowledge that the other will return the "favor."

The scientist who complained that a conversation in an elevator had been used as a pretext for listing him as an author presumably subscribed to the responsible view of authorship. Quite apart from his minute contribution, he did not agree with what the paper said and wanted no share of either credit or responsibility. That view ought to be both understandable and undeniable. As multiple authorship has proliferated, however, many have come to the comfortable belief that their appearance as authors does not indicate responsibilitiy for the paper as a whole, but only for their specific contribution to it. This attitude has gradually spread to cover even cases of limited coauthorship. As the editor of the *New England Journal of Medicine* commented after the withdrawal of a paper in which John Darsee had been one of only two authors, and another in which he was one of three,

> . . . the two formal retraction notices, as well as Darsee's supporting letter . . . seem to suggest that his coauthors at Emory had no responsibility at all for what happened, simply because they are honest and had no hand in the manipulation of the data. I cannot agree, and neither will most other editors.[24]

Other editors do share Relman's views, and are endeavoring to establish rules that would have seemed unnecessary a few decades ago. For example, the *Journal of Animal Science* adopted in 1984 a new policy in regard to submission of manuscripts:

> All authors regardless of whether senior or coauthor must provide a signed affidavit assuring that they have read the manuscript prior to submission and (or) are fully aware of its content and that no substantial portion of the research has been published or is being submitted for publication elsewhere.

In an attempt to clarify responsibilities in multiauthor papers, one Sigma Xi member recently suggested that articles in *American Scientist* (and presumably other journals as well) should include a brief section on "Attributions": "For example, in a paper by Smith, Jones and Brown, the Attributions section might read 'Smith took the data, Jones analyzed it, and Brown fed the animals.' " This would certainly help to identify the contributions made by multiple authors, and might be worthwhile for that alone. Whether the section would do much to solve the problems liable to arise from irresponsible authorship is more doubtful. It seems to define the limits of responsibility so narrowly that, in effect, Brown would be able to say "Don't blame me if there is anything wrong with the data"; Smith could argue that his data were accurate even if the use made of them was faulty; and Jones could claim that it was not his responsibility to verify that the data were collected under the right conditions.

Some of us may also be inclined to ask whether, if Brown's contribution to

How much responsibility do authors have for the accuracy of the clinical-laboratory data they describe?. . .

. . . they were not familiar enough with the technique to have been aware that their colleague had given them a factitious tracing . . . . When authors discuss and advocate the clinical use of a diagnostic procedure, and when they publish illustrations of its application in specific patients, I think they ought to know something about the procedure itself, not simply how to interpret the results. . . .

The lesson seems clear: Authors should be familiar with the laboratory tests they write about; otherwise, they risk embarrassing themselves and misinforming their readers. . . .

Arnold S. Relman, Editorial in The New England Journal of Medicine, *310, 16, 19 April 1984, pp.1048-1049.*

the research was limited to feeding the laboratory animals, that contribution really merits coauthorship. Occasionally multiple authorship is justified on the grounds that "I had to have my samples tested (or my animals fed) by so-and-so, and he wouldn't have done it if I had not been prepared to make him a coauthor of this paper." This attitude may be entirely legitimate if the colleague has an unusual expertise required by the nature of the research problem. Sometimes, however, it may be an excuse for laziness on the part of a principal researcher who cannot be bothered to master all the techniques appropriate to that type of research. At worst it may be a form of academic blackmail.

If Brown does indeed deserve to be a coauthor, then this should be on the basis suggested by Broad and Wade:

> Two principles might be established. First, all people named as authors should have made a definably major contribution to the work reported. Any minor contribution should be explicitly acknowledged in the text of the article. Second, all authors of a paper should be prepared to take responsibility for its contents in precisely the same measure as they stand to take credit.[25]

These principles are intended for all branches of science: there are no "local rules" that exempt particular disciplines.

A word is necessary about the meaning of being "prepared to take responsibility" for the contents of a paper. Taking credit is straightforward: we include the paper in our list of publications and expect other scientists, scientific employers and grant-making bodies to give us due credit. Responsibility is normally a more private matter. Even if parts of the research are subsequently found to be based on carelessness or fraud, other scientists may be unwilling to censure you severely, if it appears that one of your coauthors was the source of the errors or dishonesty.

The generosity, or the pusillanimity, of other scientists does not however allow you to evade responsibility. If the paper contains fraudulent statements, or mistakes caused by the carelessness or self-deception of others, it should not have been published and you should not have attached your name and scientific reputation to it as a coauthor. In short, the time to take responsibility for a paper is not after its errors have been exposed but before it is published. Whatever view of the matter is taken by other coauthors, it is up to you to ensure that the manuscript is free of error or bias. This may involve learning more about some areas of expertise than you might otherwise need to know, but this is seldom as difficult as it sounds. You should, for example, not have to become a mathematician to understand the analysis contained in a particular research paper. If understanding every word and symbol is really beyond you, then you should have those sections you cannot understand checked by someone who is not a coauthor but whose knowledge and judgment you trust. Errors (or even dishonesty) can still slip through such checks, but the vast majority are caught by such responsible authorship.

Authorship, then, should mean the same thing in any branch of science. If the trend in a particular system is towards multiauthored papers, this cannot justify irresponsible authorship of the type described in this chapter.

Alternative sets of values in biomedical research

A second area of concern is limited to biomedical research rather than evident in other branches of science. Because of the vast scale of biomedical research today, and the large numbers of disciplines and scientists involved, it is nevertheless of great significance. The problem can best be stated by quoting from one recent study.

> . . . there are significant differences between the values of scientists whose professional training was in a particular field of science and those who have entered research after training in medicine. In particular, the central value in what might be called the "ethos of modern medicine" is to benefit patients rather than to produce scientific knowledge . . . .
>
> It is our sense — primarily experiential and impressionistic in nature — that honesty in research work as a fundamental moral rule is valued more strongly among

> scientists than among physicians . . . physicians tend to evaluate research in terms of harm or benefit to patients rather than in terms of adherence to the rigorous norms of scientific investigation . . . .
>
> When discussing the actual or possible occurrence of fraud in research, physicians seem less distressed morally than do scientists. With respect to what is often termed "massaging data" — as distinct from what apparently is the more negatively viewed occurrence of outright data fabrication — the physician reactions that we have heard (and that others have reported to us they have heard) indicate a pattern of indifference: "So what? It happens all the time . . . ."
>
> The ethos of modern science with respect to the integrity of data may also be weaker among nonphysician researchers who work in clinical settings than it is among basic or laboratory-based researchers, probably because the former absorb the prevailing norms of their physician colleagues.[26]

If this is reasonably accurate, the physician's attitude to research may be understandable, even if it cannot be condoned. The physician may be wary of the motives behind the research interest of the scientist. The latter may be slightly less concerned with doing everything possible to save the life or improve the health of an individual patient than the physician, and slightly more interested in the reason why the patient does or does not recover. The physician may therefore try to protect his or her patients from someone who may be inclined to view the patient primarily as an element in a scientific sample. If this means that tests are not conducted with the rigor that the scientist would like, so be it. However, the physician's refusal to become obsessed with absolute scientific accuracy may also be due to an inability — shared with a much wider public — to understand why the scientist treats data as sacrosanct. This does not mean that the scientist's attitude is wrong, any more than the physician's concern for patient welfare is unjustified.

The problem is not that one set of professional mores has to be chosen over another, but rather that the choice may take place if we are not careful. Scientific research in a clinical setting requires both the physicians' and the scientists' guiding principles. In Swazey and Scher's words,

> Adopting the position of a clinical researcher makes a physician subject to the standards of the scientific community in addition to those of the medical community. Indeed, since it is primarily practicing physicians who will be using the results of clinical research, the medical community itself relies upon the physician-investigator's conducting research in accordance with the highest scientific standards.[27]

The danger is that this will not happen; it is all-too-easy for the "nonphysician researchers in clinical settings" to forget or minimize the standards of accuracy they have learned as scientists and to adopt "the prevailing norms of their physician colleagues." If the head of the clinical research team sets the right example, such slippage is unlikely, but role models do not always behave as they should.

6

Whistleblowing

> The research community's hostile response to whistleblowing often has a devastating psychological impact on the whistleblower himself. Despite the fact that a whistleblower has acted in good faith, as a matter of principle, on the basis of compelling evidence, and out of deep concern for the goals of the scientific community and for the community itself, he may continue to be plagued by self-doubt concerning the moral propriety of his act. The hostile reaction of his fellow researchers and the associated claim of disloyalty tend to be perceived by the whistleblower as a charge that he has been a bad member of the community, that he has unjustifiably threatened the community.[28]

There is, unfortunately, much evidence to support this statement, and the present chapter must necessarily make the case for whistleblowing as a necessary part of maintaining the integrity of scientific research, at the same time as it makes clear the problems and dangers that the whistleblower faces.

Whistleblowing — drawing wide attention to dishonest or unacceptable behavior by a scientist or a group of scientists — is a distasteful task for the whistleblower, who is frequently also a scientist. Provided that the whistle is blown in good faith, it should not be made more difficult because other scientists are as hostile to allegations of fraud as they are to fraud itself. Still less should the whistleblower be penalized for attempting to uphold the integrity of scientific research. Those who are inclined to argue that scientific fraud is rare — that "there is no iceberg" — tend to emphasize the self-policing character of scientific research. Yet the step from friendly criticism and the recognition of accidental error to the exposure of deliberate fraud is a very long one, and whistleblowing is seldom seen as an integral part of the research experience.

> Allegations of misconduct in federally funded biomedical and behavioral research have a profound impact on the professional, personal and financial fortunes of both the 'whistleblower' and the alleged wrongdoer. Motives and reputations of the complainant, the accused and the affected institution are inevitably called into question. Both whistleblower and accused face academic censure, dismissal, professional 'blackballing', and expensive and time-consuming lawsuits. Fear of reprisal undoubtedly has had a chilling effect on many potential complainants.[29]

Take out the words "federally funded biomedical and behavioral" in the first sentence of that quotation and it remains a valid statement.

To emphasize the difficulties that are created, consider the position of the university or other research institution concerned. To the whistleblower, and probably also to the accused scientist, it probably represents authority: it is the agency that must sort out the mess, uphold the standards of ethical conduct which the whistleblower believes have been infringed, and impose appropriate penalties if fraud is proved. But the university will almost always assume such a role with the greatest reluctance. It is normally inexperienced in dealing with such matters, especially as the investigation of scientific fraud often requires judicial skills as well as scientific ones and the rights of the individuals concerned — "due process" — must be carefully protected. The university's own prestige is also involved:

> Research institutions rightly feel that disclosing a problem of research fraud will affect their future reputation and quite possibly their future eligibility for grants, and therefore they are extremely concerned about how to deal with fraud . . . . There has to be a message to institutions that they will not be punished for doing good.[30]

It may even be significant that some of the most notorious cases of scientific fraud have been exposed at institutions with outstanding research reputations: Cornell, Yale, Sloan-Kettering, Harvard Medical School. Whatever criticisms may be made of these institutions in regard to the discovery and investigation of such frauds, their reputations can withstand the criticism. Less prestigious institutions may well fear that a major case of fraud would be difficult to live down.

One form of whistleblowing that is probably more generally accepted occurs when the whistleblower is a direct victim of the fraud. If you believe that your research data or ideas have been stolen or misappropriated, you may be right or wrong, but your right to complain will be recognized.

Such recognition, however, is likely to be affected considerably, and to some extent understandably, by the status of the person that you accuse. If your complaint is that another graduate student misappropriated your ideas, then the student, like yourself, is considered to be learning how to behave as a research scientist. If the university accepts that plagiarism has occurred, it can act appropriately, perhaps going so far as to refuse to allow the student to continue graduate studies there. In such circumstances, the university is clearly attempting to maintain honorable standards. If, however, the accusation is that a professor or other supervisor has acted unethically, then the university is in a much more difficult position, since to some extent it is responsible for that person being in the position of supervision. The professor is also part of the university in a way that a graduate student is not.

The situation becomes still more difficult if the complaint involves behavior that affected others who have not complained in the past. Say, for example,

> I suspect it is rare indeed that unscrupulous scientists really escape critical scrutiny or detection, particularly within their own institutions. More likely they will go unchallenged or unreported because "whistle blowing" . . . is an unsatisfying, difficult and unpleasant task. . . .
>
> . . . there is a need for academic and research institutions to develop an explicit policy regarding whistle blowing which guarantees protection to the individual who in good faith wishes to bring suspected fraudulent practice to departmental or institutional attention . . . .

Letter to Sigma Xi from Doris T. Penman, Burbank, California, 12 October 1983.

that a postdoctoral student objects to what Broad and Wade have described as "the inherently dishonest practice of lab chiefs signing their name to work in which they have been only peripherally involved, if at all."[31] As is evident from the previous chapter, this booklet also takes the view that such behavior is irresponsible authorship, and "inherently dishonest." The practice is also fairly widespread. If it is to be checked, it is likely to require whistleblowers, but the junior scientists who are the "victims" may feel it difficult or impossible to challenge so directly the mores of the group within which they work, and especially the behavior of the person who heads that group. Unlike the lab head, they do not have established positions, and may not even have formal research qualifications such as a Ph.D.; they are members of the group in order to obtain those qualifications. In the real world situation that the individual student faces, it is difficult to argue that the student should tackle the situation by whistleblowing. Some will, and they probably deserve encouragement, but understandably most students will feel that reform should be initiated by others with less to lose than they have.

If this is so when the whistleblower is also the victim, it may be even more difficult for a junior scientist to complain about a situation "only" out of the desire to maintain the basic standards of scientific research, and it is not the whistleblower's own ox that is being gored. In an ideal world, of course, this should not be the case. Other scientists should be ready to recognize that dishonest work ultimately harms the whole of science, and that someone who tries to expose such behavior may be assumed, in the absence of other evidence, to be acting from principles that deserve encouragement, not suspicion or even distaste. In many institutions, however, such understanding may be unlikely.

Lastly, and perhaps most difficult of all, the whistleblower may well be honestly mistaken. No one should blow a whistle without very good grounds for doing so, but even then the whistleblower may be wrong. It is very difficult indeed for the research community involved to deal with such a situation and then to get on with its activities without recriminations. Imagine your own reaction if you were — quite unjustly — accused of fraudulent research by a sci-

> I should like to emphasise the disastrous effect of weak managers. In my (limited) experience top administrators and academics are unwilling to buy into the hassles that inevitably result from bringing into the open the sort of unethical practices we have been discussing. Both at the time and in retrospect it is clear that had they had a quiet word in the ears of those stepping out of line, more serious breaches would have been aborted, systematic thievery would have been prevented, much bad feeling would have been avoided . . . and some otherwise worthwhile careers could have been improved or even saved.

Letter to Sigma Xi from C.R. Twidale, Adelaide, South Australia, 20 December 1983.

entific colleague. The fact that the colleague had no personal animus towards you, and was honestly mistaken, might not prevent you feeling, as the long and embarrassing process of investigation and exoneration proceeded, that your accuser should somehow be "punished" for the wrongful accusation. But if it is agreed that occasional whistleblowing is inevitable and in the best interests of scientific research, then it must also be recognized that the whistleblower may, despite deep conviction and the best efforts to discover the truth, be in error. To accept this, and to go on without recrimination or retaliation, is a hard task for all concerned.

Few of us are likely to be whistleblowers, but we may find ourselves as bystanders or unwilling participants in a whistleblowing situation. If we take the integrity of scientific research seriously, we cannot turn our backs on the situation, any more than we can ignore our responsibilities as witnesses in a traffic accident where people are injured or killed. Both the whistleblowers and the accused are entitled to sympathetic hearings by their scientific colleagues and to their strong support where this seems appropriate.

7

Finding Help

As implied in the preceding chapters, the task of identifying, proving and correcting unethical behavior in scientific research is not easy and can be both time-consuming and painful for all concerned. Science takes pride in being self-policing, and in many respects this is not merely desirable but unavoidable. Because the pathways that we pursue as research scientists are infinite and unfrequented, we cannot police them as we protect our streets and personal property. We depend on those other travellers — other research scientists whose work happens to take them along such lonely byways of knowledge — to assist in ensuring that the research environment is a safe one.

Yet the earlier chapters have also indicated how difficult the self-policing task can be. Replication of experiments is seldom straightforward and attempts to replicate may themselves be flawed. Scientific "facts" are seldom as clear-cut as the non-scientific public imagines: there is often scope for multiple interpretations or for clear differences of opinion. Much research is undertaken by graduate students and other young scientists who are less well-equipped to act as guardians of scientific standards than established scholars. On the one hand they are inherently inexperienced and are liable to miss errors or to see problems where none exists. On the other hand they may have good reason to complain, but are inhibited by their dependence on senior scientists for financial support, good grades and references, and guidance in the research process itself. This problem can be general rather than specific: graduate students may feel that their future careers may be damaged if they quickly acquire the reputation of trouble makers. Finally, as noted earlier, the host institution (including department heads, lab directors and similar individuals) may have a strong desire to avoid a research scandal, and the resulting investigation and publicity.

And yet it is probable that the system works reasonably effectively, if only because most scientists have a deep commitment to the general principles around which this booklet is written. If a bad situation is encountered, few scientists will be comfortable in ignoring it, however much we dislike the problems that arise when we do not ignore it. Scientists, of course, do not differ in this respect from their colleagues in the humanities and social sciences: the commitment to the search for truth is not a prerogative of particular disciplines.

If we do encounter such a situation — something which seems difficult to reconcile with the basic principles of scientific research — how do we start to do something about it? Probably, by making an initial judgment about the character of the problem. If we come across an apparent error in research findings or technique by someone else, we must first ask ourselves whether this is likely to have been deliberate or accidental. If the error appears to have been accidental, which will normally be the case, then all that should be necessary is to notify the person concerned directly. A good analogy is that of the book reviewer: if the book is riddled with typographical errors or minor mistakes, the review should state this, but if the reviewer notices a few minor slips or questionable statements these should normally be handled by a private letter to the author, who is usually both grateful and glad to make corrections in future editions. Most scientists make occasional mistakes; most of us are grateful to those who detect such errors, and we are entitled to courtesy when these are pointed out to us.

If such an approach fails to elicit an appropriate response, e.g. if the scientist concerned unreasonably denies or ignores the correction, then something more may be required. An error in a published article may, for example, justify a letter to the editor, if the author is unwilling to write to the journal himself. Or it may not: error detection and correction covers a wide spectrum of situations, and the one who believes that an error has been made needs to keep some sense of proportion. A minor error in a paper published several years ago, that is unlikely to have many repercussions on other research, may be unfortunate but it does not necessarily require a formal correction or retraction. The editorial from *The New England Journal of Medicine* quoted on p. 26 was however concerned with a diagnostic procedure that had been published several years earlier and that had entered widely into medical practice: correction of erroneous data and interpretation was therefore very important.

The moral of the preceding paragraphs is "strive for scientific accuracy, but recognize that all of us are fallible and that those of us who make mistakes are still entitled to courtesy and respect." What, however, is to be done when one's initial judgement seems to be that an error is deliberate rather than accidental, or that there is evidence of negligent research to an extent that cannot be understood or condoned? The first thing to be said is that you still may be wrong in your initial judgement. Do not forget your own fallibility in your zeal to prove that others are in error. However, the second thing that must be kept in mind is that there is a great difference between the probable reactions of a scientist whose accidental error had been drawn to his attention and one who is alarmed that deliberate fraud or negligent research is about to be exposed. Alarm is also understandable in the honest researcher who is faced by challenges to his or her integrity.

In such situations, most of us will want to begin by discussing the problem confidentially with someone else. Discussion can easily become gossip, and gossip is a destructive and unscientific way of reaching a satisfactory conclusion.

Nevertheless, we may need advice from one or two people whom we can trust, especially in regard to whether we are already over-reacting. If it seems that we should pursue the matter, the next stage is likely to involve more formal structures. Science may be required to police itself, but it should do so through appropriate mechanisms rather than by *ad hoc* reactions or witch-hunts. Such mechanisms protect, or should protect, all concerned, including both accused and accuser.

For most of us, the mechanisms for investigating research fraud or negligence are to be found primarily in our employing institution: university, research institute or industrial corporation. Not all institutions have such mechanisms in place, and those that do probably hope that they will never need to be utilized. However, most research centers are concerned with their reputations. It is not pleasant to be identified as the site of a scientific research scandal, real or imagined, but it is infinitely worse to be known as a place where such scandals are hushed up or not investigated.

Many institutions, therefore, have created mechanisms that are designed to provide for proper investigation of the more serious forms of research fraud or negligence. In many cases these mechanisms may deliberately be informal in the early stages, so that there is the opportunity for the person initiating the process to decide whether the matter should be pursued or not. The later stages, however, inevitably are more formal and carefully structured, since professional and institutional reputations are at stake, careers may be affected profoundly, and due process is an obvious necessity. What is essential for anyone who contemplates making use of such a mechanism is to understand the mechanism as a whole, and not just the first step. This may sound self-evident, but due process is seldom straightforward; it involves factors and imposes obligations that may not be anticipated by someone normally concerned only with scientific investigation. Find out what is involved at each step, what is expected of yourself and others, and what options there are at different stages of the process to pursue or to drop the issue.

Perhaps even more important is that you recognize clearly what it is you are trying to accomplish. "Maintaining the honor of science" is fine as a general statement, but what does it mean in this particular case? In suspected research fraud, for example, does it mean exposure of the inaccuracies or punishment of the individuals concerned? If plagarism is concerned, are you seeking to have your own (or someone else's) prior claim established or, again, do you believe the plagiarist deserves to be punished?

This concern with your own motives is emphasized not because they will or should determine the course that the investigation should take, nor because any of these motives is inherently bad. Others, however, are entitled to ask what you are seeking to achieve, and to expect you to act in accordance with those objectives. Put very simply, the investigation of research fraud or similar behavior should be dispassionate but in fact usually becomes very emotional for those directly concerned. We need to try to analyze our emotions and to recognize when they are liable to take us further than we need to go.

No one would pretend that investigative mechanisms are pleasant, and some would argue that they are inherently ineffective. Since deliberate fraud or gross negligence in science is comparatively rare, the mechanisms are required only infrequently at any individual institution, and those involved may be reluctant to participate and uncomfortable with the form and character of such proceedings. The wish, on everyone's part, that the situation would just go away can sometimes be distorted into an effort to ensure that it does. It may even happen that when an individual has complained, gone through the time-consuming process of investigation and found his or her complaints vindicated, the university or other body may fail at the final step: it may find for the complainant but fail to take effective action against the individual who is at fault.

Such warnings need to be given, but such situations need not be expected. Fraud is much less common than honest research, and careful investigations of research fraud are much more probable than inadequate or ineffective reviews.

For the graduate student for whom this booklet is primarily intended, this may seem more than enough; few young scientists will wish or need to become involved in such proceedings. That said, it is also true that responsibility for maintaining the honor of science is shared more or less equally among the whole research community. Just as there is no law which says that major discoveries can only be made by senior scientists, so there is no law exempting young scientists from the task of protecting research standards. Most scientists may hope to pass their entire careers without being involved in such situations. If they occur, however, they may happen at any time. What other help is available?

Apart from the employing institutions, there are other bodies that have a strong interest in maintaining the honor of science. They include the professional and disciplinary societies, especially where publication of scientific research is concerned. Bodies like the American Association of University Professors similarly have a strong interest in matters that affect the working conditions of their members.

If existing mechanisms or institutions seem inadequate or unwilling to give assistance, there is always the option of seeking one's own legal advice. One obvious deterrent is the cost involved; another is that the scientist may feel that the problem has little to do with the law. Perhaps, but a sympathetic attorney can often suggest options worth pursuing, simply because a legal training may enable the problem to be seen in a more general context of ethical or unethical behavior than the scientist perceives. Few lawyers specialize in the law of intellectual property, or in similar fields close to the work of a research scientist. On the other hand, consider the example of a scientist who inadvertently becomes involved in a case of negligent or fraudulent research: perhaps a graduate student working in the same lab as the person whose research is questioned. The case is investigated discreetly, and appropriate action is taken, also discreetly. Although the scientist was not directly affected or under any suspicion, he or she may be worried that the actions were altogether too discreet, and that four or five years later some quite unjustified difficulties may arise, of the following type: "Wasn't he/she involved in that problem they had in the ____________ lab a few years ago? I never did hear the details, but . . ." One useful protection that an attorney can offer in such a situation is to take a sworn deposition by the scientist concerned, to be kept and used only if the need arises. It is, of course, a statement rather than proof, but sworn testimony made and dated at a time soon after the events take place may be quite effective in cancelling the effect of such rumors. This is not to say that, on any provocation, everyone even remotely involved should run immediately for an attorney. On the other hand, one's reputation is important in a scientific career, and the occasion may arise when legal advice may be a reasonable way of helping to protect that reputation.

8

Conclusion — honest research in an imperfect world

Inevitably, but regrettably, this booklet has been concerned with the difficulties and responsibilities associated with scientific research ethics, rather than with the exhilaration and rewards of research. A student contemplating going into research, who has read as far as the preceding chapter, may have acquired a dispiriting view of scientific research, and may wonder if the picture has been overdrawn.

Certainly there are those who would insist that the previous chapters do exaggerate the situation that a typical scientist is likely to encounter. Few would claim that any of the dishonest practices or unfair pressures that have been mentioned are completely absent from scientific research, but many would declare that they are very rare, and that research students need specific guidance of this kind no more than the average driver needs careful instruction on what to do if a wheel falls off. As suggested earlier, many scientists would claim that the publicized cases of dishonest behavior are proof that science really is a self-regulating activity: these cases are not the tip of the iceberg but rare aberrations in what is otherwise scrupulously honest research.

That discussions of scientific honesty are liable to generate violent disagreement is evident from the reception that the book *Betrayers of the Truth* received when it was published in 1982. In that book two science writers described a number of scientific frauds, and the processes by which recent ones were investigated. A former Dean of the Harvard Medical School, itself the site of one of the situations described in the book, is quoted by the book's publisher as regarding *Betrayers of the Truth* as "a thoughtful, well-written and well-documented analysis of how fraud and self-delusion can occur in a system which too often is claimed to be immune to such deviations." To some other critics, however, the book appeared to be an attempt to diminish the prestige of science, through journalistic pretense that the cases examined were typical of science as a whole.

Broad and Wade's own closing words are in fact rather ambiguous:

> Science is not an abstract body of knowledge, but man's understanding of nature. It is not an idealized interrogation of nature by dedicated servants of truth, but a

> human process governed by the ordinary human passions of ambition, pride, and greed, as well as by all the well-hymned virtues attributed to men of science. But the step from greed to fraud is as small in science as in other walks of life. Usually the misrepresentation amounts to no more than a sweetening or prettification of the data; less often, to outright fraud . . . .
>
> Scientific authorities deny that fraud is anything more than a passing blemish on the face of science. But only by acknowledging that fraud is endemic can the real nature of science and its servants be fully understood.[32]

It depends on what is meant by "endemic." If Broad and Wade mean that fraud is so widespread as to be more common in science than its absence, few would agree. If endemic means that a typical research scientist is likely to encounter one or more cases of suspected or actual dishonesty in the course of a career, then Broad and Wade are more difficult to contradict.

Semantic arguments apart, there remain two fundamental reasons why scientists should be concerned with the ethics of their research. The first reason is that without the basic principle of truthfulness — the assumption that we can rely on other people's words — the whole scientific research enterprise is liable to grind to a halt. Truthfulness may or may not be the cement that holds together society as a whole, but certainly it is essential in science. Secondly, whereas truthfulness in a wider context can be maintained and enforced by the institutions of the society we live in, scientific research is a specialized activity, each scientist working largely on individual experiments and analysis on the fringes of knowledge. Truthfulness — honesty — therefore has to depend primarily on individual scientists themselves.

> The research community must itself deal with teaching its students and communicating to its fellow workers the importance of openness and honesty in science, and each individual and institution must accept these principles at a personal level.[33]

The principle of honesty in science may be more readily acceptable than that of openness. We may be too inclined to assume that, since honesty in research depends so much on individual commitment to the truth, therefore the matter can be left to the individual. There are not many places where beginning research students are required to take, or even have the opportunity to hear, a formal course on ethical principles in research, and on the structures and mechanisms that support an individual commitment to honesty. When ethical questions arise, this is often because of whistleblowing, which we find distasteful. Openness is not an easy principle to maintain, but if we find the investigation of suspected fraud unpleasant, then we should try to minimize the need for it. To quote Broad and Wade once more, ". . . the detection of fraud is of far less importance than its prevention. What is required first and foremost are steps to diminish the inducement to fraud."[34] In a society, and a scientific structure, that rewards brilliance and tends to take painstaking care for granted, such inducements are not easily diminished.

Tartuffe, *by Molière (English translation by Richard Wilbur), New York: Harcourt, Brace and World, 1963.*

And it is best to err, if err one must
As you have done, upon the side of trust.

Where, finally, does this leave you, the individual embarking on a career as a research scientist? Not, it is hoped, feeling disillusioned or wary of entering a potentially dishonest world where the unscrupulous will be only too ready to take advantage of the honest person. That would indeed exaggerate the message of the booklet. Apart from your own basic personal commitment to honesty and to the standards of the scientific community (which is most of what is required), you should recognize that ethical research behavior depends on group attitudes as well as on individual behavior. As an individual, not yet possessing much research experience or secure employment, you may be limited in what you can do to change situations that are inconsistent with the true spirit of scientific inquiry. But if you find that you cannot be part of the solution, you should not become part of the problem: what you may have to endure you should not be tempted to endorse or emulate. If you believe that science depends on the principle of truthfulness, that "a false statement of fact, made deliberately, is the most serious crime a scientist can commit," then the theme of this booklet will remain important to you as long as you remain a scientist.

Notes

1. Foreman, Spencer, "Commentary: The Conflicting Missions of IRBs," in Swazey, Judith P. and Stephen R. Scher, eds., *Whistleblowing in Biomedical Research*, Government Printing Office, Washington, D.C., 1982, pp. 41-45.
2. Glass, Bentley, "The Ethical Basis of Science," *Science*, 150, 3 December 1965, pp. 1257-1258.
3. See Broad, William and Nicholas Wade, *Betrayers of the Truth*, Simon & Schuster, New York, 1982, pp. 38-56.
4. Quoted by Bentley Glass (note 2 above) from J. Bronowski, *Science and Human Values*, Messner, New York, 1956, p. 73.
5. The full title is *Reflections on the Decline of Science in England, and on some of its causes*. It was republished in London by Gregg International in 1969.
6. These definitions are taken from an article in *Awake!*, 65, 10, 1984, p. 7.
7. *Betrayers of the Truth* (note 3 above), pp. 173-174. The original source is Morton Hunt, "A Fraud that Shook the World of Science," *The New York Times Magazine*, 1 November 1981, pp. 42-75.
8. *Ibid.*, p. 174.
9. Quoted by Gerald Holton, "Subelectrons. Presuppositions, and the Millikan-Ehrenhaft Dispute," *Historical Studies in the Physical Sciences*, 9, 1978, pp. 161-224, from R.A. Millikan, "On the Elementary Electrical Charge and the Avogadro Constant," *Physical Review*, 2, 1913, pp. 109-143.
10. *Betrayers of the Truth* (note 3 above), p. 35, quoted from Allan D. Franklin, "Millikan's Published and Unpublished Data on Oil Drops," *Historical Studies in the Physical Sciences*, 11, 1981, pp. 185-201.
11. Holton, *op.cit.*, pp. 165-166.
12. *Ibid.*, p. 166.
13. *Ibid.*, pp. 210
14. *Ibid.*, pp. 166-167.
15. *Betrayers of the Truth* (note 3 above), p. 206.
16. *Betrayers of the Truth* (note 3 above), p. 155, quoted from Peter B. Medawar, "The Strange Case of the Spotted Mice," *The New York Review of Books*, 15 April 1976, p. 8
17. *Betrayers of the Truth* (note 3 above), p. 63.
18. *Ibid.*, p. 70.
19. See note 4 above.
20. "Definition of Plagiarism," in *The Blue Book, Documents of Interest to Members of the Teaching Staff and the Student Body*, Wesleyan University, Middletown, Conn., 1984-85, pp. 59-60. The original source is Harold C. Martin, Richard M. Ohmann, and James Wheatly, *The Logic and Rhetoric of Exposition*, 3rd edn. (New York: Holt, Rinehart and Winston, 1969).
21. *Webster's New Collegiate Dictionary*, G.C. Merriam, Springfield, Mass., 1974 edn.
22. *Betrayers of the Truth* (note 3 above), p. 55.
23. *Ibid.*, pp. 55-56.
24. Relman, Arnold S., "Lessons from the Darsee Affair," *The New England Journal of Medicine*, 308, 1983, pp. 1415-1417.
25. *Betrayers of the Truth* (note 3 above), p. 221.
26. Swazey, Judith P. and Stephen R. Scher, "The Whistleblower as a Deviant Professional: Professional Norms and Responses to Fraud in Clinical Research," in *Whistleblowing in Biomedical Research* (note 1 above), pp. 173-192.
27. *Idem.*
28. *Idem.*
29. Oakes, Andra N., "Protecting the Rights of Whistleblowers and the Accused in Federally Supported Biomedical Research," in *Whistleblowing in Biomedical Research* (note 1 above), pp. 111-142.
30. Glantz, Leonard H., "Commentary: The Role of the IRB in Monitoring Research," in *Whistleblowing in Biomedical Research* (note 1 above), pp. 75-77.
31. *Betrayers of the Truth* (note 3 above), p. 221.
32. *Ibid.*, pp. 223-224.
33. Price, Alan R., "Dealing with Scientists who Cheat" (review of *Betrayers of the Truth*), *Chemical and Engineering News*, 13 June 1983, pp. 68-70.
34. *Betrayers of the Truth* (note 3 above), p. 220.

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