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Authors
Ayala, F
McCormick Adams, R
Chilton, MD
et al.

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On Being a Scientist

Committee on the Conduct of Science
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Designer
Frank Papandrea

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Committee on the Conduct of Science

Francisco Ayala
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Department of Ecology and Evolutionary Biology
University of California-Irvine

Robert McCormick Adams
Secretary
Smithsonian Institution

Mary-Dell Chilton
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Preface

This booklet is written primarily for students who are beginning to do scientific research. It seeks to describe some of the basic features of a life in contemporary research and some of the personal and professional issues that researchers will encounter in their work.

Traditionally, young scientists have learned about the methods and values of scientific research from personal contact with more experienced scientists, and such interactions remain the best way for researchers to absorb what is still a largely tacit code of professional conduct. Any beginning researcher who has not worked closely with an experienced scientist is missing one of the most important aspects of a scientific education. Similarly, any experienced researcher who does not pass on to younger scientists a sense of the methods and norms of science is significantly diminishing his or her contribution to the field's progress. However, the informal transmission of values is not always enough. Changes in science in recent years, including the growing size of research teams and the quickening pace of research, sometimes have had the effect of reducing contact between senior and junior researchers. The increasing social importance and public visibility of science and technology also make it essential that beginning researchers know how important they are to safeguarding the integrity of the scientific enterprise.

Some of the topics discussed in this document, such as sources of error in science, scientific fraud, and misappropriation of credit, have received a great deal of attention over the past decade, both within the scientific community and outside it. In preparing this booklet, the governing council of the National Academy of Sciences hopes to contribute to the discussion and to stimulate researchers to identify and uphold the procedures that keep science strong and healthy.

One of the most appealing features of research is the great degree of personal freedom accorded scientists—freedom to pursue exciting opportunities, to exchange ideas freely with other scientists, to challenge conventional knowledge. Excellence in science requires such freedoms, and the institutions that support science in the United States have found ways to safeguard them. However, modern science, while strong in many ways, is also fragile in important respects. For example, efforts to restrict the reporting of research results can be devastating.

Most Americans see a strong science as essential to a successful future. Yet that generous social support is based on the premise that science will be done honestly and that mistakes will be routinely identified and corrected. The mechanisms that operate within science to maintain honesty and self-correction must therefore be honored and protected. Research institutions can support these mechanisms, but it is the individual researcher who has both the capability and the responsibility to maintain standards of scientific conduct.

Frank Press
President
National Academy of Sciences

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On Being a Scientist

Contents

In 1937 Tracy Sonneborn, a 32-year-old biologist at Johns Hopkins University, was working late into the night on an experiment involving the single-celled organism *Paramecium*. For years biologists had been trying to induce conjugation between paramecia, a process in which two paramecia exchange genetic material across a cytoplasmic bridge. Now Sonneborn had isolated two strains of paramecia that he believed would conjugate when combined. If successful, his experiment would finally overcome a major obstacle to studies of protozoan genetics.

Sonneborn mixed the strains together on a slide and put the slide under his microscope. Looking through the eyepiece, he witnessed for the first time what he would later call a "spectacular" reaction: The paramecia had clustered into large clumps and were conjugating. In a state of delirious excitement, Sonneborn raced through the halls of the deserted building looking for someone with whom he could share his joy. Finally he dragged a puzzled custodian back to the laboratory to peer through the microscope and witness this marvelous phenomenon.

Moments of scientific discovery can be among the most exhilarating of a scientist's life. The desire to observe or understand what no one has ever observed or understood before is one of the forces that keep researchers rooted to their laboratory benches, climbing through the dense undergrowth of a sweltering jungle, or pursuing the threads of a difficult theoretical problem. Few discoveries seem to come in a flash; most materialize more slowly over weeks or years. Nevertheless, the process can bring great satisfaction. The pieces fit into place. The whole makes sense.

A life in science can entail great frustrations and disappointments as well as satisfactions. An experiment can fail because of a technical complication or the sheer intractability of nature. A favorite hypothesis that has consumed months of effort can turn out to be incorrect. Disputes can break out with colleagues over the validity of experimental data, the interpretation of data, or credit for work done. Setbacks such as these are virtually impossible to avoid in science, and they can strain the composure of both the novice and the most self-assured senior scientist.

To an observer of science, the presence of these human elements in research raises an obvious question. Science results in knowledge that is as solid and reliable as anything we know. Science and technology are among humanity's greatest achievements, having transformed not only the material conditions of our lives but the very way in which we view the world. Yet scientific knowledge emerges from a process that is intensely human, a process marked by its full share of human virtues and limitations. How is the limited, fallible work of individual scientists converted into the enduring edifice of scientific knowledge?

Many people think of scientific research as a routine, cut-and-dried process. They associate the nature of scientific knowledge with the process of deriving it and conclude that research is as objective and unambiguous as scientific results. The reality is much different. Researchers continually have to make difficult decisions about how to do their work and how to present that work to others. Scientists have a large body of knowledge that they can use in making these decisions. Yet much of this knowledge is not the product of scientific investigation, but instead involves value-laden judgments, personal desires, and even a researcher's personality and style.

This booklet divides the decisions that scientists make into two overlapping categories. Much of the first half of the booklet looks at several examples of the choices that scientists make in their work as individuals: the treatment of data, techniques used to minimize bias, the application of values in judging hypotheses. The second half deals largely with questions that arise during the interactions among scientists: the need to report research results honestly and accurately, the proper distribution of credit for scientific work, the difficult problem of reporting misconduct. A final section touches upon the social context in which personal and professional decisions are made and details a few of the special obligations that scientists have as members of society at large.

The nature of scientific research

Is There a Scientific Method?

Throughout the history of science, some philosophers and scientists have sought to describe a single systematic method that can be used to generate scientific knowledge. For instance, one school of thought, dating back at least to Francis Bacon in the seventeenth century, points to observations as the fundamental source of scientific knowledge. According to this view, scientists must cleanse their minds of preconceptions, sitting down before nature "as a little child," as the nineteenth-century biologist Thomas H. Huxley described it. By gathering facts without prejudice, a scientist will eventually arrive at the correct theory.

Some scientists may believe in such a picture of themselves and their work, but carrying this approach into practice is impossible. Nature is too amorphous and diverse for human beings to observe without having some ideas about what they are observing. Scientific understanding is made possible through the interplay of mental constructs and sensory impressions. Scientists may be able to suspend some prior theoretical or thematic preconceptions to view nature from a new perspective, but they cannot view the physical world without any perspective.

Other formulations of the "scientific method" have been proposed over the years, but many scientists regard such blanket descriptions of what they do with suspicion. Perhaps from a distance science can be organized into a coherent framework, but in practice research is as varied as the approaches of individual researchers. Some scientists postulate many hypotheses and systematically set about trying to weed out the weaker ones. Others describe their work as asking questions of nature: "What would happen if . . . ? Why is it that . . . ?" Some researchers gather a great deal of data with only vague ideas about the problem they might be trying to solve. Others develop a specific hypothesis or conjecture that they then try to verify or refute with carefully structured observations.

Rather than following a single scientific method, scientists use a body of methods particular to their work. Some of these methods are permanent features of the scientific community; others evolve over time or vary from discipline to discipline. In a broad sense, these methods include all of the techniques and principles that scientists apply in their work and in their dealings with other scientists. Thus, they encompass not only the information scientists possess about the empirical world but the knowledge scientists have about how to acquire such information.

"Scientists are people of very dissimilar temperaments doing different things in very different ways. Among scientists are collectors, classifiers and compulsive tidiers-up; many are detectives by temperament and many are explorers; some are artists and others artisans. There are poet-scientists and philosopher-scientists and even a few mystics."

Peter B. Medawar, *The Art of the Soluble*, London: Methuen, 1967, p. 132

The Treatment of Data

One goal of methods is to coax the facts, untainted by human bias, from a scientific investigation. In retrospect, this may seem a straightforward process, a simple application of accepted scientific practices to a specific problem. But at the forefronts of research, neither the problem nor the methods used to solve it are usually well-defined. Instead, experimental techniques are pushed to the limit, the signal is difficult to separate from the noise, and unknown sources of error abound. In such an uncertain and fluid situation, picking out reliable data points from a mass of confusing and sometimes contradictory observations can be extremely difficult.

One well-known example of this difficulty involves the physicist Robert Millikan, who won the Nobel Prize in 1923 for his work on the charge of the electron. In the 1910s, just as most physicists were coming to accept the existence of the electron, Millikan carried on a protracted and sometimes heated dispute with the Viennese physicist Felix Ehrenhaft over the magnitude of the smallest electrical charge found in nature. Both men based their findings on the movements of tiny charged objects—oil drops, in Millikan's case—in electric fields. Ehrenhaft used all the observations he made without much discrimination and eventually concluded that there was no lower limit to the size of an electrical charge that could exist in nature. Millikan used only what he regarded as his "best" data sets to establish the magnitude of the charge and argue against the existence of Ehrenhaft's "subelectrons." In other words, Millikan applied methods of data selection to his observations that enabled him to demonstrate the unitary charge of the electron.

Millikan has been criticized for not disclosing which data he omitted or why he omitted those data. But an examination of his notebooks reveals that Millikan felt he knew just how far he could trust his raw data. He often jotted down in his notebooks what he thought were good reasons for excluding data. However, he glossed over these exclusions in some of his published papers, and by present standards this is not acceptable. Scientists must be willing to acknowledge the limitations on their data if they are not to mislead others about the data's reliability.

General rules for distinguishing *a priori* "good" data from "bad" cannot be formulated with much clarity. Nevertheless, good scientists have methods that they can apply in judging the reliability of data, and learning these methods is one of the goals of a scientific apprenticeship. These methods may be unique to a given situation, depending on how and why a set of observations is being made. Nevertheless, they impose constraints on how those observations can be interpreted. A researcher is not free to select only the data that fit his or her prior expectations. If certain data are excluded, a researcher must have justifiable reasons for doing so.

The Relation Between Hypotheses and Observations

Attempts to isolate the facts and nothing but the facts in scientific research can raise philosophical as well as methodological problems. One prominent difficulty involves the line of demarcation between hypotheses and observations. For years philosophers have tried to construct purely observational languages free of theoretical constructs, but they have never been completely successful. Even a simple description such as "The temperature in this room is 25 degrees centigrade" contains a host of theoretical underpinnings. The thermometer used to measure the temperature is a complex device subject to its own systematic and random errors. And the quantity being measured is not some fundamental attribute of nature but depends in a complex way on the movements and interactions of gas particles, which are described in terms of the kinetic theory of gases, quantum mechanics, and so on.

The terms used in science also contribute to the interpenetration of hypotheses and observations. For example, Anton van Leeuwenhoek, the seventeenth-century Dutch microscopist, prided himself in describing what he saw through his lenses without any theoretical speculation. However, his descriptions were anything but theory-neutral. When he examined the water standing in the gutter outside his window, some of the microscopic creatures he saw were probably *Euglena*. Today we know that these single-celled organisms contain chlorophyll and are more closely related to plants than animals. But because the creatures moved, van Leeuwenhoek called them "animalcules," not "planticules."

Terms such as "energy," "gross national product," "pion," "black hole," "intelligence quotient," and "gene" are clearly derived from particular theories and obtain much of their meaning from their roles in these theories. But such theoretical terms can take on a life of their own and be gradually transformed into more observational terms. Similarly, as terms become unmoored from their original theories, the potential to misuse or misunderstand them increases.

The Risk of Self-Deception

Awareness of the inroads that theory can make into observations serves as a valuable reminder of the constant danger of self-deception in science. Psychologists have shown that people have a tendency to see what they expect to see and fail to notice what they believe should not be there. For instance, during the early part of the twentieth century one of the most ardent debates in astronomy concerned the nature of what were then known as spiral nebulae—diffuse pinwheels of light that powerful telescopes revealed to be quite common in the night sky. Some astronomers thought that these nebulae were spiral galaxies like the Milky Way at such great distances that individual stars could not be distinguished. Others believed that they were clouds of gas within our own galaxy.

One astronomer in the latter group, Adriaan van Maanen of the Mount Wilson Observatory, sought to resolve the issue by comparing photographs of the nebulae taken several years apart. After making a series of painstaking measurements, van Maanen announced that he had found roughly consistent unwinding motions in the nebulae. The detection of such motions indicated that the spirals had to be within the Milky Way, since motions would be impossible to detect in distant objects.

Van Maanen's reputation caused many astronomers to accept a galactic location for the nebulae. A few years later, however, van Maanen's colleague Edwin Hubble, using the new 100-inch telescope at Mount Wilson, conclusively demonstrated that the nebulae were in fact distant galaxies; van Maanen's observations had to be wrong. Studies of his procedures have not revealed any intentional misrepresentation or sources of systematic error. Rather, he was working at the limits of observational accuracy, and he saw what he expected to see.

Self-deception can take more subtle forms. For example, a researcher may stop a data run too early because the observations conform to expectations, whereas a longer run might turn up unexpected discrepancies. Insufficient repetitions of an experiment are a common cause of invalid conclusions, as are poorly controlled experiments.

Methods and Their Limitations

Over the years, scientists have developed a vast array of methods that are designed to minimize the kinds of problems discussed above. At the most familiar level, these methods include techniques such as double-blind trials, randomization of experimental subjects, and the proper use of controls, which are all aimed at reducing individual subjectivity. Methods also include the use of tools in scientific work, both the mechanical tools used to make observations and the intellectual tools used to manipulate abstract concepts.

The term "methods" can be interpreted more broadly. Methods include the judgments scientists make about the interpretation or reliability of data. They also include the decisions scientists make about which problems to pursue or when to conclude an investigation. Methods involve the ways scientists work with each other and exchange information. Taken together, these methods constitute the craft of science, and a person's individual application of these methods helps determine that person's scientific style.

Some methods, such as those governing the design of experiments or the statistical treatment of data, can be written down and studied. (The bibliography includes several books on experimental design.) But many methods are learned only through personal experience and interactions with other scientists. Some are even harder to describe or teach. Many of the intangible influences on scientific discovery—curiosity, intuition, creativity—largely defy rational analysis, yet they are among the tools that scientists bring to their work.

Although methods are an integral part of science, most of them are not the product of scientific investigation. They have been developed and their use is required in science because they have been shown to advance scientific knowledge. However, even if perfectly applied, methods cannot guarantee the accuracy of scientific results. Experimental design is often as much an art as a science; tools can introduce errors; and judgments about data inevitably rest on incomplete information.

The fallibility of methods means that there is no cookbook approach to doing science, no formula that can be applied or machine that can be built to generate scientific knowledge. But science would not be so much fun if there were. The skillful application of methods to a challenging problem is one of the great pleasures of science. The laws of nature are not apparent in our everyday surroundings, waiting to be plucked like fruit from a tree. They are hidden and unyielding, and the difficulties of grasping them add greatly to the satisfaction of success.

Values in Science

When methods are defined as all of the techniques and principles that scientists apply in their work, it is easier to see how they can be influenced by human values. As with hypotheses, human values cannot be eliminated from science, and they can subtly influence scientific investigations.

The influence of values is especially apparent during the formulation or judgment of hypotheses. At any given time, several competing hypotheses may explain the available facts equally well, and each may suggest an alternate route for further research. How should one select among them?

Scientists and philosophers have proposed several criteria by which promising scientific hypotheses can be distinguished from less fruitful ones. Hypotheses should be internally consistent, so that they do not generate contradictory conclusions. Their ability to provide accurate predictions, sometimes in areas far removed from the original domain of the hypothesis, is viewed with great favor. With disciplines in which prediction is less straightforward, such as geology or astronomy, good hypotheses should be able to unify disparate observations. Also highly prized are simplicity and its more refined cousin, elegance.

The above values relate to the epistemological, or knowledge-based, criteria applied to hypotheses. But values of a different kind can also come into play in science. Historians, sociologists, and other students of science have shown that social and personal values unrelated to epistemological criteria—including philosophical, religious, cultural, political, and economic values—can shape scientific judgment in fundamental ways. For instance, in the nineteenth century the geologist Charles Lyell championed the concept of uniformitarianism in geology, arguing that incremental changes operating over long periods of time have produced the Earth's geological features, not large-scale catastrophes. However, Lyell's preference for this still important idea may have depended as much on his religious convictions as on his geological observations. He favored the notion of a God who is an unmoved mover and does not intervene in His creation. Such a God, thought Lyell, would produce a world where the same causes and effects keep cycling eternally, producing a uniform geological history.

The obvious question is whether holding such values can harm a person's science. In many cases the answer has to be yes. The history of science offers many episodes in which social or personal values led to the promulgation of wrong-headed ideas. For instance, past investigators produced "scientific" evidence for overtly racist views, evidence that we now know to be wholly erroneous. Yet at the time the evidence was widely accepted and contributed to repressive social policies.

Attitudes regarding the sexes also can lead to flaws in scientific judgments. For instance, some investigators who have sought to document the existence or absence of a relationship between gender and scientific abilities have allowed personal biases to distort the design of their studies or the interpretation of their findings. Such biases can contribute to institutional policies that have caused females and minorities to be underrepresented in science, with a consequent loss of scientific talent and diversity.

Conflicts of interest caused by financial considerations are yet another source of values that can harm science. With the rapid decrease in time between fundamental discovery and commercial application, private industry is subsidizing a considerable amount of cutting-edge research. This commercial involvement may bring researchers into conflict with industrial managers—for instance, over the publication of discoveries—or it may bias investigations in the direction of personal gain.

The above examples are valuable reminders of the danger of letting values intrude into research. But it does not follow that social and personal values necessarily harm science. The desire to do accurate work is a social value. So is the belief that knowledge will ultimately benefit rather than harm humankind. One simply must acknowledge that values do contribute to the motivations and conceptual outlook of scientists. The danger comes when scientists allow values to introduce biases into their work that distort the results of scientific investigations.

The social mechanisms of science discussed later act to minimize the distorting influences of social and personal values. But individual scientists can avoid pitfalls by trying to identify their own values and the effects those values have on their science. One of the best ways to do this is by studying the history, philosophy, and sociology of science. Human values change very slowly, and the lessons of the past remain of great relevance today.

Judging Hypotheses

Values emerge into particularly sharp relief when a long-established theory comes into conflict with new observations. Individual responses to such situations range between two extremes. At one end of the spectrum is the notion that a theory must be rejected or extensively modified as soon as one of its predictions is not borne out by an experiment. However, history is full of examples in which this would have been premature because not enough was known to make an accurate prediction. A classic ex-

N RAYS

Self-delusion is not a danger only for individual scientists. Sometimes a number of scientists can get caught up in scientific pursuits that later prove to be unfounded.

One of the most famous examples of such "pathological science" is the history of N rays. In the first few years of the twentieth century, shortly after the discovery of X rays by the German physicist Wilhelm Roentgen, the distinguished French physicist Rene Blondlot announced that he had discovered a new type of radiation. Blondlot named the new radiation N rays after the University of Nancy, where he was professor of physics. The rays were supposedly produced by a variety of sources, including electrical discharges within gases and heated pieces of metal; they could be refracted through aluminum prisms; and they could be detected by observing faint visual effects where the rays hit phosphorescent or photographic surfaces. Within a few years, dozens of papers describing the properties of N rays had been published in journals by eminent scientists.

Other scientists, however, found it impossible to duplicate the experiments. One such scientist was the American physicist Robert W. Wood, who traveled to Blondlot's laboratory in 1904 to witness the experiments for himself. After viewing several inconclusive experiments, Wood was shown an experiment by Blondlot in which N rays generated by a lamp were bent through an aluminum prism and fell on a phosphorescent detector. At one point in the experiment, Wood took advantage of the room's darkness to surreptitiously remove the aluminum prism from the apparatus. Nevertheless, Blondlot continued to detect the visual signals that he believed were caused by N rays.

In an article in Nature published shortly after his visit, Wood wrote that he was "unable to report a single observation which appeared to indicate the existence of the rays." Scientific work on N rays soon collapsed, and previous results were shown to be experimental artifacts or the result of observer effects. Yet Blondlot continued to believe in the existence of N rays until his death in 1930.

ample involves Charles Darwin's defense of the theory of evolution. After Darwin presented his theory, physicists argued that the age of the Earth—then calculated to be between 24 million and 100 million years based on the loss of the heat generated by the Earth's formation—could not possibly be long enough for Darwinian evolution to have occurred. Doggedly, although admittedly rather miserably, Darwin hung on. Only after his death was he vindicated. When physicists discovered radioactivity and realized that natural radioactive heating must be included in the Earth's heat budget, there proved to be plenty of time for natural selection to have produced today's species.

On the other hand, history also contains many examples of scientists who held on to an outdated theory after it had been discredited. Human beings have a strong tendency to cling to long-established ideas even in the face of considerable opposing evidence. A trend in the data can always be resisted by citing uncertainties in the observations or by supposing that unknown factors are at work.

Hanging on for a while to a favorite but embattled idea is often a necessity during the initial stages of research. But scientists must also learn to give way in light of new and more insistent evidence. Knowing why an idea is so appealing, or why countervailing evidence is so strongly resisted, can help a person develop this fine sense of discrimination.

Peer Recognition and Priority of Discovery

Human values are also an integral part of the forces that motivate scientists. These forces are numerous and psychologically complex. They include curiosity about the natural or social world, the desire to better the human condition, and a feeling of awe, whether religious or secular, at discerning the workings of nature.

Another important motivating force in science is a desire for recognition by one's peers. One of the greatest rewards scientists can experience is to have their work acknowledged and praised by other scientists and incorporated into their colleagues' research. Sometimes the quest for personal credit can become counterproductive, as when time, energy, or even friendships are lost to priority disputes or ad hominem polemics. But a strong personal attachment to an idea is not necessarily a liability. It can even be essential in dealing with the great effort and frequent disappointments associated with scientific research.

In science, the first person or group to publish a result generally gets the lion's share of credit for it, even if another group that has been working on the problem much longer publishes the same result just a little later. (Actually, priority is dated from when a scientific journal receives a manuscript.) Once published, scientific results become the public property of the research community, but their use by other scientists requires that the original discoverer be recognized. Only when results have become

"A large number of incorrect conclusions are drawn because the possibility of chance occurrences is not fully considered. This usually arises through lack of proper controls and insufficient repetitions. There is the story of the research worker in nutrition who had published a rather surprising conclusion concerning rats. A visitor asked him if he could see more of the evidence. The researcher replied, Sure, there's the rat."

—E. Bright Wilson, Jr., *An Introduction to Scientific Research*, New York: McGraw-Hill, 1952, p. 34

common knowledge are scientists free to use them without attribution.

In deciding when to make a result public, a scientist weighs several competing factors. If a result is kept private, researchers can continue to check its accuracy and use it to further their research. But researchers who refrain from publishing risk losing credit to someone else who publishes first. When considerations such as public acclaim or patent rights are added to the mix, decisions about when to publish can be difficult.

Social mechanisms in science

The Communal Review of Scientific Results

Given the morass of preconceptions, fallible methods, and human values described in the previous pages, a person might wonder how science gets done at all. Yet the large and rapidly expanding body of scientific knowledge, resistant to change and eminently successful in its practical application, attests to the tremendous success of the enterprise. The link between the two domains, between the volatile microcosm of individual scientists and the solid macrocosm of scientific knowledge, lies largely in the social structure of the scientific community.

If scientists were prevented from communicating with each other, scientific progress would grind to a halt. Science is not done in isolation; nor is it done from first principles. Scientific research takes place within a broad social and historical context, which gives substance, direction, and, ultimately, meaning to the work of individual scientists.

Researchers submit their observations and hypotheses to the scrutiny of others through many informal and formal mechanisms. They talk to their colleagues and supervisors in hallways and over the telephone, airing their ideas and modifying them in the light of the responses they receive. They give presentations at seminars and conferences, exposing their views to a broader but still limited circle of colleagues. They write up their results and send them to scientific journals, which in turn send the papers to be scrutinized by reviewers. Finally, when a paper has been published, it is accepted or rejected by the community to the extent that it is used or ignored by other scientists.

At each stage, researchers submit their work to be examined by others with the hope that it will be accepted. This process of public, systematic skepticism is critical in science. It minimizes the influence of individual subjectivity by requiring that research results be accepted by other scientists. It also is a powerful inducement for researchers to be critical of their own conclusions, because they know that their objective must be to convince their ablest colleagues, including those with contrasting views.

Bypassing the standard routes of validation can short-circuit the self-correcting mechanisms of science. Scientists who release their results directly to the public—for example, through a press conference called to announce a discovery—risk adverse reactions later if their results are shown to be mistaken or are misinterpreted by the media or the public. Publication in a scientific journal includes important aspects of quality control—particularly, critical review by peers who can detect mistakes, omissions, and alternative explanations. If information transmitted through the mass media cannot be substantiated later, the public may not believe other, more careful researchers. For this reason, many journals do not accept papers whose results have been previously publicized by their authors. When a press release is warranted, it should be scheduled only when peer review is complete (normally, in conjunction with publication in a scientific journal).

While publication in a peer-reviewed journal remains the standard means of disseminating scientific results, other methods of communication are subtly altering how scientists divulge and receive information. The increased use of preprints, abstracts, and proceedings volumes and technologies such as computer networks and facsimile machines are simultaneously increasing the speed of communication and loosening the network of social controls imposed on formal publication. These new methods of communication are often simply elaborations of the informal exchanges that pervade science. But reliance on such means of information exchange should not be allowed to weaken the mechanisms of quality control that operate so effectively in science.

Replication and the Openness of Communication

The requirement that results be validated by one's peers explains why scientific papers must be written in such a way that the observations in them can be replicated. However, actual replication in science is selective: it tends to be reserved for experiments with unusual importance or for experiments that conflict with an accepted body of work. Most often, scientists who hear or read about a result that affects their own research build on that result. If something goes wrong with the subsequent work, researchers may then return to the original results and attempt to duplicate them.

Scientists build on previous results because it is not practical (or necessary) to reconstruct all the observations and theoretical constructs that go into an investigation. They make the operating assumption that previous investigators performed work as reported and adhered to the methods prescribed by the community. If that trust is misplaced and the previous results are inaccurate, the truth will likely emerge as problems arise in the ongoing investigation. But months or years of effort may be wasted in the process. Thus, the social structure of science minimizes errors in the long run through peer

"As the world of science has grown in size and in power, its deepest problems have changed from the epistemological to the social. . . . The increase and improvement of scientific knowledge is a very specialized and delicate social process, whose continued health and vitality under new conditions is by no means taken for granted."

Jerome Ravetz, *Scientific Knowledge and Its Social Problems*, Oxford, England: Clarendon Press, 1971, p. 10

verification. But in the short term science operates on a basis of trust and honesty among its practitioners.

The need for skeptical review of scientific results is one reason why free and open communication is so important in science. Different scientists can review the same data and, drawing on their own theories and values, differ in their interpretations of those data. The benefits of openness do not necessarily imply, however, that all scientific data should be available to all persons in all circumstances. In the initial, sometimes bewildering stages of research, a scientist is entitled to a period of privacy in which data are not subject to public disclosure. This privacy allows the creative process to continue without fear of professional embarrassment and allows individuals to advance their work to the point at which they can have confidence in its accuracy. Many scientists are very generous in discussing their preliminary theories or results with colleagues, and some even provide copies of raw data to others prior to public disclosure to facilitate related work. The standards of science encourage the sharing of data and other research tools at this stage, but they do not demand it.

After publication, scientists expect that data and other research materials will be shared upon request. Sometimes these materials are too voluminous, unwieldy, or costly to share freely and quickly. But in those fields in which sharing is possible, a scientist who is unwilling to divulge research data to qualified colleagues runs a great risk of not being trusted or respected. Because of the continued need for access to data, researchers should keep primary data for as long as there is any reasonable need to refer to them. Of course, researchers who share their data with others should receive full credit for the use of those data.

The sharing of data and other research tools is subject to certain constraints. Individuals requesting such information need to have demonstrated an ability to develop conclusions relevant to the field of inquiry from raw data. Scientists also are not obliged to share research materials with people who they suspect are acting solely on the basis of commercial or other private interests. For instance, a university biologist would not be obligated to turn over a potentially valuable reagent to scientists in industry. However, scientists should not deny requests for access to primary data because of professional jealousy.

In research that has the potential of being financially profitable, openness can be maintained by the granting of patents. Patents offer protection for the commercial promise of a scientific discovery in return for making the results public. However, patenting is not always an option. Therefore, many scientists, particularly in industry but also in academia, must maintain some level of secrecy in their work. Scientists working on weapons or defense-related research also generally accept the necessity for secrecy in some areas. But scientists working under such conditions should recognize the potential dangers of secrecy in fostering unproductive research and shielding results from professional scrutiny.

Scientific Progress

If there is one thing on which almost all scientists would agree, it is that science is a progressive enterprise. New observations and theories survive the scrutiny of scientists and earn a place in the edifice of scientific knowledge because they describe the physical or social world more completely or more accurately. Relativistic mechanics is a more thorough description of what we observe than Newtonian mechanics. The DNA molecule is a double helix. Our apelike ancestors walked erect before brain sizes greatly increased.

Given the progressive nature of science, a logical question is whether scientists can ever establish that a particular theory describes the empirical world with complete accuracy. The notion is a tempting one, and a number of scientists have proclaimed the near completion of research in a particular discipline (occasionally with comical results when the foundations of that discipline shortly thereafter underwent a profound transformation). But the nature of scientific knowledge argues against our ever knowing that a given theory is the final word. The reason lies in the inherent limitations on verification. Scientists can verify a hypothesis, say by testing the validity of a consequence derived from that hypothesis. But verification can only increase confidence in a theory, never prove the theory completely, because a conflicting case can always turn up sometime in the future.

Because of the limits on verification, philosophers have suggested that a much stronger logical constraint on scientific theories is that they be falsifiable. In other words, theories must have the possibility of being proved wrong, because then they can be meaningfully tested against observation. This criterion of falsifiability is one way to distinguish scientific from nonscientific claims. In this light, the claims of astrologers or creationists cannot be scientific because these groups will not admit that their ideas can be falsified.

Falsifiability is a stronger logical constraint than verifiability, but the basic problem remains. General statements about the world can never be absolutely confirmed on the basis of finite evidence, and all evidence is finite. Thus, science is progressive, but it is an open-ended progression. Scientific theories are always capable of being reexamined and if necessary replaced. In this sense, any of today's most cherished theories may prove to be only limited descriptions of the empirical world and at least partially "erroneous."

Human Error in Science

Error caused by the inherent limits on scientific theories can be discovered only through the gradual advancement of science, but error of a more human kind also occurs in science. Scientists are not infallible; nor do they have limitless working time or access to unlimited resources. Even the most responsible scientist can make an honest mistake. When such errors are discovered, they should be acknowledged, preferably in the same journal in which the mistaken information was published. Scientists who make such acknowledgments promptly and graciously are not usually condemned by colleagues. Others can imagine making similar mistakes.

Mistakes made while trying to do one's best are tolerated in science; mistakes made through negligent work are not. Haste, carelessness, inattention—any of a number of faults can lead to work that does not meet the standards demanded in science. In violating the methodological standards required by a discipline, a scientist damages not only his or her own work but the work of others as well. Furthermore, because the source of the error may be hard to identify, sloppiness can cost years of effort, both for the scientist who makes the error and for others who try to build on that work.

Some scientists may feel that the pressures on them are an inducement to speed rather than care. They may believe, for instance, that they have to cut corners to compile a long list of publications. But sacrificing quality

THE HISTORICAL ORIGINS OF PRIORITY

The system of associating scientific priority with publication took shape during the seventeenth century in the early years of modern science. Even then, a tension existed between the need of scientists to have access to other findings and a desire to keep work secret so that others would not claim it as their own. Scientists of the time, including Isaac Newton, were loathe to convey news of their discoveries to scientific societies for fear that someone else would claim priority, a fear that was frequently realized.

To ensure priority, many scientists, including Galileo, Huygens, and Newton, resorted to constructing anagrams describing their discoveries that they would then make known to others. For instance, the law "mass times acceleration equals force" could be disguised as "a remote, facile question scares clams" (though Newton would have constructed his anagrams in Latin). Later, if someone else came up with the same discovery, the original discoverer could unscramble the anagram to establish priority.

The solution to the problem of making new discoveries public while assuring their authors credit was worked out by Henry Oldenburg, the secretary of the Royal Society of London. He won over scientists by guaranteeing rapid publication in the Philosophical Transactions of the society as well as the official support of the society in case the author's priority was brought into question. Thus, it was originally the need to ensure open communication in science that gave rise to the convention that the first to publish a view or a finding, not the first to discover it, gets credit for the discovery.

to such pressures is likely to have a detrimental effect on a person's career. The number of publications to one's name, though a factor in hiring or promotion decisions, is not nearly as important as the quality of one's overall work. To minimize pressure to publish substandard work, an increasing number of institutions are adopting policies that limit the number of papers considered when evaluating an individual.

Fraud in Science

There is a significant difference between preventable error in research, whether caused by honest mistakes or by sloppy work, and outright fraud. In the case of error, scientists do not intend to publish inaccurate results. But when scientists commit fraud, they know what they are doing.

Of all the violations of the ethos of science, fraud is the gravest. As with error, fraud breaks the vital link between human understanding and the empirical world, a link that is science's greatest strength. But fraud goes beyond error to erode the foundation of trust on which science is built. The effects of fraud on other scientists, in terms of time lost, recognition forfeited to others, and feelings of personal betrayal, can be devastating. Moreover, fraud can directly harm those who rely on the findings of science, as when fraudulent results become the basis of a medical treatment. More generally, fraud undermines the confidence and trust of society in science, with indirect but potentially serious effects on scientific inquiry.

Fraud has been defined to encompass a wide spectrum of behaviors. It can range from selecting only those data that support a hypothesis and concealing the rest ("cooking" data) to changing the readings to meet expectations ("trimming" data) to outright fabrication of results. Though it may seem that making up results is somehow

FRAUD AND THE ROLE OF INTENTIONS

The acid test of scientific fraud is the intention to deceive, but judging the intentions of others is rarely easy. The case of William Summerlin illustrates both situations: an instance of blatant fraud and a previous history in which the origins of serious discrepancies are harder to determine.

In 1973 Summerlin came to the Sloan-Kettering Institute for Cancer Research in New York, where he subsequently became chief of a laboratory working on transplantation immunology. For the previous six years, Summerlin had been studying the rejection of organ transplants in humans and animals. He believed that by placing donor organs in tissue culture for a period of some days or weeks before transplantation, the immune reaction that usually causes the transplant to be rejected could be avoided. The work had become well-known to scientists and to the public.

However, other scientists were having trouble replicating Summerlin's work. Another immunologist at Sloan-Kettering was assigned to repeat some of Summerlin's experiments, but he, too, could not make the experiments work. As doubts were growing, Summerlin began a series of experiments in which he grafted patches of skin from black mice onto white mice. One morning as Summerlin was carrying some of the white mice to the director of the institute to demonstrate his progress, he took a felt-tipped pen from his pocket and darkened some of the black skin grafts on two white mice. After the meeting, a laboratory assistant noticed that the dark color could be washed away with alcohol, and within a few hours the director knew of the incident. Summerlin subsequently admitted his deception to the director and to others.

Summerlin was suspended from his duties and a six-member committee conducted a review of the veracity of his scientific work and his alleged misrepresentations concerning that work. In particular, in addition to reviewing the "mouse incident," the committee examined a series of experiments in which Summerlin and several collaborators had transplanted parts of corneas into the eyes of rabbits. The committee found that Summerlin had incorrectly and repeatedly exhibited or reported on certain rabbits as each having had two human corneal transplants, one unsuccessful from a fresh cornea and the other successful from a cultured cornea. In fact, only one cornea had been transplanted to each rabbit, and all were unsuccessful.

When asked to explain this serious discrepancy, Summerlin stated that he believed that the protocol called for each rabbit to receive a fresh cornea in one eye and a cultured cornea in the other eye. Summerlin subsequently admitted that he did not know and was not in a position to know which rabbits had undergone this protocol, and that he only assumed what procedures had been carried out on the rabbits he exhibited. After reviewing the circumstances of what the investigating committee characterized as "this grossly misleading assumption," the report of the investigating committee stated: "The only possible conclusion is that Dr. Summerlin was responsible for initiating and perpetuating a profound and serious misrepresentation about the results of transplanting cultured human corneas to rabbits."

The investigating committee concluded that "some actions of Dr. Summerlin over a considerable period of time were not those of a responsible scientist." There were indications that Summerlin may have been suffering from emotional illness, and the committee's report recommended "that Dr. Summerlin be offered a medical leave of absence, to alleviate his situation, which may have been exacerbated by pressure of the many obligations which he voluntarily undertook." The report also stated that, "for whatever reason," Dr. Summerlin's behavior represented "irresponsible conduct that was incompatible with discharge of his responsibilities in the scientific community."

"We thus begin to see that the institutionalized practice of citations and references in the sphere of learning is not a trivial matter. While many a general reader—that is, the lay reader located outside the domain of science and scholarship—may regard the lowly footnote or the remote endnote or the bibliographic parenthesis as a dispensable nuisance, it can be argued that these are in truth central to the incentive system and an underlying sense of distributive justice that do much to energize the advancement of knowledge."

Robert K. Merton, "The Matthew Effect in Science, II: Cumulative Advantage and the Symbolism of Intellectual Property," *Isis* 79(1988):621

more deplorable than cooking or trimming data, all three are intentionally misleading and deceptive.

Instances of scientific fraud have received a great deal of public attention in recent years, which may have exaggerated perceptions of its apparent frequency. Over the past few decades, several dozen cases of fraud have come to light in science. These cases represent a tiny fraction of the total output of the large and expanding research community. Of course, instances of scientific fraud may go undetected, or detected cases of fraud may be handled privately within research institutions. But there is a good reason for believing the incidence of fraud in science to be quite low. Because science is a cumulative enterprise, in which investigators test and build on the work of their predecessors, fraudulent observations and hypotheses tend eventually to be uncovered. Science could not be the successful institution it is if fraud were common. The social mechanisms of science, and in particular the skeptical review and verification of published work, act to minimize the occurrence of fraud.

The Allocation of Credit

Fraud may be the gravest sin in science, but transgressions that involve the allocation of credit and responsibility also distort the internal workings of the profession. In the standard scientific paper, credit is explicitly acknowledged in two places: at the beginning in the list of authors, and at the end in the list of references or citations (sometimes accompanied by acknowledgments). Conflicts over proper attribution can arise in both places.

Citations serve a number of purposes in a scientific paper. They acknowledge the work of other scientists, direct the reader toward additional sources of information, acknowledge conflicts with other results, and provide support for the views expressed in the paper. More broadly, citations place a paper within its scientific context, relating it to the present state of scientific knowledge.

PATENT PROCEDURES

In some areas of research, a scientist may make a discovery that has commercial potential. Patenting is a means of protecting that potential while continuing to disseminate the results of the research.

Patent applications involve such issues as ownership, inventorship, and licensing policies. In many situations, ownership of a patent is assigned to an institution, whether a university, a company, or a governmental organization. Some institutions share royalty income with the inventors. Universities and government laboratories usually have a policy of licensing inventions in a manner consistent with the public interest, at least in cases in which federal funds have supported the research.

Scientists who may be doing patentable work have an obligation to themselves and to their employers to safeguard intellectual property rights. Particularly in industry or in a national laboratory, this may involve prompt disclosure of a valuable discovery to the patent official of the organization in which the scientist works. It also entails keeping accurately dated notebook records written in ink in a bound notebook, ideally witnessed and signed by a colleague who is not a coinventor. Data scribbled in pencil on scraps of paper interleaved in loose-leaf notebooks, besides being professionally undesirable, are of no use in a patent dispute.

Under U.S. patent law, a person who invents something first can be granted a patent even if someone else files a claim first so long as witnessed laboratory records demonstrate the earlier invention. Any public disclosure of the discovery prior to filing for a U.S. patent can jeopardize worldwide patent rights.

Citations are also important because they leave a paper trail for later workers to follow in case things start going wrong. If errors crop up in a line of scientific research, citations help in tracking down the source of the discrepancies. Thus, in addition to credit, citations assign responsibility. The importance of this function is why authors should do their best to avoid citation errors, a common problem in scientific papers.

Science is both competitive and cooperative. These opposing forces tend to be played out within "invisible colleges," networks of scientists in the same specialty who read and use each other's work. Patterns of citations within these networks are convoluted and subtle. If scientists cite work by other scientists that they have used in building their own contributions, they gain support from their peers but may diminish their claims of originality. On the other hand, scientists who fail to acknowledge the ideas of others tend to find themselves excluded from the fellowship of their peers. Such exclusion can damage a person's science by limiting the informal exchange of ideas with other scientists.

It is impossible to provide a set of rules that would guarantee the proper allocation of credit in citations. But scientists have a number of reasons to be generous in their attribution. Most important, scientists have an ethical and professional obligation to give others the credit they deserve. The golden rule of enlightened self-interest is also a consideration: Scientists who expect to be treated fairly by others must treat others fairly. Finally, giving proper credit is good for science. Science will function most effectively if those who participate in it feel that they are getting the credit they deserve. One reason why science works as well as it does is that it is organized so that natural human motivations, such as the desire to be acknowledged for one's achievements, contribute to the overall goals of the profession.

Credit and Responsibility in Collaborative Research

Successful collaboration with others is one of the most rewarding experiences in the lives of most scientists. It can immensely broaden a person's scientific perspective and advance work far beyond what can be accomplished alone. But collaboration also can generate tensions between individuals and groups. Collaborative situations are far more complex now than they were a generation ago. Many papers appear with large numbers of coauthors, and a number of different laboratories may be involved, sometimes in different countries. Experts in one field may not understand in complete detail the basis of the work going on in another. Collaboration therefore requires a great deal of mutual trust and consideration between the individuals and groups involved.

One potential problem area in collaborative research involves the listing of a paper's authors. In many fields the earlier a name appears in the list of authors the greater

"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."

C. Ian Jackson, *Honor in Science*, New Haven, Conn.: Sigma Xi, The Scientific Research Society, 1984, p. 14

the implied contribution, but conventions differ greatly among disciplines and among research groups. Sometimes the scientist with the greatest name recognition is listed first, whereas in other fields the research leader's name is always last. In some disciplines, supervisors' names rarely appear on papers, while in others the professor's name appears on almost every paper that comes out of the lab. Well-established scientists may decide to list their names after those of more junior colleagues, reasoning that the younger scientists thereby receive a greater boost in reputation than they would if the order were reversed. Some research groups and journals avoid these decisions by simply listing authors alphabetically.

Frank and open discussion of the division of credit within research groups, as early in the process leading to a published paper as possible, can avoid later difficulties. Collaborators must also have a thorough understanding of the conventions in a particular field to know if they are being treated fairly.

Occasionally a name is included in a list of authors even though that person had little or nothing to do with the genesis or completion of the paper. Such "honorary authors" dilute the credit due the people who actually did the work and make the proper attribution of credit more difficult. Some scientific journals now state that a person should be listed as the author of a paper only if that person made a direct and substantial contribution to the paper. Of course, such terms as "direct" and "substantial" are themselves open to interpretation. But such statements of principle help change customary practices, which is the only lasting way to discourage the practice of honorary authorships.

As with citations, author listings establish responsibility as well as credit. When a paper is shown to contain error, whether caused by mistakes or fraud, authors might wish to disavow responsibility, saying that they were not involved in the part of the paper containing the errors or that they had very little to do with the paper in general. However, an author who is willing to take credit for a paper must also bear responsibility for its contents. Thus, unless responsibility is apportioned explicitly in a footnote or in the body of the paper, the authors whose names appear on a paper must be willing to share responsibility for all of it.

Apportioning Credit Between Junior and Senior Researchers

The division of credit can be particularly sensitive when it involves postdoctoral, graduate, or undergraduate students on the one hand and their faculty sponsors on the other. In this situation, different roles and status compound the difficulties of according recognition.

A number of considerations have to be weighed in determining the proper division of credit between a student or research assistant and a senior scientist, and a range of practices are acceptable. If a senior researcher has defined and put a project into motion and a junior researcher is invited to join in, major credit may go to the senior researcher, even if at the moment of discovery the senior researcher is not present. Just as production in industry entails more than workers standing at machines, science entails more than the single researcher manipulating equipment or solving equations. New ideas must be generated, lines of experimentation established, research funding obtained, administrators dealt with, courses taught, the laboratory kept stocked, informed consent obtained from research subjects, apparatus designed and built, and papers written and defended. Decisions about how credit is to be allotted for these and many other contributions are far from easy and require serious thought and collegial discussion. If in doubt about the distribution of credit, a researcher must talk frankly with others, including the senior scientist.

Similarly, when a student or research assistant is making an intellectual contribution to a research project, that contribution deserves to be recognized. Senior scientists are well aware of the importance of credit in the reward system of science, and junior researchers cannot be expected to provide unacknowledged labor if they are acting as scientific partners. In such cases, junior researchers may be listed as coauthors or even senior authors, depending on the work, traditions within the field, and arrangements within the team.

Plagiarism

Plagiarism is the most blatant form of misappropriation of credit. A broad spectrum of misconduct falls into this category, ranging from obvious theft to uncredited paraphrasing that some might not consider dishonest at all. In a lifetime of reading, theorizing, and experimenting, a person's work will inevitably incorporate and overlap with that of others. However, occasional overlap is one thing; systematic, unacknowledged use of the techniques, data, words or ideas of others is another. Erring on the side of excess generosity in attribution is best.

The intentional use of another's intellectual property without giving credit may seem more blameworthy than the actions of a person who claims to have plagiarized because of inattention or sloppiness. But, as in the case of fraud, the harm to the victim is the same regardless of intention. Furthermore, given the difficulty of judging intentions, the censure imposed by the scientific community is likely to be equally great.

Special care must be taken when dealing with unpublished materials belonging to others, especially with grant applications and papers seen or heard prior to publication or public disclosure. Such privileged material must not be exploited or disclosed to others who might exploit it. Scientists also must be extremely careful not to delay publication or deny support to work that they find to be competitive with their own in privileged communication. Scrupulous honesty is essential in such matters.

Even though plagiarism does not introduce spurious findings into science, outright pilfering of another's text draws harsh responses. Given the communal nature of science, the plagiarist is often discovered. If plagiarism is established, the effect can be extremely serious: All of one's work will appear contaminated. Moreover, plagiarism is illegal, and the injured party can sue.

Upholding the Integrity of Science

Perhaps the most disturbing situation that a researcher can encounter is to witness some act of scientific misconduct by a colleague. In such a case, researchers have a professional and ethical obligation to do something about it. On pragmatic grounds, the transgression may seem too distant from one's own work to take action. But assaults on the integrity of science damage all scientists, both through the effects of those assaults on the public's impression of science and through the internal erosion of scientific norms.

To be sure, "whistle-blowing" is rarely an easy route. Fulfilling the responsibilities to oneself discussed earlier in this booklet will not harm a person's career. That has not necessarily been the case with whistle-blowing. Responses by the accused person and by skeptical colleagues that cast the accuser's integrity into doubt have been all too common, though institutions have been adopting policies to minimize such reprisals.

Accusing another scientist of wrongdoing is a very serious charge that can be costly, emotionally traumatizing, and professionally damaging even if no transgression occurred. A person making such a charge should therefore be extremely careful that the claim is justified. One of the best ways to judge one's own motives and the accuracy of a charge is to discuss the situation confidentially with a trusted, experienced colleague. Many universities and other institutions have designated particular individuals to be the points of initial contact in such disputes. Institutions have also prepared written materials that offer guidance in situations involving professional ethics. In addition, Sigma Xi, the American Association for the Advancement of Science, and other scientific and engineering organizations are prepared to advise scientists who encounter cases of possible misconduct.

Once sure of the facts, the person suspected of misconduct should be contacted privately and given a chance to explain or rectify the situation. Many problems can be solved in this fashion without involving a larger forum. If these steps do not lead to a satisfactory resolution or if the case involves serious forms of misconduct, more formal proceedings will have to be initiated. For this purpose, most research institutions have developed procedures that take into account fairness for the accused, protection for the accuser, coordination with funding agencies, and requirements for confidentiality and disclosure.

Assaults on the integrity of science come from outside science as well as from within. Vocal minorities that call for a halt to whole areas of scientific research or individuals who use a few events to question the entire ethos of science can undermine the public's confidence in science, with potentially serious consequences. Just as scientists need to protect the workings of science from internal erosion, they have an obligation to meet unjustified or exaggerated attacks from without with sound and persistent arguments.

The scientist in society

This discussion has concentrated on the responsibilities of scientists to themselves and their colleagues, but scientists have obligations to the broader society as well. These obligations are most apparent when scientific research intersects directly with broader societal concerns, as in the protection of the environment, the humane treatment of laboratory animals, or the informed consent of human experimental subjects. Such obligations are also common in applied research, in which the products of scientific investigation can have a direct and immediate impact on people's lives.

Scientists conducting basic research also need to be aware that their work ultimately may have a great impact on society. World-changing discoveries can emerge from seemingly arcane areas of science. The construction of the atomic bomb and the development of recombinant DNA, events that grew out of research into the nucleus of the atom and investigations of certain bacterial enzymes, respectively, are two examples. The occurrence and consequences of discoveries in basic research are virtually impossible to foresee. Nevertheless, the scientific community must recognize the potential for such discoveries and be prepared to address the questions that they raise. The response of biologists to the development of recombinant DNA—first calling for a temporary moratorium on the research and then setting up a regulatory mechanism to ensure its safety—is an excellent example of researchers exercising these responsibilities.

This document cannot hope to describe the diverse responsibilities—and associated opportunities—that scientists encounter as members of society. The bibliography lists several volumes that examine the social roles of scientists in detail. The important point is that science and technology have become such integral parts of society that scientists can no longer abstract themselves from societal concerns. Nearly half of the bills that come before the U.S. Congress have a significant scientific or technological component. The problems facing modern society cannot be solved solely on the basis of scientific information, because they involve social and political processes over which science has no control (though the social sciences can analyze those processes). Nevertheless, science has important contributions to offer in addressing many of society's problems, and scientists will be called upon to make those contributions.

Scientists who become involved with the public use of scientific knowledge have to take time away from work to meet with community groups, serve on committees, talk with the press. Many scientists enjoy these activities; others see them simply as distractions from research. But dealing with the public is a fundamental responsibility for the scientific community. Concern and involvement with the broader uses of scientific knowledge are essential if scientists are to retain the public's trust.

Interacting with nonscientists also serves a less tangible but still important function. Many people harbor misconceptions about the nature and aims of science. They believe it to be a cold, impersonal search for a truth devoid of human values. Scientists know these misconceptions are mistaken, but the misconceptions can be damaging. They can influence the way scientists are treated by others, discourage young people from pursuing interests in science, and, at worst, distort the science-based decisions that must be made in a technological society.

Scientists must work to counter these feelings. They should not disguise the human factors that motivate and sustain research or the value judgments that inevitably influence science. They should explain and defend the scientific worldview, a prospect of great beauty and grandeur that ought to be a part of how people think about themselves and their place in nature. Scientific research is an intensely human endeavor. This humanity must not be lost in the face science presents to the world.

"Concern for man himself and his fate must always form the chief interest of all technical endeavors . . . in order that the creations of our minds shall be a blessing and not a curse to mankind. Never forget this in the midst of your diagrams and equations."

Albert Einstein in an address to the students of the California Institute of Technology

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