On Being A Scientist: Responsible Conduct in Research

COMMITTEE ON SCIENCE, ENGINEERING, AND PUBLIC POLICY

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The Social Foundations of Science

Throughout the history of science, philosophers and scientists have sought to describe a single systematic procedure that can be used to generate scientific knowledge, but they have never been completely successful. The practice of science is too multifaceted and its practitioners are too diverse to be captured in a single overarching description. Researchers collect and analyze data, develop hypotheses, replicate and extend earlier work, communicate their results with others, review and critique the results of their peers, train and supervise associates and students, and otherwise engage in the life of the scientific community.

Science is also far from a self-contained or self- sufficient enterprise. Technological developments critically influence science, as when a new device, such as a telescope, microscope, rocket, or computer, opens up whole new areas of inquiry. Societal forces also affect the directions of research, greatly complicating descriptions of scientific progress.

Another factor that confounds analyses of the scientific process is the tangled relationship between individual knowledge and social knowledge in science. At the heart of the scientific experience is individual insight into the workings of nature. Many of the outstanding achievements in the history of science grew out of the struggles and successes of individual scientists who were seeking to make sense of the world.

At the same time, science is inherently a social enterprise-in sharp contrast to a popular stereotype of science as a lonely, isolated search for the truth. With few exceptions, scientific research cannot be done without drawing on the work of others or collaborating with others. It inevitably takes place within a broad social and historical context, which gives substance, direction, and ultimately meaning to the work of individual scientists.

The object of research is to extend human knowledge of the physical, biological, or social world beyond what is already known. But an individual's knowledge properly enters the domain of science only after it is presented to others in such a fashion that they can independently judge its validity. This process occurs in many different ways. Researchers talk to their colleagues and supervisors in laboratories, in hallways, and over the telephone. They trade data and speculations over computer networks. They give presentations at seminars and conferences. They write up their results and send them to scientific journals, which in turn send the papers to be scrutinized by reviewers. After a paper is published or a finding is presented, it is judged by other scientists in the context of what they already know from other sources. Throughout this continuum of discussion and deliberation the ideas of individuals are collectively judged, sorted, and selectively incorporated into the consensual but ever evolving scientific worldview. In the process, individual knowledge is gradually converted into generally accepted knowledge.

This ongoing process of review and revision is critically important. 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 try to convince their ablest colleagues.

The social mechanisms of science do more than validate what comes to be known as scientific knowledge. They also help generate and sustain the body of experimental techniques, social conventions, and other "methods" that scientists use in doing and reporting research. Some of these methods are permanent features of science; others evolve over time or vary from discipline to discipline. Because they reflect socially accepted standards in science, their application is a key element of responsible scientific practice.

Experimental Techniques and the Treatment of Data

One goal of methods is to facilitate the independent verification of scientific observations. Thus, many experimental techniques-such as statistical tests of significance, double-blind trials, or proper phrasing of questions on surveys-have been designed to minimize the influence of individual bias in research. By adhering to these techniques, researchers produce results that others can more easily reproduce, which promotes the acceptance of those results into the scientific consensus.

If research in a given area does not use generally accepted methods, other scientists will be less likely to accept the results. This was one of several reasons why many scientists reacted negatively to the initial reports of cold fusion in the late 1980s. The claims were so physically implausible that they required extraordinary proof. But the experiments were not initially presented in such a way that other investigators could corroborate or disprove them. When the experimental techniques became widely known and were replicated, belief in cold fusion quickly faded.

In some cases the methods used to arrive at scientific knowledge are not very well defined. Consider the problem of distinguishing the "facts" at the forefront of a given area of science. In such circumstances experimental techniques are often pushed to the limit, the signal is difficult to separate from the noise, unknown sources of error abound, and even the question to be answered is not well defined. In such an uncertain and fluid situation, picking out reliable data from a mass of confusing and sometimes contradictory observations can be extremely difficult.

In this stage of an investigation, researchers have to be extremely clear, both to themselves and to others, about the methods being used to gather and analyze data. Other scientists will be judging not only the validity of the data but also the validity and accuracy of the methods used to derive those data. The development of new methods can be a controversial process, as scientists seek to determine whether a given method can serve as a reliable source of new information. If someone is not forthcoming about the procedures used to derive a new result, the validation of that result by others will be hampered.

Methods are important in science, but like scientific knowledge itself, they are not infallible. As they evolve over time, better methods supersede less powerful or less acceptable ones. Methods and scientific knowledge thus progress in parallel, with each area of knowledge contributing to the other.

A good example of the fallibility of methods occurred in astronomy in the early part of the twentieth century. One of the most ardent debates in astronomy at that time 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 from the earth that individual stars could not be distinguished. Others believed that they were clouds of gas within our own galaxy.

One astronomer who thought that spiral nebulae were within the Milky Way, 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 van Maanen's procedures have not revealed any intentional misrepresentation or sources of systematic error. Rather, he was working at the limits of observational accuracy, and his expectations influenced his measurements.

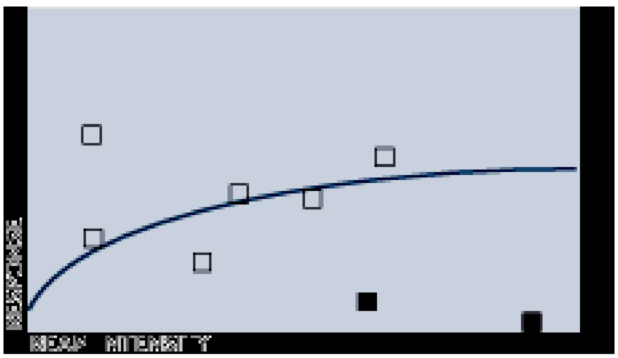
Though van Maanen turned out to be wrong, he was not ethically at fault. He was using methods that were accepted by the astronomical community as the best available at the time, and his results were accepted by most astronomers. But in hindsight he relied on a technique so susceptible to observer effects that even a careful investigator could be misled.

The fallibility of methods is a valuable reminder of the importance of skepticism in science. Scientific knowledge and scientific methods, whether old or new, must be continually scrutinized for possible errors. Such skepticism can conflict with other important features of science, such as the need for creativity and for conviction in arguing a given position. But organized and searching skepticism as well as an openness to new ideas are essential to guard against the intrusion of dogma or collective bias into scientific results.

THE SELECTION OF DATA

Deborah, a third-year graduate student, and Kathleen, a postdoc, have made a series of measurements on a new experimental semiconductor material using an expensive neutron source at a national laboratory. When they get back to their own laboratory and examine the data, they get the following data points. A newly proposed theory predicts results indicated by the curve.

During the measurements at the national laboratory, Deborah and Kathleen observed that there were power fluctuations they could not control or predict. Furthermore, they discussed their work with another group doing similar experiments, and they knew that the other group had gotten results confirming the theoretical prediction and was writing a manuscript describing their results.



In writing up their own results for publication, Kathleen suggests dropping the two anomalous data points near the abscissa (the solid squares) from the published graph and from a statistical analysis. She proposes that the existence of the data points be mentioned in the paper as possibly due to power fluctuations and being outside the expected standard deviation calculated from the remaining data points. "These two runs," she argues to Deborah, "were obviously wrong."

- 1. How should the data from the two suspected runs be handled?
- 2. Should the data be included in tests of statistical significance and why?
- 3. What other sources of information, in addition to their faculty advisor, can Deborah and Kathleen use to help decide?

Discussion:

Deborah and Kathleen's principal obligation, in writing up their results for publication, is to describe what they have done and give the basis for their actions. They must therefore examine how they can meet this obligation within the context of the experiment they have done. Questions that need to be answered include: If the authors state in the paper that data have been rejected because of problems with the power supply, should the data points still be included in the published chart? Should statistical analyses be done that both include and exclude the questionable data? If conventions within their discipline allow for the use of statistical devices to eliminate outlying data points, how explicit do Deborah and Kathleen need to be in the published paper about the procedures they have followed?

Publication and Openness

Science is not an individual experience. It is shared knowledge based on a common understanding of some aspect of the physical or social world. For that reason, the social conventions of science play an important role in establishing the reliability of scientific knowledge. If these conventions are disrupted, the quality of science can suffer.

Many of the social conventions that have proven so effective in science arose during the birth of modern science in the latter half of the seventeenth century. At that time, many scientists sought to keep their work secret so that others could not claim it as their own. Prominent figures of the time, including Isaac Newton, were loathe to convey news of their discoveries for fear that someone else would claim priority‹a fear that was frequently realized.

The solution to the problem of making new discoveries public while assuring their author¹s 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 society¹s Philosophical Transactions as well as the official support of the society if the author¹s priority was brought into question. Oldenburg also pioneered the practice of sending submitted manuscripts to experts who could judge their quality. Out of these innovations rose both the modern scientific journal and the practice of peer review.

The continued importance of publication in learned journals accounts for the convention that the first to publish a view or finding, not the first to discover it, tends to get most of the credit for the discovery. Once results are published, they can be freely used by other researchers to extend knowledge. But until the results become common knowledge, people who use them are obliged to recognize the discoverer through citations. In this way scientists are rewarded through peer recognition for making results public.

Before publication, different considerations apply. If someone else exploits unpublished material that is seen in a privileged grant application or manuscript, that person is essentially stealing intellectual property. In industry the commercial rights to scientific work belong more to the employer than the employee, but similar provisions apply: research results are privileged until they are published or otherwise publicly disseminated.

Many scientists are 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. But scientists are not expected to make their data and thinking available to others at all times. During the initial stages of research, a scientist deserves a period of privacy in which data are not subject to disclosure. This privacy allows individuals to advance their work to the point at which they have confidence both in its accuracy and its meaning.

After publication, scientists expect that data and other research materials will be shared with qualified colleagues upon request. Indeed, a number of federal agencies, journals, and professional societies have established policies requiring the sharing of research materials. 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 share research materials with qualified colleagues runs the risk of not being trusted or respected. In a profession where so much depends on interpersonal interactions, the professional isolation that can follow a loss of trust can damage a scientist¹s work.

Publication in a peer-reviewed journal remains the standard means of disseminating scientific results, but other methods of communication are subtly altering how scientists divulge and receive information. Posters, abstracts, lectures at professional gatherings, and proceedings volumes are being used more often to present preliminary results before full review. Preprints and computer networks are increasing the ease and speed of scientific communications. These new methods of communication are in many cases just elaborations of the informal exchanges that pervade science. To the extent that they speed and improve communication and revision, they will strengthen science. But if publication practices, either new or traditional, bypass quality control mechanisms, they risk weakening conventions that have served science well.

An example is the scientist who releases important and controversial results directly to the public before submitting them to the scrutiny of peers. If the researcher has made a mistake or the findings are misinterpreted by the media or the public, the scientific community and the public may react adversely. When such news is to be released to the press, it should be done when peer review is complete-normally at the time of publication in a scientific journal.

Sometimes researchers and the institutions sponsoring research have different interests in making results public. For example, a scientist doing research sponsored by industry may want to publish results quickly, while the industrial sponsor may want to keep results private‹at least temporarily‹to establish intellectual property rights prior to disclosure. Research institutions and government agencies have started to adopt explicit policies to reduce conflicts over such issues of ownership and access.

In research that has the potential of being financially profitable, openness can be maintained by the granting of patents. Patents enable an individual or institution to profit from a scientific discovery in return for making the results public. Scientists who may be doing patentable work have special obligations to the sponsors of that work. For example, they may need to have their laboratory notebooks validated and dated by others. They may also have to disclose potentially valuable discoveries promptly to the patent official of the organization sponsoring the research.

In some situations, such as proprietary research sponsored by industry or militarily sensitive research, openness in disseminating research results may not be possible. Scientists working under such conditions may need to find other ways of exposing their work to professional scrutiny. Unclassified summaries of classified work can compensate for the lack of open scrutiny that allows the validation of results elsewhere in science. Properly structured visiting committees can examine proprietary or classified research while maintaining confidentiality.

THE SHARING OF RESARCH MATERIALS

Ed, a fourth-year graduate student, was still several months away from finishing an ongoing research project when a new postdoc arrived from a laboratory doing similar work. After the two were introduced, Ed automatically asked about the work going on in the other lab and was surprised to hear that researchers there had successfully developed a reagent that he was still struggling to perfect. Knowing that both labs had policies requiring the sharing of research materials, Ed wrote a letter to the head of the other lab asking if the laboratory could share some of the reagent with him. He didn't expect there to be a problem, because his project was not in competition with the work of the other lab, but a couple of weeks later he got a letter from the lab director saying that the reagent could not be shared because it was still "poorly developed and characterized."

The new postdoc, upon hearing the story, said, "That's ridiculous. They just don't want to give you a break."

- 1. Where can Ed go for help in obtaining the materials?
- 2. Are there risks in involving other people in this situation?
- 3. What kinds of information is it appropriate for researchers to share with their colleagues when they change laboratories?

Discussion:

After a research material like a reagent has been described in a publication, sharing that material speeds and in some cases enables the replication of results and therefore contributes to the progress of science. But the reagent in this situation has not yet been described in a published paper, so the provisions for sharing it are different. Ed needs to consider the other laboratory's legitimate interest in developing that material and establishing how it works before publication. He also needs to consider the relationship between the two laboratories. If he turns to his faculty advisor for help in acquiring the reagent, how is his advisor likely to respond? Is there any way he can work with the other laboratory and thereby come a step closer to forming an agreement with them about the use of the reagent?

Misconduct in Science

Beyond honest errors and errors caused through negligence are a third category of errors: those that involve deception. Making up data or results (fabrication), changing or misreporting data or results (falsification), and using the ideas or words of another person without giving appropriate credit (plagiarism)- all strike at the heart of the values on which science is based. These acts of scientific misconduct not only undermine progress but the entire set of values on which the scientific enterprise rests. Anyone who engages in any of these practices is putting his or her scientific career at risk. Even infractions that may seem minor at the time can end up being severely punished.

The ethical transgressions discussed in earlier sections-such as misallocation of credit or errors arising from negligence-are matters that generally remain internal to the scientific community. Usually they are dealt with locally through the mechanisms of peer review, administrative action, and the system of appointments and evaluations in the research environment. But misconduct in science is unlikely to remain internal to the scientific community. Its consequences are too extreme: it can harm individuals outside of science (as when falsified results become the basis of a medical treatment), it squanders public funds, and it attracts the attention of those who would seek to criticize science. As a result, federal agencies, Congress, the media, and the courts can all get involved.

Within the scientific community, the effects of misconduct-in terms of lost time, forfeited recognition to others, and feelings of personal betrayal-can be devastating. Individuals, institutions, and even entire research fields can suffer grievous setbacks from instances of fabrication, falsification, or plagiarism even if they are only tangentially associated with the case.

When individuals have been accused of scientific misconduct in the past, the institutions responsible for responding to those accusations have taken a number of different approaches. In general, the most successful responses are those that clearly separate a preliminary investigation to gather information from a subsequent adjudication to judge guilt or innocence and issue sanctions if necessary. During the adjudication stage, the individual accused of misconduct has the right to various due process protections, such as reviewing the evidence gathered during the investigation and cross- examining witnesses.

In addition to falsification, fabrication, and plagiarism, other ethical transgressions directly associated with research can cause serious harm to individuals and institutions. Examples include cover-ups of misconduct in science, reprisals against whistleblowers, malicious allegations of misconduct in science, and violations of due process in handling complaints of misconduct in science. Policymakers and scientists have not decided whether such actions should be considered misconduct in science-and therefore subject to the same procedures and sanctions as falsification, fabrication, and plagiarism-or whether they should be investigated and adjudicated through different channels. Regulations adopted by the National Science Foundation and the Public Health Service define misconduct to include "other serious deviations from accepted research practices," in addition to falsification, fabrication, and plagiarism, leaving open the possibility that other actions could be considered misconduct in science. The problem with such language is that it could allow a scientist to be accused of misconduct for using novel or unorthodox research methods, even though such methods are sometimes needed to proceed in science. Federal officials respond by saying that this language is needed to prosecute ethical breaches that do not strictly fall into the categories of falsification, fabrication, or plagiarism and that no scientist has been accused of misconduct on the basis of using unorthodox research methods. This area of science policy is still evolving.

Another category of behaviors-including sexual or other forms of harassment, misuse of funds, gross negligence in a person's professional activities, tampering with the experiments of others or with instrumentation, and violations of government research regulations-are not necessarily associated with scientific conduct. Institutions need to discourage and respond to such behaviors. But these behaviors are subject to generally applicable legal and social penalties and should be dealt with using the same procedures that would be applied to anyone.

FABRICATION IN A GRANT APPLICATION

Don is a first-year graduate student applying to the National Science Foundation for a predoctoral fellowship. His work in a lab where he did a rotation project was later carried on successfully by others, and it appears that a manuscript will be prepared for publication by the end of the summer. However, the fellowship application deadline is June 1, and Don decides it would be advantageous to list a publication as "submitted." Without consulting the faculty member or other colleagues involved, Don makes up a title and author list for a "submitted" paper and cites it in his application.

After the application has been mailed, a lab member sees it and goes to the faculty member to ask about the "submitted" manuscript. Don admits to fabricating the submission of the paper but explains his actions by saying that he thought the practice was not uncommon in science.

The faculty members in Don's department demand that he withdraw his grant application and dismiss him from the graduate program. After leaving the university, Don applies for a master's degree, since he has fulfilled the course requirements. Although the department votes not to grant him a degree, the university administration does so because it is not stated in the university graduate bulletin that a student in Don's department must be in "good standing" to receive a degree. They fear that Don will bring suit against the university if the degree is denied. Likewise, nothing will appear in Don's university transcript regarding his dismissal.

- 1. Do you agree with Don that scientists often exaggerate the publication status of their work in written materials?
- 2. Do you think the department acted too harshly in dismissing Don from the graduate program?
- 3. Do you believe that being in "good standing" should be a prerequisite for obtaining an advanced degree in science? If Don later applied to a graduate program at another institution, does that institution have the right to know what happened?

A CASE OF PLAGIARISM

May is a second-year graduate student preparing the written portion of her qualifying exam. She incorporates whole sentences and paragraphs verbatim from several published papers. She does not use quotation marks, but the sources are suggested by statements like "(see . . . for more details)." The faculty on the qualifying exam committee note inconsistencies in the writing styles of different paragraphs of the text and check the sources, uncovering May's plagiarism.

After discussion with the faculty, May's plagiarism is brought to the attention of the dean of the graduate school, whose responsibility it is to review such incidents. The graduate school regulations state that "plagiarism, that is, the failure in a dissertation, essay, or other written exercise to acknowledge ideas, research or language taken from others" is specifically prohibited. The dean expels May from the program with the stipulation that she can reapply for the next academic year.

- 1. Is plagiarism like this a common practice?
- 2. Are there circumstances that should have led to May's being forgiven for plagiarizing?
- 3. Should May be allowed to reapply to the program?