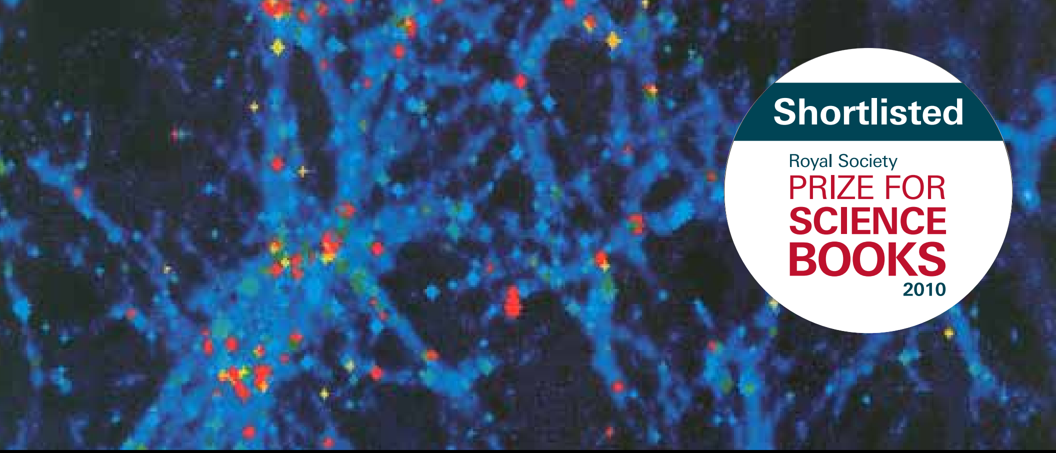

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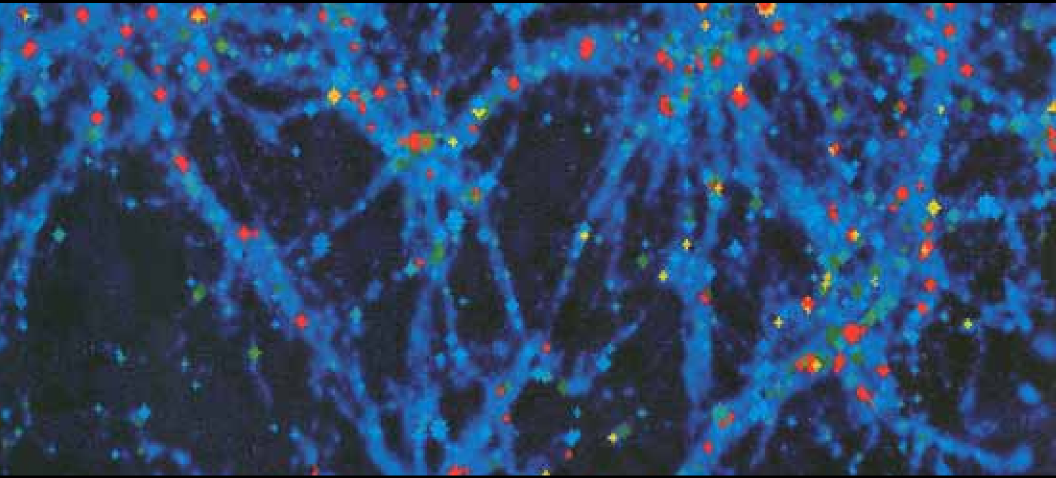
2010

Everyday Practice of Science

WHERE INTUITION AND PASSION MEET OBJECTIVITY AND LOGIC



Frederick Grinnell

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An Overview

Every year the U.S. National Science Foundation publishes a comprehensive analysis of *Science and Engineering Indicators*. As long as I can remember, the chapter on public attitudes contrasts two key points. First, Americans have a highly favorable opinion of science and technology. Second, Americans lack an understanding of basic scientific facts and concepts and are unfamiliar with the scientific process. Astronomer Carl Sagan called the situation "a clear prescription for disaster": "We live in a society exquisitely dependent on science and technology, in which hardly anyone knows anything about science and technology" (1).

In this chapter, I present an overview of the scientific process—what I call *everyday practice of science*. All of us practicing science face common problems: what to do, when to do it, how to do it, who should pay for it, and—after the work is completed—what the findings mean. I hope to provide some general insights throughout this book about these issues. Most of the examples I use come from biomedical research. If one wants “to piece together an account of what scientists actually do,” wrote Nobel Laureate Sir Peter Medawar,

> then the testimony of biologists should be heard with specially close attention. Biologists work very close to the frontier between bewilderment and understanding. Biology is complex, messy and richly various, like real life. . . . It should therefore give us a specially direct and immediate insight into science in the making. (2)

I want to distinguish everyday practice from the idealized linear model of research. According to the linear model, the path from hypothesis to discovery follows a direct line guided by objectivity and logic. Facts about the world are there waiting to be observed and collected. The scientific method is used to make discoveries. Researchers are dispassionate and objective.

Although representative of the way that we teach science, I believe the linear model corresponds to a mythical account—or at least a significant distortion—of everyday practice. Rather than linear, the path to discovery in everyday practice is ambiguous and convoluted with lots of dead ends. Success requires converting those dead ends into new, exciting starts. Real-life researchers may aim to be dispassionate and objective, but they work within the context of particular life interests and commitments.

The two conversations of science

Figure 1.1 diagrams everyday practice of science. I place the individual scientist in the center. She engages in two conversations, one with the world to be studied, and the other with other members of the research community. The former conversation gives rise to the circle of discovery—learning new things. The latter gives rise

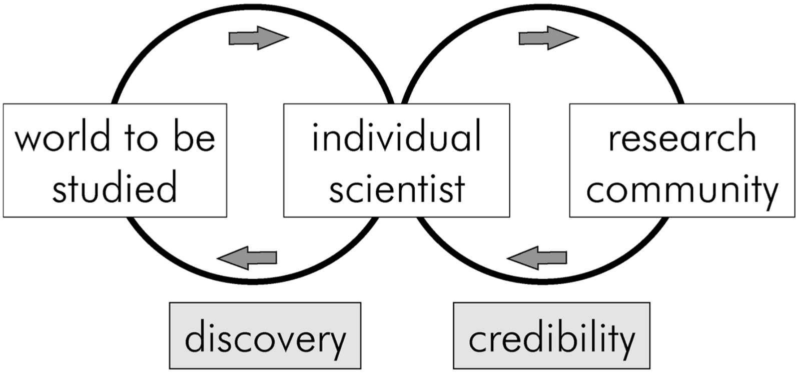

Figure 1.1. Everyday Practice of Science

to the circle of credibility—trying to convince others that the new findings are correct. These conversations are dialogs that proceed in an iterative manner. Of course, figure 1.1 is highly simplified because there are many conversations going on simultaneously. In addition, the researcher interacts only with a small part of the world, and the scientific community is itself within the world. Nevertheless, making the artificial distinctions in figure 1.1 helps to emphasize that there are important differences between these conversations. Interactions with the world typically are limited to making observations and carrying out experiments. Interactions within the research community depend largely on cooperative and competitive behavior.

Who is the individual scientist in figure 1.1? To help answer that question, I will introduce two imaginary researchers: Professor-It-Could-Be-Anybody and Professor-Somebody-In-Particular. Professor Anybody is the idealized researcher who does science according to the linear model. Professor Anybody is the scientist found in textbooks and research publications. Professor Particular, on the other hand, is the researcher engaged in everyday practice. Professor Particular experiences science as an adventure, so much so that she might write an autobiographical essay called "How to Get Paid for Having Fun" (3). A lot of us doing science feel just that way.

Science textbooks and research publications exclude everyday practice

There is no place for Professor Particular in the idealized structure of science. Sociologist Robert Merton described the norms of science as universalism, communism, disinterestedness, and organized skepticism (4). *Universalism* means that scientific claims are independent of the personal or social interests of researchers. *Communism* means that everyone owns scientific knowledge. *Disinterestedness* means that the community suppresses any tendency of investigators to behave according to their own self-interests. *Organized skepticism* means that researchers suspend and replace personal beliefs with an attitude oriented toward empirical and logical criteria. Merton's norms describe perfectly the characteristics of Professor Anybody: independent of personal or social interests, knowledge owned by everyone, disinterested, personal beliefs suspended.

It is Professor Anybody rather than Professor Particular who can be found in science textbooks. Textbooks usually present facts without clarifying where and how they arise. Space limitations may make this omission necessary. The consequence is that practice becomes invisible. The more common the knowledge, the more anonymous will be its source. Years of research are compressed into one or several sentences. At the same time, the adventure, excitement, and risks of real-life discovery disappear.

Research publications also mask the work of Professor Particular. To emphasize this point, I will describe some of the history of how researchers discovered messenger RNA. To understand this example, the following facts found in most modern biology textbooks will be useful:

- All cells store their genetic information in double-stranded molecules of deoxyribonucleic acid (DNA).
- Different cell types transcribe different portions of the sequence into specific messenger RNAs (mRNAs).
- These mRNAs then are processed and translated to make the proteins that determine in large part the specialized features of different cell types.
- Taken together, these steps represent the classic molecular information pathway of modern molecular genetics:

$$\text{DNA} \Rightarrow \text{mRNA} \Rightarrow \text{protein}$$

When a biology textbook states that mRNA is the intermediate between DNA and protein, the textbook sometimes adds a footnote to a 1961 research paper published in the prestigious scientific journal *Nature*. Evidence for the intermediary role of mRNA appeared first in the *Nature* paper (5). Research papers such as the publication in *Nature* provide the formal mechanism by which investigators report the details of their discovery claims to the scientific community.

The 1961 *Nature* paper about mRNA is titled "An Unstable Intermediate Carrying Information from Genes to Ribosomes for Protein Synthesis." The paper begins by summarizing prevailing views and controversies on the subject. Then the paper suggests a new hypothesis to resolve the controversial issues: "*A priori*, three types of hypothesis may be considered to account for the known facts." Experiments are proposed that could distinguish between the possibilities. Studies carried out are described. Conclusions are drawn from the results. The discovery claim is presented. Conceptually, the paper is arranged according to the sequence:

> prevailing views ⇒ issues requiring further understanding ⇒ testable new hypothesis ⇒ experimental design ⇒ results ⇒ confirmation of one hypothesis and falsification of others

This sequence conforms to the linear model of science and gives rise to a paper whose plot will be none other than the scientific method. This plot is not, however, the way things actually happened. Rather, the scientific method represents a formal structure imposed upon what actually happened. "Writing a paper" wrote Nobel Laureate François Jacob—one of the authors of the *Nature* paper—"is to substitute order for the disorder and agitation that animate life in the laboratory. . . . To replace the real order of events and discoveries by what appears as the logical order, the one that should have been followed if the conclusions were known from the start" (6).

Stated otherwise, a research paper converts the process of discovery into an announcement of the discovery. In a sense, the paper itself becomes the discovery claim (7). Rather than discoverers, researchers become reporters of discoveries. They write "the (or these) data show" far more often than "our data show." Even if written in a personalized fashion, underlying every research report is the implication that that any scientist could have done the experiments and made the discovery.

Because the linear model of science typifies how scientists communicate with each other when they make public their research, the misimpression easily can arise that science actually proceeds in this fashion. Autobiographical writings of researchers provide a different perspective. In the case of the mRNA discovery, Jacob's view of what actually happened can be found in his memoir *The Statue Within*. Below are several quotes from Jacob's book followed by brief comments to highlight important features of everyday practice.

> *We were to do very long, very arduous experiments. . . . But nothing worked. We had tremendous technical problems.* (6)

In everyday practice, experiments can be divided into three classes: heuristic, from which we learn something new; demonstrative, which we publish—often repetition and refinement of heuristic experiments; and failure, which includes Jacob's "nothing worked." Not surprisingly, failed experiments represent the largest class. Failed experiments arise for many reasons, including methodological limitations, flawed design, and mistaken hypotheses. Experimental failures are part of the normal process of science. Even inconclusive or uninterpretable results can still be extremely valuable if they challenge the researcher's previous assumptions and teach her what not to do the next time.

> *Full of energy and excitement, sure of the correctness of our hypothesis, we started our experiment over and over again. Modifying it slightly. Changing some technical detail.* (6)

The objective and disinterested researcher envisioned by idealized science would never be "sure of the correctness" of an unproven hypotheses. Nevertheless, investigators' intuitions based on their previous knowledge and experience sometimes lead them to continue to believe in and pursue a hypothesis even when the hypothesis appears to be contradicted by the experimental results.

> *Eyes glued to the Geiger counter, our throats tight, we tracked each successive figure as it came to take its place in exactly the order we had been expecting. And as the last sample was counted, a double shout of joy shook the basement at Caltech. Followed immediately by a wild double jig.* (6)

The exhilarating experience of success! Solving a challenging puzzle and being the first to know the answer can elicit a degree of excitement and enthusiasm uncharacteristic of serious grown men and women at work. When my seventh-grade science teacher, Mr. Perkins, told me "science is serious play," he was not exaggerating.

In summary, science comes in three different versions: (*i*) the facts—statements found in scientific textbooks—with little if any explanation of their source; (*ii*) the linear model—found in research publications and used by researchers to establish the credibility of their work and to influence the work of others; (*iii*) everyday practice—what really happened, a view rarely glimpsed by outsiders.

Science studies

In contrast to Merton's description of the idealized structure of science, philosopher Thomas Kuhn focused on individuals and their practices. Kuhn's book *Structure of Scientific Revolutions* had a great impact on development of the field called *science studies*. Rather than the idealized norms of science, the actual practices of individual scientific researchers and research teams became the focus of anthropologists, historians, philosophers, and sociologists, who together developed the field of science studies (e.g., 8, 9).

Kuhn described *paradigms* in science as sets of beliefs and values shared by members of a scientific community and as established and acceptable ways of problem solving (10). In addition, Kuhn emphasized that, beyond these criteria shared by the community, scientific judgment depends on individual biography and personality (11). Writers from other backgrounds also have emphasized the importance of individual biography and personality on how a researcher practices science. Examples include the *schemata* of psychologist Jean Piaget (12), *thought styles* described by physician-immunologist Ludwik Fleck (13), scientist-philosopher Michael Polanyi's *tacit knowledge* (14), and historian Gerald Holton's *thematic presuppositions* (15).

According to this way of thinking, the researcher's understanding of things is not simply given. Rather, understanding requires interpretation of experience. Interpretation takes place within the framework of one's life situation. Prior knowledge and interests influence what the person experiences, what she thinks the experiences mean, and the subsequent actions that she takes. Unlike idealized science, everyday practice can accommodate the remark that author Steve Martin has Einstein make to Picasso in Martin's play *Picasso at the Lapin Agile*: "What I just said is the fundamental end-all, final, not-subject-to-opinion absolute truth, depending on where you're standing" (16).

After *Structure of Scientific Revolutions*, science increasingly became of interest to study as an individual human activity characterized by, among other things, social and political aims. Given its potential impact on the world, understanding these aims would seem to be essential. Consider, for instance, questions that have been raised by the feminist movement (17). Upper-middle-class, white males have dominated science in the past and continue to do so in the present. Does this lack of gender diversity among researchers in the scientific workforce make a difference in the practice of science? If so, what difference? When it comes to getting a job, being promoted, or getting an equal salary, much evidence suggests that absence of role models and mentors has acted as a diversity barrier in science and engineering fields. Will lack of diversity also influence how science is practiced or what science is practiced?

Objectivity and the research community

Some people believe that the effect of cultural biases is limited to what is studied and not the conclusions reached by the research community. In his book *The Mismeasure of Man*, evolutionary biologist

Stephen Jay Gould argues otherwise. He uses historical examples from the late nineteenth and early twentieth centuries to describe how racist and sexist cultural attitudes influenced not only research design but also interpretation. Science progresses, wrote Gould,

> by hunch, vision, and intuition. Much of its change through time does not record a closer approach to absolute truth but the alteration of cultural contexts that influence it so strongly. Facts are not pure and unsullied bits of information; culture also influences what we see and how we see it. (18)

Gould's use of the expression *absolute truth* reflects the important distinction between truth (small "t") as we now understand things and Truth (capital "T") that no further experience will change. I will emphasize frequently that everyday practice of science is after truth. Science always is a work in progress, which makes the process exciting and challenging. Anyone who claims to know already the Truth of a matter must be depending on sources of information outside everyday practice of science.

An important example of cultural bias comes from the history of psychiatry. Until the early 1970s, homosexuality was viewed widely as an illness and was listed as such in the 1968 version of the *Diagnostic and Statistical Manual of Mental Disorders* (*DSM-II*) of the U.S. psychiatric community. That the diagnostic classification was political rather than scientific is shown by how the classification was changed. In 1973, the board of directors of the American Psychiatric Association *voted* that homosexuality was not an illness. Membership ratified that vote a few months later (19). With the link between homosexuality and psychopathology discredited, homosexual couples increasingly have been accorded the same rights and respect as heterosexual couples. Now those who oppose homosexuality, and many still do so, can less easily appeal to "scientific/medical facts" to support their objections.

For some, admitting the human associations of science can challenge the belief that science provides an objective description of reality. Especially at the fringe of the so-called postmodernist movement, the argument has been put forth that scientific facts are merely culture-dependent, normative beliefs. If there is truth to be learned, then scientific inquiry deserves no privileged status. Truth-for-the-individual likely is the best for which one may hope.

The postmodernists are wrong. Culture may influence what we see and how we see it, but the dramatic impact of technology on the world shows that much of scientific knowledge is more than mere belief. Throughout history, we humans have been attempting to overcome natural threats to our existence, such as famine and disease. Beginning with the discovery and use of fire and the invention of primitive tools, controlling and changing the environment has been a central human project. The ability of science to produce technologies with increasing impact on the world suggests that science's understanding of the physical mechanisms of the world has advanced.

So here is a paradox. How can practice of science situated within a particular cultural context give rise to knowledge that has universal validity? How does Professor Particular become Professor Anybody?

My way to begin to answer this question is by comparing scientific researchers with baseball umpires. According to tradition, there are three types of baseball umpires:

> The first type says, "I call balls and strikes as they are."
>
> The second says, "I call them as I see them."
>
> The third says, "What I call them is what they become."

What distinguishes these umpires is not the situations in which they find themselves, but rather the attitudes that they bring to their work. Because of their different attitudes, they practice umpiring differently. The first emphasizes Truth; the second, context; the third, power.

Those who have learned the idealist, linear view of science frequently identify researchers with the first type of umpire. Postmodernists identify researchers with the third. Further description of discovery and credibility will clarify why the second type of umpire corresponds most closely to the way that scientists work.

In everyday practice, discovery begins in community. Community offers continuity with the past and interconnectedness of the present. Each researcher or group of researchers initiates work in the context of prevailing experiences and beliefs—the starting point and justification for further action. We assume that this previous knowledge is incomplete or to some degree incorrect. There is little reward in science for simply duplicating and confirming what others already have done. What we aim for is *new-search* rather than *re-search.*

What I am focusing on here is discovery at the frontier of knowledge, a place where no one has been before. At the frontier, one encounters an ambiguous world demanding risky choices. What should be done first? What is the difference between data and noise? How does one recognize something without knowing in advance how it looks? Of course, not all research occurs at the frontier. Clinical investigation involving humans should begin only in much more settled territory after a great deal of preclinical work has been accomplished. The ethics of research with humans demands that the work be as unambiguous as possible.

At the edge of knowledge, incomplete understanding can result in mistaken assumptions and errors in experimental design. At the same time, incomplete understanding sometimes permits observation of unexpected results. Nobel Laureate Max Delbrück called the latter aspect of research the *principle of limited sloppiness* (20). Here, sloppiness does not refer to technical error, although some important discoveries have their origins in just that fashion. Rather, Delbrück meant sloppiness in the sense that our conceptual understanding of a system under investigation is frequently a little muddy. Consequently, experimental design sometimes tests unplanned questions, as well as those explicitly thought to be under consideration. Unexpected results can emerge and lead to important findings if the experimenter notices (21). *We do more than we intend*. The underlying ambiguity of practice makes what we call luck or serendipity a frequent feature of discovery.

Because Professor Particular cannot avoid the possibility of error, including self-deception, her initial discoveries should be thought of as *protoscience*. For protoscience to become science, the researcher not only must be able to replicate her own work, but also must turn to the community to convince peers of the correctness of the new findings. Professor Particular overcomes her subjectivity through *intersubjectivity*. Intersubjectivity assumes *reciprocity of perspectives*—if you were standing where I am, then you would see (more or less) what I see. The world is ours, not mine alone (22).

Reciprocity of perspectives makes possible the process of credibility. Other researchers usually offer responses to discovery claims that can range from agreement to profound skepticism. They react to the specifics of the research as well as to the relationship between new ideas and prevailing beliefs. Novel and unexpected discovery claims sometimes will be rejected or unappreciated by the community because the new thinking does not fit current understanding. The history of Nobel Prize–winning research is replete with such examples.

Rather than accept rejection, to succeed in scientific research often requires that researchers become advocates for their work. When the awards are given out, we frequently read:

> Why were Professor Particular's early studies ignored, neglected, and often denigrated? . . . The powerful force of the longstanding dogma made it easy for the community to brush

> aside Particular's experiments and ideas and to view them as a curiosity with little or no relevance to the mainstream. Fortunately, Particular's passionate belief in his data and his unshakeable self-confidence propelled him forward despite the criticisms of his colleagues. (paraphrased from 23)

Of course, becoming an advocate for one's beliefs when everyone else thinks that you are mistaken is risky business. What appears to be novel often turns out to be experimental artifact. N-rays, polywater, and cold fusion bring to mind some of the most famous cases of erroneous research. The only thing worse than being wrong in science is being ignored. The former frequently leads to the latter.

In the end, Professor Particular becomes Professor Anybody through the process of credibility. During this process, investigators shape and reshape their work to anticipate and overcome the criticisms that they receive from the community (24). When (if) others eventually validate the new observations by using them successfully in their own research—often modifying them at the same time—then the new findings become more widely accepted. In short, credibility happens to discovery claims. Discovery claims become credible—are made credible or incredible—through their subsequent use (25).

Returning to the baseball umpire analogy, in everyday practice of science calling things as they are is reserved for the community rather than the individual. But even the community's calling is tentative. With discovery oriented toward completion and correction, the scientific attitude defers Truth to the future and aims for credibility in the present. The realism of science remains incipient and tightly linked to practice through last year's discoveries. Last year's discoveries become this year's conceptual and technological instruments of exploration. Thus, realism of science emerges not through power, as supposed by the postmodernist critique, but by replacing individual subjectivity with communal intersubjectivity, philosopher

Annette Baier's *commons of the mind*: "We reason together, challenge, revise, and complete each other's reasoning and each other's conceptions of reason" (26).

At the ideal limit, reciprocity of perspectives means that all scientists can share the same experiences. As experience becomes typical and commonplace, the unique individual disappears and the anonymous investigator (Professor Anybody) emerges. Scientific knowledge aims to be correct for anyone, anywhere, anytime.

In summary, objectivity of science does not depend on the individual. Rather, objectivity is a function of the community. Everyday practice of science is neither truth nor power, but rather balanced on a contextual ledge in between.

In practice, biography and personality never really disappear. Intersubjectivity can be achieved only partially. Because the objectivity of science depends on the community rather than the individual, the influence of personality and biography on the researcher's scientific judgments becomes an asset to science rather than an impediment. Diversity in how people think and work enhances scientific exploration of the world. Diversity of demographics—for example, gender, race, and economic status—enhances the possibility of a multicultural approach (27). Without diversity, the community cannot really "complete each other's reasoning and each other's conceptions of reason." The judgments of a research community that is too homogeneous or isolated are just as much at risk as those of a community prevented by political interference from open exchange and dissent.

The foregoing discussion emphasizes the inherent ambiguity of everyday practice of science. Table 1.1 explicitly contrasts this ambiguity with the stages of the classic scientific method. The ambiguity evident in table 1.1 highlights Medawar's comment that there is no such thing as the scientific method, and that the idea of naive or innocent observation is philosophers' make-believe (2). Inevitably, both sides of table 1.1 blend together.

Table 1.1. The Classic Scientific Method vs. the Ambiguity of Everyday Practice

The Classic View	The Ambiguous View
State the problem to be studied.	Choosing a problem commits one to investing time, energy, and money. The wrong choices can place one's life goals and career in science at risk.
Carry out experiments to study the problem and record the results.	The important results may not be noticed. What counts for data one day may appear to be experimental noise the next.
Conclude whether the observations confirm or falsify one's ideas.	If the results don't agree with expectations, it may be because the idea is wrong or because the method used to test the idea is flawed. Hence the adage: Don't give up a good idea just because the data don't fit.
Seek verification by other researchers of the findings and conclusions.	Discovery claims are often greeted with skepticism or disbelief, especially when they are very novel and unexpected. Rejection by other scientists is a common experience. To succeed, investigators frequently have to become advocates for their work.

Education without practice

We frequently hear the question, "What ails U.S. science and mathematics?" For more than a generation, an emphasis on the shortcomings and need for enhanced science education in the United States has been recognized in every national report that addresses the subject. The huge literature that has developed offers many answers to the foregoing question but lacks consensus. "The candidates include teachers who don't know the subject matter, lousy textbooks, a badly designed curriculum, low expectations by educators and parents, an outmoded school calendar, and the debilitating effects of poverty and race" (28). In addition, maybe students are just "turned off." They think of science as a mere collection of facts rather than as high adventure. "Dry as dust," commented Nobel Laureate Leon Lederman (29).

Shortly before his death in 1994, I heard Nobel Laureate Linus Pauling lecture at a science education workshop. Pauling began his personal reflection by holding up a contemporary college chemistry text. He suggested that the book was too thick—several inches too thick. In his view, textbooks had become collections of facts divorced from understanding.

Divorced from understanding reflects at least in part the omission of everyday practice from science education. This criticism is nothing new. More than 50 years ago, Harvard University President James Conant pointed out the problem in *Science and Common Sense*:

> The stumbling way in which even the ablest of the scientists of every generation have had to fight through thickets of erroneous observations, misleading generalizations, inadequate formulations, and unconscious prejudice is rarely appreciated by those who obtain their scientific knowledge from textbooks. (30)

Even the science fair, one of the most popular and valuable science education experiences, distorts practice. The science fair judge begins by asking, "Is the problem stated clearly and unambiguously?" The hypothesis always goes near the upper left-hand corner of the poster board describing the science project, and must come first—never last. When I encouraged one of my children to put the hypothesis at the lower right as her conclusion, she lost points. After that, she questioned whether I really understood science! Traditional science fairs reward success in research and clarity of presentation.

What kind of science fair rewards success in the playfulness of discovery, including learning what not to do the next time?

Why has everyday practice not become a more central focus for science education? Whatever the reasons, ignoring practice impedes the goals of science education. When he was executive director of the National Science Teachers Association, Bill Aldridge wrote that the framework for science education should be built around three fundamental questions: What do we mean? How do we know? Why do we believe? (31). Those who do not understand the practice of science cannot, in the end, answer these questions.