Chapter 1. Practicing Science

Most people mistake the linear model of science for everyday practice. According to the linear model, researchers move from hypothesis to discovery following a direct course guided by logic and objectivity based on *The Scientific Method*. Practice is quite different. Rarely do investigators select or test a hypotheses in a completely detached manner. Rather, every scientist carries out her/his work situated within particular life interests and commitments. Moreover, instead of linear, the path from hypothesis to discovery tends to be highly ambiguous and convoluted with lots of dead ends. In the first part of this chapter, we will discuss general features of practice emphasizing ideas about ambiguity, individuality, and sources of objectivity in science. In the second part, we will discuss the implications of practice for science policy and science education.

The individual researcher is at the center

As illustrated by Figure 1.1, the individual can be found at the center of everyday practice of science. Professor-Somebody-In-Particular participates in two conversations, one with the world 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 to the circle of credibility – trying to convince others that the new findings are correct. We will see that these conversations are dialogues, which evolve in an iterative manner. Of course, the researcher interacts with only a small aspect of the world and the scientific community itself is within the world. Nevertheless, making the artificial distinction helps to emphasize that there are important differences between these conversations. An investigator's interactions with the world typically are limited to observation and intervention using previously developed technologies; whereas her/his interactions with the community also depend on cooperative and competitive behavior.

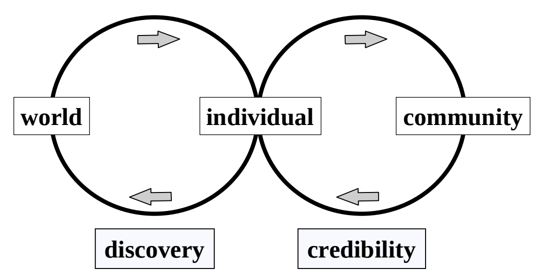

Figure 1.1: Practice of science

One reason why the linear model rather than everyday practice has become the common image of research is because there is no place for the practice of unique individuals in the normative structure of science. Robert Merton's classical analysis (1) identified the four features of the ethos of science as universalism, communism, disinterestedness, and organized skepticism. Universalism means that scientific claims are independent of the personal or social atttributes of researchers. Communism means that scientific knowledge is owned by everyone. Disinterestedness means that the community surpresses any tendency of investigators to behave only according to their own self-interest. And organized skepticism means that researchers suspend personal beliefs in favor of an attitude oriented towards empirical and logical criteria.

Rather than Professor Particular, normative science is practiced by the anonymous, disinterested, and dispassionate Professor-It-Could-Be-Anybody.

Science textbooks and research publications exclude practice

Just as practice of unique individuals is not part of normative science, the individual researcher is absent from science textbooks and research publications. Scientific facts are presented in textbooks without making clear where and how these facts arise. Space limitations may make this omission necessary. Nevertheless, the consequence is that practice becomes invisible. Years of research are compressed into one or several sentences. At the same time, the adventure, excitement and risks of real-life discovery disappear. We should consider the possibility that many students are turned off because they are taught to think of science as a mere collection of facts rather than as a highly creative enterprise. As Nobel Laureate Leon Ledermann remarked (2) in a symposium called “Who will do the science of the future?”:

> Teaching science without some appeal to its history, how do we know, how did we go wrong, and so forth, is dry as dust.

Research publications also mask the process of discovery. In what follows we will consider an example to illustrate this point. By way of background, here are some facts that can be found in most modern biology textbooks:

> 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 subsets of 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. These steps represent the classic pathway discovered by modern biology:

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

When a biology textbook states the fact that mRNA is the intermediate between DNA and protein, it also might add a footnote to a 1961 research paper (3) published in the prestigious scientific journal *Nature*. In this paper, evidence for the intermediary role of mRNA was first reported. As will be discussed in chapter four, research publications provide the formal mechanism by which investigators report the details of their work and interact with other members of the scientific community to establish its credibility.

The 1961 *Nature* paper about mRNA was entitled: "An unstable intermediate carrying information from genes to ribosomes for protein synthesis." The paper began by summarizing prevailing views and controversies on the subject and suggested a new hypothesis to resolve the controversial issues. Experiments then were proposed to test the hypothesis, carried out, and the results reported. Further discussion explained how the findings supported the new hypothesis and excluded other possibilities. Therefore, the paper was arranged according to the sequence:

> prevailing views ⇒ testable new hypothesis ⇒ experimental design ⇒ results ⇒ confirmation of the new hypothesis/falsification of the old

The above sequence conforms to the linear model of scientific discovery. As is the case with many – perhaps most -- research papers, the plot is the scientific method. This formal structure is imposed upon what actually happened. As Nobel Laureate François Jacob, one of the authors of the *Nature* paper wrote in his memoir *The Statue Within* (4):

> Writing a 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.

Stated otherwise, research publications convert the process of discovery into an announcement of the discovery. In a sense, the paper itself becomes the discovery (5). In corresponding fashion, investigators become reporters of discoveries rather than discoverers. They adopt an anonymous role in presenting the work to the scientific community.

> To get some work accepted and a new way of thinking adopted [wrote Jacob] it is necessary to purify the research of all affective or irrational dross. To get rid of any personal scent, any human smell.

In principle, any scientist could have done the experiments, any scientist could have made the discovery. The researcher in scientific publications is none other than Professor Anybody. Because this style of presentation is the usual form in which scientists communicate in public, it is natural for those outside of science to believe that science works in this fashion.

Unlike the textbook/research publication version of scientific facts, autobiographical writings of researchers sometimes provide a different perspective of discovery. In the case of mRNA story, we can get Jacob's view of what actually happened from his memoir (4). Below are several quotes from his book accompanied by brief explanations to emphasize the contrast between practice and the linear model of discovery.

> We were do very long, very arduous experiments. . . But nothing worked. We had tremendous technical problems. . .

Experiments often do not work. There are three classes of experiments. Heuristic - from which one learns something new. Demonstrative – those that get published. Uninterpretable -- the largest class – what <u>not</u> to do the next time. When high school science teachers spend a summer working in my laboratory, they are amazed at how frequently experiments fail for technical reasons.

> 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.

The disinterested researchers envisioned by normative science would never be “sure of the correctness” of their unproven hypotheses. In practice, however, investigators' intuitions often lead them to continue to believe in and pursue a hypothesis even when it is not supported 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.

Finally, the exhilirating experience of success elicits a degree of excitement and enthusiasm uncharacteristic of serious grown men and women at work. When my 7th grade science teacher, Mr. Perkins, told me that “science is serious play,” he was not exagerating.

In summary, one can distinguish three different versions of a discovery. The factual account found in textbooks, which simply is to be memorized. The linear account found in research publications, which researchers use to establish the credibility of the work. And the everyday practice account, rarely glimpsed from outside the laboratory, which is what really happened.

<u>Science studies</u>

In contrast to Robert Merton’s description of the normative structure of science, Thomas Kuhn emphasized the individual and her/his practice. Kuhn’s book *Structure of Scientific Revolutions* (6) became the most widely read and cited philosophical work of recent times. His book had a great impact on the development of the modern field of science studies. The practices of individual researchers and research teams rather than normative structure are precisely what interest most the anthropologists, historians, philosophers, and sociologists who together make up the field of science studies.

Kuhn described the role of *paradigms* in science. On one hand, paradigms refer to the set of beliefs and values shared by members of the scientific community. Paradigms also refer to established and acceptable ways of problem solving. Kuhn’s goal was to emphasize that scientific judgements depend on shared values and vary greatly according to individual personality and biography.

Writers from other backgrounds also have emphasized the importance of prior knowledge and attitudes on the practice of science, albeit nuanced in different ways. Examples include the *schemata* of psychologist Jean Piaget (7); *thought styles* described by physician-scientist Ludwik Fleck (8); chemist Michael Polanyi’s *tacit knowledge* (9); and historian Gerald Holton’s *thematic presuppositions* (10). According to this way of thinking, the researcher’s (like everyone’s) knowledge of things is not simply given. Rather, it requires interpretation of experience and takes place within the framework of one's life situation. Prior knowledge and attitudes influence what the person experiences, what s/he thinks it means, and the subsequent actions that s/he takes.

Science as practice, in contrast to its normative structure, can accommodate the remark that Steve Martin has Einstein make to *Picasso at the Lapin Agile* (11):

> What I just said is the fundamental end-all, final, not-subject-to-opinion absolute truth, depending on where you're standing.

After *Structure of Scientific Revolutions,* science became of interest not only in terms of its normative structure, but also 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 absolutely essential. At the same time, however, admitting the human associations of science has for some challenged the belief that science provides an objective description of reality. Indeed, especially among the "postmodernists," 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.

As an example of the range of opinions on this matter, consider feminist critiques of science. Praticitioners of modern science are for the most part white, male, and upper middle class. Sue Rosser and others have asked whether there might be distinct feminist methodologies for doing science (12). If so, what difference might these methodologies make? Liberal feminism argues that gender bias has a great influence on science education and policy, which in turn determines who can do science and what research will be carried out. Marxist feminism goes further and challenges the objectivity of science. Current practices of science are suggested to be accepted only because they reinforce the "historical, economic, social, racial, political, and gender policies of the majority of scientists..." African-American feminism also rejects the objectivity of science, emphasizing its Eurocentric approach and racial bias. Essentialist and existentialist feminism suggest that women would do science differently from men because of their unique biological and social roles, respectively. Radical feminism understands the world as an organic whole and thus rejects dichotomies such as rational/feeling and objective/subjective. Finally, Lesbian feminism argues that women will not truly be able to understand how to do science until they separate from ongoing oppression by men.

<u>What does objectivity in science mean?</u>

Feminist critiques of science and science studies in general point to an important paradox. Some of the "truths" established by science turn out to be culture-dependent and wrong. For instance, up until the mid-1960's, homosexuality was widely viewed in the United States as an illness and listed as such in the diagnostic manual (DSM-2) of the psychiatric community. Since then the link between homosexuality and psychopathology has been discredited, and homosexual couples increasingly are accorded the same rights and respect as heterosexual couples. Now those who still oppose homosexuality, and there still are many, can less easily base their objections on "the scientific/medical facts."

In his book *The Mismeasure of Man* (13), Steven Jay Gould describes how racist and sexist cultural attitudes influenced research design and interpretation in the late 19th and early 20th century. He wrote:

> [Science] progresses 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.

Nevertheless, despite the cultural context, the dramatic impact of technology on the world shows that scientific knowledge is more than mere belief. Including the discovery and use of fire and invention of primitive tools, humankind always has attempted to control and change the environment. Much of human history has been driven by the attempt to overcome famine and disease. Our increasing ability to have an impact on the world through technology suggests that science's understanding of the physical mechanisms of the world is advancing closer to the truth. So here is the paradox: how can a practice situated within a particular cultural context give rise to knowledge that has universal validity? How does Professor Particular become Professor Anybody?

One way to answer this question is to compare the practices of baseball umpires with researchers. According to tradition, there are three types of 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 the attitudes that they bring to their work. As a result of those attitudes, they practice umpiring differently. The first type claims truth; the second, perspective; and the third, power.

Those who have learned the linear view of science identify researchers with the first type of umpire. Postmodernists identify researchers with the third. A brief introduction to the practices of discovery and credibility will make it evident that the second type of umpire corresponds most closely to the way that scientists do research.

In everyday practice, discovery begins in community. There are no loose ends. Each researcher or group of researchers initiates the work in the context of prevailing beliefs, using these beliefs as a starting point and justification for further action. At the same time, it is assumed that this previous knowledge is incomplete or wrong. There is no reward in science for simply duplicating and confirming what others already have done. Re-search is only interesting as a basis for new-search.

The kind of discovery that investigators usually find most adventurous and full of surprises occurs 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. For instance, as will be discussed in chapter five, clinical research using human subjects should begin only in much more settled territory after a great deal of preclinical work has been accomplished successfully. The ethics of research with humans demands that the work be as unambiguous as possible.

At the edge of knowledge incomplete understanding of experimental conditions and controls often results in failure. “The worst kind of failure, and a common one” wrote Nobel Laureate Arthur Kornberg (14), “is the inability to repeat what appeared to be a novel finding enlarged by fantasy to a great discovery.” Nevertheless, incomplete understanding also can be a great benefit and give rise to unexpected and important results. Nobel Laureate Max Delbrück called this aspect of research the *principle of limited sloppiness* (15). Investigators frequently take unplanned journeys to unexpected places. Only retrospectively do they learn where they have gone. The 19th century philosopher Charles Peirce understood that this was the most important mechanism of discovery and gave the process a special name `“abduction” (16) in contrast with the more frequently used words *induction* and *deduction* .

Because Professor Particular is always subject to fantasy on one hand and self-deception on the other, we should call the initial discovery by the individual researcher something other than science, perhaps *protoscience*. For protoscience to become science, the researcher must not only be able to replicate his/her own work, but also s/he must turn to the community to show peers the new findings and convince others that they are correct. In response, other researchers usually offer profound skepticism. This skepticism concerns not only the specifics of the research itself, but also, the relationship of the new ideas to prevailing beliefs. The more novel and unexpected a discovery, the more likely that it will be rejected or ignored by the community precisely because it does not fit current understanding. The history of Nobel Prize-winning research is replete with examples of work initially rejected by the community. Therefore, to succeed in science, researchers often have to confront rejection by becoming advocates for their work. Yet to be sure of the correctness of one’s hypotheses is risky. Novelty and unexpectedness often turn out to be experimental artifact. Ambiguity leads to failure or error far more easily than to success. 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 a dialectic of credibility. During this process, investigators continually shape and reshape their work to anticipate and overcome the criticisms that they receive from the community (17). When (if) others eventually validate and use the new observations in their own work – often modifying them at the same time – then the new findings become more widely accepted. Credibility is a process that happens to discovery. Scientific knowledge becomes true, is made true by events (18).

Returning to the baseball umpire analogy, it should now be clear that in everyday practice of science, calling things as they are is reserved for the community. And even the community's calling is tentative. That is, with discovery oriented towards completion and correction, the scientific attitude defers truth to the future in favor of credibility in the present. Unchangeable truth cannot be part of science. The realism of science remains incipient and tightly linked to practice through technology. Last year's discoveries become this year's instruments of discovery. Consequently, the realism of science emerges out of community, not through power as supposed by the postmodernist critique, but by replacing individual subjectivity with intersubjectivity. This sense of community exemplies Annette Baier's *commons of the mind* (19).

> We reason together, challenge, revise, and complete each other's reasoning and each other's conceptions of reason.

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

Because objectivity does not depend solely on the individual, the influence of personality and biography on the researcher's scientific judgments is actually an asset to science rather than an impediment. Diversity of researchers – e.g., gender, race, economic – enhances scientific exploration of the world and makes possible a multiculture view of science and technology (20). In addition, diversity of the community is required for us to "complete each other's reasoning and each other's conceptions of reason." The judgements of a scientific community that is too homogeneous or isolated are just as much at risk as those of a community prevented by force (usually political) from challenge and revision.

<u>The "ambiguous" scientific method</u>

Based on the foregoing description, the table below presents the stages of the scientific method along with corresponding ambiguities of everyday practice.

State the hypothesis to be tested.	Choosing to test a hypothesis means investing time, energy, and money. Hypotheses that are wrong can place one's life-goals and career in science at risk.
Carry out experiments to test the hypothesis 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 the hypothesis.	If the results don't agree with expectations, it may be because the hypothesis is wrong or because the method used to test the hypothesis is wrong. Hence the adage: Don't give up a good hypothesis just because the data don't fit.
Seek verification of the findings and conclusions by other researchers.	New findings often are greeted with skepticism or disbelief, especially when they are unexpected. Rejection by other scientists is a

	common experience. To succeed, investigators frequently have to become advocates for their work.

Taken together and placed side-by-side in this context, the ambiguous version of everyday practice echos the comments Sir Peter Medawar wrote in an essay on hypothesis and imagination (21):

> There is no such thing as Scientific Mind.
> There is no such thing as The Scientific Method.
> The idea of naïve or innocent observation is philosopher's make-believe.

Research integrity

The foregoing sections described some general features of practice. Now we turn to a discussion of the implications of practice for research integrity, science policy, and science education. What we will see is that substitution of the linear model of science for everyday practice can actually have a negative impact on science and its goals.

In his description of the ethos of science, Merton attributed to the norm of disinterestedness "the virtual absence of fraud in the annals of science" (1)

> Involving as it does the verifiability of results, scientific research is under the exacting scrutiny of fellow experts. Otherwise put…the activities of scientists are subject to rigorous policing, to a degree perhaps unparalleled in any other field of activity.

Unfortunately, the rigorousness of this policing was called into question beginning in the late 1970's by several well-publicized cases of research misconduct involving U.S. scientists. Subsequently, numerous national committees and commissions as well as Congressional Hearings addressed the problem of misconduct in research and how it might be prevented. One consequence of these deliberations was a requirement established by the National Institutes of Health (NIH) in 1989 that educational institutions include in their NIH training programs a component on research integrity and responsible conduct of science .

For a number of years prior to 1989, I had taught a seminar course about philosophy and practice of science for UT Southwestern graduate students in the biomedical sciences. In response to the NIH mandate, the dean co-opted an abbreviated version of my course to become part of the core instructional material for all incoming graduate students in the division of cell and molecular biology. Subsequently, my experience teaching about research integrity as well as participation in workshops to help others teach about this subject made it clear to me that everyday practice confounds seemingly straightforward conclusions about integrity and responsible conduct that are based on the linear model of science. For instance, NIH defined research misconduct (22) as:

> fabrication, falsification, plagiarism, deception or other practices that seriously deviate from those that are commonly accepted within the scientific community for proposing, conducting or reporting research.

While the definition sounds reasonable at first glance, many scientific organizations including the Federation of American Scientists for Experimental Biology (FASEB) recognized the potential unintended consequences of the phrase "other practices that seriously deviate…" As FASEB president Howard Schachman testified to Congress (23):

> It is our view that this language is vague and its inclusion could discourage unorthodox, novel, or highly innovative approaches, which in the past have provided the impetus for major advances in science. It hardly needs pointing out that brilliant, creative, pioneering research deviates from that commonly accepted within the scientific community.

In another report, the Congressionally-mandated Commission on Research Integrity (24) suggested that the definition of misconduct should be based on "the fundamental principle that scientists be truthful and fair in the conduct of research and the dissemination of research results." Yet, as already discussed, research publications often present a reconstruction of what actually was done in the laboratory. While such publications aim to be truthful in an intellectual sense, they certainly are not truthful in any literal sense. Indeed, Sir Peter B. Medawar wrote an essay on this topic called: *Is the Scientific Paper a Fraud?* (25)

Ambiguity of practice also confounds simplistic notions of truth and fairness. In their 1992 report *Responsible Science: Insuring the Integrity of the Research* (26), representatives of the Committee on Science Engineering and Public Policy of the U.S. National Academies wrote:

> The selective use of research data is another area where the boundary between fabrication and creative insight may not be obvious.

As an example, consider the difficulty of distinguishing data from experimental noise. While heuristic principles can be helpful, it will be an investigator's experience and intuition -- in short, her/his creative insight – that determine what counts and what does not. To some, how results were selected might appear arbitrary and self-serving, or even an example of misconduct. The case of the Nobel laureate Robert A. Millikan, who selected 58 out of 140 oil drops from which he calculated the value of the charge of the electron, provokes precisely this kind of debate (10).

In 2000, the U.S. Office of Science and Technology Policy (OSTP) finally adopted a government-wide definition of misconduct (27) in which they narrowly focused misconduct to include "fabrication, falsification, or plagiarism in proposing, performing, or reviewing research, or in reporting research results." In addition, for there to be a finding of research misconduct, it was required "that there be a significant departure from accepted practices of the relevant research community." The OSTP definition shifted the emphasis in two important ways. First, it excludes from misconduct "practices that seriously deviate from those that are commonly accepted." Second, it makes clear that even behaviors that might otherwise be considered misconduct such as fabrication, falsification, or plagiarism should be evaluated in the context of the accepted practices of the relevant research community.

Many scientists and policy-makers view conflict-of-interest as a greater challenge to integrity in science than research misconduct. Chapter four will focus on the relationship between self-interest and conflict-of-interest. Individuals (and institutions) have interests in the outcome of the research, and success in research leads to advancement and advantage. This potential for advancement need not undermine science. Indeed, it can encourage scientists to exhibit a high degree of integrity because, as David Hull has pointed out (28):

> more often than not, it is of their own best self interest to do so. By and large, what is good for the individual scientist is actually good for the group. The best thing that a scientist can do for science as a whole is to strive to increase his or her own conceptual inclusive fitness.

By "conceptual inclusive fitness," Hull means intellectual influence, having one's own ideas adopted as the prevailing beliefs of the scientific community. Conflict of interest occurs when a scientist's primary goal or significant motivation becomes something other than intellectual advance, for instance, financial reward or accomplishing a social or political agenda. These other agendas, whether they are known or not, create a situation in which what is good for the individual scientist no longer is necessarily good for the entire community. When these agendas exist, they have the potential to undermine the foundation of interpersonal trust on which the commons of the mind depends.

While conflict of interest among scientists is not a new issue, concern has intensified in recent years because of increased scrutiny of human clinical research trials. The Bayh-Dole Act passed by the U.S. Congress in 1980 has exacerbated the situation. This act, designed to increase the transfer of technology from universities to industry (and it has done so successfully), permitted for the first time that universities own what their employees had invented with the help of federal funds. As a result, academic researchers and their universities find themselves in a sense part of a pharmaceutical/biotech industry whose <u>legitimate</u> values are very much different from the ethos of science. Because profitability is the primary motive for research in the pharmaceutical/biotech setting, privacy is valued over common ownership of knowledge, and success means meeting the goals of the company board-of-directors and shareholders.

<u>Science policy</u>

Establishing a federal definition of misconduct and authorizing universities to patent intellectural property are science policy decisions. Such decisions affect every aspect of research, including what will be done, who will do it, and how will it be funded. Not surprisingly, therefore, diverse societal interest groups have a stake in and seek to influence science policy. The issues are complex and increasingly public as exemplified by the first prime time television speech of George W. Bush's presidency (29), which began as follows:

> Good evening. I appreciate you giving me a few minutes of your time tonight so I can discuss with you a complex and difficult issue, an issue that is one of the most profound of our time. The issue of research involving stem cells derived from human embryos is increasingly the subject of a national debate and dinner table discussions. The issue is confronted every day in laboratories as scientists ponder the ethical ramifications of their work. It is agonized over by parents and many couples as they try to have children, or to save children already born.
>
> The United States has a long and proud record of leading the world toward advances in science and medicine that improve human life. And the United States has a long and proud record of upholding the highest standards of ethics as we expand the limits of science and knowledge.

What is required to understand the President's speech? At the minimum, some knowledge of human embryos, including how they are viewed from different scientific and religious perspectives. These different perspectives give rise to different conclusions about whether doing research with human embyos is ethical. Humans have been doing research on each other for a long time. The question of what makes human research ethical, however, is a relatively new subject for analysis, generated especially by the human research abuses of World War II.

The President's speech also raises general questions about practice of science. For instance, do researchers really ponder the ethical ramifications of their daily work? (If not, should they?) And if advances in science improve human life, then should the scientific research agenda be linked to specific national goals?

In the United States, the goal of using science to serve humanity was already articulated in pre-Revolutionary writing. Benjamin Franklin founded the American Philosophical Society in 1743. The inaugural volume of its *Transactions* offered the following perspective (quoted in (30)):

> Knowledge is of little use, when confined to mere speculation: But when speculative truths are reduced to practice, when theories grounded upon experiments, are applied to common purposes of life; and when, by these, agriculture is improved, trade enlarged, the arts of living made more easy and comfortable, and, of course, the increase and happiness of mankind promoted; knowledge then becomes really useful.

In Franklin's time, the evolution of technology was slow enough so that one might clearly distinguish between basic research aimed at expanding theoretical knowledge vs. applied research aimed at reducing theoretical knowledge to practice. The dividing line between basic and applied research has become increasingly difficult to identify, however, either intellectually or temporally. Today's discoveries in basic research depend upon applications and tools developed last year (or sooner) just as much as next year's new applications and tools depend on today's basic research discoveries.

Given the advances in science and the increase in rate of transformation of speculative truth into technology, how are we progressing towards the goal of improving human life? Measured by average life expectancy and stabilized population growth, we have achieved considerable success. At the same time, however, much of the world's population experiences massive proverty, inadequate health care, and lack of pure drinking water. And the consequences of environmental degradation are experienced by us all. In response, science policy decision-makers want to ask: Could such social problems be fixed by more science? If so, what science?

In the United States, with the costliest health care system in the world and greatest expenditure on biomedical research, public health statistics are only average compared to other industrial countries. Moreover, large numbers of individuals including children lack adequate health care by many measures. So what is missing? Dan Sarewitz, in *Frontiers of Illusion* (31), suggests that many political decision-makers propose “science as a surrogate for social action.” By taking this path, these politicians can avoid making the tough political, social, and economic decisions necessary to solve the problems directly. Consequently, Sarewitz's advice to political and cultural institutions if they really want to advance the public good is to act:

> as if scientific and technological progress had come to an end and the only recourse left to humanity was to depend upon itself.

The expectation that science will be the solution of social problems distorts the founding vision of modern United States science policy set forth at the end of World War II in a report called *Science –The Endless Frontier*, written by President Roosevelt’s science advisor Vannevar Bush (32). The report makes it clear that science is necessary to advance the nation’s goals but not by itself sufficient.

> Progress in the war against disease depends upon a flow of new scientific knowledge. New products, new industries, and more jobs require continuous additions to knowledge of the laws of nature, and the application of that knowledge to practical purposes. Similarly, our defense against aggression demands new knowledge so that we can develop new and important weapons. This essential, new knowledge can be obtained only through basic scientific research.
>
> Science, by itself, provides no panacea for individual, social, and economic ills. It can be effective in the national welfare only as a member of a team, whether the conditions be peace or war. But without scientific progress no amount of achievement in other directions can insure our health, prosperity, and security as a nation in the modern world.

The Endless Frontier also argues against creating tight linkage between basic research and national goals and adopts a view of basic research consistent with the ambiguity of everyday practice as opposed to the linear model.

> Scientific progress on a broad front results from the free play of free intellects, working on subjects of their own choice, in the manner dictated by their curiosity for exploration of the unknown.
>
> One of the peculiarities of basic science is the variety of paths which lead to productive advance. Many of the most important discoveries have come as a result of experiments undertaken with very different purposes in mind. Statistically, it is certain that important and highly useful discoveries will result from some fraction of the undertakings of basic science; but the results of any one particular investigation cannot be predicted with accuracy.

When science policy decision-makers attempt to identify in explicit fashion the specific social needs that basic research would help remedy and establish investment criteria by which to evaluate the basic research enterprise, they substitute the linear model of science for everyday practice. By denying the ambiguity inherent in practice and asking the unpredictable to be predicted, the performance-based approach to science policy cannot succeed. On the contrary, it has the potential to undermine and distort basic research and thereby retard rather than advance the national interest.

<u>Science education</u>

Along with science policy, science education plays a major role in determining every aspect of research. Science education during the years K-12 plays a foundational role because it is:

- when most future scientists and engineers are attracted to science and receive the foundation for their subsequent studies.
- when those who will go on to non-science careers acquire a large part of their science background.
- when the public at large obtains much of the scientific and technical understanding that will be needed later to make (hopefully informed) decisions as consumers and voters.

In general, the news about education is not good. Despite decades of intense effort, the situation has not improved much since the 1983 report *A Nation at Risk* (33), which focused attention on the declining state of the educational system in the United States. American high school students continue to perform poorly on studies of math and science knowledge compared to students from many other countries. By the time they graduate, the percentage of American college students who complete degrees in science or engineering is now less than that found in many other countries. These deficiencies are troubling. “The life enhancing potential of science and technology cannot be realized,” wrote George Nelson (34), former director of the American Association for the Advancement of Science's science literacy initiative (Project 2061):

> unless everyone understands the nature of these subjects and acquires basic scientific habits of mind. Without a science-literate population, the outlook for a better world is not promising.

Carl Sagan was more blunt (35):

> We live in a society exquisitely dependent on science and technology, in which hardly anyone knows anything about science and technology. This is a clear prescription for disaster.

Shortly before his death, I heard Nobel Laureate Linus Pauling deliver one of the lectures at a science education workshop. Other lectures in this session were on “Why is science literacy important in society?” and “How can science literacy be achieved?” Pauling began his personal reflection by holding up a contemporary college chemistry text. He suggested that it was too thick – several inches too thick. In his view, textbooks had become collections of facts divorced from understanding.

While textbooks are only one aspect of the overall problem, they do reflect the point made at the beginning of this chapter, namely, the omission of practice of science from the science education curriculum. Identification of the textbook omission is not a new revelation. More than 50 years ago, Harvard University president James Conant wrote in *Science and Common Sense* (36) :

> 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.

Ironically, one of the best opportunities for K-12 students to experience science as practice -- science fair – also distorts practice. Science fair rewards success and leaves little room for failure or ambiguity. As a science fair judge, I have been told that the number one criterion to assess scientific thought is: "Is the problem stated clearly and unambiguously?" The hypothesis always goes near the upper left hand corner and must come first, never last. [When I encouraged one of my children to put the hypotheses at the lower right, she did so and lost points. After that she questioned whether I really understood science.] Overall, there is nothing in science fair to encourage the playfulness of discovery. Critical thinking, logic, and problem solving are the focus. While these skills certainly are important for managing life in a complex world, the intuitive and artistic student must wonder what role s/he could play in science.

What prevents everyday practice of science from becoming a central focus for science education? I suspect that the answer is at least in part resistence by the scientific and educational communities to deal with issues like the ambiguity of practice or the cultural context of research. This resistance was evident, for instance, in the widespread criticism generated by the Smithsonian Institute's exhibit *Science in American Life*. The exhibit began by showing the arrival of laboratory science in the United States embroiled in controversy over credit for discovery and patent rights. Alongside the documentation of many scientific advances, one could also find the negative unanticipated consequences. *Science in American Life* reminded the visitor that research makes possible great benefits for humankind but also can open paths that threaten our continued existence.

By sticking to the linear model of science, the ambiguity of practice and cultural context of research can both be ignored. To do otherwise, it is feared, might confuse the public and their political representatives thereby decreasing enthusiasm for continued federal research support and making it harder to distinguish science from other ways of practicing the world such as religion or art. I would argue precisely the opposite. 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 (37):

1. What do we mean?
2. How do we know?
3. Why do we believe?

Those who do not understand practice of science cannot in the end answer these questions. Indeed, for them to accept the claims and explanations put forth by science is truly a matter of faith.

<u>Summary of Key Points</u>

- In this chapter, we have introduced the features of everyday practice of science. Rather than practice of science, most people are familiar with the the linear model of science according to which the researcher moves from hypothesis to discovery following a direct course guided by logic and objectivity and the scientific method. Practice is quite different. The path to discovery tends to be ambiguous and convoluted with lots of dead ends.

- Ambiguity plays a paradoxical role in everyday practice. On one hand, it can slow down the pace of discovery and increase controvery. On the other, it can make discovery possible by providing an investigator with unexpected opportunities to notice new things about the world.

- The linear model rather than everyday practice has become the common image of science. There is no place for the unique individual in normative science. In science textbooks and research publications, the adventure, excitement and risks of real-life discovery disappear.

- In practice, the researcher works within the framework of her/his life situation. Prior knowledge and attitudes influence what the person experiences, what s/he thinks it means, and the subsequent actions that s/he takes. Because researchers are always subject to fantasy on one hand and self-deception on the other, discovery by the individual can be at most protoscience. To become science, the researcher must turn to the community. This gives rise to the dialectic of credibility. Credibility happens to discovery. Scientific knowledge becomes true, is *made* true, by events.

- Some of the "truths" established by science turn out to be culture-dependent and wrong. Nevertheless, the dramatic impact of technology on the world shows that scientific knowledge is more than mere belief. The realism of science emerges out of community. Objectivity of science does not depend on the individual. Rather it is a function of the community and the goal of research. Everyday practice of science is neither truth nor power, but rather balanced on a contextual ledge in between.

- Diversity of researchers – e.g., gender, racial, economic – enhances scientific exploration of the world. The judgements of a scientific community that is too homogeneous or isolated are just as much at risk as those of a community prevented from dissent by force.

- Science policy decisions affect every aspect of research including what will be done, who will do it, and how will it be funded. When science policy makers substitute the linear model of science for everyday practice, their decisions can undermine and distort research.

- Modern science education ignores everyday practice of science. Instead of ambiguity, students learn only the linear model. Critical thinking, logic, and problem solving are the focus. While these skills are important for managing life in a complex world, the intuitive and artistic student must wonder what role s/he could play in science.

- Those who do not understand practice cannot explain how scientific knowledge arises. For them to accept the claims and explanations put forth by science becomes simply a matter of faith.

References

1. Merton, R. K. (1973) in *The Sociology of Science: Theoretical and Empirical Investigations* (Merton, R. K., and Storer, N. W., eds), pp. 267-278, University of Chicago Press, Chicago, IL
2. Lederman, L. (2000) in *Who Will Do the Science of the Future* (National Academy of Sciences, ed), pp. 7-11, National Academy Press, Washinton, DC
3. Brenner, S., Jacob, F., and Meselson, M. (1961) *Nature* **190,** 576-581
4. Jacob, F. (1988) *The Statue Within*, Basic Books Inc., New York, N.Y.
5. deSolla Price, D. J. (1981) in *Coping with the Biomedical Literature* (Warren, K. S., ed), pp. 3-16, Praeger, New York, NY
6. Kuhn, T. S. (1962) *The Structure of Scientific Revolutions*, University of Chicago Press, Chicago, IL
7. Piaget, J. (1970) *Genetic Epistemology,* Trans. Duckworth, E., W.W. Norton & Co., New York, NY

8. Fleck, L. (1979) *Genesis and Development of a Scientific Fact (1935)*, University of Chicago Press, Chicago, IL
9. Polanyi, M. (1958) *Personal Knowledge: Towards a Post-Critical Philosophy*, University of Chicago Press, Chicago, IL
10. Holton, G. (1973) *Thematic Origins of Scientific Thought: Kepler to Einstein*, Harvard University Press, Cambridge, MA
11. Martin, S. (1996) *Picasso at the Lapin Agile and Other Plays*, Grove Press, New York
12. Rosser, S. V. (2001) in *The Gender and Science Reader* (Lederman, M., and Bartsch, I., eds), pp. 123-144, Routledge, New York
13. Gould, S. J. (1981) *The Mismeasure of Man*, W.W. Norton and Co., New York
14. Kornberg, A. (1989) *For the Love of Enzymes: The Odyssey of a Biochemist*, Harvard University Press, Cambridge, MA
15. Benzer, S. (1992) in *Phage and the Origins of Molecular Biology* (Cairns, J., Stent, G. S., and Watson, J. D., eds), pp. 157-165, Cold Spring Harbor Laboratories, Cold Spring Harbor, N.Y.
16. Hartshorne, C. W., Paul. (1965) *Collected Papers of Charles Sanders Peirce* (Hartshorne, C. W., Paul, Ed.), Vol. V - Pragmatism and Pragmaticism; Vol. VI - Scientific Metaphysics. 2 vols., The Belknap Press of Harvard University Press, Cambridge, Massachusetts
17. Ziman, J. M. (1968) *Public Knowledge: An Essay Concerning the Social Dimension of Science*, Cambridge University Press, Cambridge, U.K.
18. James, W. (1975) in *Pragmatism and The Meaning of Truth*, pp. 95-113, Harvard University Press, Cambridge, MA
19. Baier, A. C. (1997) *The Commons of the Mind*, Open Court Publishing Co, Chicago
20. Frankel, M. S. (1993), American Association for the Advancement of Science, Washington, D.C.
21. Medawar, P. (1982) *Pluto's Republic*, Oxford University Press
22. Department of Health and Human Services. (1989) in *Federal Register* Vol. 54, pp. 32446-32451
23. Schachman, H. K. (1990) *Congressional Testimony to the Subcommittee on Investigations on Oversight, Committee on Science, Space, and Technology, U.S. House of Representatives*, U.S. Government Printing Office, Washington, D.C.
24. Commission on Research Integrity. (1995), U.S. Department of Health and Human Services, Washington, D.C.
25. Medawar, P. B. (1963) *The Listener* 377-378
26. Committee on Science Engineering and Public Policy. (1992), National Academy Press, Washington, D.C.
27. Office of Science and Technology Policy. (2000) *Federal Register* **65,** 76260-76264
28. Hull, D. L. (1988) *Science as a Process*, University of Chicago Press, Chicago, IL
29. President George W. Bush. (August 9, 2001) *http://www.whitehouse.gov/news/releases/2001/08/20010809-2.html*
30. Greene, J. C. (1984) *American Science in the Age of Jefferson*, Iowa State University Press, Ames, Iowa
31. Sarewitz, D. (1996) *Frontiers of Illusion*, Temple University Press, Philadelphia, PA
32. Bush, V. (1945) *Science -- The Endless Frontier*, U.S. Government Printing Office, Washington, D.C.

33. National Commission on Excellence in Education. (1983), Department of Education, Washington, D.C.
34. Nelson, G. D. (1999) *Educational Leadership* **57,** 14-17
35. Sagan, C. (1993) *Mercury* 52-55
36. Conant, J. B. (1951) *Science and Common Sense*, Yale University Press, New Haven, CT
37. Aldridge, B. G. (1992) *J. Science Education and Technology* **1,** 13-21