

resolution is not always clearly marked. As the distinguished biologist Peter Medawar has reminded us, there is "no procedure of discovery that can be logically scripted." Rather, the scientist uses a variety of "exploratory stratagems" that have as much to do with personal style as with logic. Acute powers of observation, a delight in surprise, a willingness to grapple with the unexpected, and a "feel for things" are but some of these stratagems.

Part III explores some of the contexts and conditions in which discovery takes place. In the first essay, Robert S. Root-Bernstein argues that discovery often results not from the logical process of verification but rather from problems or anomalies exposed during the course of verification. For him, our understanding of the way science is actually done must be broadened to account for the "messy process of discovery." In the essay that follows, Mahlon Hoagland discusses discovery from a different perspective. He argues that because science is a "dynamic accumulating body of knowledge," and because it is an essentially collaborative enterprise, we can assume that if any given discovery had not been made by one scientist, it would sooner or later have been made by another (or others). A discovery, he believes, is "uniquely the discoverer's only in terms of priority and in the way it was made." These issues of uniqueness and collaboration in scientific discovery are implicit in Anne Sayre's examination of Rosalind Franklin's role in the discovery of the double-helix structure of DNA. Sayre discusses the ethical issues surrounding this discovery and shows that they are rooted in the inherent contradiction between the essentially collaborative and the inevitably competitive nature of science. These contradictory characteristics loom large in Luis W. Alvarez's account of his near misses in the discovery of nuclear fission and other breakthroughs in atomic physics. In the next essay, Evelyn Fox Keller, in her discussion of Nobel Laureate Barbara McClintock, returns to issues raised earlier by Root-Bernstein. As Keller shows, McClintock's greatness as a scientist lay in her ability to achieve a remarkable intimacy with her material ("a feeling for the organism"). Intimacy implies a collapsing of the distance between the observer and the observed (between subject and object). For McClintock, as for a number of the scientists discussed by Root-Bernstein, scientific objectivity (and, more broadly, scientific method) as traditionally understood limits our apprehension of nature's mysteries. The importance of this deep familiarity with one's material is humorously underscored by Samuel Scudder in the penultimate essay. In the final selection of Part III, the Poinars convey something of the excitement—and deep humility—that accompanied their discovery of soft tissue (insect cells) in ancient amber. For the Poinars, as for so many of the scientists represented/discussed in this collection, science involves far more than the purely rational self. Nature's mysteries demand our reverence as well as our reason.

ROBERT S. ROOT-BERNSTEIN

Setting the Stage for Discovery*

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*Originally appeared in *The Sciences*, May/June 1988. Reprinted by permission of the publisher.

His Rethinking AIDS: The Tragic Cost of Premature Consensus appeared in 1993. Root-Bernstein was one of the first MacArthur Prize Fellows and was, for several years, a contributing editor of *The Sciences*, published by the New York Academy of Sciences.

In the following essay, Root-Bernstein broadens the traditional definition of scientific method to include a range of attitudes and activities that can set the stage for discovery. He stresses the importance of intuition and even of deep personal engagement as a means of achieving scientific insight. On this, see also Keller and the Poinars (this Part).

Anyone familiar with the history or philosophy of science has heard some version of the story in which a researcher is going patiently about his daily grind—growing cell cultures or mixing chemicals or peering into a microscope—when, quite by accident, he makes some earthshaking discovery. Variations on the tale are myriad, but the moral is always the same: Great breakthroughs can be neither planned nor predicted; you just have to get lucky.

Consider, for example, the legend of how Louis Pasteur developed the cholera vaccine. According to the standard account, the French chemist might never have realized that weakened microorganisms can activate the immune system without causing serious illness had he not gone away on vacation during the summer of 1879. Pasteur had been experimenting with chicken cholera, the story goes, and happened to leave his germ cultures sitting out when he left Paris for more than two months. Upon his return, he found that the cultures, though still active, had become avirulent; they no longer could sicken a chicken. So he developed a new set of cultures from a natural outbreak of the disease and resumed his work. Yet he found, to his surprise, that the hens he had exposed to the weakened germ culture still failed to develop cholera. Only then did it dawn on Pasteur that he had inadvertently immunized them.

Equally fortuitous, according to conventional wisdom, was the German pathologist Oskar Minkowski's 1889 discovery that diabetes stems from a disorder of the pancreas. Minkowski had removed that organ from a dog to determine its role in the digestion of fat. After the operation, the dog happened to urinate on the laboratory floor. The urine drew flies, and the flies drew the attention of a sharp-eyed lab assistant. Puzzled, since flies are not normally attracted to urine, the assistant questioned Minkowski, who analyzed the urine and found it to be loaded with sugar. The obvious conclusion was that the pancreas was somehow involved in metabolizing that substance. (We now know that the pancreas contains islets of Langerhans, which secrete insulin, a hormone responsible for sugar metabolism.) The depancreatized dog turned out to be a perfect experimental model for diabetes. Yet, as legend has it, Minkowski would never have recognized this had the dog not relieved itself in the company of a swarm of flies and an alert lab assistant.

Still another breakthrough usually described as a fluke is the discovery of lysozyme—a bacteria-killing enzyme in tears, saliva, mucus, and other bodily fluids and tissues—by the British bacteriologist Alexander Fleming, in 1921. When Fleming began the work that led to this discovery, during the First World War, it was well known that the body had three lines of defense against infection—the skin (a physical barrier); macrophages (a type of white blood cell that ingests foreign material); and antibodies (proteins that neutralize toxins by adhering to them). But no one had even suggested there might be a fourth. Thus, we are told, Fleming was not looking for lysozyme; the

discovery resulted from a series of chance occurrences. First, some contaminant from the air fell into a culture dish in Fleming's laboratory, where it spawned a bacterial colony. Then, when Fleming leaned over his microscope to take a close look at this germ population, his nose dripped into it (he suffered from frequent winter colds). To his surprise, the drippings dissolved colonies of bacteria in the petri dish. He developed other cultures from the first one, subjected them to the same treatment, and obtained the same result. Further experiments confirmed that the mucus contained an antibacterial agent and showed that the agent was a proteinaceous substance that did not reproduce itself. He concluded it was an enzyme manufactured by the body.

Stories such as these (there are countless others, ranging from Wilhelm C. Röntgen's discovery of the X-ray to Jocelyn Bell Burnell's discovery of pulsars to Charles R. Richet's discovery of anaphylaxis—an extreme allergic reaction that can cause death) have led philosophers of science to draw a bold distinction between the process of *discovery* and that of *proof*, and to insist that logic and reason apply only to the latter. According to most standard texts—W. I. B. Beveridge's *Art of Scientific Investigation*, R. B. Braithwaite's *Scientific Explanation*, Carl Hempel's *Philosophy of Natural Science*, David Hull's *Philosophy of Biological Science*, Karl Popper's *Logic of Scientific Discovery*—discovery is a product not of particular methods of logical inquiry but of being in the right place at the right time. It could happen to anyone at any time. In contrast, the process of testing a hypothesis is said to be a more logical operation—one that only a rational inquirer, trained in the methods of science, can successfully perform. Unlike discovery, scientific validation is thought to consist of two distinct mental activities: induction (deriving general rules from particular instances) and deduction (making specific predictions based on general rules). The objective of science, according to this philosophy, is simply to validate or invalidate inexplicable insights.

By limiting themselves to explaining scientific validation, philosophers save themselves the trouble of trying to account for the rich, messy business of discovery. But this approach has drawbacks. It suggests, paradoxically, that illogical processes have led to the most logical constructs known to mankind—mathematics and science. And it fails to explain where problems come from, what scientists do from day to day, and how they actually think. Real scientists do not spend their lives cataloging the facts that follow from established principles, or noting the principles that are implicit in particular facts. As the French mathematician Henri Poincaré argued in *Science and Method*, such exercises would be largely pointless and endlessly boring. The passion of any real scientist is to expand our knowledge of the world, not merely to confirm it. That means searching out instances in which the codified rules of science fail to account for our experience, looking for paradoxes, contradictions, anomalies—in short, for problems. It is only after a problem has been identified that induction or deduction can serve a purpose, and only in relation to such a problem that an observation becomes a discovery.

So something is clearly amiss. The notion of accidental discovery assumes that anyone else seeing what Pasteur, Minkowski, Fleming, Röntgen, Burnell, or Richet saw would have come to the same conclusions. Yet, in each case, someone else was there and did not make the discovery. Pasteur's collaborator Émile Roux, Minkowski's unidentified lab assistant, and Fleming's colleague V. D. Allison, Richet reports that the experiment that caused him to invent the concept of anaphylaxis was so bizarre, his collaborators refused even to

countenance the results. And several people, including the English chemist William Crookes, observed the same phenomena that Röntgen did—fogged photographic plates and fluorescing barium platinocyanide screens—but did not appreciate the fact that these effects were created by previously unknown rays—X-rays—emitted by nearly cathode-ray tubes. Clearly, it is not sufficient simply to be in the right place at the right time. How a scientist interprets what he sees depends on what he expects. Discoveries do not just walk up and present themselves from time to time, disguised as chickens, dogs, or nasal drips.

Why not admit that discoveries derive from the ways in which particular scientists logically go about their work? Then, given that different scientists practice different styles of research, and that not all of them make discoveries, it should be possible to identify the styles that most often pay off. Surely, any mental activity that contributes directly to scientific discoveries should be recognized as scientific method. If such activities are not acknowledged by the prevailing view of how scientists use logic and reason, that does not mean the activities are illogical. It means that the prevailing view is too narrow to account for how scientists really think. The task, then, is to redefine the scientific method in a way that accounts for the process of discovery.

Were Pasteur's and Minkowski's and Fleming's breakthroughs really just accidents? A recent analysis of Pasteur's notebooks by the historian of science Antonio Cadeddu suggests that the discovery of the cholera vaccine was anything but. It was well known during the late nineteenth century that people who survive certain infectious diseases tend not to come down with them again. Pasteur had noted as much, and his experiments with chicken cholera were clearly designed to explore that phenomenon. He seems to have been consciously pursuing a problem: how to produce a microbe strong enough to cause some degree of illness (and thus to protect against future infection) yet not strong enough to kill. So he was not aimlessly inoculating chickens when he discovered the cholera vaccine; he was trying to use his own term, to "enfeble" the infectious agent.

Moreover, the breakthrough did not come about from his leaving flasks of germs unattended while he went on vacation. In fact, he left them in the care of Émile Roux. Pasteur did, upon his return, inoculate chickens with material from the flasks, and the birds did fail to become ill. But when the same chickens were later injected with a more virulent strain, they died. No discovery here. Indeed, the notebooks reveal that Pasteur did not even initiate his first successful enfeeblement experiment until a few months later, in October of 1879. He and Roux had tried to enfeble the germs by passing them from one animal to another, by growing them in different media, by heating them, by exposing them to air—anything that conceivably might weaken them—and only after many such attempts did one of the experiments succeed.

That winter, Pasteur managed, by placing germ cultures in acidic mediums, to enfeble them in varying degrees. For some time, the strains that failed to kill chickens were also too weak to immunize them. But by March of 1880, Pasteur had developed two cultures with the properties of vaccines. The trick, according to his notebooks, was to use a mildly acidic medium, not a strong one, and to leave the germ culture sitting in it for a long time. Thus, he produced an attenuated organism capable of inducing an immune response in chickens. The discovery, therefore, was not an accident at all; Pasteur had posed a question—is it possible to immunize an animal with a weakened infectious agent?—and then systematically searched for the answer.

Minkowski, too, was less reliant on dumb luck than is widely presumed. He probably was surprised to find sugar in the urine of the depancreatized dog; he had, after all, set out to investigate the role of the pancreas in the metabolism of fat. But Minkowski's own account of the discovery, published long after the popular version took hold, suggests it was neither a swarm of flies nor an alert lab assistant that brought the undigested sugar to his attention. It was, rather, his own carefully honed skills of observation and diagnosis, which he applied to an unexpected change in the dog's behavior.

Minkowski recounts that the dog, though fully housebroken before the operation, became an invertebrate floor wetter afterward. In medical terms, it developed polyuria—unusually frequent urination. Polyuria is a classic symptom of diabetes, and Minkowski had learned in medical school that if a patient developed that symptom, the way to find out whether he had diabetes rather than, say, a bladder infection was to test the urine for sugar. Once Minkowski had asked the right question—namely, Why does a depancreatized dog suddenly develop polyuria?—standard medical procedures provided a ready answer: the urine was found to contain sugar and, as expected, the dog eventually developed all the symptoms of diabetes. So it followed that diabetes stems from a pancreatic disorder.

Minkowski's discovery was clearly a surprise (he had not even set out to study diabetes), but that is not to say it was a random occurrence. The dog's indoor accidents did not just happen; they were an inevitable result of the pancreas experiment. Nor was it by fluke that Minkowski found the dog's problem significant. His response was a consequence of his expectations. Had he not known the dog, he might have assumed that it always urinated on the floor. And had he not been familiar with the symptoms of diabetes, he might never have suspected that he had induced it in the dog. In short, Minkowski's discovery consisted not of what he saw but of how he saw it.

What about Fleming's discovery of lysozyme? According to the accepted accounts, there was no logic whatever to this breakthrough; it grew out of at least three totally unpredictable occurrences: first, Fleming got a severe cold; second, at about the same time, his petri dish was mysteriously contaminated by one of the few bacteria sensitive to lysozyme; third, he happened to contaminate the same dish with a drip from his nose and still did not discard it. In fact, one need only consult Fleming's notebooks to see that he quite literally cultivated the circumstances surrounding his discovery. The initial contaminant turns out to have been a bacterium harvested from his own nose—and his faithful drip into this culture, part of a deliberate experiment.

The purpose of the experiment was to determine whether colds might be caused by bacteriophages—viruses that cause illness by destroying resident bacteria in a host's body. The bacteriologist Frederick W. Twort had discovered bacteriophages in 1915, and a few years later, another bacteriologist, Félix d'Hérelle, isolated them in locusts with diarrhea and in humans with dysentery. The cold-prone Fleming was personally interested in learning the cause of the common cold, and a simple, flippancy play on words may have led him to suspect bacteriophages. Might not "runny noses" and "runny bottoms" be the work of related agents? The way to find out was to extract bacteria from normal nasal mucus and then determine whether a cold sufferer's mucus contains agents capable of destroying it.

Fleming reported that it took him four days to cultivate a suitable bacterial colony. (The chance-drip version of the story is further belied by his assistant W. Howard Hughes's recollection, in *Alexander Fleming and Penicillin*, that Fleming had attached a leather gourd to his microscope to prevent such accidents from occurring.) A few drops of his cold-infected mucus are holes into this lawn of bacteria—just as bacteriophages do. Fleming spent the next few weeks conducting additional tests to make sure. But things started going wrong.

The easiest way to find out whether a solution contains bacteriophages is to dilute it repeatedly. Because bacteriophages are self-replicating, a solution containing them will return to its original potency within a few hours. Thus, when Fleming's solutions did not regain their strength, he began to suspect that he was dealing with an enzyme. (Because enzymes are body products, not organisms, they are not self-replicating; the more an enzyme preparation is diluted, the less activity there is.) Furthermore, Fleming found that the agent he had isolated could be inactivated by heat, as other enzymes can, and chemical tests demonstrated that it had the proteinaceous composition of an enzyme. The original hypothesis was foiled: the antibacterial agent obviously was not an invading bacteriophage. Instead, Fleming had discovered a new enzyme. He soon published his discovery, adding lysozyme to the pantheon of recognized bodily defenses. But he never publicly explained how he had happened upon lysozyme, and, hence, the story of the contaminated cell culture and the accidental drip was invented to make up for historical ignorance.

As Minkowski and Pasteur did, Fleming succeeded only after failing, but he did not succeed by chance. Had he not conceived of a possible link between intestinal disease and the common cold, he would not have been looking for bacteriophages in his nasal mucus. And had he not expected his would-be bacteriophages to reproduce in solution, he would not have performed the tests that led to his recognition of a new enzyme.

Virtually every so-called chance discovery that has been reexamined in the light of additional historical evidence has had to be revised in the manner of the Pasteur, Minkowski, and Fleming stories. Again and again, the record reveals that the discovery is not a fluke but the inevitable, if unforeseen, consequence of a rational and carefully planned line of inquiry initiated by a scientist. It follows that, contrary to philosophical orthodoxy, the tests of an incorrect hypothesis often result in surprises that lead to discovery, and that discoverers are not just beneficiaries of fate. They seem to have ways of courting the unexpected, which improve their chances of making novel observations. So there must be a logic, or at least a set of strategies, in discovery. The question is, Why are discoveries made by certain scientists rather than others? Can their strategies be learned?

I think they can. But such strategies are not so easily codified as are the rules of scientific proof, for they pertain to everything from recognizing interesting problems to appreciating unexpected results. How a scientist handles these matters is a function of his entire personality—the sum of the interests, skills, experiences, and desires that define him as a human being. Still, it should be possible to identify some of the habits of thought that are particularly advantageous.

It is striking how many great scientists have incorporated play into their lives and work, how many have consciously avoided being overly cautious or orderly, or narrowly dramatic. Fleming, for one, was famous for his love of games. He was raised in a family that played everything from poker and bridge to table tennis and quiz games. As an adult, he played croquet, bowls, and snooker at his club, and pitched pennies at

his office whenever he lacked patients. He took up golf, too, but rarely played a straight game; he would put holding the club as a snooker cue, or revise the rules to make the game more interesting. Life was essentially a game to him, and so was research. "I play with microbes," he once said, adding, "It is very pleasant to break the rules."

In the laboratory one of Fleming's favorite pastimes was to fashion art from germs. He would start with an assortment of microorganisms and, knowing which color each one would produce as it multiplied, paint them onto a petri dish. After incubating the dish for a day, he would unveil a picture of his house or a ballerina or a mother nursing a baby. Fleming was no great artist, but his hobby fostered a rare intimacy with the bacterial world. To paint his pictures, he had to know not only which germ would produce which color but also how rapidly each would proliferate at a given temperature. To maintain a diverse palette, he also had to be constantly on the lookout for bacteria that might suit his purposes. To this end, he made a point of creating environments in which unusual germs might crop up. V.D. Allison recalls in a lecture to the Ulster Medical Society, just how conscientiously Fleming practiced this method:

At the end of each day's work I cleaned my bench, put it in order for the next day and discarded tubes and culture plates for which I had no further use. He, for his part, kept his cultures . . . for two or three weeks until his bench was overcrowded with forty or fifty cultures. He would then discard them, first of all looking at them individually to see whether anything interesting or unusual had developed.

Fleming was not alone in his tendency to mix things up a bit to see what would happen. Konrad Lorenz, the great animal behaviorist, was equally scrupulous about cultivating fruitful confusion. Lorenz lived among his research subjects: dozens of species of mammals, birds, reptiles, and fishes. He did not quantify, control, or consciously experiment. He got to know each creature individually, then threw them together, watching for the unexpected, the unusual, or the bizarre in the chaos that followed. For example, his interest in one of ethology's most important concepts, that of intention movements (motions with meaning, such as the head bobbing in birds that serves as an alarm signal before flight), derived from an inadvertent experiment. He had trained a free-flying raven to eat raw meat from his hand and had been feeding the bird on and off for several hours one day. He would reach into his pants pocket and take out a piece of meat, and the raven would swoop down to grab it in its bill. By and by, Lorenz went to relieve himself near a hedge. When the raven saw him put his hand into his pants and pull out another morsel of meat, it swooped down, hungrily grasping the new mouthful in its bill. Lorenz howled in pain. But the event left a deep impression on him—about how faithfully animals respond to intention movements, that is.

One mental quality that facilitates discovery, then, is a willingness to goof around, to play games, and to cultivate a degree of chaos aimed at revealing the unusual or the unexpected. Looking back on the scientists who misused discoveries—Allison in Fleming's lab; Richter's collaborators on the anaphylaxis experiments; Crookes, Röntgen's colleague—we see that, in each case, they refused to credit a phenomenon with significance because it was not what they were looking for. "It's just a contamination." "You must have injected the wrong solution." "Send the photographic plates back to the manufacturers and tell them they'd better deliver good ones tomorrow or we'll cancel our order."

A classic example of such a reaction was reported by Jocelyn Bell Burnell in an interview concerning her discovery of pulsars. She had been pointing her radio telescope toward a region of the heavens at a time when she expected to pick up only a weak signal, when the pen on the recording device started jiggling. Repeating the observation at weekly intervals yielded the same result, and rest after rest revealed nothing wrong with the equipment. Eventually, Burnell realized she had detected the presence of stellar sources of pulsating radio waves, or pulsars, which astronomers had hypothesized but never found. Sometime later, she heard that a colleague had observed the same phenomenon, given his equipment table a good kick, and written off the result as a mechanical aberration. We may presume he later kicked himself.

Not every anomaly or unexpected result leads to discovery, of course. As Sherlock Holmes once said, "It is of the highest importance in the art of detection to be able to recognize out of a number of facts which are incidental and which vital." However, Charles Richter, in *The Natural History of a Seism*, and the physicist George P. Thomson, in *The Strategy of Research*, both warn that there is no correlation between the difficulty of a problem and its importance. The most trivial observation can, in the mind of a scientist possessed of imagination, yield surprises of the greatest significance.

To elevate the trivial to the universal, the scientist must first of all, be a global thinker; that is, he must be able to perceive how certain principles apply to diverse phenomena. The biochemist Albert Szent-Györgyi provides a good example. His discovery of the universal principles by which oxygen reacts with living tissue stemmed from his observation that bananas and lemons react differently with oxygen: bananas turn brown when they are bruised, but lemons do not. He concluded that lemons contain something that affects the way they react with oxygen and later found that something to be ascorbic acid—Vitamin C. But Szent-Györgyi's insight did not end there. He realized that similar oxidative reactions must occur in all living organisms and went on to demonstrate how muscle tissue uses oxygen. "Looking back on this work today," he said years later, "I think that bananas, lemons, and men all have basically the same system of respiration, however different they may appear."

In the search for universal truths, a scientist is also wise to know intimately, even to identify with, the things or creatures he studies. Lorenz was fully aware of all his animals' normal behaviors—feeding, fighting, mating, nesting, imprinting, rearing, and so on—so he could recognize when a behavior was exceptional. And Pasteur and Fleming had the same complete familiarity with microbes. But intimacy means more than mere knowledge. In an interview, the geneticist Barbara McClintock, winner of the 1983 Nobel Prize in medicine, described her method of research as having "a feeling for the organism." Speaking of her work on the chromosomes of the *Neurospora* fungus, she said:

I found that the more I worked with them, the bigger and bigger [they] got, and when I was really working with them I wasn't outside, I was down there. I was part of the system. . . . I even was able to see the internal parts of the chromosomes—actually everything was there. It surprised me because I actually felt as if . . . these were my friends. . . . As you look at these things they become part of you. And you forget yourself. The main thing about it is you forget yourself.

The mathematician Jacob Bronowski, in an essay in *Scientific American*, wrote that it is this "personal engagement" of the scientist that differentiates him from a mere technician. The physicist-philosopher Michael Polanyi calls this "personal knowledge."

The reward for such internalization of subject matter is intuition. The scientist learns to sense what is expected, to *feel* how the world ought to work. Peter Debye, a Dutch-born American who won the 1936 Nobel Prize in chemistry for his work on molecular structure, said once that he would ask himself, "What did the carbon atom want to do?" The virologist Jonas Salk, discoverer of the polio vaccine, writes in *Anatomy of Reality*, "I would picture myself as a virus, or as a cancer cell, for example, and try to sense what it would be like to be either. I would also imagine myself as the immune system . . . engaged in combating a virus or cancer cell."

In essence, intuition is the ability to sense an underlying order in things, and thus is related to still another mental tool that is indispensable to the working scientist: the perception of patterns, both visual and verbal. The Russian chemist Dmitri I. Mendeleev's periodic table of elements is a classic example of how ordering facts yields new insights. Before he conceived it, in 1868, chemists had had great difficulty perceiving relationships between the elements. Mendeleev noticed that when he arranged all the elements on a chart, according to their atomic weights, the chemically related elements appeared at regular, or periodic, intervals. (For example, magnesium, calcium, and strontium, all of which occur in the same column, have the same valence, or number of orbitals available for bonding.) His table had many gaps, but Mendeleev correctly predicted the existence of missing elements, and scandium, gallium, and germanium, among others, were duly discovered during his lifetime.

All good theories contain, at heart, an ordering process that reveals hidden patterns. Consider how Pasteur discovered the phenomenon of molecular asymmetry—the way organic molecules exhibit what are called right-handed and left-handed forms. The discovery grew out of Pasteur's search for the molecular differences between racemic and tartaric acids, both of which are wine byproducts that form crystals on the inner surfaces of casks during the fermentation process. The German chemist Eilhard Mitscherlich had concluded that the two acids not only had the same chemical composition and specific gravity but also seemed to form identical crystal structures. The only difference between them, he believed, was that a beam of polarized light would pass directly through a racemic acid crystal but would be bent, or deflected, as it passed through a crystal of tartaric acid.

Pasteur was puzzled by the notion that two crystals, identical in structure, would differ in this key respect, for studies of quartz crystals had suggested that differences in their ability to bend polarized light always corresponded to differences in crystal form. He hypothesized that Mitscherlich had been wrong and that the light-deflecting racemic molecules would turn out to be asymmetrical in structure and the tartaric ones, symmetrical. Pasteur placed samples of both acids under the microscope and noted that there were in fact slight irregularities in the racemic molecules. Yet he also discovered, to his surprise, that the tartaric molecules were slightly irregular. This finding shot down Pasteur's initial hypothesis—that a crystal's optical activity reveals whether its molecules are symmetrical or asymmetrical—but the paradox led him to a better theory. By designing further experiments, he figured out that tartaric acid had only one asymmetrical form, a right-handed form that caused it to bend polarized light, whereas racemic acid had two asymmetrical forms—right-handed and left-handed—which nullified each other's ability to deflect light.

Pasteur's advantage over the various crystallographers who had studied the same molecules and failed to detect their asymmetry was not that he had better eyesight (he was nearsighted) or that he was better at constructing hypotheses (indeed, his first one was wrong). It was that his logic and perceptual skills (as a teenager, he had been trained as an artist) gave him an edge. First, he insisted that the tartarates fit the pattern of previously studied compounds, and, second, he looked at molecules that other scientists regarded as identical, and he recognized differences in their structures.

Verbal patterns are sometimes just as suggestive as visual ones. Fleming, you will recall, had no reason beyond the verbal symmetry of runny bottoms and runny noses to seek a biological connection. And more than one valuable idea has begun as a pun. In 1943, when the biologist Ralph Lewis of Michigan State University started a study of how fungus is disseminated by insects, he could not get his flies to pick up the fungal spores he was placing in their mids, so his experiment came to a halt. The problem was that the spores were drying out in the laboratory. In nature, spores are released by the fungus in a sweet, moist substance called honeydew. Free-ranging insects are attracted to the fungus by the honeydew, and it sticks to their legs, carrying the embedded spores with it. The question that occurred to Lewis was, Would honey do? In fact, honey would do just fine, and the experiment could proceed. Lewis's idea was not a formal, logical inference, but it was a perfectly good one.

It should be clear by now that scientific discovery is never entirely accidental. It holds an element of surprise, to be sure, the effective surprise that changes a person's perception of nature. But the best scientists know how to surprise themselves purposely. They master the widest range of mental tools (including but certainly not limited to, game playing, universal thinking, identification with subject matter, intuition, and pattern recognition) and identify deficiencies or inconsistencies in their understanding of the world. Finally, they are clever enough to interpret their observations in such a way as to change the perceptions of other scientists, as well. As Albert Szent-Györgyi put it, "Discovery consists of seeing what everybody has seen and thinking what nobody has thought."

We are forced to accept still another conclusion. The process of discovery is not distinguishable from that of logical testing. In each of the examples discussed above—Pasteur, Minkowski, Fleming, and all the rest—the tests neither validated nor invalidated the initial hypotheses but ended in surprise. What each of these scientists discovered was a new problem, an anomaly that led to the discovery of something else. Thus, it appears that the most important discoveries arise not from verification or disproof of preconceptions but from the unexpected results of testing them.

This has practical implications. At present, we fund experiments whose results are foreseeable rather than those that are most likely to surprise. Similarly, we train scientists almost solely in the methods of demonstration and proof. And students are evaluated on their ability to reach correct, accepted conclusions. This sort of education is necessary, but it is also insufficient, serving only to verify what we know, to build up the edifice of codified science without suggesting how to generate problems of the sort that lead to new discoveries.

A startling conclusion? Perhaps, but this is the message contained in the work of Pasteur, Minkowski, and Fleming, and dozens of other successful scientists. What is intriguing is how historians and philosophers persist in ignoring their testimony. Is it that we perceive only what we expect to see?