#601

In R.L. Kuhn (Ed.), Frontiers in creative and innovative management. Cambridge, MA: Ballinger Publishing Co., 1985.

Chapter 1

WHAT WE KNOW ABOUT THE CREATIVE PROCESS

Herbert A. Simon

Research on creativity has been carried out most often in the natural sciences, to a lesser extent in the arts and humanities, and to a very slight extent in professional domains like management or the law. Under these circumstances, unless we are willing to assume that creativity, in whatever domain it appears, relies upon essentially the same processes, there is little we can say about the processes of creative management. However, I think there is much reason to believe that there is, indeed, a great commonality among the creative processes, wherever they appear. If that is so, a review of the processes as they evidence themselves in scientific discovery will be of interest and value to all concerned with managerial creativity.

In this paper, I should like to review some recent research on scientific discovery and to describe the creative process as that research reveals it. Then, in the final sections, I would like to draw some lessons for creativity in management.

CREATIVITY

At one point in history, about forty years ago, the federal courts put themselves in the position of requiring that for an invention to be patentable there must be proof that a "spark of genius" had occurred. The language was Mr. Justice Hand's, and it plagued the courts for at least a decade until it was mercifully more or less forgotten.

The trouble with sparks of genius, and similar evidences of creativity, is that they are not photographable, hence are difficult to introduce into evidence in a federal courtroom. As long as we refer to acts of creativity with awe and emphasize their unfathomability, we are unlikely to achieve an understanding of their processes. And without such an understanding, we are unlikely to be able to provide usable advice as to how to encourage and enhance them.

Fortunately, it is not necessary to surround creativity with mystery and obfuscation. No sparks of genius need be postulated to account for human invention, discovery, creation. These acts are acts of the human brain, the same brain that helps us dress in the morning, arrive at our office, and go through our daily chores, however uncreative most of these chores may be. Today we have a substantial body of empirical evidence about the processes that people use to think and to solve problems, and evidence, as well, that these same processes can account for the thinking and problem solving that is adjudged creative.

Symbol Systems

The evidence to which I have just referred, and which I will presently develop in more detail, supports two central hypotheses:

1. Thinking is information processing that involves reading symbols, writing symbols, assembling symbols in relational symbol structures, storing symbols, comparing symbols for identity or difference, and branching on the outcome of the comparison. Intelligence calls for these, and only these, processes.
2. The processes required for creative acts are the same as those required for all intelligent acts.

The first hypothesis, sometimes referred to as the Physical Symbol System Hypothesis, has as corollaries, first, the assertion that computers (since they are symbol systems with the requisite processes) can be programmed to behave intelligently, and second, that human beings use these same symbolic processes (embodied in distinctly different "hardware" from computers) to accomplish thinking and other intelligent acts. None of these assertions need be taken on faith: they are all empirical hypotheses that can be (and have been, extensively) tested in the laboratory. The first corollary can be tested by programming computers to behave intelligently, the second by analyzing the processes that people use in handling difficult intellectual tasks.

Definition of Creativity

But let me start at the beginning. My basic claim is that creativity is "thinking writ large." Before we can test this claim, we must have a definition of creativity. The simplest way to find such a definition is to observe when people apply the term "creative" to some human act. What is the basis for such an attribution?

Acts are judged to be creative when they produce something that is novel and that is thought to be interesting or to have social value. Interesting or valuable novelty is the touchstone of the creative. Acquaintance with a creative act, one's own or another's, is often accompanied by surprise: "How did he (or she) manage to do *that?*" This quality of unobviousness partly accounts for the sense of mystery and awe that creativity often evokes.

Novelty can have either of two meanings: it can mean wholly new in the world or it can mean new to the discoverer. Usually, the medal of creativity goes only to the *first* discoverer. Second discovery, however independent, wins no awards from the U.S. Patent Office. There are exceptions, however. We celebrate the birthday of Columbus, although his discovery was rather thoroughly anticipated by the American Indians. Histories of science are also sometimes kind to independent discoverers. They remember Leibnitz as an inventor of the calculus, although the historical record shows that Newton had ten years' clear headstart. Newton failed to publish promptly, however, and it was Leibnitz's version of the calculus that was diffused and developed.

Independent discovery may also be used as evidence of the discoverer's abilities, for the processes must be the same as those employed in first discovery. When the young Gauss immediately found the formula for the sum of the first N integers, his teacher correctly predicted that he would be a creative mathematician, even though the formula was old hat to trained mathematicians. Thus, we have creativity in the weaker, or individual, sense, and creativity in the stronger, or social, sense, and rightly regard the former as a harbinger of the latter.

Of other uses of the term "creativity"—"creative advertising" or "creative writing"—I have little to say. One has the impression that such language is loose, or at least generous to the products to which it is applied. But in the last analysis, each field must make its own judgments of creativity; each must decide what is novel and what products are interesting or valuable. There are no reasons to suppose that the basic processes underlying the humbler forms of creativity are different in kind from those that account for the great leaps (which are not really leaps but successions of tiny steps) of a Newton or a Leibnitz. I think that even applies to singing commercials. What we wish to understand, then, are the sequences of processes that enable a man or a woman or a child to bring into being something that is novel and interesting or valuable.

The Discovery of Planck's Law

People make little discoveries daily or hourly in their everyday lives. Great discoveries are, by definition, rare events. Our hypothesis is that the processes that underlie both little and great discoveries are basically the same. Since we cannot ordinarily produce great discoveries in the laboratory, most of our evidence for the processes of significant invention must be historical. Let me recount to you one piece of evidence that is a little more direct.

The so-called problem of black-body radiation was posed by the great physicist, Kirchoff, in about 1860. The problem was to find the formula describing the intensities of radiation of different wavelengths that would be given off by a perfectly absorbing cavity ("black body") at a given temperature. Many distinguished physicists struggled with the problem over the next forty years, among them the young Max Planck. The problem was both experimental (to devise apparatus permitting the radiation to be measured over wider and wider ranges of wavelengths) and theoretical (to find both a formula that would fit the empirical observations and a physical explanation for the formula).

By about 1896, it was thought that an answer had been found. A formula called Wien's Law provided an excellent fit to the data then available. Moreover, in 1899, Planck believed that he had proved, from basic classical physical principles, that Wien's Law was the only acceptable formula. Alas, in science, new experimental data can always be counted on to cause trouble. By the middle of the year 1900, experimentalists, penetrating a new range of wavelengths, had made observations that could in no way be fitted by Wien's Law. Where Wien's Law called for an exponential function, in the new range the observed function was linear.

On Sunday afternoon, 7 October 1900, Heinrich Rubens, one of the experimentalists in Berlin working on black-body radiation, called with his wife upon the Plancks and described the new findings that clearly violated Wien's Law. Before he went to bed that night, Planck had conjectured a new formula to replace the defective one and had mailed a postcard to Rubens describing it. The new formula is what we have known ever since as Planck's quantum-theoretic law of black-body radiation. Its public announcement on 19 October 1900 marked the beginning of twentieth century physics.

How did Planck do it? In the spirit of casual empiricism, I have carried out the following experiment. On eight occasions I have sat down at lunch with colleagues who are good applied mathematicians and said to them: "I have a problem that you can perhaps help me with. I have some very nice data that can be fitted very accurately for large values of the independent variable by an exponential function, but for small values they fit a linear function accurately. Can you suggest a smooth function that will give me a good fit through the whole range?"

None of my colleagues recognized the problem as Planck's—and there was no reason why they should have. Five of the eight proposed Planck's Law as the answer, each within the space of two minutes or less. When asked how they arrived at it, all five described rather standard methods of interpolation. (The most common was to expand the exponential into a Taylor's series and to observe that for small values of the independent variable the resulting function, less unity, was linear through the origin.)

The moral of the story, I suppose, is that Planck was the right person at the right place at the right time. The role of time and place are obvious. None of my colleagues will receive the Nobel Prize for solving Planck's problem. As to the person, Planck clearly possessed both an ardent interest in the problem and the mathematical skills to solve it.

But the story has a sequel. Finding Planck's Law did not provide a physical explanation of why it should hold. Planck discovered such an explanation in the two months following his discovery of the law and published it before the year was out. He later described the activity of these two months as the hardest work of his life.

He began with the rationalization he had earlier derived of the faulty Wien's Law, seeking the loophole in his derivation. He also had the "correct" new formula to guide him—he could work backward from this answer and could reject any purported explanation that did not lead to it. In the event, he succeeded by introducing a probability assumption that was quite unusual and that had no particular justification except that it produced the desired result. Moreover, it required the introduction of a particular constant (which we now call "Planck's constant" or the quantum constant, h), which first appeared to him as merely a computational-trick. In fact, it took five years, and the work of other physicists including Einstein and Ehrenfurst, before the truly revolutionary import of these assumptions began to become apparent. One could say that physics backed into quantum theory through the constraints imposed by an empirical law upon the possible physical explanations for a phenomenon.

It does not in any way demean Planck's achievement to trace its history in this detailed way, to observe how little of the final result was anticipated, and to note that the sequence of events seems to have proceeded along a quite normal course of problem solving. In science, we do not wonder at natural phenomena because they are mysterious. We wonder at them because we find that the beauties and complexities of nature can be understood in terms of relatively simple and orderly underlying mechanisms. The magic is that there is no magic. The natural phenomena are truly "natural."

Moreover, it was no accident that it was Planck who provided a physical rationalization for the new black-body radiation formula. By 1900 he had already devoted a decade of his life to intense work on the problem. There were probably not more than two or three other theoretical physicists who came even close to him in the amount of effort spent in probing the problem and conjecturing sets of possible physical mechanisms that could contribute to its solution. As Thomas Kuhn's detailed study of the background of the discovery has shown, Planck had studied thoroughly all of the branches of physics—electromagnetism, thermodynamics, statistical mechanics—that played a role in the mechanism he finally postulated. Chance, in the words of Pasteur, had favored the prepared mind.

THE (RE)DISCOVERIES OF BACON

There are not many opportunities to test the processes of scientific discovery in the way in which I was able to recreate Planck's discovery of his formula. An alternative way is to see how far we can go toward constructing a computer program capable of making significant discoveries. A computer is patently a physical symbol system—nothing more. We know exactly what operations it can perform, and we can examine its programs to determine exactly what operations those programs employ to accomplish their work. If a program can make discoveries that, if made by a human, would be regarded as creative, then the processes it used (unless they amounted to nothing more than blind trial and error) will tell us something about the creative process.

The program I should like to tell you about is named BACON, in honor of Sir Francis Bacon, whose theory of discovery by induction provides much of the groundwork for BACON's procedures. In its earliest form, BACON (the program, not the man) was created by Pat Langley as his doctoral thesis, and it has since undergone extensive development at the hands of a research team including Langley, Gary Bradshaw, Jan Zytkow, and me.[1]

At least in its simplest forms, BACON, following the principles proposed by its namesake, is a data-drive inductive system. Its inputs are raw observational or experimental data, and its outputs, when it is successful, are scientific laws that describe the data parsimoniously. Now I do not wish to suggest that data driven induction is the only mode of scientific discovery, although a study of the history of science shows that it is an exceedingly common one. In many cases, discovery is guided not only by data but also by relevant theory, and in domains of science where strong theories are already in place, logical deduction can often lead to the prediction of new data or empirical phenomena, which are subsequently observed.

Nor do I wish to claim that the extraction of laws from data and/or theory is the sole important aspect of scientific research. The process of defining problems for study and selecting relevant data is also important (although it is worth observing in passing that the problem Planck solved was one that had been defined for him by others, and that there was no ambiguity as to what data were relevant). Also important are the design of experiments and the devising of new instruments of observation. All of these activities, and others, call for creativity. Nevertheless, the induction of laws will provide us with a useful domain within which we can examine creative processes.

BACON at Work

When BACON is given data on the distances of the planets from the sun and their periods of revolution about the sun, it produces (in less than a minute on a computer of moderate size) Kepler's Third Law: the period, P, varies as the 3/2 power of the distance, D. Kepler's Third Law was a scientific discovery of the first magnitude; hence BACON's independent rediscovery must be accounted to be creative. How was it accomplished?

BACON generates and considers possible laws very selectively, being guided in its search by selective heuristics, or rules of thumb. In the search for Kepler's Law, two heuristics allow BACON to arrive at the result very quickly. A scientific law expresses some invariant of the data. BACON's first heuristic leads it, when it finds that two variables are positively correlated, to compute their ratio and test it to see if it is invariant. (If the variables are negatively correlated, BACON computes and tests their product.) Its second heuristic leads it, when the result of the first step is unsuccessful, to add the newly computed variable to its set of data and try the same process over again.

Thus, BACON notices that period and distance are positively correlated, and computes the ratio, P/D, which is not a constant. It now notices that D and P/D are positively correlated and computes the ratio, P/D^2, which again is not constant. Next, BACON notices that P/D and P/D^2 are negatively correlated, hence computes their product, obtaining P^2/D^3, which is an invariant. Hence, P varies as the 3/2 power of D.

An even more interesting case is BACON's rediscovery of the law of conservation of momentum. In this case, the only data given BACON are the relative accelerations of pairs of bodies connected by a stretched spring that is released. BACON first discovers, using the same heuristics as before, that for any given pair of bodies, the ratio of accelerations is always constant—but with different constants for different pairs. When BACON discovers an invariant relation between pairs of objects, it conjectures that this relation can be stated more simply by attributing a new property to each of the objects, and expressing the relation in terms of that property. In this case, BACON invents, and assigns to each body, a property (which we call *inertial mass*) and finds that the product of these masses by the corresponding accelerations, summed over the pair of bodies, is zero. This, of course, is the law of conservation of momentum.

In the same way, when BACON is given data on the temperatures of liquids and their mixtures, it introduces the concept of specific heat and discovers Black's Law of temperature equilibrium. When given data on the refraction of light passing from one medium to another, it introduces the concept that we know as the index of refraction. BACON is not limited, then, to discovering numerical laws; it can also invent new concepts.

Various extensions of BACON, which we have named GLAUBER and STAHL in honor of distinguished chemists who made important discoveries in the early history of modern chemistry, are capable of using qualitative information about the inputs and outputs of chemical reactions to discover qualitative laws. GLAUBER, for example, by searching for common components in different reactions, is able to define such classes as *acid, base,* and *salt.* STAHL, given information about reactions involving combustion, arrives at either the (erroneous) phlogiston theory of combustion or the (correct) oxygen theory, depending upon the way in which the reactions are described.

More recent experiments with BACON are aimed at discovering laws that do not merely describe phenomena but explain them as well. For example, if it is known or conjectured that the quantity of heat is conserved when liquids are mixed and that the mass of the mixture is equal to the sum of the masses of the components, then simple inferences can be used to *deduce* Black's Law of temperature equilibrium, instead of inducing the law from the experimental data. Thus, conservation laws, laws of symmetry, and other a priori assumptions can be introduced to guide the search for scientific laws, making the search (when successful) far more efficient than when it depends solely on examining the empirical data and providing explanations of the regularities in terms of conservation or symmetry.

Another offspring of BACON, which we call DALTON, uses a simple atomic hypothesis to guide its search. It starts with the assumption (as nineteenth century chemists did) that chemical substances are made up of atoms "packaged" in molecules. It assumes further that the total numbers of each kind of atom are conserved in chemical reactions, and (Gay-Lussac) that equal volumes of gases under standard conditions of pressure and temperature represent equal numbers of molecules. With these assumptions, it is able to infer the chemical makeup of many molecules and to demonstrate, for example, that gases like oxygen and hydrogen are diatomic, a fact that eluded chemists for many years in the nineteenth century. With small modifications in its structure, DALTON can induce from Mendel's original data on sweet peas the laws of Mendelian inheritance.

All of these experiments were aimed at showing that scientific discovery is an understandable phenomenon that can be explained in terms of the same kinds of basic information processes that account for other kinds of human problem solving and thinking. It involves search through large spaces of possibilities, the search being guided and made efficient by the use of heuristic principles and previously developed theory.

The Prepared Mind

Earlier, I quoted the saying of Pasteur that "chance favors the prepared mind." "Accidental" discoveries are exceedingly common in the history of science. All of us are familiar with the story of Becquerel's discovery of radioactivity, or Fleming's of penicillin. Those discoveries could have been made by other scientists than Becquerel or Fleming, but they could not have been made by just anyone. Assigning the "accidents" to randomly chosen members of the population would not have done the trick.

To exploit an accident—the image that appeared on Becquerel's photographic plate or the destruction of bacteria in proximity to the penicillium molds—one must observe the phenomenon and understand that something surprising has happened. No one who did not know what a dish of bacteria was supposed to look like could have noticed the pathology of the dish that was infected by the mold nor would have been surprised if it had been called to his or her attention. It is the surprise, the departure from the expected, that creates the fruitful accident; and there are no surprises without expectations, nor expectations without knowledge.

A study by my colleague John R. Hayes of world-class experts in a number of different domains, including chessplaying, painting, and musical composition, shows that no one reaches a world-class level before he or she has devoted a decade or more of intensive effort to acquiring knowledge and skill about the domain of expertise. (Bobby Fisher, who became a grandmaster only nine years and some months after learning the game of chess, is a near-exception, but the only one.) Child prodigies are not exempt from this rule. Mozart was composing music (but not especially creative music) by age four, but his first world-class compositions were written no earlier than his late teens or early twenties (depending on one's standard). Picasso, whose father was a professional painter, painted from early childhood, but his productions were not world class until after his move to Paris in early adulthood.

Expertness, in turn, is the prerequisite to creativity. One need only visit a regional art exhibit and then an international one to realize that amateurs are not a major source of the world's important innovations. In making his claim, we must be careful: the vital point is the possession of *relevant* skill and knowledge, and at certain key periods in the history of science and of other domains, the relevant knowledge comes from a field other than the one to which it is applied. That is why many of the major discoveries of modern molecular biology were made by biochemists or even physicists, rather than by traditionally trained biologists. The ten years of dues that the world-class expert must pay must be paid in the right field, and choosing that field may itself involve accident and gambler's luck.

We even have some knowledge about how *much* knowledge the world-class expert needs and how it is organized in his mind and brain. A college graduate is likely to have a vocabulary of 50,000 words (or even twice or four times that) in his or her native language vocabulary. Each word is immediately recognizable when it is heard or seen in print and, upon recognition, evokes from long-term memory a more or less rich set of meanings and associations. A psychologist would say that each person has 50,000 familiar chunks of knowledge, each accessing information in long-term memory through an act of perceptual recognition. A computer specialist might call each of these chunks a production, a pair consisting of a set of conditions to be tested and an action to be taken whenever these conditions are tested. The conditions are, of course, the recognition cues; the action is the evoking of the associated information from memory.

Several estimates have been made of the number of chunks (in this case, patterns of pieces that recur frequently on the chessboard during games) held by a chess grandmaster. These estimates again range around 50,000. Fifty thousand is not a surprising number, given the ten years of effort during which the grandmaster is acquiring these chunks. Now that we are beginning, in artificial intelligence, to build expert systems in a number of domains, these projects are also providing us with estimates of the amounts of knowledge required. None of the systems built thus far, with the possible exception of the CADUSEUS medical diagnosis program, comes close to 50,000 chunks; but of course, most of the extant systems are relatively primitive and quite restricted in the range of their expertness.

Until we have better numbers, ten years and 50,000 chunks will serve as informative parameters for indicating the effort and knowledge that is prerequisite to expertness and, hence, to creativity. If these are necessary conditions, it would be unreasonable to claim they are sufficient conditions. Yet they suggest that hard work and persistence represent a very large part of the ingredients that go into creative performance. We should not be surprised if we find that many (most?) highly creative people behave like workaholics.

Taking Risks

Science is an occupation for gamblers. Of course, journeyman science can be done without much risk taking, but highly creative science almost always requires a calculated gamble. By its very nature, scientific discovery derives from exploring previously unexplored lands. If it were already known which path to take, there would be no major discovery—and the path would most likely have previously been explored by others.

In this respect, successful scientific research has much in common with successful stock market investment. Information is only valuable if others do not have it or do not believe it strongly enough to act on it. The investor is pitting his knowledge, beliefs, and guesses against the knowledge, beliefs, and guesses of other investors.

In neither domain—science or the stock market—is the professional looking for a "fair bet." On the contrary, he or she is looking for a situation where superior knowledge—knowledge not yet available to others—can be made, with some reasonable assurance, to pay off. Sometimes that superior knowledge comes from persistence in acquiring more "chunks" than most others have. Sometimes it comes from the accidents that have already been mentioned. But whatever its source, it seldom completely eliminates the element of risk. Investors and scientists require a "contrarian" streak that gives them the self-confidence to pit their own knowledge and judgment against the common wisdom and belief of their colleagues.

CREATIVITY IN MANAGEMENT

If we wish to talk about creativity in management, we must use the same definition of creativity that we use when we talk about scientific discovery. We attribute creativity to behavior when it produces interesting or useful novelty. What are the evidences we can use to detect or identify managerial creativity?

The peculiar characteristic of managerial creativity is that we must assess it, not by the personal accomplishments of managers, but by the achievements of the organizations for which they are responsible. Because of this characteristic we may expect that the motivation for managerial creativity may be rather different than the motivation for individual creativity of other sorts. We may also wonder whether there are fundamental differences in the creative processes—that is, the cognitive aspects—as well.

Motivations

There is no reason to believe that the basic motivations of managers are different from those of other people, although the admixture may not be exactly the same. People receive satisfaction from accomplishment (solving the problem), from material rewards, from the esteem of others, and from power. Undoubtedly there are other motives, but these are generally acknowledged to be prominent and powerful ones, and they will suffice for our purposes. In our kind of society, management probably offers more than average opportunities for material rewards and for power, but it differs in these respects from other occupations only in degree. Preeminence in the arts and in science can also lead to wealth and to power.

What would seem to distinguish management most sharply from other kinds of work is the nature of the sense of accomplishment it provides. In most other endeavors, accomplishment is a highly personal matter—the direct product of the working of one's own head and hands. An author writes books, a scientist carries out research and publishes papers, a painter produces canvases. The sense of accomplishment of managers, on the other hand, arises out of what they see others doing. For this to provide satisfaction, managers must see or imagine a causal nexus connecting the works of their organizations with their own efforts in organizing, directing, and staffing them.

In all human affairs, the assignment of credit and blame is a difficult matter. I have already alluded to the role of accident, hence of luck, in scientific discovery. Management inserts another step of indirectness in the causal chain connecting personal behavior with outcomes, thus making assessment correspondingly more difficult. Moreover, "hands on" accomplishment generates, for many people, a qualitatively different affect from accomplishment by indirection. Even the intervention of a power tool may alter radically the feelings associated with handicraft activities. True, there has been more romantic speculation about these matters than hard empirical evidence, but we have only to consult our own feelings to know that there are differences, and often important ones.

Inability to delegate effectively is a very common managerial failing. It is usually attributed to feelings of responsibility for the results and unwillingness to depend on others for the discharge of that responsibility. It may also be due to the diminished satisfactions some managers feel when delegation deprives them of the opportunity to participate directly in the problem-solving process. They may get greater satisfactions from the exercise of their problem-solving skills than from the exercise of their skills of influencing other people.

Creative managers, then, are people who, by their own propensities or through learning, can receive great satisfaction from creative outcomes even when their role in producing those outcomes has been an indirect one—specifically, a managerial one.

We should not suppose that this peculiarity of motivation, though characteristic of management, is wholly limited to business occupations. It has often been noticed in recent years that, with the growth of Big Science, scientific activity itself becomes more and more managerial, carried out through organized research teams. This has been a cause of no little malaise to scientists attached to the traditional values and modes of operation; but probably also a source of satisfaction to scientists who have good managerial skills and who can now participate in the scientific enterprise with a success they could not otherwise have had.

Even before the advent of Big Science, the scientist was often also a teacher. In teaching, and especially in guiding the activities of graduate students, there is a major shift in satisfactions from problem solving to facilitating the work of others, and one can see the proclivities of scientists reflected in their attitudes toward the direction of graduate work and their styles of supervision and guidance.

When all is said and done, however, management is the discipline *par excellence* that depends for its achievement satisfactions on influencing the accomplishments of others. No one is likely to succeed in management, or to be creative in it, for whom this particular kind of achievement is not congenial.

The Cognitive Aspects of Managerial Creativity

Since my thesis is that creativity consists of good problem solving, in considering the creative process in management I need mainly to point out that the principal kinds of problems that confront managers where creativity is called for. First, however, I should like to make some comments on the nature of managerial expertness. In what are managers expert, and how does that expertness reveal itself in their behavior?

We have seen that a major component of expertise is the ability to recognize a very large number of specific relevant cues when they are present in any situation and then to retrieve from memory information about what to do when those particular cues are noticed. Because of this knowledge and recognition capability, experts can respond to new situations very rapidly—and usually with considerable accuracy. Of course, on further thought, the initial reaction may not be the correct one, but it is correct in a substantial number of cases and is rarely irrelevant. Chess grandmasters, looking at a chessboard, will generally form a hypothesis about the best move within less than five seconds, and in four out of five cases, this initial hypothesis will be the move they ultimately prefer. Moreover, it can be shown that this ability accounts for a very large proportion of their chess skill. For, if required to play very rapidly, the grandmaster may not maintain a grandmaster level of play but will almost always maintain a master level. But in rapid play, there is time for almost nothing but to react to the first cues that are noticed on the board.

We usually use the word "intuition"—sometimes also "judgment" or even "creativity"—to refer to this ability of experts to respond to situations in their domains of expertise almost instantaneously and relatively accurately. The street-wise slum resident has good intuitions about how to react to the situations that are often encountered in a slum environment. The manager has good intuitions about how to react to the situations that are often encountered in organizations. Both skills have the same basis in knowledge and recognition capability.

Present a capable and experienced business manager with the summary accounts of a business firm and he or she, within a matter of minutes, will make some shrewd conjectures about the firm's strengths and weaknesses. Present the same manager with a case describing a personnel problem, and a diagnosis of the difficulty and comments on possible courses of action will be forthcoming almost at once.

The point is not that managers either do or should act on impulse. Rather, it is that the expert ones have learned their 50,000 chunks and, with them, their ability to respond "intuitively" to business situations as they present themselves. It follows from this that schools of business, even the best, do not produce expert managers. They do not charge the ten years' dues that expertness would call for, nor can they provide the full environment of organizational situations in which the perceptual cues can be learned and practiced. But I think that this conclusion will surprise none of us.

As a surrogate for some of this experience and as an alternative means for developing the perceptual recognition skills that underlie expertness, business schools often use the case method as one of their instructional techniques, as well as the business game. These techniques could probably be used more effectively if they were recognized for what they are: methods for giving students opportunities to practice searching for relevant and important cues in business situations and for associating potentially useful responses to these cues. By these methods, the business school can at least start its students on the way toward accumulating the 50,000 chunks they will need as managers.

A more difficult question has to do with the content of the chunks. What does a streetwise—or, more accurately, organization-wise—manger know? The requisite inventory has never been taken, but we can conjecture that managerial knowledge falls into two main categories: on the one hand, knowledge about human behavior in organization and about how organizations operate, and, on the other, knowledge about the content of the organization's work—knowledge that may be largely specific to an industry or even to a particular company or plant.

It has sometimes been argued that managerial expertise is a general skill that can be transferred from any organizational environment to any other. I don't think the evidence bears out this claim. The Peter Principle is a refutation as it applies to vertical transfer, and we can see as many instances of failure as of success in horizontal transfer between organizations. The hypothesis of transferability probably appoaches most closely the truth toward the top levels of very large corporations or governmental organizations. In the former, the responsibilities at the top levels, in addition to the selection of key personnel, are most likely to resemble those of an investment banker. In the latter, responsibilities for mediation between political and administrative levels are likely to bulk large.

In any case, it would seem that knowledge of technical content of an organization's work can be harmful to managers only if it tempts them to resist delegation of responsibility. Nevertheless, it is characteristic of managerial jobs that managers are continually in the position of directing operations whose technical content they cannot fully master. To cope with this difficulty, they develop a number of strategies. One strategy is to encourage multiple channels of communication from below, so that they will not be the captives of any one set of experts. Another strategy is to develop skills of cross-examination—specifically, skills in inducing experts to reveal the hidden assumptions on which their conclusions and recommendations are based. A third strategy is to strengthen the identifications of their associates and subordinates with the top-level goals of the organization, weakening their attachments to subgoals.

One way to probe the content of the knowledge (whether possessed by executives or by members of their organization) that underlies organizational success is to enumerate the various ways in which an organization can enjoy an advantage over its competitors and then to assess the historical role these different forms of competitive advantage have played in the growth of especially successful organizations. I don't know that a systematic study of this kind has ever been undertaken and can therefore only guess what it would reveal.

Even in the absence of systematic data of this kind, we can point to a very large number of different dimensions in which organizations have behaved creatively and prospered as a consequence. We can think of instances of innovation in manufacturing methods (interchangeable parts and the assembly line) and even a few instances of innovation in organizational form (divisionalization by product groups).

Technical innovation, the creation of new products, undoubtedly is the major factor accounting for the rise of whole new industries. But within individual industries, the forms of creativity that provide particular firms with competitive advantage are more difficult to specify. The identification of these factors of advantage would provide an excellent focus of research on the species of creativity that are specifically managerial.

Managerial Risk Taking

Is every expert manager creative? What are the additional ingredients, beyond the intuitive skills based upon the 50,000 chunks of knowledge, that are required for creativity? Perhaps we can return to our understanding of scientific creativity for part of the answer. There are, in science as in business, competent journeymen and especially creative masters.

From our review of scientific discovery, we have seen that at least three stigmata seem to characterize scientists who are unusually creative: first, sensitivity to "accidents" and readiness to respond to them, even abandoning an ongoing program (as the Curies did in their search for radium); second, care and thoughtfulness in defining and selecting research goals and research problems; third, a propensity for risk taking. (Of course, we must interpret this last characteristic with care, for the creative scientists we know of are the ones whose bets paid off.)

Translated into terms of business and management, these traits sound rather familiar. The first is sensitivity to opportunity and the ability to marshal fluid resources to initiate new programs of activity. The second is attention to strategic planning, to understanding relevant future trends and developments, and to the setting of long-term goals. The third is a willingness to adventure, even with risks of failure. You will understand that I am not recommending any particular level of risk preference but simply claiming that the opportunity to be creative can seldom be fully separated from the opportunity to fail.

The common romantic scenario for the creative hero postulates an underdog who is willing to risk all to achieve his or her visionary goals and who finally reaches those goals after surviving many perils and overcoming many obstacles. We have seen that a more realistic scenario pictures the creative person as a professional gambler who prefers odds that are stacked in his or her favor and who secures those odds by acquiring superior knowledge about the domain in which the gamble is taking place. I put the matter this way not to discount the genuine element of risk associated with most creative accomplishment but rather to emphasize the skill and knowledge (the 50,000 chunks) that form the foundation to most successful risk taking.

CONCLUSION

William Larimer Mellon, the benefactor of the Graduate School of Industrial Administration at Carnegie-Mellon University, said: "Industrial opportunity means the opportunity to create." That motto is engraved in stone in the lobby of the school's building, where I hope it is still read and pondered by the students who pass by it. The motto does not spell out, of course, how the opportunity is to be seized.

What I have tried to do in this paper is to review what is known about the creative process. Most of my evidence was derived from research on scientific creativity, but I am confident that the foundations of creativity are the same in management as they are in science.

My review of the evidence emphasizes the conclusion that the creative processes are problem-solving processes—that we do not have to postulate any special kind of "genius" to explain the creative act. The evidence shows, further, that effective problem solving rests on knowledge, including the kind of knowledge that permits the expert to grasp situations intuitively and rapidly. But intuition is no mysterious talent. It is the direct byproduct of training and experience that has been stored as knowledge.

Creative performance results from taking calculated risks, where the accuracy of the calculations rests, again, on the foundation of superior knowledge. What appears to be the reckless gamble of the successful creator may be just that; more likely it was much less a gamble than it appears, just because the risk taker understood the situation better than competitors did.

Earlier, I maintained that science does not demean phenomena by explaining them. Creativity is not less challenging or exciting when the mystery is stripped from the creative process. The most beautiful flowers grow under careful cultivation from common soil. The most admirable products of human effort grow from the cultivation of ordinary knowledge by the solid processes of problem solving. Understandable, but no less admirable for that.

NOTE

1. This section of my paper is based upon our joint work. An introduction to it, and references to other publications, will be found in Gary F. Bradshaw, Patrick W. Langley, and Herbert A. Simon, "Studying Scientific Discovery by Computer Simulation," *Science* 222 (2 December 1983): 971–75.