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Science and Its Applications: How to Succeed

Michel Boudart

The research enterprise is defined as the organization that generates science and its applications. Success in this endeavor depends on a combination of attitudes that are rarely found in a single individual but may be assembled among members of a winning team. Some of these attitudes are examined in this essay based on external advice and personal reflection.

THE RESEARCH ENTERPRISE

Many arguments over semantics could be avoided if the following words of Louis Pasteur were more widely disseminated:

> There are not two sciences. There is only one science, and the application of science, and these two activities are linked as the fruit is to the tree.

To separate science from its applications is most often counterproductive. Many leading scientists were deeply involved in applications: Louis Pasteur, Lord Kelvin, Walther Nernst. Some famous industrialists were fascinated by science, or perhaps started as scientists: Ernest Solvay, Charles L. Reese, the Varian brothers. Ralph Landau started from chemistry and its applications, developed and commercialized several innovative processes, and is currently devoting much of his energy to the science of economics as related to technology.

Thus, there exists a continuous spectrum of related activities from long-range, fundamental, basic, pioneering, academic, and corporate research to short-range, applied, mission-oriented, industrial research. Whether the motivation of the work is the *right* to know rather than the *need* to know, curiosity rather than the marketplace, good science can be recognized whether it is far removed from or very close to its applications. Good science never drills a dry well: It leads to discovery, invention, or innovation. Its product is a scientific paper, a patent, or high technology. I define good science as successful science wherever it is managed, directed, or conducted from the viewpoint of the student, scientist, or engineer doing it. Unfortunately, there is also bad science that does not add to the pool of knowledge and does not lead to applications. My remarks shall be confined to small science, done in a group of 15 people or so, in opposition to big science. The examples will be borrowed from chemistry and its applications because that is the field I know firsthand.

The question I shall try to answer is one that is of current interest: how to attract the best creative talent to science and its applications? One answer is, by managing success in the research enterprise, be it a research group at the university, a research institute, a national laboratory, or an industrial company.

Yet, there is a fundamental difference between a corporate entity and academe. Milton Friedman stated that the social responsibility of business is to maximize profit within the rules of the game. Perhaps one could add that the social responsibility of academe is to formulate and disseminate the rules of the game. Thus, there are differences in the style of management of a research team, depending on its social responsibility. In looking for proven reasons behind success, I have restricted myself to one field—catalysis, and catalytic technology. I have borrowed from six industrial leaders in the field six key ideas that, in part, contributed to their success. My sources, in alphabetical order, are: Heinz Heinemann, Jim Idol, Wolfgang Sachtler, John Sinfelt, Haldor Topsoe, and Paul Weisz. I have distilled the elements of their success from talks they gave at Stanford University in 1983 on their philosophy of research. These six ways to success in catalytic science can be expressed by six short exhortations: Exploit luck through observation, optimize chaos, persevere, think fundamentally, moonlight, and be arrogant. These recommendations are not in the same order as the alphabetical order of names, and the following commentaries are my sole responsibility.

EXPLOIT LUCK THROUGH OBSERVATION

This advice is a variation on the aphorism of Pasteur: Luck favors the prepared mind. A lot has been said about luck, chance discovery, or serendipity. It seems that the creative act cannot be planned consciously, so it appears accidental. But most often the *eureka* is preceded by months or years of study, inquiry, discussion, silent reflection, and meticulous experimentation. The deeper the thought, the more personal it is. Brainstorming has its uses in the research enterprise, as expressed by the well-known statement, discussion is the lifeblood of physics. Nevertheless the creative research climate must encourage meditation rather then agitation. Pressure does not hasten gestation of ideas. Yet, agitation and pressure have their roles in the development stages following creative research, but that is another story.

The *eureka* is not possible without observation. The mind must be prepared. Giulio Natta discovered isotactic polypropylene because he had the past expertise of X-ray diffraction that permitted him to recognize the ordered structure of his Nobel award-winning polymer. Without *quantitative* observation, there is no science. The creative research climate rewards prepared minds.

OPTIMIZE CHAOS

This sounds like an oxymoron, and it is one. In 1967, Herman Pines wrote in *Science* about Vladimir Ipatieff, the Russian chemist who left his imprint on the Universal Oil Products Company:

> Ipatieff was a general in the artillery of the czar. As befitted an officer at the time, he was a consummate horseman. Applying this skill to human relations, he used to say: "Give the subordinates enough rein, but let them know who the master is."

Another way to express the idea is to paraphrase Thomas Paine. The best research management is the least research management. Or, according to Paul Janssen, founder and chief of Janssen Pharmaceutica, research management should adapt its goals to the ability of the research staff to meet these goals. This is another way to optimize chaos. Who is a good research manager? A Ph.D. is neither a necessary nor a sufficient condition for a research manager. Many Ph.D.'s do not learn to *do* research: They may have been used only as a pair of hands by their thesis adviser. This is well recognized by the need for an assistant professor to demonstrate ability to do independent research before promotion to tenure rank.

The quality of a research laboratory is no better than that of its director. The director manages things, not people. By shrewd allocations of human and material resources, the research director creates a climate in which people believe that they are better than they really are. This climate is that of optimized chaos.

PERSEVERE

Nature is always more complex than it first seems. It always takes much more time than anticipated to solve a scientific problem. Even a patient director becomes impatient. The researcher needs courage, sometimes beyond the call of duty, to persevere against heavy odds. Loyalty commands to fight the establishment. Perseverance requires character. Faith must overcome expediency.

Yet, if perseverance means to stay on course, unforeseen results may demand a sudden sharp shift of direction to follow new leads. Managers do not like this either; they are committed to an orderly battle plan and hate the hazards of guerrilla warfare.

Toleration of perseverance and the flexibility to shift course are other expressions of the art of optimizing chaos. Academe provides a very opportunistic climate for science. Although research proposals fund specific problems, it is understood that the principal investigator is free to switch course on the basis of new information. Yet, the graduate student in the third year of doctoral work is better advised to persevere. These situations illustrate again the delicate balance between order and freedom in the research enterprise.

THINK FUNDAMENTALLY

To think fundamentally gives the scientist a chance to understand, or at least to understand better, the problem under study. But understanding means different things to different scientists. To the physicist, it means the ability to predict; to the chemist, it provides a way to explain. The engineer understands when design is at hand. At the lowest level, understanding is related to an orderly description of the facts.

Fundamental thinking in science must be quantitative. A nonquantitative activity is not science. All too often, the chemist thinks in terms of qualitative mechanisms. Mechanistic obsession is fun, at least to some, but it rarely helps, except when it leads to the invention of chemical reactions, as argued by Derek Barton (1990), or to the discovery of new molecules, as explained by Roald Hoffmann (1990).

No matter how it is done, thinking is always hard. It takes time and detracts from getting things done. Time to think is not the favorite allocation of managers. In fact, the scientist may well be forced to borrow thinking time from free time, following the imperative discussed in the next section.

MOONLIGHT

In the world of management, employees are nonexempt or exempt, depending on whether they get paid for overtime or not. But a creative scientist, like a creative artist, does not sell his or her time. The very idea of a creative scientist filling out time sheets is ludicrous. Fascination in science cannot be turned on or off on demand. The creative mind continues to wander while the body eats, exercises, or sleeps. Interruptions in or out of the laboratory, the library, the office, or the home study may ruin a promising effort. Hence the well-established practice of moonlighting. Do what you need to do to keep the wolf away from the door, the wolf being your thesis adviser, team leader, or laboratory director. Then use the rest of your time, perhaps at night or on the weekends, to do what you really want to do. All of us who have done science know how to moonlight effectively. So many scientific books are prefaced by remarks such as "I thank my spouse, who tolerated my awful antics while this book was being written." I acknowledge that some of the most creative results to come out of my laboratory were obtained by graduate students or postdoctoral assistants in the absence of my instructions or even occasionally against them.

The creative mind has a vision. And vision, following Jonathan Swift, is the art of seeing things invisible. To try to explain a vision can get one into serious trouble, as Joan of Arc found out. It is better to moonlight until things become visible.

By now, the reader must have noted many connections between the various admonitions to succeed in the research enterprise: the director must optimize chaos where the scientists count on luck, think endlessly, pursue seemingly hopeless avenues on or off the workplace, and sometimes act in a rather arrogant manner.

BE ARROGANT

At the end of their famous book on *The Conservation of Orbital Symmetry*, Woodward and Hoffmann (1970) consider violations of the rule that bears their name. They write, "Violations. There are none! Nor can violations be expected of so fundamental a principle of maximum bonding."

I define arrogance as the utterance of such a statement. To find something new is always risky. Is it really going to work? Is it right? The discoverer needs a champion. The best champion is the discoverer. This applies to new concepts, compositions of matter, processes, or technologies. Being arrogant does not mean acting as a bully. On the contrary, the successful inventor or entrepreneur combines arrogance with charm, being sometimes forceful in some sort of a shy manner. In any event, I far prefer the word arrogance to salesmanship, although they are not unrelated in the present context.

A SEVENTH KEY TO SUCCESS IN SCIENCE AND TECHNOLOGY

It would be foolhardy to claim that there are only six ways to success in the long road from science to an economically rewarding technology. To lengthen my list, I interrogated a man who has walked along this road not once but several times, Ralph Landau, to whom this essay is dedicated. What had been for him a guiding principle distinct from the six others that I already knew about? Ralph did not answer, but Claire Landau said, "Timing." Later she added, "And daring." So I propose a seventh pillar of success in the research enterprise: timing and daring. They seem inseparable, as timing, in the face of incomplete information, requires the daring of a risk taker. Albert Einstein talked about the value of timing in science, and daring he certainly was.

In conclusion, I have tried to comment on a number of attitudes that lead to success in the research enterprise of science and its applications. I have attempted to remain as general as possible, covering the gamut of activities from discovery to innovation. I fully realize that there are differences of style and substance as an idea moves from its inception to the industrial plant. But it is important to reemphasize that the creative and successful scientist-technologist-engineer is not just another professional and must be treated accordingly in hiring, promoting, rewarding, and if necessary, retiring or dehiring. Otherwise, the best creative minds will avoid science and its applications and will choose other pursuits.

In summary, let me rephrase some of my remarks. The best research management is the least management, alert to exploiting luck, avoiding a yo-yo style of starts and stops, convinced that *good science* always pays off. It is the lone guy in a corner of the laboratory who is likely to be the discoverer or inventor. But it is the arrogant guy who is likely to be the innovator with the seventh sense of timing and daring.

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Papers commemorating
Ralph Landau's service to the
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