RESEARCH MANAGEMENT TODAY

Research managers should aim to maintain an environment in which practitioners can be creative, and leave the choice of the object of research to the researchers themselves.

John J. Gilman

The general problem besetting research management was stated succinctly centuries ago:

> There is nothing more difficult to take in hand, more perilous to conduct, or more uncertain in its success than to take the lead in the introduction of a new order of things, because the innovator has for enemies all those who have done well under the old condition, and lukewarm defenders in those who may do well under the new.
>
> —Niccolò Machiavelli, *Il Principe* (1513)

Modern times have seen this problem compounded by a number of factors: the use of muddled language to describe the research scene; counterproductive management policies; unfavorable financial conditions. These impediments to better scientific research are the subject of this article.

We badly need clear statements about the conduct of research. With an output that cannot be described in advance because it does not yet exist, how can research organizations present rational purposes, strategies and tactics? With the "rule of tens" operating, so that about 90% of the activities of a research organization produce results that are not substantially edifying or useful, how can you convince people that the organization is adequately productive before the fact? Why should research be done? Out of an infinity of possibilities, which should be pursued? Who should pursue them? How should they be pursued to maximize output? These questions and more face research managers. (Figure 1 shows one of the first managers to face these questions in a large-scale industrial research organization.)

My views on the subject are the result of observing and participating in research management from about 1948 to the present. I started by watching an inventor, Peter Payson, discover remarkable new steels for use in engine valves, struts in aircraft landing gear, and other demanding structures. He did this by applying a very personal style to a small research organization. Then I watched the unforgettable J. Herbert Hollomon operate within the large General Electric Research Laboratory. The management traditions established by Willis R. Whitney at that laboratory, together with Hollomon's innovations, provided some strong guidelines for forming effective policies—and avoiding ineffective ones. My personal work included the first direct measurements of dislocation velocities and the first direct measurements of the surface energies of crystals.

I then participated in the academic scene as a professor at Brown University and at the University of Illinois. When an opportunity arrived to organize and manage a new materials research center at the Allied Chemical Corporation (now Allied–Signal), I seized it. At this center we invented such things as metallic glass transformer cores, alexandrite lasers and polydiacetylene time–temperature indicators. Later, I obtained further experience managing the Amoco Corporation's research department. This included a group devoted to biotechnology. More recently I have directed the Center for Advanced Materials at Lawrence Berkeley Laboratory. During the entire period, I also served as a consultant at various government operations. Thus I have seen many sides of the research community.

Rule of tens

While managing Allied Chemical's materials research center, I tabulated various measures of output: publications, invention memos, lectures given by staff members, promising ideas and so on. After some years had passed, patterns became clear. The most important of these is the "rule of tens" related to inventions (see figure 2). A group of researchers that averaged about 75 in number over a decade produced about 10 000 casual ideas. These resulted in about 1000 written invention memos, which yielded 100 applications to the US Patent Office, most of which became issued patents. Of these, about 10 were commercially significant, and 1 was important enough to change an industry. The actual logarithmic progression was not as neat as this, and the numerical factor might be eight rather than ten, but the logarithmic character of the progression was nonetheless clear.

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Thomas Edison, model manager. Edison was remarkably successful at managing his research laboratories, and today's research managers should not dismiss his policies. Edison, with hands in lap and wearing a cap, appears with his staff to the left of center in this 1880 photo of his second-floor laboratory at Menlo Park, New Jersey. **Figure 1**

Others have reported observing similar rules. In the Soviet Union, for example, V. Lytkin of the State Committee on Inventions and Discoveries has commented on the system for certifying inventions[1]:

> Results submitted for certification must undergo a lengthy screening process. Expert review of applications is conducted in the All-Union Scientific Research Institute of State Patent Examination; appropriate scientific organizations; an expert council of the State Committee on Inventions and Discoveries; and suitable departments of the Academy of Sciences. The screening process is a rigorous one. Of the more than 1000 applications that the All-Union Scientific Research Institute of State Patent Examination receives each year, only about 200 are accepted for further consideration, and only 15–17 works per year are ultimately registered as inventions.

Thus one feature of the research process is that it condenses large amounts of activity. This is true not just for inventions, but also for other forms of output such as prototypes, papers and lectures. A corollary is that costs also progress logarithmically, although this is not commonly recognized. It implies that effective management is very important for maintaining a competitive position. Another corollary is that research activity is a poor measure of research productivity. Real output is the only good measure, yet standard methods for managing the "performance" of researchers tend to emphasize activities.

What is the function of research?

Terminology in research management is muddy, beginning with the coupling of the words "research" and "development" (or worse, "R&D"). The functions that these words represent are like oil and vinegar: Both are needed in the "salad," but they don't mix. Their purposes and underlying values are different; people who are good at one are often poor at the other; they are budgetary enemies; and so on. Citing them as a couplet is inappropriate, just as the coupling of the words "science" and "technology" is.[2]

But what about "research" by itself? The word is probably best defined in terms of the output research produces, which may lie anywhere along a lengthy coordinate that runs from the sublime to the ordinary. This output may be philosophy, science, invention, exploration, design, measurement, marketing or consuming. The definitional difficulty indicates why there are communication problems between workers in different research communities, especially the academic and the industrial. Although the name is the same in both communities, the content of research activity is different. The difference is centered on the fact that different social values dominate academia and industry.

From the viewpoint of the larger society, the broad purpose of research is the creation of new assets. These range in character from intangible intellectual systems to highly tangible tools, software, medical techniques or consumer products. The assets may be consumed or used to organize knowledge, to develop still newer products, to make processes more efficient, to improve the productivity of people, to trade with other organizations, to do more sophisticated research and so on.

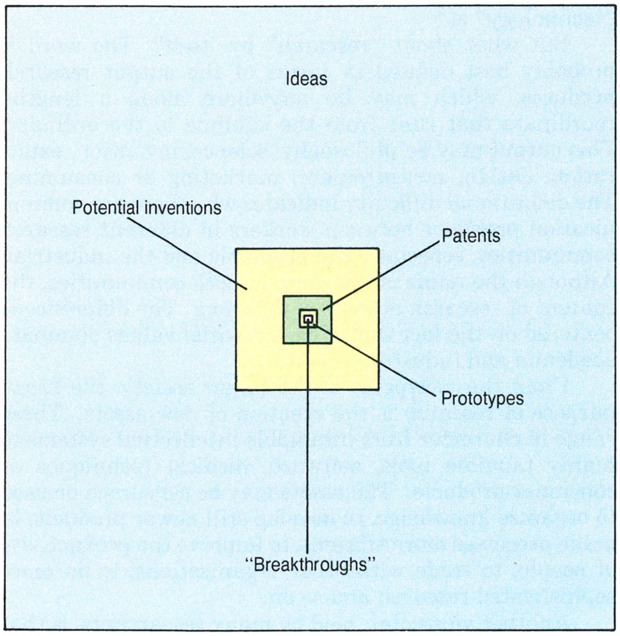
Another viewpoint, held by many researchers, is that research is "the glorious entertainment," a purpose in itself.[3] Because the notion that others should fund your entertainment is elitist, this purpose is not often stated explicitly. Instead it is shrouded with bunkum about cultural or potential technological values. While acting as entertainment, research sometimes produces side effects that prove to be important to society at large. But when it acts simply as entertainment, who should pay for it? The entertainers? Patrons? The taxpayers?

As mentioned above, the values underlying academic and industrial research are markedly different. Academic research aims to relate phenomena to one another to form patterns. This requires *analysis* and is closely related to the teaching functions of academic institutions. Without relational patterns of phenomena to condense myriad facts, it would be virtually impossible to pass comprehensive knowledge from one generation to the next. Relational patterns—theories—are also very useful for design work, data management, guiding future research and so on. Finding relational patterns is not the only concern of academic research, but it is the principal one.

In contrast, the primary aim of industrial research is *synthesis*, manifested in the invention of new techniques, materials, devices or systems. Not all inventions are of interest; the ones that are have potential for substantial economic usefulness. Sometimes a synthesis, or invention, can be accomplished through pure thought. More often invention involves systematic, intelligent experimentation together with "accidental" discoveries. Analysis plays a role as well, but not the primary role.

Why do it?

Privately funded research needs no justification, providing it stays within the bounds of elementary ethics. But when research is done on a large scale using other people's money, it is appropriate to ask, "Why do it"?



'Rule of tens' as it applies to product evolution following research. **Figure 2**

Finding simple cause-and-effect relations between doing research and realizing benefits is extraordinarily difficult because of the large amount of time that often elapses between the two. Also, it must be noted that much research—roughly 90%—does *not* lead to recognizable benefits.

The first sizable, organized effort to do industrially oriented research was that of Thomas Edison.[4] (Others have claimed the distinction,[5] but their cases are weak.) The Edison organization was extraordinarily successful. It not only invented new technologies but also established new businesses and made some purely scientific discoveries. Among its successes were incandescent electric lamps, central electricity generation and distribution systems, practical telephone transducers, motion picture systems and phonographs. The assets and income generated by these developments dwarfed the original research costs. This left little doubt about the answer to "Why do it?" at the time.

Later, a similar sequence of events occurred in the chemical industry, especially at the Du Pont Company, which converted itself from a narrowly based explosives manufacturer into a broad-based and very large chemical company through the development of its own inventions as well as the acquisition of inventions from others.[6] These inventions included synthetic fibers (amides, polyesters, acrylics) and the complex technology needed to manufacture them, tetraethyl lead, synthetic rubbers, fluorocarbons, safety glass, herbicides, titanium dioxide pigments and many others.

In addition to such general observations, there is statistical information that connects research with benefits. Let us consider the 100 companies that spend the most on research. It has been shown that the relative values of the companies in this diverse group (as measured by their price/earnings and price/sales ratios) are statistically proportional to the relative amounts that they spend on research (for small amounts).[7]

Because research spending must be subtracted from pretax earnings, a company's value as a continuing source of revenue passes through a maximum as research spending increases. This determines an optimal spending level, which can be calculated by simply maximizing a quadratic equation (see figure 3). Once the optimum has been reached, further increases become counterproductive because they subtract from current production.

By analogy, there exists an optimum fraction of the national budget for spending on research (and another fraction for development). For this case, however, there are no quantitative models, and so the optimum level for the country is not known. This is unfortunate, because it is clear that spending at levels near the optimum is essential for maximizing the total return on the assets of the country and thereby maintaining its competitive position. Note also that figure 3 suggests that if the country's output became more specialized, a larger fraction of the national income could be put into research effectively.

What is worth doing?

Decision making about what research is worth doing illustrates how research activities are very different from development activities. For long-term scientific research (defined by its output, namely, papers published in reputable archival journals) such decisions should be left to the practitioners. The wise have known this for a long time. The purpose of good management is to enable practitioners to be creative; management can bask in the glory of the result. Results cannot be generated by management, and especially not by the sponsor. The "trickle down" approach doesn't work any better in research management than it does in general economic management. Just as the music of an orchestra must come from the instrumentalists, research results must come from the researchers at the bench.

This seems almost tautological, but consider Peter Medawar's description of what has happened in our time; what he describes for the United Kingdom occurred earlier in the United States:

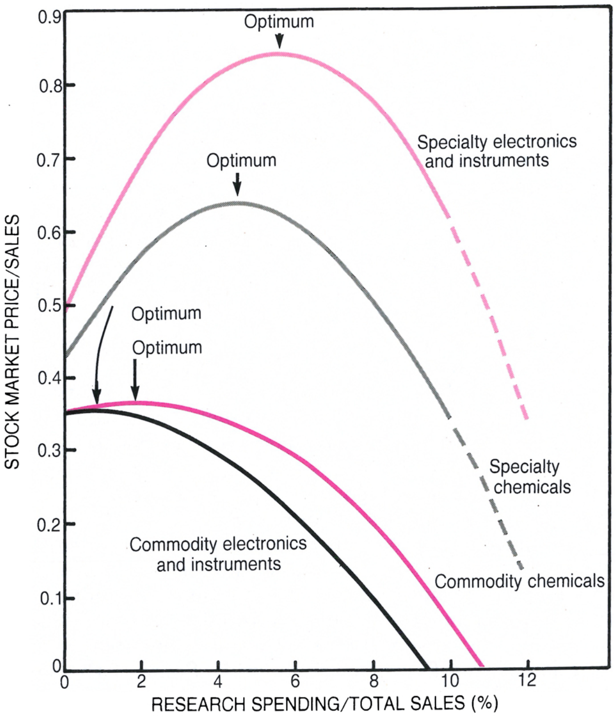
> The government was engaged in reorganizing the funding of the Research Councils on the basis of the retail trade: Customer–contractor was to be the principle, in which the scientist was to put up a research proposal to the government and if the government approved, the scientist would be given a contract to undertake it and bring about the desired result. This was a very bold innovation and had not been the basis on which research had hitherto been conducted. Nor was it the proceeding that had given us penicillin, insulin, the discovery of the blood groups, the elucidation of the causes of myasthenia gravis, the transplantation of tissues, or the discovery of the genetic code. Scientific discovery cannot be premeditated.[8]

This is a powerful condemnation of the current scene in research management at nearly all levels. The "Big Brother knows best" approach is irrational because in reality Big Brother knows nothing. That is, he knows nothing about the most important aspect of the research enterprise, namely, the unknown. He knows nothing about undiscovered territories, unconceived ideas, undetermined facts, inventions yet to be made.

In the case of development, the management situation is quite different. Now the unknown moves from the front to the back row. The shape of the activity and its goals are clear enough that one can apply various analytic tools. Costs can be estimated. Tasks can be outlined and their completion times estimated, yielding expense rates. Markets can be studied, demand estimated based on prices, and market shares deduced. Substitution theory can be used to generate information about expected revenue rates. From these and other factors, one can postulate a quantitative model of the new business and subject it to various tests. Finally, one can make a decision, albeit partially subjective, as to whether it is worthwhile to go ahead with the work.

Trouble arises when these modes of management get interchanged: when excessive analysis is applied to research or too much intuition is applied to development.

The fraction of "research" that is being done at the discretion of its practitioners is continually decreasing as the fraction of all "R&D" that I would judge to be development increases. The impact of this trend is exacerbated by the fact that much of the development work is not worth doing: It could not pass the simplest technical or economic screens.



Market value of companies plotted against normalized research spending. For companies of different types, market-value maxima occur at different spending levels. (Adapted from ref. 7.) **Figure 3**

The researcher himself should be the one to decide what research is worth doing because he is better informed than others and is therefore most likely to make the best choices. And making choices is the essence of research, as stated so well by a great mathematician in the last century:

> Invention consists in avoiding the constructing of useless combinations and in constructing the useful combinations which are in the infinite minority. To invent is to discern, to choose.
>
> —Jules-Henri Poincaré

Consistent with this quotation, research management consists largely of making selections. Good management selects good people, good technology, good ambience and so on. Some people in management positions express the opinion that their function is to "make decisions." That is, they see themselves as judges, rather than selectors. In my experience these people are usually bad managers because they practice the wrong art. The difference is subtle, but real.

Who is most prolific?

Let's start at the top of an organization. The call to direct a research organization is most properly a call to service the needs of other people. But many of those called have other motivations—often a desire for personal glory.

As Hollomon, a prominent research spokesman of the 1960s, was fond of saying, a leader is "someone who can get an above-average performance from an average group of people." Remember that any large group of people contains mostly average people, although it may have been screened so that all of its members meet some minimum skill level. Then think of what an Arturo Toscanini or a Georg Solti can do with such a group in the world of music, or, in research, what an Edison, a Lawrence Bragg or a Robert R. Wilson can do.

Although no one knows what makes individual researchers effective, it is well known that their effectiveness varies widely, following a distribution named after the economist and sociologist Vilfredo Pareto. The mathematician Alfred James Lotka showed this many years ago for the written outputs of chemists and physicists,[9] as have others for other disciplines.[10] Figure 4 shows Lotka's data. In the sample of 6900 chemists who wrote at least one paper, 3400 were listed as authors on 1 paper abstracted during the given ten-year period, about 50 were authors of 10 papers, and 1 authored 100 papers, while another was named on 340 papers! The latter must have had many helpers, of course. Seven authors (1/1000 of the sample) averaged 6 or more papers per year. It would be interesting to know how many practicing chemists wrote nothing (the distribution is indefinite on this point, but suggests many thousands). The production of patents by inventors follows the same distribution function with different parameters.[11] Some of the same data are plotted in figure 5 as a cumulative fraction, a form that emphasizes the large deviation from uniform productivity.

One can get some sense of the nature of the Pareto distribution function (which is followed by many additional things, including city sizes, incomes, biological genera and the frequencies of words in texts) by considering its analytic form. Let $p(n)$ represent the probability that an author writes exactly n papers in a given time span, and let $P(n)$ be the probability that the author writes more than n papers:

$$P(n) = \sum_{j=n+1}^{\infty} p(j)$$

In probability theory, the ratio of the two probabilities is known as the "failure rate," $F(n)$:

$$F(n) = p(n)/P(n)$$

For illustration, let us consider an exponential law and use a continuum limit to make the math easier,

$$p(n) = \lambda \exp(-\lambda n)$$

and

$$P(n) = \exp(-\lambda n)$$

Hence $F(n) = \lambda$: The failure rate is a constant. This means that the fact that someone has published n papers so far gives no indication about his future success. This is a well-known fact about the exponential distribution—that it contains no memory.

For Lotka's data,

$$p(n) = 1/n^2$$

This case of Pareto's law leads to $F(n) = 1/n$, a failure rate that decreases the more one has published (that is, with increasing n). If one author has published 2 papers and another 35 papers, then you can bet that the more prolific author is more inclined to remain prolific. In other words, the Pareto distribution indicates that authorship begets authorship, cities beget larger cities, income begets more income and so on. With an exponential law, you cannot draw such conclusions.

Small changes in the slopes of distributions such as those in figure 4 will cause large changes in the integrated productivity. It seems to me that these distributions are a more fruitful thing to study to gain understanding of creativity than the output of creative individuals. Such statistical studies would be no less difficult than biographical or case studies, but might yield more systematic information.

Does management make a difference?

Researchers who have worked in more than one organization can often give anecdotal evidence (or opinions) concerning the effects of good, and less good, management. Excellent management is usually discounted as an explanation for productive research, of course, in favor of the talent of the individual researcher.

Objective evidence that management has some effect is provided by the patent literature. For all of its faults, this is the only literature in which the manuscripts have been systematically studied for originality by professionals. Although it only represents a particular fraction of the research community, it does represent that fraction critically.

One study of the patent literature found R&D expenditures per patent to vary by a factor of 100—from 10^5 to 10^7 dollars.[12] The study covered 145 companies that were granted at least one US patent per month (on average) for each year of the five-year period 1976–80. This criterion selected for companies that were serious about protecting inventions. Part of the large variation in the cost per patent is simply due to different management goals at different companies. For government research organizations, where invention is usually not a goal at all, the ratio can reach 10^8 dollars per patent or more. Another part of the variation is related to the size of the organization (as indicated by the size of its budget). Figure 6 shows this effect. The equation for the regression line is $P = 8.9\sqrt{R}$, where P is the average number of patents in a five-year period and R is the annual research expenditure in millions of dollars; this shows that the dependence is not linear and that the cost per patent increases with budget size.

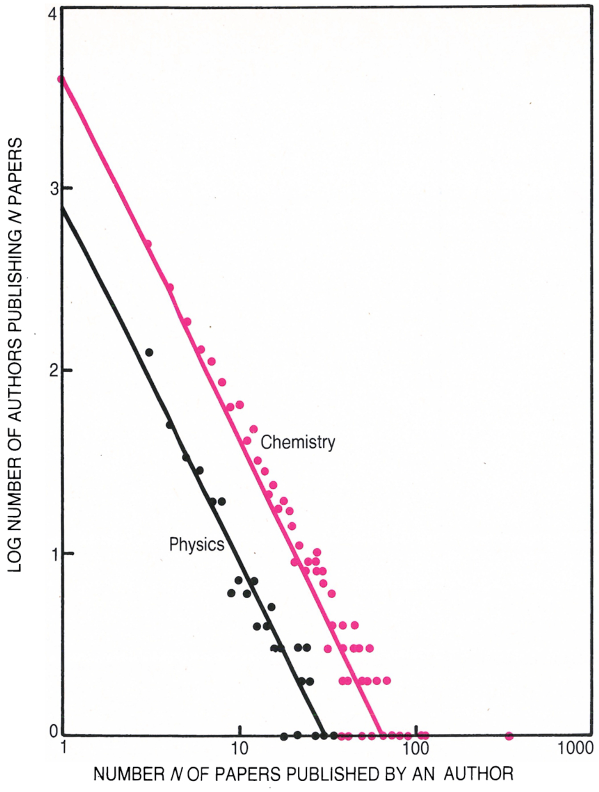
The size effect can be interpreted in various ways, but it is consistent with the idea that decentralization is good for research productivity. The opposite effect holds for development projects, so once again, the evidence is that one style of management does not fit all activities.

A parallel with jazz

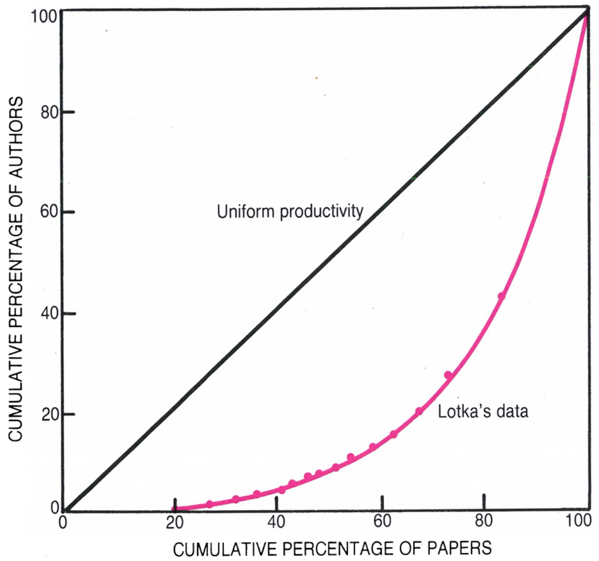
An analogy is often helpful, and in this case I think a musical ensemble is a good one. Musicianship alone does not yield an excellent ensemble. Jazz ensembles are especially pertinent because they create new music in real time and have divergence as a purpose. In contrast, classical ensembles converge so as to play particular compositions.

The best jazz musicians invent new musical phrases in real time with amazing proficiency. They also discover new sounds by pushing at the frontiers of their instruments. They play higher, lower, faster, smoother, rougher, with greater rhythmic complexity and with increasingly subtle harmonics. These activities parallel those of research workers, except that for the researchers the results are new materials, processes, devices, systems, theoretical relationships and so on.

Although jazz is played according to well-defined harmonic and rhythmic rules, and although the skills of the musicians are often exquisitely meshed, a given



Pareto distributions of numbers of technical papers written by authors in physics (black) and chemistry (red), as obtained by Alfred James Lotka.[9] Chemistry data are from *Chemical Abstracts* and cover author names starting with A or B for the years 1907–16. Physics data cover all names from *Auerbach's Geschrichtstafeln der Physik* (1910). Both lines have slopes of −2. **Figure 4**



Cumulative-fraction plot (red) of Lotka's *Chemical Abstracts* data from figure 4, representing nearly 7000 authors and nearly 23 000 papers. The divergence from uniform productivity (black) is striking. **Figure 5**

composition is not played in exactly the same way twice. Thus a jazz ensemble does not "produce" music. And the people in it are fiercely independent.

Whether an ensemble aims to create new music or research results, it must have certain basic elements:

▷ leading performers (inventors)
▷ supporting performers (experts)
▷ arrangement (organization)
▷ conductor (coordination)
▷ instruments (apparatus)
▷ performance hall (laboratory).

All of these must be first rate—not just some of them—if better than mediocre results are to be achieved.

The most important element in this list is the leading performers. If the players of the lead instruments in a jazz ensemble are not exceptionally talented, no music of consequence will be created, regardless of the qualities of the other elements on the list. Creative performers are characterized by intense motivation. This has usually been awakened in them during an apprenticeship under a past master of their particular instrument. They create new musical or technical ideas by first assimilating existing ideas and then combining them in new ways.

Inventors of music or technology must also have a high tolerance of risk and must be able to accept imperfections. Thus creative musicians push their instruments beyond the "state of the art." This sometimes results in wrong notes, poor phrases or other failures. Similarly, inventors and those who manage them must expect many failures to precede most successes. If the failures do not occur, the organization is not pushing the state of the art hard enough.

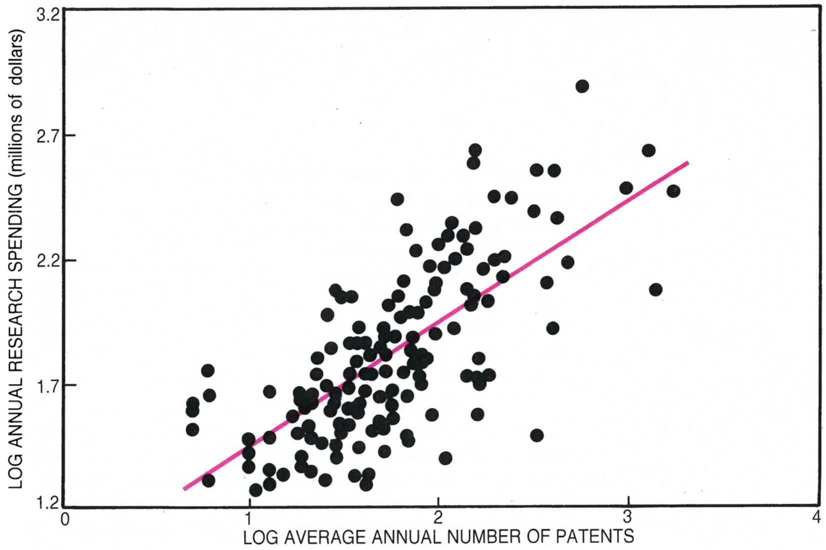
But leading performers alone are not enough. Their efforts can be greatly strengthened by supporting performers. This is clearly illustrated in the world of music by the rhythm instruments that support the lead voices. A poor drummer or bass player can ruin an otherwise promising ensemble, and a superior rhythm section can convert an otherwise good ensemble into an outstanding one. Similarly, first-rate technical, analytic, library and shop services can mean the difference between a good and an excellent research organization.

A creative ensemble also needs the guidance of a musical arrangement, or an organizational structure, that has a beginning, a middle and a purposeful overall direction.

Many pressures act on and within any organization. These will tend to randomize its efforts if they are not counteracted in some way. This is the job of the conductor or director. If the organization functioned ideally, a conductor would not be necessary; but in reality he must coordinate its various activities and continually restrain wayward trends. Also, the conductor or director ensures the continuity of the organization by recruiting new members.

Through the accuracy of their tuning, their timbre and their compatibility, instruments affect music; apparatus likewise affects research.

The final essential element in the operation of a creative ensemble is its facilities—that is, its housing. This cannot be treated casually, because it strongly affects the interactions between people in the ensemble and between the ensemble and its audience. In music, the acoustics and the general ambience of the performance hall influence both the musicians and the audience. In research, there are analogous effects on communication



Nonlinear distribution of numbers of patents issued annually over a five-year period to 145 companies with variously sized research and development budgets. Expenditure per patent increases as the square root of a company's total research expenditures. The correlation coefficient is 0.7. **Figure 6**

and attitudes. These include effects on communication to the group, within the group and outward to patrons of the group.

There is a strong tendency to homogenize organizations for administrative convenience. However, as I have explained, large, homogeneous laboratories do not invent efficiently any more than large, instrumental ensembles create interesting music. In both cases special internal structuring can markedly improve the situation. Lack of nimbleness is the basic problem—so nimble subgroups are a solution. In the case of music these are quartets, quintets and sextets, pulled out of a full orchestra. In the case of research, small informal groupings can be arranged with productivities well above that of the parent organization.

The financial squeeze

From the viewpoint of accounting, research is analogous to inventory—a cost now, to be converted into income later. When real interest rates rise, the cost of carrying inventory rises, as does that of research. Pressure develops to turn over inventory quickly and to cash in on research quickly.

The late 1960s saw quantitative changes in the US financial system, caused by the financing of the Vietnam War through debt instead of current tax revenues. This led to both inflation and a seller's market for money. These effects of the war financing were further aggravated (or perpetuated) by the jump in the price of oil, induced by the OPEC cartel.

Secondary effects intensified the situation. Because growing assets is a slower process than buying them, companies turned to buying. This increased the demand for money, thereby pushing up the price—that is, the real interest rate. Direct financial factors were exacerbated by less direct ones, including tax policy, regulation and antitrust policy.[13]

These various factors led to a shift from homogeneous companies with technical depth to heterogeneous, technically shallow conglomerates. Power moved from technical to financial managers, and the focus of management moved from deferred to current income. Research managers and their superiors changed their outlook to concentrate on the short term, not because this was natural for them but because there was no other realistic response to the changes in the financial environment. The changes affected industry first, then government and finally universities.

No number of researchers talking to each other or talking at research managers is going to undo the current short-term focus. For such a change to occur, the quantitative parameters of the financial system must be changed. But no one has socially acceptable ideas for accomplishing this.

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