Knowledge and Research in High Technology Companies

A Case Study by
Phillip M. Yelland

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PREFACE

Dr. William ("Bert") Sutherland,
Director Emeritus,
Sun Microsystems Laboratories

This Sun Labs report presents Phillip Yelland's analysis of high-tech research lab management prepared as a paper for his M.B.A. studies at U.C. Berkeley. As a member of the Sun Labs research staff, Phillip chose to ask me, his former Lab manager, for comments and opinions on my style of Lab management. I find in this report some of my familiar concepts couched in a larger context of external scholarship about management practices to foster innovation. I found it particularly interesting to see my instinctive techniques placed in juxtaposition to the published literature on this topic. This Sun Labs report can be viewed itself as a result of several management practices. It comes from encouraging the individual curiosity of a staff member pursuing additional knowledge through an M.B.A. program. It brings in an external perspective from outside the organization. Publishing it is an attempt to make available whatever value the reader can glean from the analysis. And perhaps some manager may find the ideas useful enough to modify their management behavior or style in a way that enhances their organization's innovative abilities. One of Sun's Fellows remarks, "Technology transfer is a contact sport." The effective dissemination of ideas is both important and difficult. Thank you, Phillip, for this analysis of how value can be created from a contemporary research laboratory.

Bert Sutherland

August 1999

Notes from the Author

This report is a revision of a paper I prepared originally for a course entitled *Management of Hi-Tech Knowledge Workers*, given by Dr. Homa Bahrami under the auspices of the M.B.A. program at the University of California at Berkeley. A marked counterexample to the stereotype of business education as obsessed with things quantified and quantifiable, Dr. Bahrami's course explored the social aspects of management as it applies to a breed of workers pivotal to modern business. Broadly speaking, these *knowledge workers* are characterized by a high degree of professional skill that's usually most productive in a work context offering significant individual freedom and empowerment. Naturally, balancing such requirements against the needs of the company as a whole represents a significant challenge for managers of knowledge workers.

As modern economies move into a phase of "post-industrial" or "information-based" development, such workers are increasingly widespread and prominent. But though the prevalence of knowledge workers throughout the economy is relatively recent phenomenon, they have long played key roles in many aspects of business. Managers of industrial research organizations, for example, have been charged with the governance of knowledge workers since the inception of such organizations in the late part of the nineteenth century. In my studies, therefore, I determined to draw what lessons I could from the conduct of industrial research that might be applicable in a larger context. I chose to examine industrial research in high technology companies (the very quintessence of the "information-based" organization) since I was particularly interested in the performance of research in a setting in which the distinction between research and product development is far less clearly-defined than has been the case historically.

I was particularly fortunate in this regard, in that I had access to an illustrious career's worth of collected wisdom concerning the management of industrial research in high technology companies: Dr. Bert Sutherland—erstwhile director of the Xerox Palo Alto Research Center, and of Sun Labs, the research arm of Sun Microsystems Inc.—generously offered to share his vision for the management of research in high technology companies, and the lessons yielded by his experience.

In this report, I've endeavored to match many of Bert's precepts with the body of scholarly research whose proliferation has mirrored the recent rise in the importance of knowledge and knowledge workers in the economy. The result, I hope, is at least a modest contribution to the literature that shows how academic research and empirical practice may be combined to inform an aspect of business whose importance in the coming millennium is sure to grow.

Phillip M. Yelland,

August 1999

Editor's Notes

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About the Author—Phillip M. Yelland is a staff engineer at Sun Microsystems Laboratories. Since receiving his M.A. and Ph.D. in Computer Science from the University of Cambridge in England, he has worked in a wide variety of settings, ranging from software development management in a number of high-tech startups to industrial research for major corporations. He has made a number of contributions to academic and industrial publications. Throughout, his activities have reflected a preoccupation with the application of theoretical knowledge to the practical conduct of business. In addition to his current research, which encompasses formal methods for the analysis of object-oriented programming systems and the use of statistical techniques in supply-chain management, he is also completing an M.B.A. at the University of California at Berkeley.

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About the Essay—In the words of the author: "Management in general is an exacting discipline; the management of research is one of the most challenging of its instances. Historically, general prescriptions for the effective management of research have been very difficult to formulate. Many difficulties stem from the long-term nature of many research projects and the great uncertainty that usually attaches to their payoffs. Furthermore, it is not clear that familiar management accounting measures such as discounted future cash flows necessarily encapsulate the fruits of many research projects. Such factors are particularly acutely felt in competitive and labile high technology industries, where traditionally, activity has centered on the commercialization of existing science and technology. This paper attempts to derive an approach to the problem of effective research management in high technology by construing the counsel of a highly regarded research director (Bert Sutherland, Director Emeritus of Sun Microsystems Laboratories) in light of current thinking concerning the origination and management of knowledge for competitive advantage. Certainly, the results of this exercise may be dismissed as idiosyncratic. But so compelling is the alignment in the model between Dr. Sutherland's empirical wisdom and many of the commonly-accepted precepts of knowledge management that it at least partially endorses its broader applicability."

—Ed.

KNOWLEDGE AND RESEARCH IN HIGH TECHNOLOGY COMPANIES

A CASE STUDY

INTRODUCTION

There is little doubt that in recent years, mastery of technological innovation has become an increasingly vital source of competitive advantage. In a competitive global economy where product life cycles are diminishing rapidly, any company that fails to sustain innovation is bound to suffer materially. Nowhere is this observation more applicable than in computer-related industries—which for the purpose of this paper are denoted *high technology*, or simply *high-tech*. High-tech is littered with the emaciated husks of once-dominant companies (Data General, Wang, Ashton-Tate, WordPerfect, etc.) that lost the ability to innovate.

With such a premium attached to innovation, one might expect that research laboratories would be common fixtures of high technology companies. After all, the conventional depiction of product development begins in the company's research laboratory, whence it moves into manufacturing. Curiously enough, however, "research laboratories" as such are appreciably rarer in high technology than they are in traditional industry. Naturally, research labs are almost unknown in smaller companies, but institutions as august as Intel, Apple, Cisco and Oracle also lack distinct organizational units devoted to research. Exceptions to this trend, such as IBM or the scions of Bell Telephone, are long-established companies whose research laboratories date from an earlier *industrial age*.[1]

The apparent antipathy towards research in high technology firms is not without justification. As is demonstrated below, even under the best of circumstances, accurately calibrating the returns on research projects is difficult. It is doubly so when markets are as dynamic and competition as intense as in high-tech. Conversely, the commercial potential of existing technology is so great that many firms feel disinclined to risk valuable capital on more dubiously lucrative long-term research. Given these considerations, therefore, one might wonder whether research laboratories have any role at all in high technology companies. The premise of this paper contends that they do, though in a capacity rather different from that in more conventional industrial settings.

The following section lays out the conventional model of industrial research, as it was generally accepted throughout the first seven decades or so of the twentieth century. The

[1] We use the term "industrial age" somewhat invidiously to set in counterpoint traditional, manufacturing-oriented industrial practice and that in high technology. Of course, as Leonard-Barton points out [18], even the most traditional of businesses is susceptible to modern management techniques.

difficulties associated with the management of research in this context are highlighted, and we point out how these difficulties are particularly nettlesome for high technology firms. We go on to show how we can derive a cogent role for a research lab in a high-tech company from recent work on *knowledge management*—the generation, acquisition, codification and transmission of knowledge for competitive advantage. From this statement of purpose, we are able to develop a framework for the management of such a research lab that maximizes its utility to the firm. As corroboration of our proposed framework, we reproduce a digest of an interview with Bert Sutherland—a highly regarded director of one of the few research laboratories in high-tech, Sun Microsystems Laboratories. We are able to demonstrate that the framework can effectively accommodate many of the precepts that Bert has employed in his role as a research manager.

RESEARCH LABORATORIES IN INDUSTRIAL AGE FIRMS

In the industrial age model—where great emphasis was put on the production and distribution of material products—the role of a research laboratory was fairly clearly defined. Broadly, it was expected to act as a "fountainhead" for new products, which were developed more-or-less *ab initio* in the laboratory before being transferred into downstream (*operating*) divisions for refinement and manufacture [10]. Naturally, the laboratory was expected to consult with customers and with other parts of the company, but it was generally expected to *innovate*—that is, to conceive new products—itself. The task of the remainder of the organization was to turn such innovations into saleable artifacts, which it would manufacture and sell. Thomas Edison's laboratories—established in the latter part of the nineteenth century—were the archetypes of this kind of research (though technically, Edison provided contract rather than in-house research), and the model remained predominant during much of the first three quarters of the twentieth century.

In aggregate, there is no denying that industrial research made enormous contributions to human welfare throughout the 20^{th} century. On an individual level, however, the problems associated with the management of research in this mode were manifold. The most fundamental concerned the selection of projects to pursue. The general view was that the worth of a project should be related in some way to the probable profits yielded by products that might result from it. Indeed, a number of proposals sought to place this sort of reasoning on a formal basis (see [19] or [12], for example). Unfortunately, few proved at all useful in practice [2] [26]. It turned out to be so difficult to estimate the probability of ultimate success in a project, and the size and profitability of the markets for any products, that such estimations rarely constituted a worthwhile basis for day-to-day decisions, no matter how principled the derivation.

When scholars tried to formulate metrics to serve as reliable proxy indicators for ultimate success in research, matters proved just as unsatisfactory [8] [15] [19]. Tangible artifacts such as publications, patents and so on were signally unassociated with commercial success [20]. As Fusfeld et al. point out in [10], it was difficult even to decide upon a clear definition of the task of industrial researchers over the short term.

Another problem with the conventional model of research—and a significant factor in the inaccuracy of forecasts concerning project success—was the difficulty in ensuring congruence between the objectives of the company's management, research laboratories and operating divisions. This was manifest in two ways:

1) Projects were often undertaken with objectives that were at odds with the company's overall strategic goals.

2) *Technology transfer*—the process whereby the results of research projects were turned into commercial products—was a perennial problem. It was common for the fruits of research to fail to find a home in product divisions, with the latter either unwilling or unable to take them on [16].

The consequences of the problems outlined above were appreciable; in [23], Ramsey states that "fewer than 20% of the product ideas that began in technical development were successfully introduced into the market place, and … about three-quarters of all applied R&D funds were spent on unsuccessful projects."

If the efficacy of research in industrial age companies was dubious, its applicability in high technology industries is even less certain. Competition in high technology markets can be particularly intense and industry-wide seismic upheavals are not uncommon. As Bahrami and Evans state in [1], "[the high technology] environment is subjected to a frenzied pace of change due to the confluence of technological uncertainty (affecting both product designs and manufacturing processes), market uncertainty (in relation to end-user preferences and evolving distribution channels), competitive uncertainty (due to formation of spin-offs and strategic alliances), and arena uncertainty (such as emergence of new industry standards and converging industry standards)." These factors make assessing the probability of commercial success of long-term research projects almost impossible. Furthermore, they can often render wholly obsolete the products of even the most impeccably run project before they even enter the market (see [1] for a fuller discussion and illustrations from practice).

An equally compelling argument against research of the conventional stripe in high-tech arises from the considerable success that many companies in the industry have educed from the commercialization of science and technology developed by public institutions or elsewhere in industry. Sun Microsystems, for example, (the company which figures in the next section) was incorporated to manufacture a computer workstation the prototype of which had been developed at Stanford University. Later on, it derived a pivotal component of its product line (the SPARC™ CPU) from technology developed at IBM, Stanford and Berkeley [3]. Even the recent explosion of Internet-related start-ups owe their existence to technical infrastructure formed under the aegis of the U.S. Defense Department[2] and the laboratories of CERN.[3] Thus the central activity of many high technology companies—though it is often referred to loosely as "research and development"—is most accurately termed "technology development," in that it concentrates largely on near-term exploitation of others' research.

Given the observations above, it is clear that research as conventionally practiced in industrial age firms has few attractions for companies in high-tech. The fact remains, however,

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[2] Through the sponsorship of the Department of Defense's Advance Research Projects Agency (ARPA).

[3] Conseil Européen pour la Recherche Nucléaire.

that some high technology companies are enduringly committed to the maintenance of research labs; clearly, such long-term commitment must stem from a perception that their labs are sources of competitive advantage.[4] In the next section, we recount an interview with the director of one such research laboratory—that of Sun Microsystems. By formulating a model that appears to inform the management of research at Sun, we hope to delineate a paradigm for successful research in high-tech.

AN INTERVIEW WITH BERT SUTHERLAND, DIRECTOR EMERITUS OF SUN LABS

Sun Microsystems Inc. is a major manufacturer of premium computer equipment based in Palo Alto, California. It was established by two graduates of Stanford University in 1982, on the premise that innovative computer architectures could incorporate standard, off-the-shelf parts to produce machines that offered high performance and attractive prices. The company thrived in the highly competitive market for technical workstations, vanquishing many of the other participants and achieving a dominant position by the late 1980s. When cheaper IBM PC's began to invade the market for workstations in the early 1990s, Sun shifted the focus of its business to server computers—powerful, expensive machines that underpin the information technology infrastructure of many large firms. By 1999, the company's revenues amounted to over $10 billion. Throughout its 17-year history, Sun's growth has been underpinned by its ability to deliver innovation in a timely and consistent fashion.

Pivotal to that ability is the company's research division, Sun Labs. In technical areas ranging from microprocessors to programming languages, Sun Labs has been instrumental in the development of many of the most significant features of Sun's product line. During much of its existence, Sun Labs was directed by Bert Sutherland ("Bert")—a highly-regarded research manager whose extensive career in high-tech research also encompasses a position as manager of the Systems Science Laboratory at Xerox's renowned Palo Alto Research Center (PARC).

Bert articulated some of his approach to research management in two interviews—one with the author [28], and the other recounted in a transcript of [14]. A redaction of these interviews as a series of "precepts" is presented below. We have categorized Bert's precepts under one of two headings: 1) the functions of a research laboratory in a high technology company, and 2) techniques whereby those functions may be effectuated. (Each list is numbered for reference in later sections.)

[4] Admittedly, a research laboratory staffed with scientific luminaries may have considerable public relations appeal, but for the purposes of this paper, we seek to uncover the potential for more tangible benefits.

THE FUNCTIONS OF A RESEARCH LABORATORY IN HIGH-TECH

f1 A source of technology and innovation for the rest of the company.

f2 A magnet for technical talent, attracting expertise to the company.

f3 An "intellectual trading post" for exchanging technology and know-how with organizations outside the company.

f4 The company's center for "institutional curiosity" (a phrase coined by Seely Brown [24]).

f5 An entry-point for information about the customer.

f6 A source of ready technical skill for "internal consultancy" within the company.

f7 A source of experts who are acculturated to the company, for transfer into operating divisions.

f8 Custodians of technological options, allowing the company the flexibility to address changes in the industry.

TECHNIQUES FOR CONDUCTING RESEARCH IN HIGH-TECH

t1 Broadly speaking, activity should be based around projects. To start a project, four ingredients are desirable:[5]

- A good idea that's novel and relevant,
- A team leader (a "champion") who will assemble and direct a team to work on the project,
- A customer outside of the lab in one of the operating divisions with an interest (and a willingness to participate) in the proposed activity,
- Sufficient available resources to ensure effective execution.

t2 Ensure a reasonable "turnover" of projects, with new ones replacing old on a fairly frequent basis.

t3 When projects are sufficiently mature, move the project members—along with the fruits of their labors—into the receiving operating division to effect further development. Such moves may be temporary or permanent.

t4 As people move out of the lab, move new people in from inside or outside the company. Try to maintain a diversity of projects and people.

t5 Recruit people who are experts in their field (usually, with advanced formal qualifications).

[5] While it's possible to dispense with one or more of these desiderata, projects with such deficiencies should be reviewed with a greater degree of caution.

t6 When in doubt, trust the technical expertise of the researchers themselves, but arrange for experts to referee each other's work.

t7 At the conclusion of each project, produce a formal write-up of its results.

t8 In other regards, try to reduce the amount of "bureaucratic busy-work" required of researchers.

t9 To the greatest extent possible, publish publicly the results of all projects, so as to encourage exchange with other institutions.

t10 Supplement the research conducted in the lab with collaborative research undertaken in conjunction with universities and strategic partners.

t11 Encourage researchers to visit customers regularly, so as to sound out their present and future requirements.

Bert points out that in Sun's case at least, the research laboratory accounts for a mere 1% of the company's total headcount. To be effective, therefore, Sun Labs must facilitate innovation in other parts of the company, rather than attempting to shoulder the entire task itself. As Bert says: "We're effective only in so far as we are able to alter the behavior of the other 99% of the company."

A MODEL FOR MANAGEMENT OF RESEARCH IN HIGH-TECH

In this section, we propose a model for research in high technology companies that we postulate is capable of accommodating most—if not all—of Bert's precepts, as set out above. The model rests upon the fundamental role that *knowledge* plays in the operations of most high technology companies. (Here, "knowledge" can be considered to comprise capabilities that confer competitive advantage on a company. The term is discussed in greater detail in the next section.)

In Sun's case in particular, Sveiby [29] points out that in 1996, only one third of the company's market value was accounted for by its tangible assets. The remainder—over $40 billion—consisted of *intangible assets*, a substantial proportion of which was comprised by knowledge. Sun is by no means unique in this regard. The activities of almost all high technology firms revolve around the generation, acquisition and exploitation of knowledge [27]. By way of counterpoint, the focus of affairs in an industrial age company was the management of physical assets for production; one of the few departments concerned mainly with knowledge was the research laboratory.

The centrality of knowledge to the operations of a high technology firm has important ramifications for the conduct of research. For whereas in an industrial age company, knowledge *per se* was of little interest outside of the research laboratory, in a high technology company, it is a vital concern of almost every operating division, too. It is not unreasonable, therefore, to consider that knowledge itself is exchanged between a research laboratory and the rest of a high-tech firm, rather than products, as was the case in the conventional model of research.

A corollary of this assumption forms the central tenet of the model, namely:

> *The purpose of a research laboratory in a high technology company is to transfer knowledge into the rest of the company so as to promote innovation.*

From a general statement such as that above, of course, there depend a host of practical questions. Amongst the more salient are the following:

- What precisely is knowledge, and how is it expressed materially?
- How does one go about obtaining it?
- … and keeping it?
- How do I determine the value of the knowledge possessed by a research organization?
- How is knowledge most effectively transferred into the remainder of the company?
- What is innovation, and why is a research laboratory qualified to promote it?

The next section advances some answers to the above in the context of current research on knowledge management.

DETAILS OF THE MODEL

Answering the questions posed in the previous section effectively particularizes research management according to the proposed model. To show how Bert's precepts fit into this framework, a running commentary is provided, demonstrating how the precepts help fulfill the imperatives embodied in the model.

Knowledge: Definition and Expressions

Unfortunately, the still-nascent corpus of knowledge management still offers no really consensual definition of the term "knowledge" itself.[6] The working definition most appropriate for this paper derives from Leonard-Barton's [18], viz. *capabilities used to sustain a company's competitive advantage*. According to Leonard-Barton [18], knowledge may be expressed in four "dimensions":

1) The competence of company personnel,
2) Physical systems (databases in paper or electronic form, machinery, software, etc.) owned by the company,
3) Management systems in the company, encompassing education programs, reward/incentive systems, and such,

[6] Contrast, for example, the treatments of [18], [27] and [29].

4) Norms and values propagated by company culture—these include systems of status, rituals, shared beliefs, and so on.

Sveiby [29] places particular emphasis on the human aspects of knowledge (the first dimension above). He resolves "competence" into explicit knowledge, skill, experience, judgment and social relationships.

Almost all writers in the field point to the *tacit* nature of knowledge when it is manifested as personal competence. A concept originally expounded by Polanyi [22] and amplified at length in [21], tacit knowledge is unarticulated, and implicit in the approach of an expert to a problem. As such, its transfer is more difficult than that of explicit (articulated or codified) knowledge. By the same token (as Foss [9] points out), its value is correspondingly greater, since it is much less amenable to duplication by competitors.

Commentary

Bert's approach to research management appears to center on the people-oriented management of knowledge. Assertions f2, f6, f7 all speak of "expertise"—that is, knowledge as it manifests itself in supreme technical competence—and technique t6 further affirms its value. By stressing the human dimension of knowledge management, Bert's approach is particularly attuned to the garnering of tacit knowledge—which as we indicated, is of especial advantage to the company. Conversely, Bert ascribes a lesser role to explicit knowledge. This is a logical corollary of his avowed practice of publishing all explicit knowledge generated by the lab (see t9); since it's thus freely available to competitors, its value to Sun is far less than that of the tacit knowledge of lab personnel.

Obtaining Knowledge

Leonard-Barton delineates four "knowledge-building activities" [18]:

1) Shared, creative problem-solving,

2) Implementation of new methods and tools,

3) Experimentation,

4) Pulling in knowledge from outside the organization.

Of these, perhaps the latter two are most germane to the activities of a research lab, though of course, all four are apropos in some degree. One would expect, naturally, that experimentation would figure prominently in research, and that lab members of a scholarly bent would be wont to assimilate explicit knowledge published elsewhere. As Sveiby, Stewart and others observe [29] [27], however, one of the most expedient means of acquiring that all-important tacit knowledge is simply to recruit those who possess it. Such writers stress the importance of a recruiting strategy to a knowledge-based business, averring the necessity of drawing into the organization those who (in Sveiby's words) "will increase its stock of knowledge."

In terms of content, Coombs, Hull and Peltu [5] distinguish three areas in which knowledge may be most usefully acquired:

1) Technology relevant to the company's operations,
2) The disposition of the company's markets and customers,
3) Processes conducted internally within the company.

Though by no means exhaustive, such a categorization—particularly when combined with Leonard-Barton's typology above—might form the basis of a useful taxonomy of research activities that would help guide and explain the operations of a lab.

Commentary

Clearly, Bert promotes the activities listed above. In f4, for example, can be seen a statement of the importance of experimentation as well as the acquisition of knowledge from outside—that latter also figures in the notion of the "intellectual trading post" in f3.

The "recruitment of knowledge" also features prominently; witness f2 and t5, for example.

Evidently, Bert's precepts encompass the first two of the Coombs-Hull-Peltu content areas (see f5, t1 and t11). Interestingly, Hiltzik [13] reports that during his tenure at Xerox PARC, Bert retained anthropologists to study social processes (content area three). In [14], however, Bert disavows such a practice—though he does point out that comparable expertise can be obtained through collaborative research with universities and the like. It's possible that the political pressures elicited by "non-technical" research of this type (also documented in [13]) played a part in this volteface.

Maintaining Knowledge

Much of the current eruption of commercial interest in knowledge management pivots on the development of tools for storing and managing knowledge in its explicit form—that is, as codified in paper or (more likely) electronic databases (see [17], for example). No doubt such tools can be of immense value, as in the knowledge management programs at consultants Ernst & Young or Anderson, for example [11]. All such exercises, however, should be undertaken in light of two major caveats: First, much of the valuable tacit knowledge in an organization may defy codification, and second, knowledge (especially in an area like high-tech research) is in constant danger of obsolescence. Given the sizeable amounts of capital required to implement an effective knowledge management system, that the costs involved should be outweighed by the benefits is not self-evident in a high-tech research setting.

There is little doubt, however, that assiduous management of human capital—the repository of much of a research lab's knowledge—is almost always cost-effective. Counsel concerning the retention and motivation of the modern workforce abounds. As Sveiby [29] and Drucker [7] observe, however, management of experts such as those found in a lab presents particular challenges, since their professional values may not always align with those of the organization. (Experts, for example, are notoriously disinclined to engage in bureaucratic procedures, even where such procedures are vital to the smooth functioning of the organization [27] [29].)

One interesting proposal that Sveiby makes is the adoption of "tandem leadership" like that found in some artistic organizations, where management responsibilities are shared by an administrator and an expert professional.

Commentary

We have already pointed to the priority Bert gives human competence over knowledge in its codified form. For the reasons rehearsed above, this seems appropriate in high-tech research, though the increasing availability and efficacy of commercial knowledge management tools may justify a reassessment of this judgement in the near future. By no means does Bert entirely neglect codification, however, as t7 and t9 evidence.

There is a notable congruence between Bert's notion of project structure (as described in t1) and the concept of tandem leadership that Sveiby advocates. In many ways, Bert's "champion"—an expert in the subject of the project who will manage it on a day-to-day basis—plays the role of Sveiby's adjunct professional leader, reducing the danger of misalignment between the project team and lab management.[7] Bert's concern with the minimization of bureaucracy (t8) also plays nicely to the concerns of Drucker, Stewart and Sveiby.

Valuation of Knowledge

Certainly, an ample store of knowledge within a research lab is a prerequisite of its transfer into the operating divisions. Thus some assessment of the "knowledge assets" in the lab is of compelling interest to its management.

Reckoning the quantity of explicit knowledge generated or acquired by an organization is fairly straightforward; a simple count of papers, reports, patents, etc., should suffice. There are also methods proposed in the literature that allow the knowledge of personnel in an organization to be quantified in a more-or-less formal fashion. Stewart [27], for example, provides "human capital measures" recording (amongst other parameters) employee attitudes, qualifications and output in terms of patents and publications. Sveiby does likewise [29].

In both cases, however, the question of *quality* must be addressed; as Drucker [7] observes, quality is often a far more pertinent yardstick for knowledge than is quantity. In the area of research, the time-honored practice of peer review is still amongst the most effective means of assessing quality; in this case, peers may be drawn from the lab itself, from the operating divisions, from among the company's customers, or from other research organizations. Historical trends (the rate of uptake of a given researcher's output, for example, in the operating divisions) may also provide clues.

Another issue concerns the relative evanescence of a great deal of knowledge, particularly in high-tech (this is a point magnified in [27] and [29]). Thus a lab might want to apply some form of "depreciation schedule" to its knowledge assets, to reflect the superannuation of older stocks.

[7] Though of course, there remains the possibility of cleavage between the project champion and lab management.

Commentary

None of Bert's remarks concern assessments of the quantity of knowledge in a lab, though implicitly, recruitment of experts (t5) should serve to maximize it. Given Drucker's qualms, this is perhaps unsurprising. Bert's procedures for assessing quality rest largely on peer assessment, both internal and external (t6 and f3).

Transferring Knowledge

As we indicated above, the transfer of knowledge from research lab into a company's operating divisions ("technology transfer," as it is commonly entitled) has been a perennial concern of research management [16]. Knowledge transfer in a more general context is a preoccupation of writers in knowledge management, too.

In their pioneering work, Nonaka and Takeuchi [21] distinguish four processes for effecting knowledge transfer:

1) Socialization—person-to-person interactions leading to the transfer of tacit knowledge (a form of apprenticeship),
2) Externalization—the codification of tacit knowledge as explicit knowledge,
3) Combination—synthesis of explicit knowledge from multiple sources,
4) Internalization—the assimilation of explicit knowledge to form tacit knowledge.

Along these dimensions, Coombs and Hull [4] propose the institution of formal processes to promote knowledge transfer—together with Peltu [5], they go on to devise a checklist to guide the implementation and conduct of such processes. Davenport and Prusak [6] emphasize the importance of such formal processes in overcoming the possibility of cultural and social discontinuities retarding knowledge transfer from the lab to the divisions.

Many of the difficulties associated with technology transfer, Davenport and Prusak aver, stem from the problems associated with the transfer of tacit knowledge—socialization can be an expensive and time-consuming process. Both Leonard-Barton [18] and Sveiby [29] advocate the intimate involvement of the receiving party in the origination of the knowledge in question as a means of expediting the process. Drucker [7] highlights the importance of a holistic approach; truly effective transfer of knowledge requires adequate preparation and effort on the part of the receiver (the operating divisions, in our case), as well as the originator (research lab).

Commentary

The mainstay of Bert's approach to knowledge transfer is simple, effective and increasingly widely practiced: To transfer the knowledge of an individual from one department to another, merely transfer the individual (f7, t3). (By extension, where knowledge is manifest as social structures in a group, transfer the group as a whole.) There are, of course, potential hazards associated with this approach—resistance to transfer on the part of researchers and culture clashes with the receiving division come most directly to mind. Bert tries to mitigate these difficulties by (respectively) allowing for temporary transfers or secondments (t3, [14]) and by ensuring that the operating division has an interest in the research process from the outset (t1).

Innovation

In attempting fully to depict innovation, the first point to draw out is that the precise character of "innovation" varies greatly from firm to firm, depending upon strategic stance (implicit or explicit). One might contrast (as do Drucker [7] and Sharma [25]) the risk-averse *dirigiste* approach of the sort of firm portrayed in [23] with the much more fluid and venturesome enterprises exemplified in the study of Bahrami and Evans [1].

Whatever the disposition of the company as a whole, however, a research lab should be accorded a rather more daring remit than that of the rest of the company. For as Sharma [25] explains, too conservative an approach to innovation can be deleterious in the long run, and the limited confines of a research laboratory allow ideas to be tested without imperiling the firm's main operations. Further, there are a number of reasons that suggest that a lab is better able to innovate (at least on a limited scale) than are operating divisions:

1) The very necessary preoccupation of an operating division with revenue generation can lead to a resistance to innovation that Leonard-Barton characterizes as "core rigidities" [18]. She notes that the very factors that lead to commercial success can often constitute core rigidities—a theme taken up again in [25]. Being somewhat removed from revenue earning, a research lab may comprise a more fecund medium for innovation.
2) A high concentration of experts allows the staff of a research lab, as Sveiby opines [29], to transcend accepted practice in a given field. The latter is often a prerequisite for innovation.
3) The small size of a research lab, as well as appealing to the inclinations of many experts [29], allows it to circumvent the retardant bureaucracy associated with larger divisions, and allows the sort of frequent social interaction that aids innovation.

In order to promote innovation in a research laboratory, management should take steps to foster a more risk-seeking and less formal culture than that of the rest of the company. Furthermore, ensuring some degree of "creative friction" [18] by recruiting personnel with diverse backgrounds and views (and carefully directing any discord into creative activity) usually helps, too.

For the reasons rehearsed above, it is important to stress that according to the model, the focus of a research lab is the production of *knowledge* leading to innovation (often in concert with the rest of the company), rather than innovative products in themselves. This notion resonates well with the notion of "strategic enveloping" coined in [25].

Commentary

Bert quite clearly recognizes the necessity of innovation in a research lab (f1, f4), and takes steps to ensure it along the lines suggested in the previous paragraph (t2, t4, t5, t6).

CONCLUSIONS

This paper has proposed a model suggesting one possible direction for research in high technology companies. The model—which centers around the origination, acquisition and transfer of knowledge—tries to address many of the exigencies that are particular to high technology industry. We have also tried to demonstrate how the articulated practice of a distinguished high-tech research manager fits into the model, as it is elaborated with respect to current research in knowledge management.

Of course, the space of two interviews (and that of a paper) permit only the most perfunctory summary of a career's hard-earned wisdom, and there are without doubt important principles in Bert's practice of research management that have not been ventilated here. Nonetheless, we would observe that in so far as we have managed to capture some part of Bert's approach, it substantiates the proposed model in a convincing fashion.

Should the efficacy of the model be accepted, one might wonder if there are worthwhile principles that one can extrapolate from it that add to the aphorisms we extracted from Bert's conversations. If there are, we conjecture that they concern the importance of routines and procedures (formal or informal) that aid the capture and transmission of knowledge. Such activities—termed *knowledge management procedures (KMP's)* by Coombs and Hull [4]—can constitute an important source of competitive advantage. Furthermore, once sufficiently established, KMP's remain in place in spite of turnover in personnel. It's quite probable that Bert has perfectly sound reasons for eschewing an emphasis on KMP's—not the least of which is the above-mentioned aversion to bureaucracy shared by many professionals. Nonetheless, in the absence of such procedures, the possibility remains that management and staff changes may undermine the effectiveness of a research lab.

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