Project Selection Models or Professional Autonomy?[1]

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ABSTRACT *Scholarship on managing professionals has emphasized the centrality of autonomy to industrial scientists in selecting research projects, but has proposed alternative selection models. This article describes the project selection processes in centralized corporate laboratories of high-technology industries, as reported by scientists and managers. It finds that project selection models are rarely utilized in industry because different projects have different levels of uncertainties and benefits. Scientists enjoy autonomy in selecting projects and deciding how to carry them out in industrial contexts. Research projects in corporate laboratories are supported when several elements—research choices made by scientists, demands conveyed by R&D and business managers, and constraints generated by funding, time, and resources—are aligned at a specific point in time. The process appears to be one of resource allocation rather than of project selection.*

Keywords: autonomy, centralized corporate R&D laboratories, control, evaluation, idea generation, project selection models.

Introduction

The professional autonomy of scientists in research is considered to be one of the most fundamental norms of the scientific profession. It is understood as scientists having the discretion to determine the problems that they will examine, as well as the means they will use in confronting the problems. It is proposed that scientists working in industry must enjoy autonomy in conducting research in order to be creative and inventive.[2] Proletarianization theorists also suggest that scientists, subordinate to those who control the capital, must be granted autonomy from industrial management.[3] The consensus about the centrality of autonomy has led to studies on how scientists are able to maintain autonomy in industry.[4]

While all camps converge on the importance of autonomy to industrial scientists, research project selection has been described as being directed and controlled by managers. Scholars have proposed many models to explain how projects are selected in an industrial laboratory. Most of these models are analytical, based on isolating and quantifying variables, forming equations on mathematical principles, and utilizing computer-based information. The common idea behind these models is that managers need to select projects that will maximize the return from their investments. Return is calculated in terms of the commercial benefits that the project is likely to generate and the cost of research needed to achieve it.[5] Various project selection models differ in inputs to calculate the cost and the benefit. If, and how, these models are utilized by managers to select projects suggests a limit on the autonomy of scientists.

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This paper contributes to the discussion on scientists' autonomy by studying the actual process of project selection in centralized corporate laboratories of high-technology industries in the United States. There has been some research on autonomy in the scientific profession, but the focus has been on scientists' preferences for varying degrees of autonomy. This paper focuses on the research activity itself, which is conducted through projects, defined as a set of interrelated technical tasks with a specific goal and a commitment of resources.[6] Projects are the primary organizational means for scientists to engage in research, and generate new knowledge, products and processes.

This research is based on in-depth interviews with scientists and managers working in centralized corporate laboratories of high-technology industries in 1991–92 and 1996–97. Interviews with 47 scientists and six managers in 1991–92 indicated that the image one gets of project selection from the literature is rather different from what actually takes place in corporate laboratories. Five years later interviews with 72 scientists and 18 managers found little evidence that R&D project selection models were being utilized in corporate laboratories. Professional autonomy among scientists appears to be compatible with corporate goals. Projects are supported when research choices made by scientists, demands conveyed by R&D and business managers, and constraints generated by funding, time, and resources, are aligned at a specific point in time. The methodology used for the research is described after a brief review of the literature on project selection models and professional autonomy.

Project Selection Models

The literature divides project selection into two activities: project generation and project evaluation. Scholars have described idea generation for projects either as internal to scientists, coming from their creative behavior, or as external, coming from outside sources. An internal perspective[7] suggests several methods of idea generation such as:

- *brainstorming,* that is unrestricted discussion with deferred judgment, free wheeling fantasy and spontaneous associations;
- *brain-writing,* that is spontaneous and associative writing down of ideas on slips or special forms;
- *creative orientation,* that is guidance by heuristic principles in order to look at the problem from a new point of view;
- *creative confrontation,* that is stimulating ideas by transmitting structures to the problem; *systematical structuring,* that is decomposing the problem into sub-problems to display all possible ways to find a solution; and
- *problem specification,* that is revealing the crucial relations within a problem by systematic structuring approaches.

Since creative behavior and systematic approaches to idea generation are viewed as necessary in the industry, scholars have focused on external factors to foster creativity. The marketing department has been viewed as a source of good ideas because of its close contact with customers.[8] The performance gap theory[9] describes the idea generation process in terms of discrepancies in what the organization could do because of a goal-related opportunity and what it is actually doing in terms of exploiting that opportunity. With the transformations in the global economy in the 1980s, corporate executives have proposed businesses as a source of good research ideas.[10] They believe that businesses understand the strengths and weaknesses of the current products or processes, the relative position of the competitive products or processes, and unsatisfied needs of customers.[11]

Once projects are generated, scholars have proposed various models 'to provide managers with a set of guidelines to help them to choose the project evaluation and selection model that best suits their business environment'.[12] While differing in complexity, all models have the same goal to maximize the return from investing in research. Steele notes that the literature on project evaluation describes a very large number of analytical techniques for applying: $V = RP/C$, where V = index of value, R = net return, P = probability of attaining commercial success, and C = cost of research to achieve success. There are several popular models in the literature.

- *Q-sort procedure*[13] involves a group of people classifying items into a series of preconceived categories, which are then displayed in a tally chart, evoking group consensus on these classifications.
- The *checklist model*[14] involves evaluating each prospective project on the basis of a checklist such as fits into overall objectives and strategy, cost, likelihood of technical success, size of potential market, patent status, availability of resources, market trends and growth, development time and available skills.
- *Technological forecasting*[15] seeks to reduce the uncertainty of the cash flow by incorporating the life span of the project, the future price of the product, the market size, the trade-off between operating cost and the capital intensity of the production process.
- *Decision trees method*[16] generates the range of outcomes in the form of a probability density function and then locates the optimum path by calculating the expected value at each chance node.
- The *cost–benefit ratio*[17] calculates the total future costs of selecting a project and the future benefits that would be realized from future product or process sales by using standard discounting techniques to establish current values.
- The *scoring model*[18] computes overall project scores from ratings assigned to each project for each relevant decision criterion such as benefit potential, benefit-to-risk ratio, resource allocations, scientific potential, urgency, criticality, uniqueness, technical merit and program relevance.
- *Expert system*[19] utilizes a number of software programs in project evaluation and suggests criteria for developing new ones.

These models are more complex than what is presented here. Scholars continue to generate additional variations in project selection rather than convergence.

Professional Autonomy

Professional autonomy is considered to be a prerequisite for scientific work. It is defined as the freedom to choose the problems on which to work, to pursue them independently of directives from anywhere except the precepts of a discipline and to publish freely the results of such research.[20] The professional model views science as the autonomous pursuit of knowledge where scientists evaluate scientific contributions in an objective fashion and maintain scientific standards by scrutinizing the findings of fellow scientists. According to the model, scientists perform scientific work without any concern for self-interest since it carries its own reward. The knowledge they generate becomes the property of the entire scientific community.

The US National Labor Relations Act, which is empirically official and consequential, differentiates professional employees from other employees on the grounds of technical autonomy.[21] In the eyes of the labor law, scientific work, by definition, is discretionary.[22] Professionals are expected to exercise judgment and discretion on a routine, daily basis in the course of performing their work; other workers perform the routine task and have little technical control over their work. Scientists enjoy autonomy mainly because they belong to occupations which demand highly specialized knowledge and skill acquired by prolonged education that is certified by some professional society.[23] Scientists are involved with that which cannot be done by people who do not have similar knowledge and training. This is not to deny that some professions are more diverse in higher education and standards than others.

The consensus regarding the importance of autonomy to scientists has led scholars to study whether industrial employment is compatible with professional autonomy. In the 1960s, scholars suggested the existence of conflicts among scientists due to industrial control of autonomy.[24] Many scholars, however, felt that conflicts between scientists and industrial management were inflated.[25] Conflicts between scientists and industry received renewed attention in the 1970s, when proletarianization theorists argued that autonomy of professionals was withering away in industry.[26] Mainstream scholars have made an explicit comeback with the claim that autonomy and independence of professionals are necessarily curtailed in industry.[27]

It is believed that without autonomy, scientists may quit their jobs, take frequent leaves, do inferior work, or have a low commitment to the employing company.[28] Scholars, therefore, suggest a number of strategies to overcome the problems of autonomy such as the dual ladder, transition to management, managing ends and not means, and professional participation.

Methodology

This study is based on primary empirical data acquired by interviews with scientists and managers in 1991–92 and 1996–97. Both times the objective was to obtain a detailed description of the research process as it actually unfolds in industry; it was not to test specific hypotheses concerning project selection models or professional autonomy. With the first interviews, the goal was to study the changing links between corporate goals and science and their implications for the effectiveness of centralized corporate R&D laboratories. The purpose of the second set of interviews was to understand the affects of restructuring in corporate R&D on scientific activities. Nonetheless, the process of project selection was explored each time with scientists and managers.

In both sets of interviews, the focus was on corporate R&D laboratories in high-technology industries because they are associated with innovation, high-value added, success in global markets and spillover effects.[29] Economic activities in these industries drive national economic growth around the world. Corporate R&D laboratories were selected based on total R&D funds as a percentage of net sales and the number of R&D scientists per 1000 employees. In 1991–92, one corporate R&D laboratory in the machinery industry was selected along with another in the chemical industry. In 1996–97, the number of research sites was increased by the selection of two corporate R&D laboratories from each of the following industries—computers and office machinery, electronics-communications and chemicals—that had been recently restructured. Most R&D is concentrated in a few large corporations and many have gone through restructuring since the 1980s to gain greater efficiency.[30] All eight laboratories selected operate independently of any business division and employ over 1000 scientists and engineers from a broad range of scientific and technical disciplines. They are, in many ways, typical of other corporate R&D laboratories in terms of size, expenditures and research activities.

Scientists were identified as having a degree in scientific and engineering disciplines and by employment as research scientists in corporate R&D laboratories. Managers were identified by their organizational status, irrespective of any similarities with scientists in training and credentials. Since there are layers of managers in any corporate R&D laboratory, attention was focused only on the immediate managers of the scientists interviewed because they are the links between the scientists on technical possibilities and the upper management on business interests. In 1991–92, both Ph.D. and MS scientists were selected; however, in 1996–97 only Ph.D. scientists who had been in the company for at least 5 years before the restructuring began, were selected. In 1991–92, 31 scientists and six managers from two corporate R&D laboratories were interviewed. In addition, 16 scientists were interviewed who had left these corporate R&D laboratories and joined academic institutions to get an outsider view. In 1996–97, 72 scientists and 18 managers—approximately 12 scientists and three managers from each laboratory—were interviewed. Scientists were selected from the company's directory and the names of their immediate managers were taken from the scientists interviewed.

Both times, the interviews combined structured and unstructured formats. They were structured in the sense that certain topics were covered; they were unstructured in the sense that they resembled a private conversation with the participants. Such a combination allowed the scientists and managers to express themselves in depth, while I could maintain control over the topics and probe interesting leads. The interviews were tape-recorded and lasted from 40 minutes up to a little less than 2 hours. Individual scientists and managers voluntarily participated in interviews. One subject declined to participate on initial contact in 1991–92, and eight in 1996–97. Most participants chose their offices for interviews, while some preferred to meet in a conference room or outdoors, if weather permitted. Most preferred lunch hours though some chose office hours.

I interviewed scientists on what is the process of selecting projects in which they are involved. How do they generate ideas for research? What are the roles of technical, business, and financial criteria in the project selection process? How do they persuade managers to get support for projects? How much weight do they believe managers put on scientists' ideas as opposed to other considerations in selecting projects? Have they been involved in research projects that have been terminated? How is the decision made to have the project terminated? How do they view the connections between their research goals and the company's goals and interests? Does working in an industrial laboratory affect their professional autonomy in decisions about research projects, and if so, how? Does someone tell them how to do the research or do they decide the way the research will be conducted? Managers were asked: How are projects selected in their labs? How do they evaluate prospective projects? What kind of information do they desire and receive to make selection decisions? How do technical, business, and economic criteria influence the evaluation of projects? How do they balance the three criteria in evaluating prospective projects? How is the decision to terminate projects made? How do they commit resources at their disposal to various projects? In 1996–97, additional questions were added on how scientists are building a partnership with business divisions.

The interviews were transcribed verbatim for data analysis. In 1991–92, Professor Richard Worthington of Pomona College and, in 1996–97, Professor David Hess of Rensselaer Polytechnic Institute were consulted on the data. Both times, interviews suggested that project selection models are more popular in academia than in corporate laboratories, and that scientists enjoy professional autonomy in the industrial context.

Project Generation

In corporate R&D laboratories, scientists generate most projects, scientists and managers jointly initiate some, and only managers decide a few. The most common method of generating projects is one in which scientists make their own choices about what they will do. They generate ideas, write proposals, recruit colleagues and get managers interested in supporting those projects. In one scientist's words: 'Scientists come up with research ideas and sell them to managers'. Another said: 'Managers don't assign projects ...When I joined the lab, I asked my manager: "what shall I do?" He answered: "I don't know". This is typical'.

Managers may generate projects for junior scientists when they join the laboratory. However, as time goes by, junior scientists are expected to develop their own projects. Generally, a company plans workshops for newcomers so that they can learn the company's history, business interests, products, processes, and the skills of other members in the laboratories. The most common process of management assignment of projects involves consultation or discussion with scientists, with managers taking the initiative. Managers believe that their task is 'to get scientists interested in proposed work rather than assigning projects to them'. As one explained: 'We work in a team. Scientists and managers together decide new research programs. But, scientists play the key role. They are the ones who organize the research and get time commitments from other scientists and support from us ... We do this intentionally so scientists have a sense of ownership of programs. We want to make sure that our scientists feel that the programs belong to them'. Managers know that if scientists have significant input in projects, they will be highly motivated.

Actually, if managers want scientists to do something, they will talk scientists into working in that area. Instead of assigning projects, managers will try to generate interest among scientists in the particular areas. Managers will introduce scientists to people in the interested areas, ask scientists to read relevant articles or organize workshops on the subject. One scientist noted: 'Once my manager said that the great challenge of management is to get scientists interested in a job that they really do not want to do, and make those scientists like doing it'.

Scientists generate research ideas for projects by critical thinking, speculation, debate, interpretation, judgment, refutation, confrontation, analysis, synthesis, and specification. They employ what Schlicksupp called six methods of idea generation. As one said: 'Most of the research ideas are decided by researchers themselves by thinking, reading papers, interacting in the scientific community, and working in a group'. Scientists' brainstorming, however, is influenced by the industrial context, and it is not purely internal to them. Since corporations seek to make profits and be responsive to shareholders by increasing the value of their shares, corporate research is conducted to enhance profit making. Industrial interests influence even extreme speculative ideas. Technical and business motivations are interwoven. Scientists deal with scientific and technical questions, but the usefulness of their research ideas is business-oriented.

The most common reason to generate research ideas is to help the company in business by improving the existing products and processes or by innovating new products and processes. Scientists, however, do not calculate the gap with respect to its potential consequences for the laboratory and its perceived technical and psychological rewards for them as suggested by the performance gap theory. Instead, scientists match their education, training and technical interests to what the company is interested in. In one scientist's words: 'There is an understanding of what business the company is in and what business the company wants to be in'. Another said: 'I select my research projects. But, I tie my research interests to the overall objectives of [the company]'. When it comes to generating specific projects, scientists are influenced by the problems the company has in making its products and processes. One noted: 'We base our decisions on how likely the research is going to be useful to the company's activities. It could be in the form of long term strategic objectives that managers have set forth. Or it could be in the form of some specific problems that the business components' are having'.

Increasingly, more weight is being placed on innovation and commercialization than on technical or scientific merits. Since the mid-1980s, in many leading companies, research has been carried out in the context of immediate business interests and this imperative is present from the beginning. Earlier, research was carried out on the basis of the generic interests of the company. The objective was to generate technical knowledge, which would eventually be beneficial to the company. The business considerations were in the background. Now the new goal is to fit business needs into research, and not the other way around. Scientists are generating research ideas that are closely aligned to the company's products and processes. As one scientist said: 'Earlier, my managers believed that we needed to focus on fields that [the company] was interested in. And we did ... The center did not have a set of defined goals. Instead, it had a general goal that scientific research was healthy for the company ... Now [the company] wants to see the return from its investment ... We have to produce research, which can be shown in [the company's] best interest'.

The availability of research funds has become a major reason to generate research ideas. In many corporate laboratories, the balance of funding has shifted from corporate sources to business divisions, which are more closely monitored through customer–contractor relationships. Earlier, corporate laboratories received most of their funds from a flat tax on the sales or profits of the company's business divisions. Now, most of the research funds are being generated from direct contracts from the company's business divisions. Leading companies in high-technology industries have changed their funding structure from one third being generated by business divisions to more than half. Scientists can generate all kinds of research ideas, but without funding their ideas would remain an intellectual exercise. Scientists invariably generate projects for which they could get funding or contracts from internal and external sources. They know 'where the money is concentrated for research'. As one said: 'we know who has the money. We don't go to appliances because they contribute very little for research'. Another said: 'We also find out if there is an opportunity to get funds from NSF [National Science Foundation], DARPA [Defense Advanced Research Project Agency], or other funding agencies'. Often, managers will inform them that 'there is an opportunity to get research funds in this area because [business] is interested in that area'.

When the desires of business divisions and the availability of funds influence ideas generation, scientists' work tends to become development-oriented instead of research-oriented. The market mechanism of funding leads scientists to generate projects that are short term and mission oriented. As one said: 'Business people have a bias for incremental improvements in the technology. They prefer to keep what they have been using. If we tell them to fund our work, which will replace or make major changes in their technology, they will get very nervous'. Another said: 'Managers are unlikely to fund brilliant ideas if they take a few years. They like to see the results from the research soon ... They do not like to risk the failures'. Managers feel that 'in the existing financial environment it is difficult to undertake time consuming projects'.

Also, the salary, rewards, and other compensations encourage scientists to choose research problems of interest to their laboratory. One scientist explained: 'You are recognized more if you generate something that would be beneficial to the company than if your efforts are more directed on things that do not impact a product or a process. If your project does not have a lot of visibility, then you do not get the recognition you want'.

Since managers do not assign projects or tell scientists what projects to work on, scientists enjoy professional autonomy. Scientists are advocates of their own research. They make their own choices about what they will do. This, however, does not mean that scientists have absolute autonomy. Instead, scientists generate research ideas within the framework of industrial objectives. Scientists have to find out how their research interests coincide with the company. Yet, they make specific choices on the research topics which interest them the most from a technical standpoint. As one scientist said: 'We decide based on what we know our technical expertise ... We do this to have a control over technology we work on. We do not want to be in a situation when projects are assigned to us'.

Project Evaluation

Projects generated by scientists are rarely self-justified; instead, they are evaluated and selected by managers who decide on a particular kind of research by allocating funds. Scholars, therefore, draw the attention of managers to various analytical models to facilitate project evaluation in a rational scientific way. These models are proposed as a tool in the decision-making process. Lowell Steele, however, has argued that rigorous collection and quantification of information has placed severe limits on the utility of project selection techniques for managers. According to Steele, since none of these models have been adopted in corporate laboratories, they might as well not have been written. This study provides empirical support to Steele's reasoning and observation.

In corporate laboratories, research projects are not submitted at a given point in a year for selection. Instead, project selection decisions are made throughout the year, with R&D managers, scientists, and business managers influencing decision-making at different levels. Scientists are involved because they have insight in research useful to the company in the future. R&D managers are involved because they place technological decisions in the context of overall corporate goals. Now business managers are being included because they fund the research and know what is needed from the laboratory. Scientists and managers are involved in many types of research projects such as explanatory, application-oriented, developmental, long-term, short-term and high-priority, which all have different tasks. Further, they are engaged in research to benefit not one but diversified businesses of their company. Project selection in corporate laboratories, therefore, cannot be reduced to an analytical model, which is applicable throughout to all types of projects and to different audiences.

Unlike analytical models that assume centralized and formal evaluation, the process of project selection in corporate laboratories is decentralized. Scientists propose ideas to their immediate managers who then financially support the work. If support demanded by scientists is outside the budget allocated to the first level managers, then both will approach the next level of managers. If scientists have to get support from business divisions, either they or their managers will contact business managers. The process of getting R&D and business managers' approval for a project is rather informal. Often, scientists 'toss' their ideas to managers on the phone, via electronic mail, in the hall or in the dining room. If managers react favorably then scientists send a small report to them. If the work has to be approved by the upper management, scientists make a formal presentation to them.

The first and most important thing managers seek in projects is 'the relevance of the proposed technical knowledge for the company'. It is the job of management to ensure that corporate interests are always at the forefront in determining how the money for research is spent. Since the 1980s, R&D managers have faced increasingly stringent demands for the performance of research for the company's growth. There has been a shift from a situation where a high technological capability was uncommon to a situation where high technological capability is distributed among many countries. US companies are facing intense competition from Japan, Europe, South Korea, and Taiwan. Corporate management has come to question the return from investment in research. Business managers are expecting a larger contribution from research. R&D managers are redefining the role research has to play for the company. Business and R&D managers are carefully examining whether the work proposed by scientists is really needed. Managers are asking the basic question: 'What is there in this work for the company?'

Scientists have to find a way to persuade R&D and business managers that their work will be valuable for the company. As one scientist explained: 'We have to be a good salesperson in a sense that we should be able to get our managers excited about a totally abstract idea. Salesmanship has a lot to do with getting support for a project. We have to sell the idea that our work is significant'. Scientists have become conscious that often research results have not been commercialized. They are building a 'partnership' with business and thus have incorporated some business elements into research. Increasingly, scientists are proposing research with a business format such as: risk factors to technical possibilities, cost–benefit, and possible manufacturing potential. As one scientist said: 'You try to come up with a business plan associated with the project, namely how, if the project is successfully completed, will it benefit the business. It is always helpful if you can identify a specific business component ... Letters from managers of business components supporting the project are always helpful'.

Similarly, R&D managers, working with business managers, are thinking about cost of capital, investment analysis, manufacturing strategy, commercialization of new products, competitive analysis and distribution strategy. As project selection models suggest, cost of the project as a proportion of the total R&D budget allocated to managers and the estimated time completion period are important factors to support a project. Cost–benefit has acquired some legitimacy in corporate laboratories because it treats R&D expenditures as an investment. Earlier, research was viewed as a valuable investment for the company's growth and a premium was placed on stability in funding. Yet, managers do not apply investment or budgeting methods to calculate future costs and returns to compare projects or terminate existing projects at a given time. One manager said: 'We have to take a hard look at the cost of the programs. Some programs are easy to support. But, this is not the case with all programs ... If we can see the need from the company's business perspective then we go ahead and support the project ... It is a simple decision at one level, but not so simple at another level'. One scientist acknowledged: 'They give you a chunk of money. You use the money for the project ... When funding gets down to a critical mass; you go and get more funds. If managers agree you are making really good progress, they will give you more money. Otherwise, the project will stop'.

Unlike cost–benefit models, managers do not rely solely on financial criteria. Instead, they take prior decisions, human resources and technical feasibility, among other factors, into consideration. The probability of technical success is considered as important as cost and time until a project has come closer to the developmental stage. Analytical models consider the probability of commercial success but do not elaborate the probability of technical success. Managers support many minor projects that have many unanswered technical questions. These questions can only be answered by additional research, whereas analytical models point in the direction of non-technical criteria. While making a technical judgment, managers may face uncertainty. They often rely on their intuition about proposals and scientists. Managers pay close attention to the 'scientists' expertise, competence, commitment, vision, and leadership quality'. As one said: 'We rely on the track record of the researcher. If the researcher has had a successful track record then chances are his project will be supported'. Scientists acknowledge that 'the seniority of scientists' influences managers' decision'. One said: 'If they like what you have been doing, they will literally pour money for you'.

When projects have more input in the areas of application, managers find out a project's relative attractiveness by examining cost estimates, likelihood of business success, time required, conformance with the company's goals, scientific merit and business attractiveness. With such a list they identify sources of agreement and disagreement to give serious thoughts about the criteria to be used in project evaluation. Yet, they do not take a comprehensive checklist developed by others to use as a basis for project evaluation. Managers 'employ a set of criteria which is compiled by [them] to suit the particular needs of [their] group'. When they assign scores to compare different projects, they do not spend too much of their time collecting enormous amounts of data and manipulating sophisticated mathematical techniques because they work under intense pressure. As one manager said: 'We really don't have much time. In industry, you have to decide fast. Some times within a few seconds. You don't have a luxury to take your own sweet time'. One scientist explained: 'If at any point managers are convinced that the area the project is in is not going to serve the company's needs, they have no qualm to terminate the project. They don't take more than a day to decide'. Other than scoring and checklist, most project selection models require extensive data gathering which imposes an additional burden on managers. In highly diversified companies, information requirements become so extensive that it is not clear that project selection models are cost effective.

Managers are cautious on long-term projects because of the risk involved. According to them, investors in equity markets demand a return on their investment in a short time. As one manager said: 'The main problem with the long-term is that it competes for the resources with the short term. Short term has more power behind it. But one has to realize that if we do not make things work in the short term, we would not be here for the long-term'. Different companies, however, classify projects long- and short-term according to their needs. In pharmaceutical companies most research is long-term because, as one scientist said, 'it takes a minimum of 10 years and millions of dollars to discover a drug'. These companies support long-term research even though products may die in clinical trials for a number of reasons, or the product may not be needed in the market by the time research is completed. Cost–benefit models, on the other hand, favor short-term goals, do not acknowledge the differences among companies and suggest one single technique for all.

Different levels of managers give different emphasis and importance to factors associated with project evaluation. Usually the evaluation of projects is more technical at the lower level of management than at the higher level. Analytical models, on the other hand, do not differentiate among audiences. Immediate managers are usually trained in scientists' areas of expertise and evaluate proposals for what ideas are there and for the technical content of proposals. The higher levels of managers can no longer evaluate projects on the basis of their knowledge and their interactions with scientists because of the sheer number of scientists working under them and the enormous body of scientific knowledge that they represent. These managers rely on the assessment of the lower level of managers to make a decision. As one manager said: 'We do not have a superstructure ... I manage four groups. Each group has about four to five scientists, a group leader, and a couple of technicians ... They give me the progress report and then I report to [X] who is our branch manager. He pretty much relies on what scientists and I have to say'.

With the marketing mechanism of funding in many leading companies, business managers are increasingly involved in selecting research projects. They know about the areas of application, the strengths and weaknesses of the current products or processes, the relative position of competitive products, what is needed from the lab, how the developed technology would be implemented, the work procedures of workers, problems faced by operators and material in use. These managers tend to have an obsession with financial success and cost effectiveness. They fund research in output mode. Scientists are increasingly translating their own research into the language appropriate to other agendas. As one scientist said: 'We go out and sell contracts to business people ... Since I am a selling party, I sell what I am interested in doing ... If I don't succeed in selling, then I lose control'. Another said, 'We are dealing with business managers who do not understand research. So we have to learn what they do understand, i.e. the basics of the businesses'.

Project evaluation has little affect on scientists' autonomy because managers do not employ analytical models. Only checklist and scoring models, which are developed by managers for internal use, are used for some projects. Both models are simple and do not have formal, rigid structure. Cost–benefit analysis has become appealing only as a concept and corporate laboratories have taken some elements of it. Managers do not rely solely on financial criteria to evaluate projects; prior decisions, technical expertise, intuition, resources and leadership quality of scientists play an important role. Scientists' autonomy is constrained by a company's business needs and growth. Since research is being carried out in the context of immediate business interests, scientists are incorporating business value in their research projects.

Discussion

The goal of this research was to outline the whole process of project selection in corporate laboratories in order to understand the extent of autonomy enjoyed by scientists. The goal was not to check the validity of various project selection models. In corporate laboratories, project selection is a process that requires an interaction of technical knowledge with business goals and the participation of scientists, R&D managers and business managers. Project selection models do not curtail scientists' autonomy because scientists and managers rarely employ them. Scientists' autonomy, however, is affected by the main industrial goal of business opportunities. Ultimately, corporate laboratories are supporting technical work in fields that will be beneficial to the company.

Corporate laboratories have been physically separated from other business units of the company to maintain an environment conducive for good creative research. There is an understanding that research differs from other industrial activities in its requirements for effectiveness and in the nature of its contribution. So when the whole process of project-oriented research activity is outlined, absolute autonomy of scientists does not seem crucial simply because they have it in carrying out research. Scientists select projects and decide how to carry them out, not because they are forced or required to do so, rather they are internally committed to research for the company.

Even when some form of evaluation criteria is employed in project selection, industry does not completely control scientists like other workers. The organizational structure in corporate laboratories is professional, based on the authority of expertise; it is not based on bureaucracy where subordinates are expected to obey officials by virtue of their formal positions.[31] Scientists are expected to follow their knowledge and skill rather than their managers' orders. Managers do not dictate their research work because it requires knowledge, skills and technical judgment that only scientists' possess.[32] Managers, therefore, hand authority over the research projects to the scientists themselves. Managers set broad goals, but they do not set means to goals. They allow scientists discretion at work. Scholars, who continue to propose the existence of the inherent conflict between scientists and managers, need to address the point that centralized corporate laboratories deviate sufficiently from the Weberian model of rational–legal bureaucracy.

Some studies have shown that there is little evidence that the analytical models for project selection are being utilized in industry.[33] Scholars, however, continue to propose models for project selection because of the belief that 'each criteria may be of particular significance at some point of time and should not be lost sight of'.[34] This faith reflects a belief that increased productivity of corporate laboratories can only derive from analytical models. Managers and scientists are highly educated and are likely to be open to better techniques in project selection, but, current project selection models do not appear functional because they are removed from the context of corporate laboratories.

Other than checklist and scoring, scholars have built complicated analytical models, which are perhaps better suited for organizations with large numbers of projects remote from their sponsors. Or the project selection models could be used to support decisions made on the basis of other criteria.[35] Analytical models provide little practical guidance to managers of corporate laboratories because different projects have different levels of uncertainties and benefits and a uniform criterion is difficult to apply. Many criteria in these models are non-technical, of limited importance, and difficult to assess at the time of project evaluation.

Furthermore, the project selection models are costly and time consuming even though the common idea behind these models is to minimize the cost and maximize the benefits. Scientists and managers with their knowledge and experience make the same intelligent guess about a project, which sophisticated models propose to do, and the former is less costly. It should be noted that the level of uncertainty and risk associated with typical corporate projects can only be reduced by additional research and not by economic and financial analysis. Considering the decline in fundamental long-term research, there is a need to suggest to managers to be bold and daring at least with some projects; instead of devising models which would make managers more cautious and risk-averting.

Scholars, therefore, need to design models that can become effective tools for corporate laboratories. Ultimately, scientists cannot be left alone if corporate laboratories want to succeed. At the same time, analytical project selection models do not provide the best solution for the effectiveness of corporate laboratories. Scholars on project selection must understand that the project selection process is rather complex. It is not a process that starts with a clean slate, takes place at a particular instance in time, and to which any single model can be applied. It is an ongoing process that has to be understood from an organizational perspective. History and culture of corporate laboratories, human resources, technical expertise, prior decisions, global constraints on industry, the scale of R&D, types of research and managerial judgment play a key role in selecting research projects.

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