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Factors influencing advances in basic and applied research: variation due to diversity in research profiles

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Introduction

Despite the increasing importance of basic and applied research to the economy and to national defense, despite increased calls worldwide for accountability and demonstration of results, particularly for publicly funded research, and despite the increasing diversity of research laboratories, there is no adequate theory about what factors influence advances in science, or how the different structures and strategies of research laboratories or units influence those advances. There is not even agreement on how to describe the strategies or intended outcomes, or how to measure the outcomes of these science advances. Some refer to our current situation as the "globalization learning economy" (Archgibugi & Lundvall 2001). In addition to globalization, there has been increasing specialization in knowledge production and in organizations, bringing with it the need for interdisciplinary work and collaboration.

Such a theory could build on contingency theory, which suggests that organizations can have different strategies and structures, and that they perform best if strategy and structure are aligned. We suggest here that, given the increasing diversity in research structures and strategies, a contingency theory for science management is needed in order to discuss the factors that influence scientific advances, and to assess research and research advances appropriately.

Both Crow & Bozeman (1998) and Larédo & Mustar (2000) have suggested that the current classification of research projects does not capture their essential differences, but their solutions do not provide a theory about the diversity of structures and strategies involved in scientific and technological research. Innovation theory stemming from Burns & Stalker (1961) similarly is inadequate in that it typically focuses on the entire organization and, we would suggest, one organizational model (the organic organization, characterized as having few authority levels, informal, decentralized, and extensive communication), rather than the specific units where different types of research are conducted (Zammuto & O'Connor 1992). In the industrial innovation literature, there is only one study that examines the structure and performance of research laboratories as such (Hull 1988). Read (2000) has suggested that, in general, there is not an adequate theory of innovation that explicates clearly the organizational contingencies and how these might vary. We would suggest that the monolithic treatment of research, technology and product development (R&D) and, within that, basic and applied research stems from the lack of studies of diverse research projects, especially those involving basic and applied scientific research. Further, this monolithic treatment limits the opportunities to describe the different outcomes of research projects in ways that can be generalized across a number of projects with similar aims (Bozeman 2004). Performance is measured as discrete outputs, such as the development of a new algorithm or sensor, rather than the contribution of either of these to increased accuracy of predicting wind speed in hurricanes, or the soundness of the science underlying weather prediction more generally.

Although we are beginning to see many studies of research laboratories and organizations (see bibliographic review of Jordan, Streit & Matiasek 2003 and, more specifically, Auditor

General of Canada 1999; Brown 1997; Ellis 1997; Joly & Mangematin 1996; Menke 1997; Szakonyi 1994a & b), the fact remains that none of these studies have focused on the measurement of incremental versus radical innovation, and then assessed which factors maximized these outcomes. Furthermore, most of these studies focus on new product development in industrial laboratories; few involve an important arena relative to innovation, namely basic science. And while there is now a new and growing literature on projects (Kim & Wilemon 2003; McDermott & O'Connor 2002; Shenhar 2001; Shenhar 1998; Thanhaim 2003), most of the emphasis here has been on developing dimensions of complexity or uncertainty at the project/product level, rather than an overall theory of diversity of scientific research projects. However, since complexity and uncertainty are critical elements that must be included, the concerns of these papers can inform the development of a satisfactory theory about research diversity.

There are three reasons why a theory about the diversity of any and all organizations is not sufficient for R&D, or for basic and applied research. First, the management of a large number of researchers is very different from the typical management issues involved in contemporary firms or public bureaucracies (Clarke 2002). Among the differences is the implied motivation of the researchers, which may be more about curiosity than money. Also, it is generally understood that the time frames for successful outcomes can be quite long and uncertain, and tasks are not routine or repetitive. Second, research is increasingly separated into distinct units: more and more basic research is being conducted either outside the firm (Hage & Hollingsworth 2000; Lundvall 1992, Meeus & Faber, forthcoming; Oosterwijk & van Waarden, 2003) or in special kinds of inter-organizational relationships, such as research consortia and global alliances. Finally, as with, for example, nanotechnology, scientific research is increasingly organized into highly fluid research projects, making the issue of how these projects should be grouped and managed more and more problematic.

Even though a theory for the diversity of research projects is different from the theory of organizations, it can be informed by the latter's suggestions of basic sources of tension in the management of multiple researchers engaged in a variety of projects. Proposed here as a first step in building a theory of research diversity is a Research Profiles Framework: it has been developed from a combination of review of the innovation and R&D management literature and lessons learned in focus groups with scientists and engineers. In particular, it builds on insights from the Competing Values Framework (Cameron & Quinn 1999) which, in turn, grew out of contingency theory (Hage 1980; Lawrence & Lorsch 1967; Mintzberg 1979; Perrow 1967). In particular, the older theories from the 1960s emphasized contingencies of strategy and structure as related to their environmental context. Although the proposed framework was developed independently of the growing literature on projects (Kim & Wilemon 2003; McDermott & O'Connor 2002; Shenhar 2001; Shenhar 1998; Thanhaim 2003), this literature complements the Research Profiles Framework in its diversity of research projects, and can be synthesized with it.

In this chapter, the Research Profiles Framework is put forward as a developing theoretical framework for understanding the diversity of research projects and organizations with multiple projects, and how this affects scientific advances. The first section indicates the various reasons for a separate framework for research projects. The following sections describe, and provide the justification for, the choice of specific strategic and structural contingencies that define distinctive profiles of research activity. Then managerial problems in the generic research profiles are described. Prior to concluding, implications for managers of organizations with multiple research profiles are discussed: these include the possibility of using the Research Profiles Framework as an organizing principle, beyond specialties and problem areas, for creating, within a research organization, groups of research projects that have specially trained managers.

Scope and terminology

The primary focus of this framework is the project, but implications for managers of the larger organization within which projects usually sit cannot be avoided. The term ‘project’ is used here to encompass a set of activities in a knowledge area that are focused on a particular question or problem area at a particular time. A research project may be an entirely separate organization, or a project within a firm or mission agency, such as the US Navy or NASA, or a research consortium. Even within organizations where research is basically the only activity, research activities are increasingly organized in projects and teams. Researchers are frequently engaged in multiple projects, raising the question of what is the best way of grouping these projects. Furthermore, the fluidity of the project’s personnel across time also raises questions about the best way of grouping projects into administrative units of a higher order. Given this diversity, for our definition of ‘research organization’ we use Westley's definition of an organization as a “series of interlocking routings, habituated action patterns that bring the same people around the same activities in the same time and places.” (Westley 1990: 339)

Although we think that the Research Profiles Framework will apply equally well to technology and product development, this paper is focused on strategy, structure and management of basic and applied research, not on technology and product innovation. This separation of science from technology is deemed impossible by some, but most recognize a blurry distinction between science and technology (Faulkner 1994). Some technologies are strongly science-related. Similarly, some science is strongly technology-related. Further, the non-linear models of R&D all show interactions and flows between research and technology (Kline & Rosenberg 1985; Hage & Hollingsworth 2000). A few in the social studies of science, starting with Bruno Latour (1987), speak of “technoscience,” and see science and technology as a single methodology, where the research process and what is deemed a legitimate scientific or technological advance from that process is socially constructed and political in nature. There is a benefit, however, in distinguishing between science and technology, even while recognizing that, in the process of creating either scientific or technological advance, the two are often tied together. After reviewing past and current literatures, Faulkner concludes that there are nuances, and that these are useful because they serve as vague umbrella terms that roughly define limits. The limits are useful precisely because, she says, the socio-technical organization of science is different from technology (with technology being more hierarchical), as are the purpose or orientation, and the cognitive and epistemological features (Faulkner 1994).

In the Research Profiles Framework we describe here, we define the advances in science more broadly than others would, but in ways that have emerged since 1995 as requirements for documenting the outcomes of science increased. The National Science Foundation (NSF) has described its outcomes in terms of ideas, tools, and people. People can be individuals or “communities of practice.” Of course, transitions from advances in science to further research or application in a problem area are also considered important outcomes of the research. These five areas were agreed upon at a workshop on knowledge benefits sponsored by the US Department of Energy (Lee *et al*. 2003). Thus our definition of the results of science is not limited to advancing knowledge for the sake of knowledge or potential application, or of gaining certainty by replicating the findings. In addition to work that advances knowledge of science (understanding of phenomena), there are advances that are new research technologies, instruments, or procedures, including new ways of organizing how research is accomplished. (See the chapter by Terry Shinn for examples of these.) Thus 'advance' includes advances in theories, laws, and general principles, measurement tools, operating principles, or in properties of materials. As mentioned, there are also advances in knowledge and skills as these are embodied in people (human capital), and in the growth of communities of practice around a discipline or problem area. Outcomes are also marked as transitions to further work, such as occurs when others extend what is known, or enter a field, as they have done with biomaterials, or when a concept or research tool is moved into commercial development.

Why a theory for profiles of research projects?

Increasingly, research is being pursued in a range of organizations of all sizes and shapes, from large, high-budget research laboratories to small research facilities. At one extreme are the many small high-tech research companies (Powell 1998), as represented by biotechnology start-ups, where applied research and product development are the major activities. At the other extreme, also in the private sector, are the chemical, pharmaceutical, and semiconductor industries, all of which have very large research projects, sometimes in a central headquarters, sometimes in multiple units clustered by product line or in a country. But it is in the public sector, which has been frequently ignored in the literature, where more and more basic and applied research is typically conducted. Here, as in the private sector, extremes exist: they range from the many small research projects funded by the National Institutes of Health (NIH) or the NSF to the mission-oriented research conducted at the large Department of Energy (DOE) national laboratories at Livermore, Los Alamos, Chicago and elsewhere. Parallels to these exist in Europe, with the European Organization for Nuclear Research (CERN) perhaps the most prominent example.

We suggest four reasons why we need a theory about the diversity of research and how this research is managed. First, science is too important for industrial innovation and meeting public goals, such as health, to be ignored. Second, knowledge production has changed, and current frameworks do not reflect those changes. Third, more study is needed of the management of large-scale research. Fourth, pressures to assess and demonstrate progress and outcomes using a one-size-fits-all approach and current measures can put perverse incentives into the system, as well as understate the value of the science advance. We will explain each of these in turn.

First, scientific research, and especially basic research, is increasingly becoming the basis for success in industrial innovation and achieving public goals. The importance of basic research is obvious in the case of the pharmaceutical, semiconductor and other large high-tech industries, in which to be on the cutting edge necessitates exploratory research. Less obvious, but also increasingly important, is the need to make products neutral relative to their environment - that is, to reduce products' negative health and safety consequences for individuals, as well as their consumption of energy and scarce resources. This has amplified the importance of basic and applied research in, for example, material sciences. Basic sciences are also now seen as contributors to national goals such as health and a competitive economy. Indeed, the terrorist attacks of 11 September 2001 have made national security as important as the health of the economy: this has spurred applied research into, and technological development of, a variety of sensors for detection of biological and chemical weapons, as well as other kinds of anti-terrorist technologies.

Second, the Burns & Stalker (1961) organic model is obsolete because, in both the private and the public sectors, the world of research has changed dramatically. There are now more varieties of innovation than the simple incremental vs. radical innovation distinction: for instance, the hypercube innovation model of Afuah & Bahram (1995) has drawn attention to more complex views of types of innovation, including the architectural notion of innovation, though it does not include a discussion of the different organizational models needed to produce these kinds of innovation. More generally, there has been a movement away from the linear model of research towards a chain-link model (Kline & Rosenberg 1986) and the differentiation of the idea-innovation network - that is, the movement back and forth between manufacturing research, basic research, applied research, quality research, product development and marketing research (Hage & Hollingsworth 2000). Given this, neither functional arrangement by managerial specialties nor a product arrangement appears appropriate for describing research activities, particularly with the change to multiple projects of short to medium duration (Davis & Lawrence 1977; Cleland 1984).

The dimension of external relationships, essential to understanding both the strategy and structure of science (and R&D), is not explicit in the organic model and we suggest that it should be. Most complex projects frequently involve multiple scientific and engineering disciplines that change across time because, as new problems present themselves, new kinds of expertise become essential. Indeed the complexity of many of these projects is far beyond what is usually considered in the organizational literature (but for an interesting exception on the complexity of new product development see Kim & Wilemon, 2003). Separate organizations are now specializing in one or two of these areas of research (Hage & Hollingsworth, 2000; Oosterwijk & van Waarden, 2003). As companies specialize more and more in product research, basic and even applied research has been moving out of the firm. This shift toward specialization accompanied by increased intra and inter organizational collaboration has led to a whole series of new ideas about the organization of science, including the triple helix (Etzkowitz & Leydesdorff, 1997), new systems of knowledge production (Gibbons 1994), and distributed innovation processes (Rammert 2003). See the chapter by Rammert for a discussion of functional specialization and fragmental distribution of research.

Third, also missing in the analysis of innovation is an understanding of large-scale technical systems (Mayntz & Hughes 1988), such as the electrical, railroad, and telephone systems, large-scale scientific research such as global climate change, and large-scale projects about the physics of space. The White House Office of Science and Technology is interested in how to choose and manage big science projects. Shenhar (2001) has recently included project scope in his discussion of engineering projects, using the language of systems. But further research is needed to determine whether the distinctions in science between a concept, a hypothesis, a theory, and a paradigm (which roughly parallel Shenhar's dimension of concept, creation of a new component, creation of a new system involving the component and, finally, a system of systems) can be related to the scale and size of basic and applied research. It appears the same distinctions can be made. For instance, in a quick glance at the research conducted at the US National Oceanic and Atmospheric Administration (NOAA), we see that there is work on both the science and the technology involved in the development of a new cloud sensor, research on a suite of sensors that measure a variety of properties, research on the oceanographic system and, of course, the system of systems (ocean, atmosphere, solar, etc.) as reflected in the science of weather forecasting.

Fourth, following the lead of industry, which wants to know the return on investment for R&D activities, there has been a major shift toward assessing the progress and outcomes of all public research programs, part of a larger movement toward increasing public accountability. A number of countries, including Japan and France, have created national committees for research evaluation, or offices within ministries with this objective. Within the US, the Government Performance and Results Act of 1993 (GPRA) and the Office of Management and Budget's Program Assessment Rating Tool (PART) require most public agencies, including research organizations, to develop and implement performance measures and evaluate the quality, relevance and outcomes of R&D that they fund. Measurement always perturbs a system, and research is no different. That you get what you measure is true. Since it is easier to measure tangibles, current assessment tends to be on discrete results such as papers, awards, and citations. Ideally there would be more general variables that described the progress of science toward ten- and fifteen-year goals. These general variables would be related to the dimensions of a diverse set of outcomes, and the process of measurement would also differ. Geisler (2000) concludes that current metrics miss the temporal dimension between scientific outputs and technological accomplishments, and that motivations for measurement differ. The short-term criteria for control are often in conflict with the long-term criteria for judging value and strategy realization.

Choosing strategic and structural dimension

What theoretical characteristics might one desire in a framework of research project diversity? Ideally, a framework should encompass dimensions that tap into the fundamental dilemmas, tensions and problems of conducting research. Since our whole interest is in making meaningful distinctions, we want to isolate multiple dimensions of both strategy and structure. These dimensions should be most appropriate for describing differences in research outcomes and tasks or activities. Furthermore, a theoretical concern is to connect these choices as much as possible with the existing literature.

To arrive at the dimensions for strategy and structure, we conducted an inductively-based exploratory study to determine the critical factors facing the research worker and manager. This is what Quinn & Rohrbaugh did when developing the Competing Values Framework, though they focused on the "cognitive structure of the organizational theorist" (1983: 365). We sought to gain a good understanding of the cognitive structure of the participants in the research process. As a result of the exploratory focus groups, representing a diversity of project types, we identified aspects of organizational structure and management practices specific to research projects, which we then used to design a survey instrument for assessing an research organizational environment (Jordan forthcoming; Jordan *et al*. 2003). The findings of the exploratory study and of subsequent surveys suggest that the diversity of research organizations can be sorted according to two primary dimensions for strategy and two for structure, which in turn are highly related to two other structural dimensions.

For scientific research, the task environment is the knowledge world or the 'state of the art' - that is, how much is known in a problem area, and what is considered to be an important scientific concern or requirement. The first strategic choice reflects to what extent, given the concern or requirement, an advance will be attempted, or, if measured after the fact, has occurred in this state of the art. This strategic choice connects to the research on innovation (Hage 1999), where the distinction is frequently made between incremental or evolutionary vs. radical or revolutionary advances. To this we would add the more recent developments in the suggestion of a hypercube of innovation (Afuah & Bahram 1995) regarding the existence of modular and architectural innovations, and how radical the change is for various parties involved (the innovator, the user, those supporting innovators and suppliers to the innovator.)

We define as 'revolutionary' or 'radical' research that is fundamentally new, or that makes a significant advance in the science involved. 'Significance,' in addition to the amount of change from the current state of the art, includes the centrality of the advance to the field or problem area, and the difficulty of achieving that advance, which is related to the number of the unknowns involved. Radicalness is a continuum; the distance of the advance compared to the state of the art would be determined by peers. The hypercube theory for new products reminds us that, for science to be successful, it must be absorbed and supported. If new science falls into a science trajectory and is relatively easily assimilated, this must be need-sustaining.and, though it could be radical, it is not as radical as it would be if it created a new need. Explicating modular and architectural scientific advances could also be helpful in considering the influence of the existing technical environment on the level of advance in a particular area.

Not all radical advances are seen as that at the time, and some radical advances happen without planning for them. Managers can and do, however, set out to make radical advances, and want to understand how to improve the chances that one will occur within some pre-determined time frame. But because strategies or intentions represent aspiration levels, errors are possible on both ends of the evolutionary vs. revolutionary continuum: not setting the bar high enough, and setting the bar too high. The organization must decide whether to pursue new science and new areas of application, or exploit those that exist. The differences are large in terms of risk, size and timing of the pay-off from the research. This is one of two dilemmas related to strategy and the intended outcomes of the research.

A second dimension of research strategy, which also gives rise to a dilemma, is the scale, in terms of breadth, depth and reach, of the outcomes of the research. For this dimension, we have to consider the scale of outcome in relation to some pre-determined period of time, because the scale of impact may not be recognized as large for decades. In addition to being of obvious interest to stakeholders, scale of impact is important because it affects decisions about the resources required to undertake the research successfully. For science, we suggest there are at least four characteristics that determine the scale of the research undertaking and outcome:

- one is the number of variables and iterations of experiments involved, related to knowledge outputs and to requirements for people hours or equipment to collect and process test data;

- a second is the extent of coverage of conditions for these variables, such as temperature range or geographic area;

- a third is the extent to which the conditions examined are extreme - conditions usually requiring special instruments or physical structures, or locations that are expensive - and the outcomes therefore of great interest;

- a fourth is the number of disciplines, fields, or problem areas that are affected by the advance.

Scale is a dimension commonly used in science. A 1994 National Academies of Science report states that large projects are required for problems that can be pursued only by using large, complex facilities and platforms, extensive campaigns, or multipoint observations (NAS, 1994): in all of these areas, there must be one project that is responsible for managing the combined activities. Scale is dependent in part on the cognitive definition of the scope of the scientific research: many sciences - for example, the earth sciences - are inherently systemic and, in these instances, studying a component or part of the problem is not easily accomplished in isolation, and can indeed give false information. Also, more scientific problems are perceived to be complicated both because of external influences that must be taken into account, and internal processes that must be modeled more or less well to improve the quality of the prediction (Boesman 1997; Kodama 1992; Miller & Morris 1999). Almost by definition, new understanding of a system and its multiple relationships affects a wide array of subsystems and components. The relative scale of the research outcomes thus leads to another strategic choice: the dilemma of choosing a scale for a research project that is considered value for money and one that covers enough of a problem area to give correct answers. If large problems can be divided into components, or if there are avenues for accessing and feeding a larger system, project scale may remain smaller.

External Influences on Intended Outcomes

Although we have employed the word ‘choice’ about an organization’s research strategy, there still remains the question of how freely managers can select particular strategies. The choices may be dictated to them by the environment, such as agendas of funding agencies, or control exerted by the state, or certain crises that demand attention (DiMaggio & Powell 1983). Choices are also bounded by resource availability and the state of the art in the area of science.

For example, to recognize, absorb and take advantage of new knowledge and technologies generated elsewhere, there may be a need for investment in incremental work to build a knowledge and competency base. This has happened in parts of the world in the field of biotechnology. Projected shortages of physicists or engineers, or building a capability in biomaterials are other examples of agenda setting. Preferences and funding for research areas change as social values, demographics, and pressing problems change: thus we now notice increased funds for cancer research, geriatrics research, and science seeking to understand how to safeguard the stockpile of nuclear weapons using simulation techniques. Environmental concerns have pressured both public research laboratories and private companies to focus on such radical research agendas as hydrogen energy, the non-polluting car, and the ‘green warship.’ Pressing needs for immediate improvements in national security after the events of 9/11 might mean greater emphasis on incremental advances to ensure the quick transfer of current technology into security applications.   Furthermore, the culture surrounding the peer panel review process creates a bias toward what is often termed ‘normal science,’ that is, toward incremental advances in knowledge (Braun 1998).

There are also considerations related to the science itself and its related market. There is a discussion of markets in the chapter by Luke Georghiou, but not much has been written about this elsewhere that we are aware of. An area of research is needed is to see if there are parallels in science with the discussions of strategy in the literature on technology and product development. Balachandra & Friar, looking at project selection for innovation, suggest that three key areas of context (contingencies) need to be examined, and that they will have different relative importance. They are:

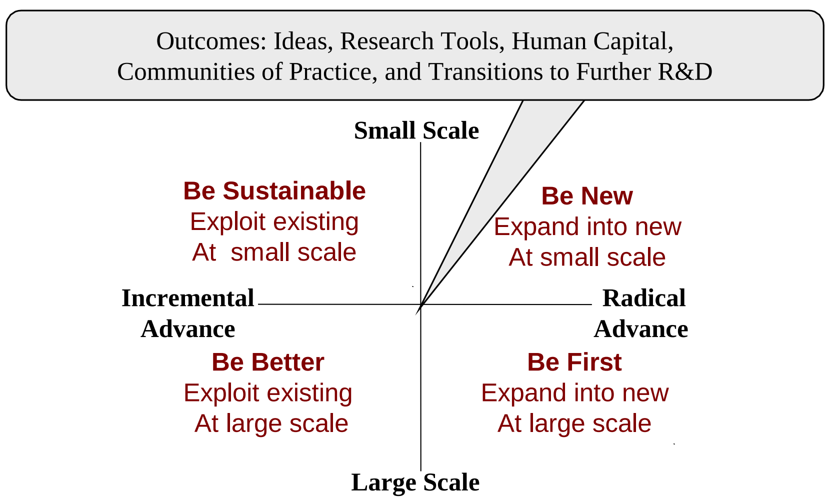
- type of innovation (radical vs. incremental) which the Research Profiles Framework covers;

- technology (high to low);

- market (new or existing).

Pathways in science, in contrast to technology and product development, have not been studied, apart from speaking of emerging fields and mature science.

Figure 1.
Research Profiles: Strategic Dilemmas & Outcomes In Scientific Research



Characterizing four distinctive research strategy profiles

Four distinctive kinds of strategic focuses are generated by the intended outcomes of the relative emphasis on the revolutionary or radical aspects of the scientific discovery, and the scale of the advance from the research project. They can be labeled with terms used in the Competing Values Framework (DeGraff & Lawrence 2002):

- *Be New*, which is the combination of radical and small scale;
- *Be First*, which is the combination of radical and large scale;
- *Be Better*, which is the combination of incremental and large scale; and
- *Be Sustainable*, which is the combination of incremental and small scale (see Figure One).

In the technology realm, these four profiles are easy to see as the phases of technology development and maturation. The *Be New* profile is the creative period during which many revolutionary and perhaps radical new ideas are generated and tested by individuals or small groups. After proof of concept, there may be a decision to move forward with large-scale development efforts in order to *Be First* in producing a new product. Once the product is commercialized, there is a phase where the strategy is to *Be Better*, and to improve or differentiate the product and the way in which it is produced through incremental R&D, often on a large scale. To *Be Sustainable*, a mature product may require some incremental R&D. This profile can also be seen as the beginning of new product development, where mastering the current knowledge and capabilities in an existing area of research is a precursor to exploratory research on related new product concepts.

The dynamics of moving across four outcome profiles for science are not quite so familiar, but that is the subject of this paper. We will provide examples in each of the four areas of outcomes: ideas, research tools, human capital and communities of practice. There are exceptions to these profiles, of course, and projects may not fall into just one profile.

Be New: The combination of small scale with revolutionary discovery is typically found in the development of a new concept, or theory, or field of research, as in the first research on the genetic model (Kohier 1979) or on DNA (Watson & Crick 1953). Organizations rely on this type of small scale research to provide new ideas that may proceed to research at a larger scale of data collection or application development. The discovery could be a new research instrument or technique, such as a micro-fluidic drop ejector for very small platforms. An individual or small group becomes knowledgeable in this new concept or tool, and as others begin to work in the area, a new community of practice would emerge. People would perhaps refer to ‘an emerging field.’

Be First: The combination of larger scale with revolutionary or radical advances is ‘big science’ that attempts to do something never done before. The project goal might be to be first with a whole new system, such as the research to map the human genome, or to build a new scientific instrument that will allow otherwise impossible research to be done. Fusion energy research is another example, as is the physics of space. In the latter two cases, tests are done under extreme conditions and, because of the difficulty of the questions addressed, all three cases require expensive measurement instruments or sustained funding over many years. In technology, the development of the Apollo rocket is an example from the past; a present one is the work on the international space station. People must capture the new knowledge and learn how to build and operate new instruments. Communities of practice must produce and absorb this knowledge. In the case of large expensive projects, key stakeholders must also know enough about the science and its promise to agree to the investment.

Be Better: In contrast, the combination of less radical and large scale can be an improvement in a more mature field or problem area. An example is the systematic collection of data from multiple points to study aspects of the environment such as air quality. Another is research in materials and improving simulation methods in order to improve the science behind stockpiling atomic weapons, so that these arsenals are safeguarded against the ravages of time and other dangers. Likewise, NOAA is concerned with improvements in the amount of time for warnings of impending violent weather. In the social sciences, examples are the slow improvements in the data collection methods associated with national surveys, such as the Census, or the General Social Survey. In the area of research tools, the operation and upgrading of large scientific user facilities fits this profile. Large-scale efforts to build human capital in a discipline, such as support of physics or mathematics graduate students, is also a possible intended outcome, as is the informal network these graduates form when they enter the workforce.

Be Sustainable: The last combination - less radical research with small scale - involves sustaining or extending existing science concepts, tools, human capital or networks. A general example would include research to extend a concept to an application not far removed, a specific one to modify a vacuum chamber for a different set of experiments. Many of the research projects that are funded by the NIH or the NSF, while doing excellent and challenging research, fit the general example because they are not aimed at radical advances: a second specific example might be attempts to improve the capability of weather detection systems, such as cloud sensors; in terms of human capital goals, coursework to keep technical staff current in their fields or train university students in research fits this profile as does investment to bring different communities of practice together to determine new directions or build excitement around an existing problem area is another.

Characterizing four distinctive research structures

As we will discuss in more detail in this section, the strategic dimensions of radicalness and scale suggest structural dimensions and tensions that are important to research. Radicalness is related to complexity and to organizational autonomy: the more radical the aim of the research, the more complex the problem and the team, including external parties, needed to address that aim successfully. Scale is related to the resources and the amount of autonomy required for the research. Given the axiom of contingency theory that structure should follow strategy, research outcomes will be determined in part by how managers handle these structural tensions.

First, we will examine the dimensions of project size and research autonomy. To borrow from contingency theory: from the dimension of scale flows the obvious dimension of size of the project; from size flow a number of consequences that create tensions about coordination and control mechanisms; these inevitably impact project autonomy. We define autonomy as the extent to which research direction and technical decisions are made by researchers, rather than coordinated for them. As we discussed earlier, project size is often a reflection of the scale of the intended outcome. Research project size includes monetary costs, the number of researchers, and the amount and variety of equipment involved. Scale is relative, of course, and what is large in one sub- field or group of organizations may not be large in another. In dollar terms, three typical project sizes are annual budgets of under $1 million, between $1 and $10 million, and over $10 million. Obviously, the range of over $10 million is enormous, and can reach into the billions when it involves a new energy system, such as one based on hydrogen. Some would classify the cost of a project as an indicator of the radicalness of the innovation (McDermott & O'Connor 2002); but because some projects of this nature are of low cost and others high, we would argue that this should be kept quite separate from measures of revolutionary breakthroughs in science or in technology.

Research projects of a large size and scale require a significant investment in management control and coordination, though this would probably still be less control than would be true for non-technical, repetitive work. Large projects typically need a substantial support staff and systems for required services: accounting, human resources, and libraries. Larger teams need leaders who can allocate resources and maintain communication and focus among team members. Management also helps to define and communicate clear goals and strategies, and align groups with them. Indeed, the success of a research project often depends on management correctly positioning the research to fulfill a need or fill a niche. In this manner, it is essential for managers to be technically competent and able to orchestrate new ideas through the organization. Overall, large projects have a unifying planning process that makes it possible to set specific scientific goals and to track progress against those. Organizational structures required for large projects are similar to the mechanical (as opposed to organic), bureaucratic structures found in the chemical and electrical product research laboratories in terms of hierarchy, coordination and control, rewards for administrative ability, and interdependence among organizational subunits. However too much coordination can stifle creativity. Shenhar (2001) provides a vivid description of how managers in this profile attempt to bureaucratize the coordination of the disparate parts of the research.

Research projects of small size do not require a great deal of management control, oversight, or support. The researchers themselves largely determine the research direction of these small projects, and individuals have a great deal of autonomy to make technical decisions. The independence and autonomy of academics are well known, as is the fact that researchers are motivated as much by recognition of their work and the intrinsic pleasure of directing and doing their research as they are by extrinsic rewards. However, there is evidence that researchers do best with some pressures to deliver (Pelz & Andrews 1976). It is possible that, left alone, the research will not continue to be cutting edge. When it is also important to have a critical mass of projects in an area, and to have research that is related to the larger organization's mission, a manager also worries about how much autonomy to allow small projects in setting the research direction. Another tension is being able to meet corporate requirements such as safety, security and accountability for these small projects with minimum burden on researchers' time. At the level of the larger organization, there can be many of these small, often one-of-a-kind projects. Managers cannot look across these disparate projects easily; thus, the organizational system requires agility and flexibility.

The other dimension of structure, namely the complexity of research as represented by the variety of scientific and engineering disciplines involved, is essentially a measure of the amount of expertise required to accomplish the research project. This could be the expertise and perspectives of an individual as well as a group. At one end of the spectrum are ongoing 'specialized'teams with the specific expertise known to be required to complete the task. At the other end is highly complex and diverse research teams. Like size and research autonomy, complexity is equally well established in the literature - in this case, the literature on innovation (Hage, 1965 and 1980), and technology (Perrow, 1967; Shenhar, 2001), and the literature on creativity (DeGraff & Lawrence 2002) and science more generally (Hagstrom 1965). The dimension of complexity, which is multi-faceted, has also emerged in the new project management literature (Kim & Wilemon 2003). Indeed, this is one of the single most important findings in the innovation literature (Damanpour 1991; Hage 1999). Innovation is more likely to occur when there is a complex division of labor and knowledge or experience and mind sets, if the knowledge sets are integrated,.

There are areas where the notion of complexity appears: the idea of Pasteur's quadrant (Stokes 1995) is bringing together the perspectives of advances of knowledge in both science and an application area; we often hear that discovery occurs on the margins of disciplines. Recognizing the absence of the studies of complexity relative to scientific research, and funded by several countries including NSF in the United States, Hollingsworth and Hage launched a large-scale historical study of radical innovation in biomedicine. Preliminary results indicate that, at the level of the research project, the relationship between complexity and radical innovation holds without qualification. Studies of multiple institutions found that when diverse groups of researchers were integrated, they were more likely to make significant contributions, as measured by winning Nobel and other prizes (Hollingsworth 2002). For example, groups often included both a biologist and a physician. Diversity can also be the mix of researchers and technologist. Our research at Sandia National Laboratories has found that many research projects have representation from six or more departments. One of the more interesting aspects of these complex research projects is the nature of the equipment that must be used to develop new ideas in science. Because typically this equipment did not previously exist, or could not be purchased, the research unit had to build these measurement instruments themselves to demonstrate the correctness of some new concept or idea. This requires another dimension of knowledge, namely the presence of technical specialties that have the capability for pursuing the development of new instrumentation.

This structural dimension also includes whether this expertise is within the project's own organization, or whether organizations external to the project's are involved. Thus an external focus within the research unit becomes a second aspect of complexity. This notion is the basis for the growing literature on inter-organizational networks, but it is not part of existing contingency theory (Lawrence & Lorsch 1967, Cameron & Quinn 1999). Complexity is essentially a measure of the knowledge pool of the research project and, even in large organizations such as the national laboratories or mission agencies, it is possible that not all of the necessary skills and attributes are to be found in the same research unit or organization. Therefore, as the complexity of research grows, there is usually an increasing need to search for expertise or information outside of the organization. This need was one reason behind the emergence of the inter-organizational network literature (Alter & Hage 1993; Dussauge & Garrette 1999; Doz & Hamel 1998; Hagedoorn 1993; Harbison & Pekar 1998; Häkansson 1990; Inkpen & Dinur 1998; Jarillo 1993; Kogut, Shan & Walker 1993; Lundvall 1993; O'Doherty 1995; Powell 1998; Van de Ven & Polley 1992).

We note that there is a certain irony in this. Those projects that are already more complex because of their revolutionary strategy are precisely the ones that are most likely to recognize the need for other pools of knowledge. As Zammuto & O'Connor (1992) argued in explaining the adoption of the radical process technology of flexible manufacturing, this flows from their aspirations, and thus makes the strategic choice so critical. Meeus & Faber (forthcoming) observe this to be true on the inter-organizational side of the structure as well.

Managers face tensions related to complexity and organizational autonomy. As Nooteboom (1999) has observed, radical innovation is created by increasing cognitive distance; but as it increases, the tendency is for people to communicate less. The managerial problem is to develop a number of mechanisms to encourage the sharing of tacit knowledge (Nonaka & Takeuchi, 1996). We would suggest that, within the context of science and smaller research projects that address only a component of a larger system or area, this requires more than the classical mechanisms suggested by Lawrence & Losrch (1967). Similarly, interactions that bring in resources beyond those available to the project, often from outside the research unit/organization, have both benefits and costs. The first and most obvious cost is the time and effort involved in the knowledge search process. Where is the needed expertise located? The second is the loss of project or organizational autonomy when bringing in external parties. Whether they bring funds, people, equipment, or ideas to the table, they will want a share in the decision making and the outcomes of the research.

Figure 2.
Research Profiles: Structural Tensions in Scientific Research

Small Size Projects
Research Autonomy

Small, autonomous projects
Specialized Research
Organizational autonomy

Small, autonomous projects
Complex Research
Inter organizational ties

Specialized Units
Organizational Autonomy

Complex Units
Inter organizational Ties

Large, coordinated projects
Specialized Research
Organizational autonomy

Large, coordinated projects
Complex Research
Inter organizational ties

Large Size Projects
Research Coordination

The two dimensions of size/research autonomy and complexity/external ties (see Figure 2) yield four distinctive structural profiles of research projects.

- *Be New*: small, autonomous, complex research projects with inter-organizational ties.

- *Be First*: large, coordinated, complex research projects with inter-organizational ties.

- *Be Better*: large, coordinated, specialized research projects with organizational autonomy.

- *Be Sustainable*: small, autonomous, specialized research projects with organizational autonomy.

Managerial challenges, strategy and structure

The central idea in contingency theory is that structure must follow from strategy. Thus the two basic dimensions of strategy dictate how the research tasks should be structured: if the combinations described above are not selected, then there is a misfit, which will be followed by related poor performance in papers, patents, citations and other measures of scientific progress.

Before introducing more specific management challenges related to the four profiles, here is a summary of the strategic dilemmas and structural tensions that managers face.

- One strategic dilemma is the setting of the aspiration level: not too low to achieve radical advance, and not too high for existing capabilities and opportunities in an area.

- A second strategic dilemma is the choice of a scale of research: too small or too large for the number of variables, systemic relationships, situations covered, and areas or field affected.

- A structural tension that follows from strategy is the complexity of the project: the more that managers choose to pursue a radical breakthrough in science, the more diverse the perspectives and knowledge sets needing to be integrated in the research unit.

- A structural tension follows from the degree of complexity: coordination tension in the complex research projects, because of collaboration with external organizations, where more specialized research projects can maintain organizational autonomy.

- Another structural tension that follows from strategy is the size of the project in terms of funds, people, and tools. As the scale of the research broadens, the resources needed grow.

- A structural tension related to the dimension of project size is how much autonomy can be allowed, with more coordination necessary for large projects than for small.

These strategic dilemmas describe the characteristics of intended science advances and the related structural tensions that help determine if the project will reach its intended outcomes; in addition, this DOE study has identified twelve areas mentioned by researchers and their managers as necessary for them to do excellent research (Figure 3) (Jordan *et al*. 2003a, 2003b, 2003c). By ‘excellent research’ they meant research that is innovative, stands the test of time, and transitions to further research or helps solve a real-world problem. All of these aspects of the research environment are important, but some are more important for some research profiles than others (Jordan *et al*. 2004). For example, the uncertain nature of research means that time to think and explore is required but, if the aim of the research is a radical breakthrough, more time for exploration is needed.

The twelve areas of key management challenges that also determine advances in basic and applied research are as follows:

Aspects more important for research that strives for radical advances:

1) Encourage exploration and risk taking: includes ensuring that researchers have time to think and explore, resources and freedom to pursue new ideas, and a systematic way (even if that is researcher action) of identifying new projects, partnerships and opportunities.

2) Integrate Ideas Internally and Externally: includes internal cross-fertilization of technical ideas, external collaborations and interactions, and an integrating and relevant research portfolio.

3) Build Strategic Relationships: includes maintaining good relationships with research sponsors, building and maintaining a reputation for excellence in research, and having a relationship with the larger organization such that senior managers are champions for basic and applied research.

4) Clear project goals and strategies: includes defining and communicating a clear research vision and strategy (often more specific for larger projects), having sufficient and stable research funding, and investing in future, as well as current, capabilities.

Aspects more important for research that strives for less radical advances:

5) Build Teams and Teamwork: includes having a climate of cooperation rather than competition within the project and organization, good internal project communication, and managers who add value to projects.

6) Commitment to employee growth: includes a good process for technical career advancement, good educational and professional development opportunities, and high- quality staff internal to the project's organization.

7) Excellent capital and knowledge resources: includes in-house, near-state-of-the-art research equipment and a good physical work environment, good research competencies, and resources to pay competitive salaries and benefits.

8) Good technical management: includes managers who are both technically informed and decisive, a process for giving rewards and recognition that researchers respond to, and allocation of internal research funds perceived by researchers to be reasonable and fair.

Aspects more important for small scale projects:

9) Encourage change at the project level: includes autonomy in decision making about research, a commitment to critical thinking about research direction and progress, and maintaining a sense of challenge and enthusiasm.

10) Value the individual: includes the optimal use of each person's skills, having respect for people, and an atmosphere of trust and integrity.

Aspects more important for larger-scale projects:

11) Plan and execute projects well: includes, in addition to planning and executing well, having measures of technical progress appropriate for the project, so that performance is driven the right way, and organization-wide measures of progress that are appropriate for the project's profile.

12) Efficient, low-burden internal support systems: includes user-friendly laboratory services such as library, good laboratory systems such as accounting and security at the lowest possible cost in dollars and time, and overall overhead rates low enough for research projects to be cost-competitive.

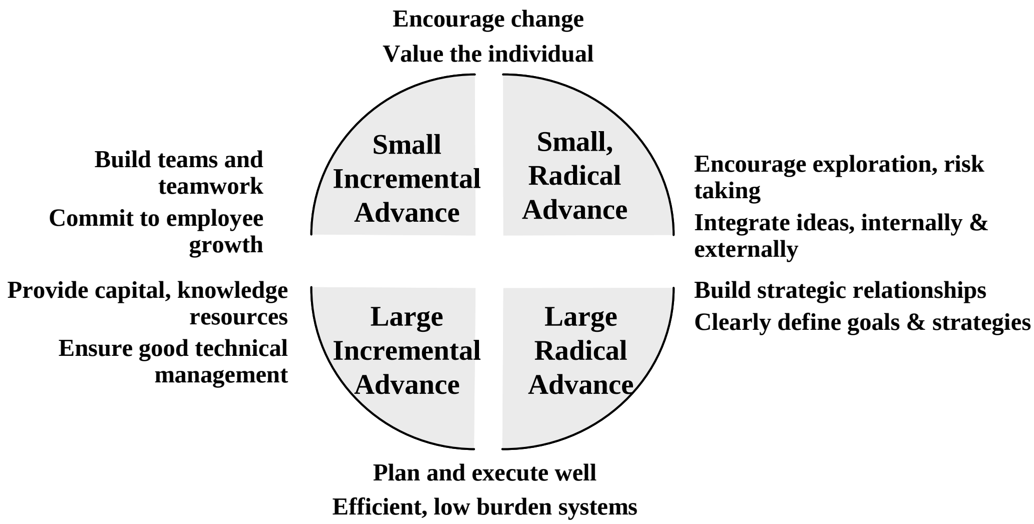
For each of these twelve areas, the DOE study has a list of sub-areas of researcher concerns. Questions in the DOE Research Environment Survey (Jordan *et al*. 2003a) probe for, given the characteristics of their work, the percentage of time these aspects are true in the researchers' work environment. Continuing research will inquire about the specific mechanisms and skills that managers can apply in each of these areas. The expectation is that these will vary according to the profile of the research, and that managers will want to recognize the differences in the nature of the work, and adjust their management practices accordingly. Here are just a few examples.

One aspect of project planning and execution is having appropriate measures of project success. There are a number of challenges for managers seeking to recognize differences in how different kinds of research should be assessed (NAS 1999). The benefits of review and of strategic relationships are openness to external events and strong feedback loops that keep the research on the cutting edge or on target for satisfying a specific need. Some of the mechanisms for keeping abreast of the literature and in planning research - interlocking spheres of information through such means as external review, advisory committees, and the involvement of the science and technology community - can provide both the stability and the stimulation necessary for making science advances, particularly revolutionary advances. The expertise of people involved in the reviews, both formal and informal, will differ depending on the profile, as will the questions asked, the criteria for assessment, and data considered. Another question is how often projects should be reviewed. Overlapping oversight currently has some projects at some of the DOE national laboratories feeling that they are over-reviewed, taking time away from research without sufficient additional benefit to offset the costs.

Another challenge is to improve technical management within the research unit: it requires skill in administration and management of people as well as knowledge and skills in the research area; the actual expertise may lie in separate departments not under the control of the project manager; and finding technical managers for larger projects is likely to be particularly difficult. Indeed, it is this kind of profile that led to the creation of matrix authority structures and, as is well known, these have not always functioned well, because researchers find it difficult to serve two masters, discipline and project (Davis & Lawrence 1977).

Another managerial problem is having an optimal mix of staff on small projects. It could be that the research unit is so small that technologists and assistants are not part of the team, and the researcher has to do many tasks, such as setting up samples and filing reports, that do not make optimum use of his or her skills. A manager can alleviate some of this by providing shared technologist and administrative resources. Paradoxically, another problem of the small specialized research profile is that many of the researchers may have multiple projects - perhaps too many. As Pelz & Andrews (1976) demonstrated many years ago, a few were beneficial for creativity, but the benefits dropped rapidly with an increase in numbers.

Figure 3.
Research Profiles: Key Management Challenges



A final example is that of researchers in small projects who are typically given greater levels of autonomy. Management challenges here are the costs for the research organization of unrestricted research; if there is too much autonomy, researchers may not make any connection with others with whom they share similar interests. This is a problem in some American universities, where researchers in the same discipline, but located in different departments, do not interact. Such solitary tendencies of researchers in small, less radical research projects make encouraging teamwork a managerial problem. In the other three profiles, teamwork is more or less forced upon the researchers because of either the scale or the radicalness of the objective; but that is not to say that there are not problems with the integration of those teams.

Organizational implications of research profiles

We have described the Research Profiles Framework for projects but, as we have mentioned, there are obvious and important implications for organizations that are groups of projects, because these decisions are not made just at the project level. The outcomes of the research depend upon a match between strategy and structure. Three implications are discussed briefly here. How can an organization use the Research Profiles Framework to examine it portfolio of projects? How does an organization structure itself to manage groups of projects that have very different intended outcomes? Finally, what are the implications for measuring the performance of basic and applied research?

An advantage of the Research Profiles Framework is that it can make explicit the composition of a portfolio of projects along multiple dimensions, and show this diversity in one picture that stimulates discussion of trade offs among small and large-scale projects, and radical and more specialized research projects, within a particular area of research. Given the current state of the art in an area, the resources available, the importance of moving forward in this area, and the scientific and technical opportunities to do so, what should the current mix be? Are there enough projects aimed at radical breakthroughs? Is now the time for large data collection efforts that require large projects? Are projects transitioning from one profile to another, such as from extending an internal knowledge and skill base to then move forward to working on cutting edge research in that area? A portfolio can get out of balance. For example, a recent National Academy of Sciences study of the US space physics program concluded that, while big science was uniquely suited for accomplishing some scientific goals, " these projects have also been accompanied by implementation delays…and the sapping of the base-funded programs" (NAS 1994). The recommendation was that future portfolio assessment should specifically address the balance between large and small projects.

What are the implications for the structure of organizations? Given these alternative research profiles, the Research Profiles Framework suggests that, in addition to grouping disciplines and specialties by their content and by the nature of the problem that they are working on, another basic organizing distinction within a research organization be made on the basis of the research profile. For example, within a problem area such as Pulsed Power Sciences, all of the research projects associated with the *Be New* profile could be in the same group, with a manager trained in how to manage that profile. There would be a different manager for those responsible for the day-to-day operation of the experimental facility. The profile managers and the Pulsed Power Sciences manager would meet together to look across the profiles at the performance as a whole. The evident differences in the nature of the work, in the structural tensions, and the managerial challenges, mean that the managerial skills needed in each profile also quite different: so different structures and management practices would be in place in the different groups. At a minimum, managers could be made sensitive through training to the differences in the profiles.

Just as Ciba Vision and other "ambidextrous" organizations have recognized the need for selecting different managers for different kinds of business activities (O'Reilly & Tushman, 2004), the same principle can be applied to these four kinds of research profiles. In effect, we have expanded existing contingency theory beyond the distinction between organic and mechanical to include the dimension of complexity and inter- organizational ties. In effect, the Research Profiles Framework approach is a return to the basic insight of Chandler (1962) in his famous work on the relationship between strategy and structure. In his study of Dupont, he observed that such aspects as the technology of production and the marketing varied by product line. Basically, the recognition of distinctive research profiles is making the same argument, except that we are suggesting that it is the differences in the strategic choices of evolutionary versus revolutionary and small scale versus large scale that entail different structuring of the research projects, as well as distinctive managerial problems.

Turning to research assessment, the worldwide movement toward performance- based management of science and technology programs aggravates the problems of not recognizing diversity and the part that each research profile plays in the larger scheme of advancing scientific and technological knowledge. It can lead to the undervaluing of incremental innovation, setting up reward systems that reward occasional, serendipitous accomplishments but which, at the same time, eliminate research that might provide radical advances because someone insists that there be measures of tangible outcomes now. The Research Profiles Framework can help set performance expectations for the different profiles. The dimensions also suggest general variables to measure – the radicalness and scale, as determined by peers, of the research progress and outcomes. Within a problem domain, there are continuous variables related to the domain and the knowledge; they can be assessed on the dimensions of the research profiles, in addition to the traditional measures of papers, awards, citations, discrete milestones met, and single-word ratings, such as ‘outstanding,’ from peer reviewers.

Such measures as a per cent increase in the accuracy of prediction of ozone levels, the reliability of estimates, the speed with which a measurement is achieved, or how important a science advance about ozone is to geophysical science are all examples of these continuous measures. Real progress in ozone research, for example, could then be measured by the per cent gain in each of these various attributes, rather than just a single milestone, such as development and implementation of a new algorithm. The basic argument is that the use of general measures of scientific or technical progress or, at a minimum, estimates of the amount of technical progress likely with certain kinds of research (for example, radical versus incremental), makes it possible to better determine the critical pathways of progress, both within and across research projects, as well as larger research units.

Conclusions

We have argued that, in the study of innovation, learning and macro-institutional change, the study of the management of basic and applied science projects is too important a topic to be ignored. Basic and applied research is important to the success of the economy and the strengthening of national security. However, there is increasing diversity in research that is not captured in current theory on how it should be managed to achieve scientific advances. In addition to radical and incremental research outcomes, there are other types of innovation, such as architectural and modular. The view of the production of research has changed from the linear model to the chain-link or idea-innovation-chain model. Because collaboration is the norm, the old distinctions between research done in industry, government and academia are blurred. With increasing knowledge specialization, a major question is how to access and share tacit knowledge across the specialties. A theory that describes this diversity is needed. Such a theory can build on contingency theory, which suggests that different organizations can have different strategies and structures, and that their performance will be best if strategy and structure are aligned.

The Research Profiles Framework proposed here is a first step in developing a theory of diversity in the management of research. The Research Profiles Framework extends the Burns and Stalker organic/mechanistic model, with its tensions between flexibility and control, to include the structural tensions between having inter-organizational ties or organizational autonomy. The framework links tensions in the nature and structure of the research work to tensions in two other arenas: the strategies of desired outcomes and specific management challenges. Dimensions of nature of task and structure are small- size, autonomous vs. large-size, coordinated projects, and complex projects with inter-organizational ties vs. specialized projects with organizational autonomy. These dimensions are linked to dimensions of hoped for outcomes in some specified period of time, which are small vs. large-scale of impact, and the radicalness of the outcome in terms of the degree to which it advances the state of the art and is central to the area of research. As has been indicated in our discussion of each of the four profiles, in addition to whether or not the organizational structure matches the research strategy, specific management styles will also determine whether or not the desired scientific and organizational learning and advances - be they finding new fields or exploiting existing competencies - actually take place. In each instance, different obstacles to advances present themselves. When the objective is radical innovation, then usually the problem is how to integrate diverse research teams (and consider impact with up- and downstream actors). As more and more of the research occurs in an inter-organizational setting, this presents an obstacle to how much advance occurs. In contrast, when the objective is incremental innovation, the problem is frequently low levels of intra-organizational integration and aspiration, especially in small research projects where the researcher has considerable autonomy.

The four profiles resulting from the Research Profiles Framework not only delineate these issues effectively but also provide some interesting insights into how to arrange research projects, how to assess their progress, and how to analyze a portfolio of projects within a research organization. In addition to the usual criteria of scientific and engineering specialty and program focus, we suggest that projects in larger research organizations can be grouped on the basis of their research profile. Once divisional structures are established, managers can specialize in the kind of profile for which they have expertise in handling the dilemmas, tensions, and key management challenges. Furthermore, with this organizing device, research organizations can assess progress along general variables that are the profile dimensions and, within a given area of research or research problem, assess their balance across profiles. Thus, we suggest that the Research Profiles Framework has the potential to provide solutions to a number of basic issues in the management of basic and applied research, while recognizing fundamental managerial challenges as well.

Throughout, we have suggested a number of areas where further research can be done. The DOE Study agenda is to build a store of knowledge from collected data and observations at the project and laboratory levels and, from this, to validate the Research Profiles Framework, define new measures of progress and value, and elucidate links between particular management actions achievement of desired outomes within the planning horizon, and characteristics of the research profile. There are obvious linkages with the study of organizational learning, organizational change and inter-organizational networks. This is an exciting area of research, and one that is increasingly recognized as important to the study of innovation.

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