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Strategic management of government-sponsored R&D portfolios†

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Abstract. Although strategic management of R&D portfolios is common practice in private sector R&D, government R&D management tends to be more discrete and ad hoc, focusing on generating maximum output from individual projects. Often, there is no clear notion of the desired public sector output. Whereas private sector R&D evaluation is generally straightforward, with the function of R&D being measured in terms of a company's internal return on investment, the benefits of public-sponsored R&D tend to be more diffuse with respect to both type and impact. Drawing from evidence developed in twenty-four case studies of R&D projects sponsored by the Department of Energy's Office of Basic Energy Sciences, one of the primary sources of funds for basic research in US universities and national laboratories, we examine implications of R&D project impacts for a sort of 'portfolio management' of government-sponsored R&D. Although the meaning of 'portfolio' is not the same in public sector R&D management, it is nonetheless possible to think strategically about projects and their cumulative impact. Indeed, it is clear that many government R&D managers already do so. In this study, we contrast two types of 'portfolio': (1) R&D output portfolios (focusing on one type of scientific output, such as, for example, fundamental knowledge or technology development); and (2) a balanced portfolio that considers both R&D outputs and 'scientific and technical human capital', the capacity created by R&D projects. The case-study evidence shows different approaches to achieving each portfolio type. An analysis of the relation of project attributes to output types focuses on aspects of projects potentially under the control of strategic public managers, including magnitude of funding, degree and type of management oversight, and interorganizational and intraorganizational linkages. Each of these variables affects the type of project outputs obtained. From the results, we suggest that a balanced approach to government R&D portfolio management is appropriate for many government agencies. That is, government managers may wish to consider the extent to which their projects both produce traditional outputs such as articles and patents, as well as provide contributions to scientific and technical human capital.

1 Introduction

In the United States, government managers distribute more than $40 billion in research and development (R&D) funds each year. Each of the chief funding agencies, including the National Institutes of Health, the Advanced Research Projects Administration, the Office of Naval Research, the National Science Foundation, and the Office of Science in the Department of Energy (DOE), has developed its own procedures and norms for the planning, selection, and evaluation of research. In some agencies, government program managers exercise a good deal of autonomy in the selection of projects to fund, in other agencies, program managers have limited discretion and implement judgments made by peer-review panels or boards of advisors. Similarly, the funding agencies differ a great deal in the average amount of funding, the composition of funding recipients, the longevity of funded projects, and preferred reporting mechanisms. Arguably, the great diversity of government funding programs is a singular strength of the US national innovation system (Crow and Bozeman, 1998).

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1.1 Limits to strategic management of government research

Despite the magnitude of government funding decisions, there are no simple rules for public-sponsored R&D 'portfolio management'. Our objective is to describe and assess a sort of 'portfolio management' approach to strategic management of government R&D. Systematic and strategic R&D program management is difficult to achieve in the public sector. Even in agencies where program managers are relatively constrained, such discretion as exists is usually exercised with 'seat of the pants' decisionmaking, heuristics developed after years of experience, and organizational tradition. In the first place, government R&D management, unlike that in the private sector, usually seeks public-domain knowledge and technology products, and, thus, the analytical convenience of internal rate-of-return is not available to government managers. Whereas the proof of success in industrial R&D is usually a product or commercial process, most government-funded R&D does not have commercial products and processes as a near-term objective. Indeed, the most often cited rationale for public sponsorship of R&D is that there is no market for the information produced from the research [see Callon (1995) for a discussion of the relationship of public authority to economic authority in government research programs].

Another factor constraining strategic management of government R&D is that agencies are subject to external controls quite different from those emanating from the environment of a private firm. For example, agencies are subject to annual federal budget cycles, a severe limitation in R&D planning. Agencies that are line offices in departments, such as the Department of Defense or the DOE, must be attuned to the priorities of officials in the department hierarchy. These officials must, in turn, be constantly vigilant in responding to dictates of political superiors in Congress and the White House, and to political control executive agencies such as the Office of Management and Budget. Thus, government R&D managers are highly constrained. They seek to allocate R&D funds as effectively as possible, but have no clear-cut criteria by which to demonstrate effectiveness. What is lacking in effectiveness criteria is made up in competing demands. Allocation decisions sometimes need to take into account the diverse, often competing, priorities of bureaucratic superiors, federal budget controllers, political institutions, and researcher stakeholder groups who invariably have strong views about why their own research field is particularly deserving of increased resources. It is no wonder that 'strategic' is not often a term taken together with 'government R&D management'.

Another limiting factor on government adoption of private sector R&D portfolio management is the diversity, and even conflict, among the goals of the different government agencies. Efforts to develop hierarchical rationalization to agencies' goals, such as integrating program, division, agency, cabinet, and department strategic goals, have shown again and again the discontinuities among the various bureaucratic units of government. The current difficulties of implementing in research agencies the Government Performance and Results Act of 1993, an initiative requiring a hierarchical ordering of goals, has most recently shown the difficulties of transagency goalsetting (National Academy of Sciences, 1999; US General Accounting Office, 1997). We conclude, therefore, that strategic management of government R&D portfolios should focus on the program or agency level, the highest level at which one might expect a consistent goal structure.

A fourth factor is that government R&D managers have somewhat different time horizons than their private sector counterparts, and different factors governing their time horizons. On the one hand, one might well assume that the fact that government generally focuses on longer term R&D, especially basic research, would be conducive to a strategic focus. But the very open-ended nature of much government R&D makes any linkage to goals difficult. Moreover, the 'real' time horizon often is a matter of political dictates as much as the character and composition of R&D.

Despite the obvious difficulties of strategic management of government-sponsored R&D, many public managers nonetheless seek to manage R&D strategically, and often succeed. The approach is usually quite different from that taken in the private sector and often occurs despite organizational procedures rather than in conformance to them. But, in many cases, government R&D managers, although laboring under the constraints mentioned above, often think of their funding 'portfolios' in strategic terms (see Averch, 1985; Gover, 1997). However, any approach to such portfolio management should recognize the inherent differences in government R&D—public-domain nature, different time horizons, poorly integrated transagency goals, and external political constraints.

1.2 Data for analyzing R&D portfolios

In developing our portfolio approach we employ methods developed under the rubric 'research value mapping' (Bozeman and Klein, 1999; Kingsley and Bozeman, 1997; Kingsley et al, 1997), focusing on the need to combine qualitative and quantitative data. We draw from evidence developed in twenty-four[(1)] case studies of R&D projects sponsored by the DOE's Division of Basic Energy Sciences (BES). Using a mix of qualitative and quantitative approaches, we examine implications of R&D project impacts. Rather than provide a detailed framework for portfolio management, we analyze the results of the cases and discuss implications and possibilities for portfolio management of government-sponsored R&D. We show, consistent with the arguments above, that the impacts of the cases are highly diverse, and we seek to develop an approach that accommodates diversity of objective and R&D output.

2 A 'portfolio' approach to government R&D management and evaluation

In strategic management of industrial R&D (Bozeman et al, 1984), the concept of portfolio management often plays a central role. One definition of R&D portfolio management is: "a dynamic decision process, whereby a business' list of active new products and R&D projects is constantly updated and revised. In the process, new projects are evaluated, selected and prioritized; existing projects may be accelerated, killed or de-prioritized; and resources are allocated and reallocated to the active projects" (Cooper et al, 1997, page 16).

Calculating risk is one of the major factors in firms' R&D portfolio management. As Taggart and Blaxter (1992, page 241) note in their analysis of R&D portfolio management in the pharmaceutical industry:

> "The management of any research-intensive company must weigh carefully the expenditure on any particular development product with the sales revenues that are likely to be obtained when the new product is introduced to the market. In particular, the technical risks (whether the development product results in a marketable product) and the market risks (whether the new product can be introduced to a suitable market niche where it will generate the required revenues) have to be scrutinized."

The above definition of R&D portfolio management and the discussion of the role of risk highlight differences between the public and private sectors. Public-sponsored R&D rarely permits the type of vetting process and the flexibility implied in the definition of portfolio management. Public-sponsored R&D is usually performed under grants and contracts and so is not amenable to rapid-fire changes in the composition of

[(1)] Some analyses include twenty-eight cases. At the time this paper was completed not all cases had been fully developed.

the portfolio. Even more different is the nature of the 'product'. Companies calculate risk (as we see from the above passage) in connection with product development and, ultimately, sales revenues. Only a small fraction of government projects could, or should, be evaluated in this manner.

Cooper and colleagues (1997) surveyed thirty-five leading firms about their R&D portfolio approach and identified the major goals. These included 'value maximization', enhancing the value of the portfolio with respect to return-on-investment; 'balance', obtaining the best mix of low-risk and high-risk projects, and long-term and short-term projects; and 'strategic direction', the fit of the portfolio to business strategy in terms of market selection and niche. With the exception of the balance criteria, none of these objectives fits well (except perhaps by strained analogies) with government R&D missions.

In sum, traditional approaches (for example, Gear, 1974; Oehmke, 1990) to R&D portfolio management do not appear to be appropriate for government management (Vonortas and Hertzfeld, 1998). Given the more direct and limited approach to evaluating private-sector R&D—payoff in terms of a company's profit and growth—private sector portfolio management approaches are not directly adaptable to government-funded research. But something approaching portfolio development and evaluation is appropriate to government-funded research, even in cases of basic research intended to advance fundamental knowledge. Averch (1993) is among those who has made the comparison between government R&D management and private sector R&D management. Although he notes (page 264) that "any [government-based] free-standing information investor has severe handicaps in even knowing the value of the information it charters", he nonetheless presses a relevant portfolio approach. We quote him at some length because this is one of the few extant conceptualizations of *government* R&D portfolio management and evaluation. Averch first explains that the usual approach to management does not, in fact, resemble portfolio management:

> "Suppose that for m years some free standing information investor has been selecting or 'purchasing' a portfolio of research projects in some field or discipline. For example, consider the portfolio of solid state physics projects funded by NSF or the portfolio of EPA projects concerned with green house gases.... The information produced by the projects in any portfolio is supposed to benefit multiple users, not just the investor, at some future time $n > m$. At some time t between m and n, the investor wants an estimate of the expected quality and utilization at n of the information in the portfolio.
>
> ...Ordinarily [the agency managers and external program reviewers] make judgments of merit on an absolute basis without direct comparisons of the alternatives facing the investor.... Thus, in each year up to m, projects have not been explicitly chosen to achieve significant incremental gains in expected information content in the portfolio, *conditional on its current content*. Projects have been chosen on a one by one basis up to the point of resource exhaustion without reference to their incremental value to the portfolio" (page 265, emphasis in the original).

Then he explains (page 266) what portfolio management in government might entail:

> "Because the investor's *ex ante* selection of projects is not automatically geared to maximizing the expected informational contributions of project portfolios, *ex post*, the investor should assess the informational value of its entire portfolio in addition to evaluating individual projects. Not only do the individual projects in a portfolio have to provide results of high quality and relevance to the users, but also the portfolio itself should have the 'right' composition, and, for rapid scientific or technological progress, the projects in the portfolio should be connected in an 'optimal' way."

2.1 A 'constrained' portfolio approach

Our concept of government R&D portfolio management is in some ways similar to that of Averch (1993). However, we do not assume a necessary interconnectedness, or even the possibility of a 'right' or 'optimal' composition. Our approach is more limited and perhaps closer to the informal heuristics that government program managers actually use. We use the term 'constrained portfolio approach' to refer to government R&D management.

Government R&D program managers face a number of constraints that limit their ability to practice true portfolio management of the R&D they oversee. In the first place, government budget processes are quite different from those of the private sector in the sense that budgets are less fungible and there is only a limited ability to roll funds over. Second, government managers often not only have a greater autonomy, but also experience generally limited interconnection among programs. They tend to be individual 'portfolio managers'. (This is a constraint on the ability of the agency to take a portfolio approach, but it may enhance the individual's ability.) There are often multiple stakeholders to government R&D, some of whom may have conflicting objectives and preference functions. Most important, evaluating government R&D is generally more difficult because of the expected externalities and the multiple objectives. Because of all these factors, we suggest that a constrained approach to portfolio management and evaluation is all that is realistic.

Our constrained approach to government R&D portfolio management entails:

(1) a recognition that government-funded R&D has highly diverse outputs, and an interest in understanding the attributes of project and management likely to be conducive to various output types;

(2) a concern with some balance of output types and an awareness of the opportunity costs associated with pursuing particular portfolio mixes; and

(3) a dual investment orientation, not only aimed at understanding, at least in an approximate way, the costs of outputs, but also with an interest in longer term investments to help sustain the portfolio.

2.2 Contrasting government R&D portfolio approaches

We examine two distinct portfolio types: (1) output-maximization portfolio; (2) a balanced portfolio that considers both discrete outputs and scientific and technical human capital. Each of these is described below.

2.2.1 *Output-maximization portfolio*

In many instances, government R&D managers seek to maximize the output or impact of one or a few categories of scientific and technical knowledge. For example, several funding units in the National Science Foundation and the National Institutes of Health seek to support research that advances fundamental scientific knowledge without any consideration (at least in the short term) as to the applications that may flow from that knowledge. Even if there is some general expectation that the knowledge will ultimately lead to application, if the application is, essentially, a black box, then maximizing fundamental research is not about application. Other R&D managers in government have an explicit mission to support projects that lead to technology development and commercialization, and to economic development impacts. Thus, our earlier studies (Kingsley et al, 1996) of the New York State Research and Development Authority provide a nice contrast to our studies of basic research impacts (Rogers and Bozeman, 1997; Rogers and Bozeman, in press) because the managers in the organizations we examined are seeking to maximize quite different outputs and production functions. The more general point is that many government managers do see themselves (sometimes as a matter of perception, sometimes as a result of formal missions) as maximizing one or another type of output. One might argue that a portfolio approach necessarily entails mixing output types. We do not agree. The majority of private sector R&D portfolio managers focus on applied research or development, and seek to integrate and make efficient their applied research investments.

In section 4, we focus on three types of output-maximization portfolios: (1) basic research, (2) technology development and transfer, and (3) software and algorithms. In each case we begin with a mini case analysis of one or more of our projects that seems instructive for the output type. We then examine empirical results for all the projects, attempting to determine aspects of the projects relevant to a particular portfolio type.

2.2.2 *Balanced portfolio: discrete outputs and scientific and technical human-capital portfolio*

Many public managers are as concerned about building up scientific and technical capacity as they are about producing discrete impacts from particular projects. Some public managers, including those at BES, speak eloquently of their roles in nurturing science. This approach leads to different assumptions about program management and to a different portfolio approach. If one seeks to develop an R&D portfolio based on capacity building, then the 'production function' is improvements in scientific and technical human capital.

Scientific and technical (S&T) human capital includes not only the formal educational endowments usually encompassed in traditional human-capital concepts (for example, Becker, 1964), but also the skills, know-how, 'tacit knowledge', and experiential knowledge embodied in individual scientists (Bozeman et al, in press). S&T human capital is the sum total of scientific, technical, and social knowledge and skills embodied in a particular individual. It is the unique set of resources that the individual brings to his or her work and to collaborative efforts. As the production of scientific knowledge is by definition social, many of the skills are more social or political than cognitive. Thus, knowledge of how to manage a team of junior researchers, postdocs, and graduate students is part of S&T human capital. Knowledge of the expertise of other scientists (and their degree of willingness to share it) is part of S&T human capital.

Our (see Bozeman et al, in press) S&T human-capital evaluation model assumes that:

(1) science, technology, innovation, and the commercial and social value produced by these activities depend upon the conjoining of equipment, material resources (including funding), organizational and institutional arrangements for work, and the unique S&T human capital embodied in individuals;

(2) although the production function of groups is not purely an additive function of the S&T human capital and attendant nonunique elements (such as equipment), it closely resembles an additive function (the 'missing ingredient' in such aggregation is the salubriousness of the fit of the elements to the production objectives at hand); and

(3) most important, effectiveness is measured in terms of enhancement of the ability of R&D groups and collectives to produce knowledge. Thus, the object of evaluation is best viewed in terms of capacity, not discrete product.

The cases we describe in section 5 build on these assumptions and show how resources are brought together to increase capacity. The empirical analysis presented focuses on training of students and postdocs, increments to the skills of project researchers, and benefits of equipment use and other such tangible research capacity assets.

3 Research design and procedures: the research value mapping project

This study is part of an ongoing project aimed at developing new approaches for assessing the impacts of government-sponsored basic research projects. The 'research value mapping program' (RVM) entails implementation of large numbers of intensive case studies that are, in turn, used as points of departure for gathering quantitative data. In the current paper, we draw from two sources of data: detailed case studies of research projects funded by BES, and an Internet survey of those projects. The focus of the Internet survey is on the outputs of the project, both type of output (for example, patent or article) and substantive content.

The list of cases chosen was determined by a combination of three factors—interviews with DOE program managers about projects and their effects, a desire to have a representative base of projects [which produced a sampling method documented elsewhere (Bozeman et al, 1999)], and access. The latter factor turned out to have less impact than expected as the great majority of principal investigators (PIs) contacted agreed to contribute data to the study.

In this research we focus on just a fraction of the case studies performed under the RVM project. The purpose of the case studies is to consider their implications for strategic management of government R&D portfolios. Given the limited use of the cases, and given space limitations, we present them in condensed form. (See the appendix for a list of institutions, project titles, and abbreviations used in this paper.) The full set of original cases is available from the authors.

The results of the Internet survey are reviewed in terms of their implications for strategic management. Thus, we consider each type of 'portfolio' and examine factors related to production of the research 'products' favored by the portfolio.

When collecting data for the RVM project, we were interested in identifying as many different types of output as possible, not only such factors as scientific articles and patents, but also outputs usually unmeasured, including contributions to technical assistance, testing and standard setting, and educational outputs. We also gathered data on factors that seemed pivotal in explaining variance among projects' outputs. The preliminary RVM impact model is presented in figure 1 (see over). It is not to be viewed as exhaustive, but rather includes those variables that could be obtained by the methods employed. (Other possibly important variables are considered in the qualitative analysis of the case studies.)

The basic assumption of the model presented in figure 1 is that the diverse outputs produced by government-sponsored R&D are subject to two direct determinants: the technical focus of the R&D, and the project's initial objectives and motivations. Several factors are indirect determinants including the particular configuration of personnel associated with the project, the amount and sources of funding, the project's timing and duration, and its institutional setting (that is, federal laboratory or university). We employ these factors as 'independent variables' (or, more properly, analytical categories) in the subsequent analysis.

We present the findings according to 'portfolio type', beginning with an illustrative case and then providing data from all the cases. We expected that this mixed-method approach to analysis would likely yield results quite different from either one approach by itself.

4 Portfolio 1: output maximization

The output-maximization portfolio seems appropriate for some government agencies and, particularly, some programs within agencies. In the case of the BES, the primary objective is clear cut: maximizing contributions to knowledge of fundamental scientific phenomena. Output maximization is quite defensible because it reflects the core BES mission.

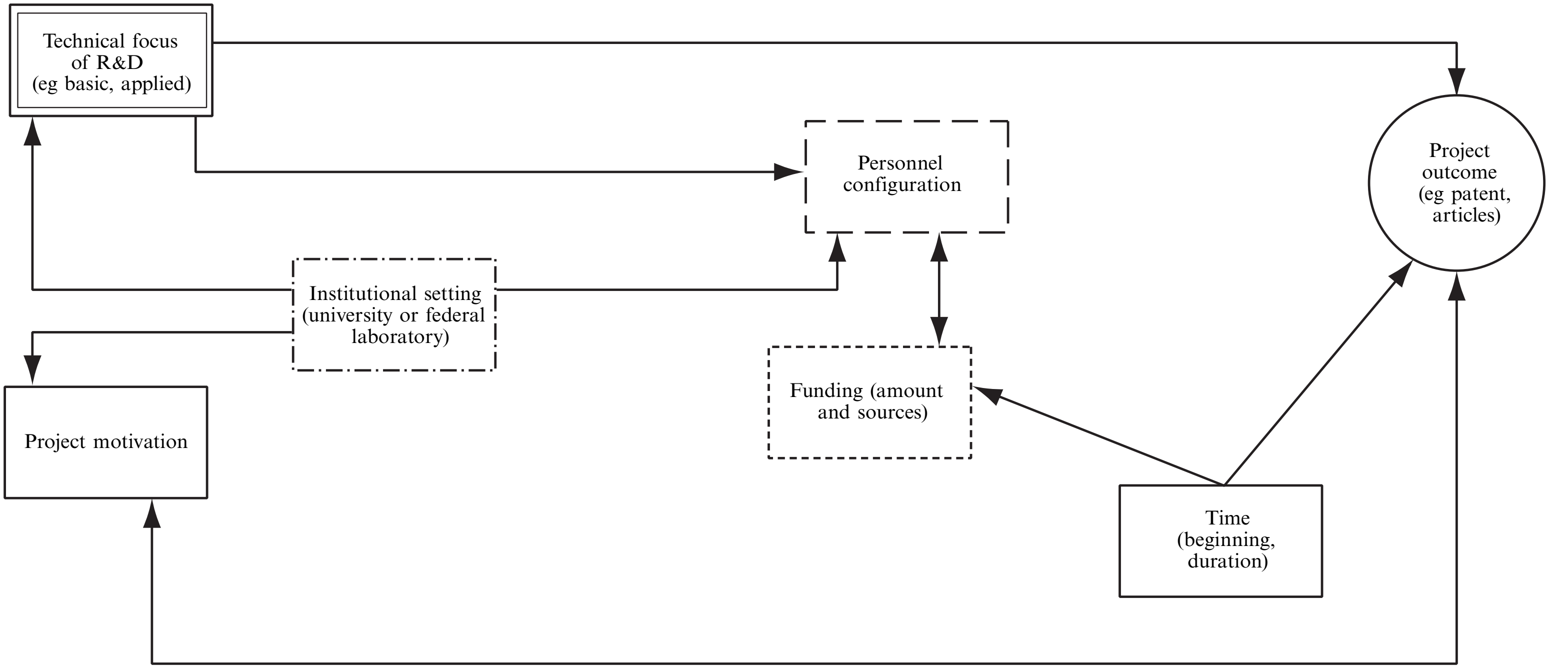

Figure 1. General outcome model for the research value mapping analysis.

In considering output maximization, we examine three output types—scientific contribution (measured in terms of articles published), technology development (measured in terms of patents and licenses), and algorithms (measured in terms of algorithms produced or copyrighted). There are many other outputs a manager might seek to maximize; we focus on these three because each is important to BES and each provides instructive lessons for portfolio management.

4.1 Output portfolio: basic research and scientific knowledge

Virtually all the projects funded by the BES result in scientific publications. But there is considerable variance in the quality and quantity of those publications. Some BES-funded projects could best be described as 'normal science' projects making important but incremental advances in familiar areas of research. Other projects have revolutionized, or even initiated, fields of scientific knowledge.

Our focus here is on projects that have resulted in more than 100 refereed scientific articles. We do not claim that quantity substitutes for quality. However, there are threshold effects such that it is quite difficult for relatively pedestrian research from a project to yield more than 100 articles. Moreover, we know from our case studies that each of the projects yielding more than 100 articles was an extremely significant scientific endeavor and tended to include several research reports generally considered path-breaking. The question, then, is 'If a government manager wished to maximize the likelihood of projects producing high levels (in both quantity and quality) of basic research output, what project management features support such an objective?'

Figure 2 (see over) depicts the projects that have produced more than 100 scientific articles, along with various characteristics of the project, including project motivation, institutional setting, technical focus of R&D, personnel configuration, and funding sources. Before discussing the details of the figure, however, some brief explanation of the method we used to construct it is required. Each of the boxes in the figure represents an attribute variable for the project, one which may either be present or absent (for example, university or federal laboratory, focus on commercialization, a motive to develop new processes or to finance graduate students). The schematic indicates the number of projects in which the attribute was present and includes the specific project abbreviations. The line drawn to the 'dependent variable' (that is, producing 100 or more scientific articles) varies in its breadth according to the number of projects having the attribute in question. If a particular attribute is not present in any project that has produced 100 or more articles, there is no arrow between the box for the attribute and the output variable at the center.

Six of the twenty-four project cases have produced more than 100 publications to date. There are a few surprises. Interestingly, half the projects are conducted in national laboratories and half are university based. Regarding *project motivation*, one of the points almost all have in common is a self-conscious desire to shape a discipline, subdiscipline, or field of science. This is not a motive for some projects, but is reported as one for five of the six projects producing unusually large numbers of published papers. By contrast, only one of the projects with more than 100 articles has developing new technology as a motive (this is despite the fact that nearly 60% of the projects report having developed technology). This seems to imply that technology development and shaping the scientific field are motive sets quite distinct from one other with quite distinct outcomes. If so, this is in accordance with the effectiveness model employed here and elsewhere, a 'contradiction' (Hall, 1991) or 'opportunity cost' (Crow and Bozeman, 1998) model which recognizes that accomplishments in one area are often at odds with accomplishments in another.

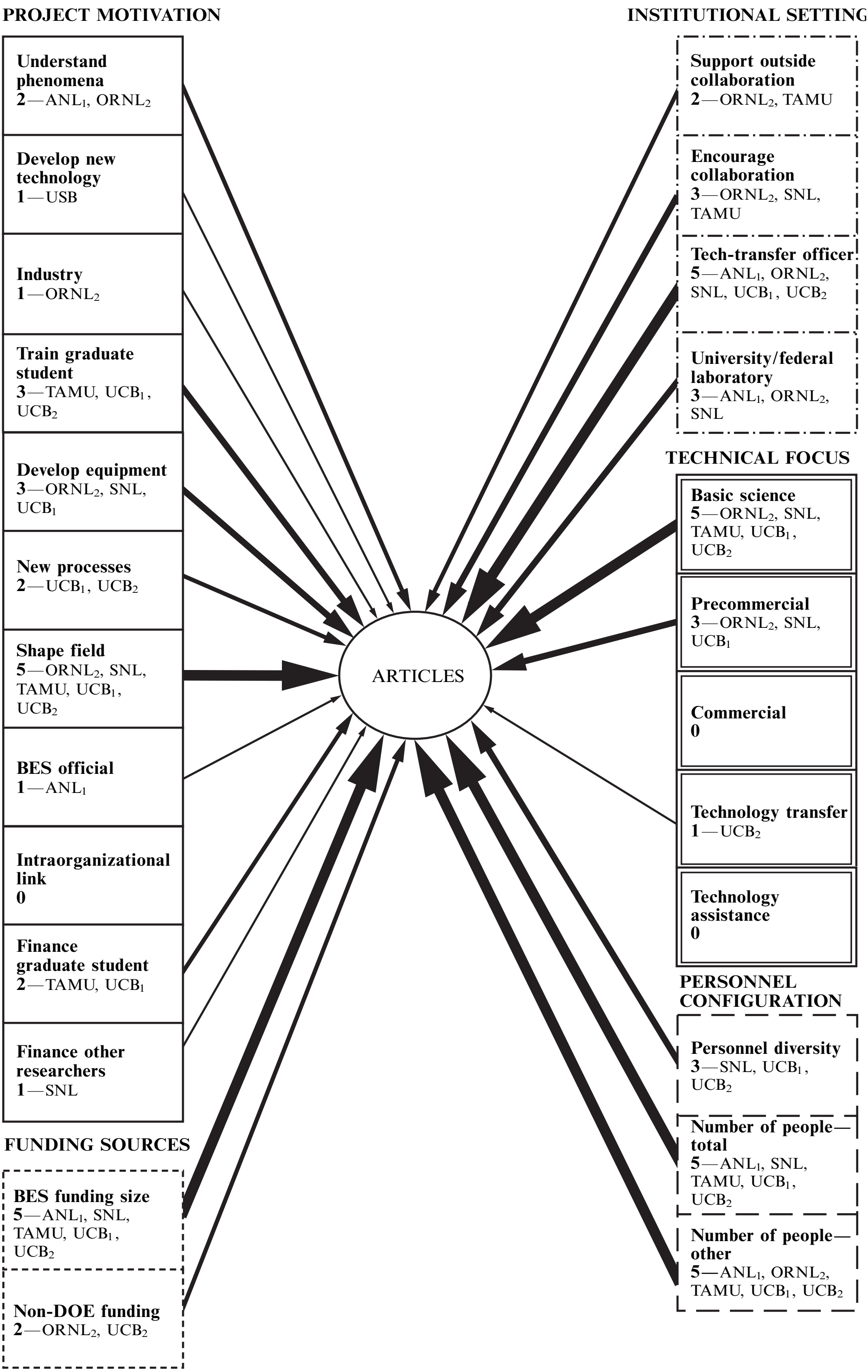

Figure 2. Articles in scientific and technical journals.

One particularly interesting factor pertaining to high-productivity projects is that the people involved are much more likely to have worked with the laboratory's technology-transfer office. Five of the six projects in question (compared with only half of other projects) report having worked with the laboratory's technology-transfer office. This seems to imply that, despite the differences in motivation, the high-scientific-productivity projects are likely to generate intellectual property with possible market value.

One would expect that the *research focus* of projects with large numbers of scientific publications would be on basic research, and, in fact, that is the focus of five of the six high producers. But half also list precommercial applied research as a focus. Interestingly, given the interaction with technology-transfer offices, none of the commercial foci (for example, product development, technical assistance) applies to these projects. Even more interestingly, half of the high producers who do not list technology development or technology transfer as a motive nonetheless have patents and licenses from these projects. This implies, perhaps, a bit of a mismatch between, on the one hand, R&D focus and intent, and, on the other hand, outcome. Put simply, high scientific producers often produce patents and licenses even if it is not a focus of the project or a major motivation.

It would seem that the projects producing exceptional quantities of scientific papers would have long gestation periods. But there is no relationship between the age of these projects and production of scientific articles. This is, in all likelihood because twenty-one of the projects began before 1990, implying that most have had sufficient time to come to fruition.

The *personnel* configurations for the high-productivity projects show a slightly greater number of people than for the other projects. Only eleven of the twenty-four projects, overall, have more than ten people associated with the project, but five of the six high-producing projects have more than ten people. They are also more likely to involve people from outside their own laboratory (five out of six of the projects).

Generally, the high-productivity projects are ones that have received relatively high *levels of BES funding* and are, as one would expect, of longer duration. If we divide projects into those having received $1 million in funding and those receiving less, the overall population has eleven projects with more than $1 million and thirteen projects with less than that. But among the high-productivity projects, all but one have more than $1 million funding. In sum, large-scale scientific production takes large-scale resources in terms of funding, equipment focus, personnel, and time. So-called 'little science' projects may produce high quality but they do not seem to have as much potential either to produce in great quantity or to produce sweeping changes in scientific disciplines and fields. These types of changes require large, stable infusions of resources.

4.1.1 *Case 1: basic research output at Pacific Northwest National Laboratory*

The 'Irradiation-Assisted Stress Corrosion Cracking' (IASCC) research program conducted at the Pacific Northwest National Laboratories (PNNL) under the direction of Stephen Bruemmer and Edward Simonen is an example of high-publication-output basic research. The research focuses on the mechanisms controlling the development of irradiation-induced microstructures and microchemistries, and their influence on the interfacial properties and environmental cracking. The primary goal is to understand the fundamental mechanisms of cracking of various materials exposed to radiation. The implications of this work for problems with cracking in nuclear-power reactors and nuclear-waste storage has generated interest in the electric utility industry creating opportunities for collaborations, especially with the Electric Power Research Institute (EPRI).

This research effort began in 1987 with core support from BES and has resulted in more than 100 publications in the intervening period. It operated almost exclusively with BES funding for the first five years at approximately $200 000 a year. In 1992 BES funding tripled to around $600 000 a year, and this level has been maintained until the present. At that time, a change in policy by DOE allowed Bruemmer and Simonen to enter collaborative agreements with private corporations, enabling them to increase the funding base for the research effort to around $1.8 million a year. The main advantage of this arrangement was the access to very expensive specialized equipment and materials samples that could not be afforded with DOE funding alone. The steady core funding of BES plus the fact that they were allowed to enter agreements with private corporations for joint research provided the IASCC team with greater flexibility to pursue their research. Even though the research that was done in collaboration with industry had shorter time tables and was oriented towards solutions to problems with cracking in systems of interest to industry, it involved characterization of new phenomena and properties of materials under a variety of conditions, all legitimate contributions to fundamental knowledge in the field. Therefore, the inherent tensions between industry-related problem solving and basic research were not acute with respect to the ability to publish research results. The collaborations themselves have a basic science orientation so the research program has not produced any patents or licenses in spite of the fact that it is very relevant commercially.

Bruemmer and Simonen thought BES program managers expected them to strive to lead and shape the field of corrosion cracking, and were encouraged to pursue these strategies to achieve that goal. As a result, Bruemmer and Simonen have been leaders of the respective professional associations, and editors of the main journals and field review issues during the last five to six years.

The high publication count and basic research orientation does not translate, however, into a high number of graduate students involved in the program. There have been five students and two postdocs working on parts of the program, some of them having advisers other than the program PIs.

4.2 Output portfolio: technology development and transfer

Given the close relationship between patents and licenses, we consider the two together here. Eleven of the twenty-four projects have produced patents. Figures 3 and 4 (see over) provide maps depicting the relationship of project attributes to patents and licenses, respectively. There is little difference according to setting, with five university projects having produced patents and six government laboratory projects. Similarly, four of the projects with licenses are government laboratories, four are universities.

The patents from these projects are diverse and include ones for technology pertaining to high-resolution study of materials, ion optic modeling software, and codes used by theoretical chemists. Unfortunately, those with patents often report that they have since had relatively little impact. Successful licenses have been provided for Surface CHEMKIN software commercialized by small businesses and for a micronebulizer (the 'Direct Injection High Energy Efficiency Nebulizer').

The *motivations* behind projects with patents do not vary much from other projects. They are somewhat more likely to report an orientation towards helping industry and developing new products. But they are also motivated to understand basic phenomena and to train graduate students (and at about the same rate for all projects). The most significant departure is that five of the eleven projects with patents are motivated to contribute to new manufacturing processes, whereas only two of the remaining thirteen projects are so motivated. With respect to licenses, the profile is quite similar, but even more oriented towards fundamental science. Indeed, all the projects with

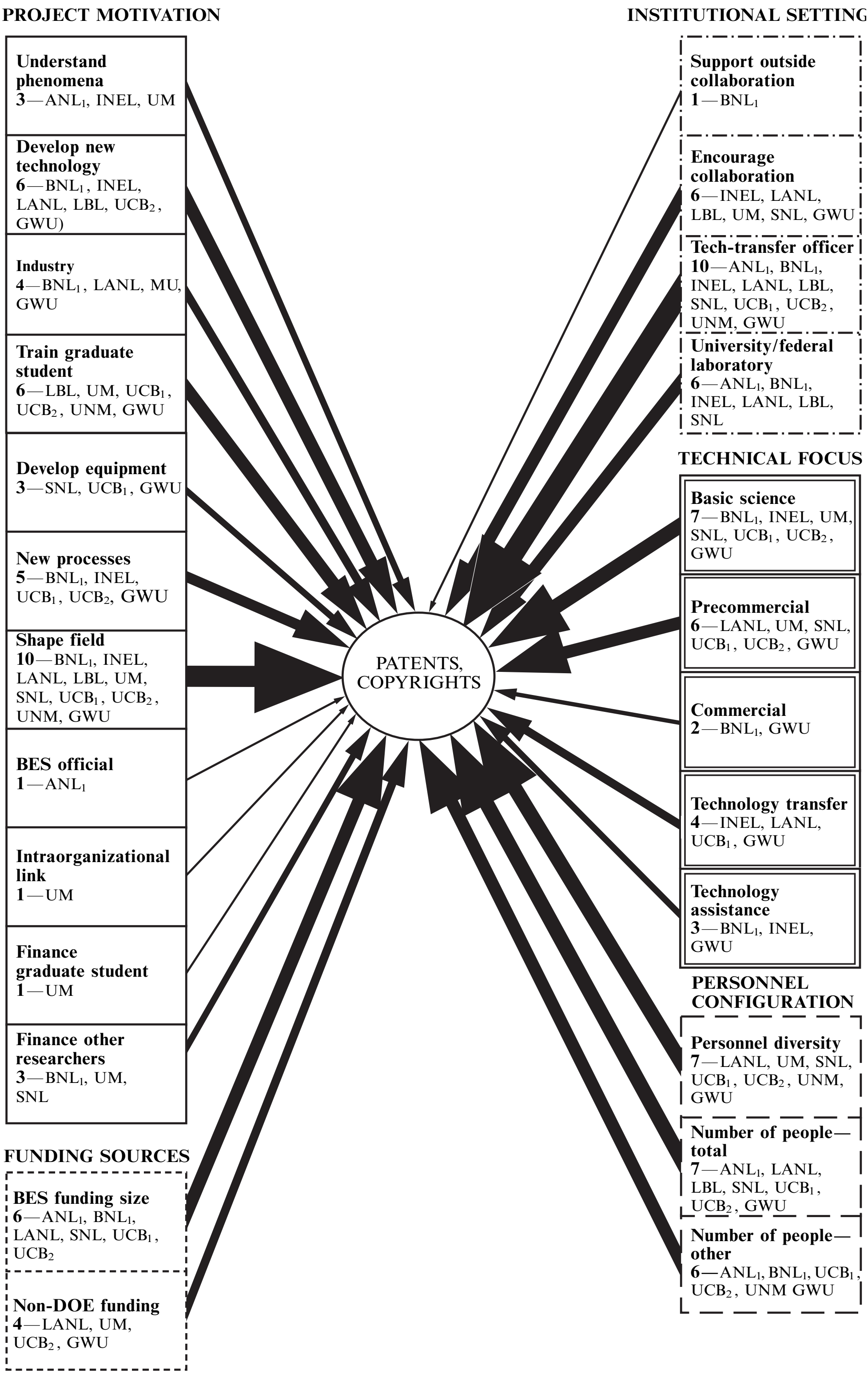

Figure 3. Patents and copyrights.

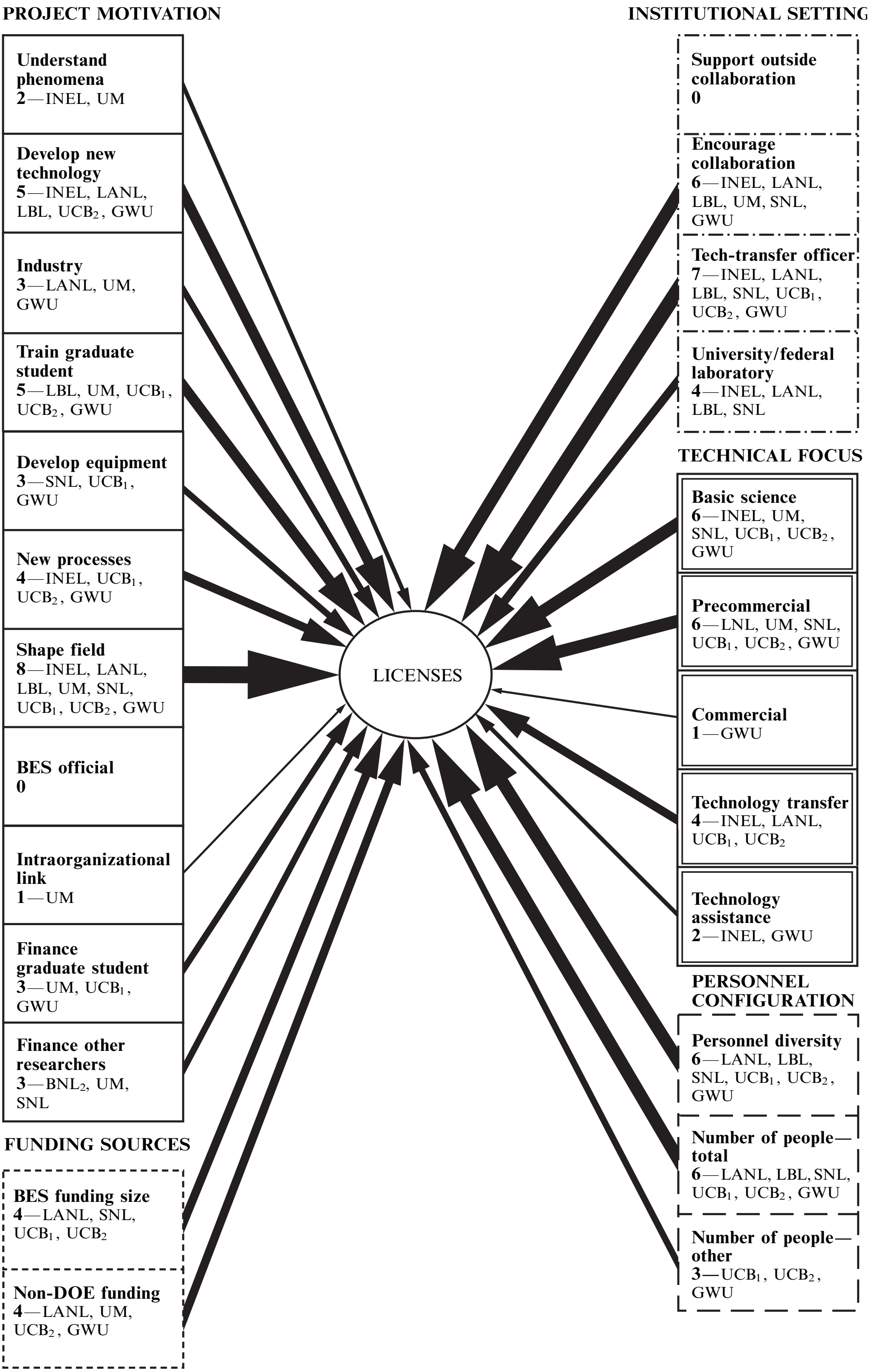

Figure 4. Licensing agreements.

licences report a motivation to reshape the field or discipline, and five of the eight have graduate student education and support as motives.

One would expect that projects with patents or licenses would report a *research focus* on commercial applied research or product development. Interestingly, nine of eleven projects with patents report a very important or most important focus on basic research and all the projects with licenses report such a focus. By contrast, only two projects with patents report a focus on applied development and only one on product development. The profile is similar for license projects. On the one hand, one expects that BES-sponsored projects will be oriented to basic research. Nonetheless, it is noteworthy that those projects with patents and licenses are even more likely than others to have an emphasis on basic research and less likely than others to have an emphasis on commercially oriented applied research or technology development.

The *personnel configurations* for projects with patents and licenses are unlike others, at least with respect to the number of people working on the projects. These projects are likely to have more than ten project personnel (six of eight license projects, seven of eleven patent projects). These projects are no more likely than others to have people from other organizations working on them.

Like most of the projects, the ones with patents and licenses (with two conspicuous exceptions) began before 1990. The *funding levels* for the projects with patents and licenses are quite similar to other projects and they are about as likely as others to have non-BES funding.

4.2.1 *Case 2: technology development and transfer at Oak Ridge National Laboratory*

The 'Rolling-Assisted Biaxially Textured Substrates' (RABiTS) case involves an intensive research and development effort at Oak Ridge National Laboratories (ORNL) to obtain a high-temperature superconducting wire. This is a highly directed research effort towards obtaining an industrially relevant and transferable result. The development of superconducting materials could have enormous implications for the generation and use of electrical power. However, the phenomenon has only been controlled at extremely low temperatures that make applications of practical scale virtually impossible.

The RABiTS project at ORNL began in 1990 and achieved one of its more important results in 1996 with the presentation at the Materials Research Society meeting of a superconducting wire with many desirable features that seemed to bring practical applications into the near future. The effort brought together three teams of scientists at ORNL working in different fields that overlapped on the development of a high-temperature superconducting wire. The contacts for this collaboration were initiated by Rosa Young, a scientist formerly employed at ORNL and then in the employ of a private industry. The experimental arrangements needed to perform certain tests on materials she was working on at the company were available at ORNL and she sought help from former colleagues. They saw the potential of these materials and proposed a collaboration taking advantage of the emphasis on cooperative research in the DOE laboratory system at that time. As a result of the National Cooperative Research and Production Act of 1993, the ability of the laboratories to engage in agreements with industry was greatly expanded. The Act authorized Cooperative Research and Development Agreements (known throughout the federal laboratory systems as CRADAs) as an instrument of choice for engaging in cooperative research. ORNL has been a leader in developing CRADAs, and the RABiTS team and Ms Young were quite aware both of ORNL's commitment to cooperative research and of the widespread use of CRADAs.

The specific result these researchers pursued required parallel efforts by the three teams, which needed to be coordinated. As the effort showed promise, greater administrative and funding support was made available by DOE including the unusual arrangement of naming a nontechnical manager to oversee the program and facilitate coordination between the teams.

Even though the explicit intent of the program, including its collaboration with industry, was to develop a commercially viable technology (as a matter of fact, a patent was obtained), the research produced several basic research results in thin-film superconductors, epitaxial deposition of buffers, and the use of e-beam evaporation to make palladium – nickel samples. In order to protect the intellectual property of the results, the researchers postponed publication of results until all the components were ready for filing a patent. However, several publications resulted from the research in the adjoining fields of research.

This case seems to follow along the lines of large-scale application-oriented efforts with a well-defined technological object as its goal. It did achieve this goal. However, this seemed to have been not incompatible with important basic research outcomes or with the incentives associated with fundamental research for those involved in the effort. Key factors in this seemed to have been the facilitative role of the manager, who coordinated the effort while allowing and encouraging each unit in its own pursuits, and the flexible collaboration with industry through the CRADA.

4.3 Output portfolio: software and algorithms

Figure 5 depicts the relationship of project attributes to production of algorithms. (The logic of the figure is as before, but rather than using arrow density to depict number of relationships, darkened boxes represent the presence of an attribute related to the production of an output.) Unlike many other activities supported by BES, the *institutional setting* makes a considerable difference. Of the eleven producers of algorithms, eight are national laboratories. The algorithms developed are diverse and include the COLUMBUS program for electronic structure calculations, DeltaE for thermoacoustics research, and algorithms for determination of electron number densities in plasma.

The *project motivations* associated with production of algorithms differ little from other projects except, understandably, training of graduate students is a lesser objective given the national laboratory setting. Production of algorithms is associated with less encouragement of collaboration with similar laboratories but somewhat more encouragement of collaboration with industry.

The *research foci* of projects producing algorithms are distinctly different in that they tend to provide stronger emphasis on product development, technology transfer, and technical assistance than do other projects. Projects developing software or algorithms are much more likely to entail CRADAs and to have licenses and patents. From every standpoint, algorithm software production seems to be the major category of product development and application produced by BES funding. To the extent that BES projects end up (near term) in the market, they tend to produce this sort of technology rather than durable physical mechanisms.

Compared with other projects, the *personnel* on projects producing algorithms tend to be greater in number and are likely to be from more than one division of the laboratory. They are no more or less likely to include personnel from other organizations. They are larger with respect to *funding resources*, with more than half having had at least $1 million funding.

4.3.1 *Case 3: software and algorithms at Massachusetts Institute of Technology*

The production of software and algorithms seems to be the major category of product development and application leading to commercialization that results from BES funding. The production of software can become the vehicle for significant two-way links between industry and BES-funded basic research, as the case 'Synthesis and Optimization of Chemical Processes' illustrates. This stream of research spans about

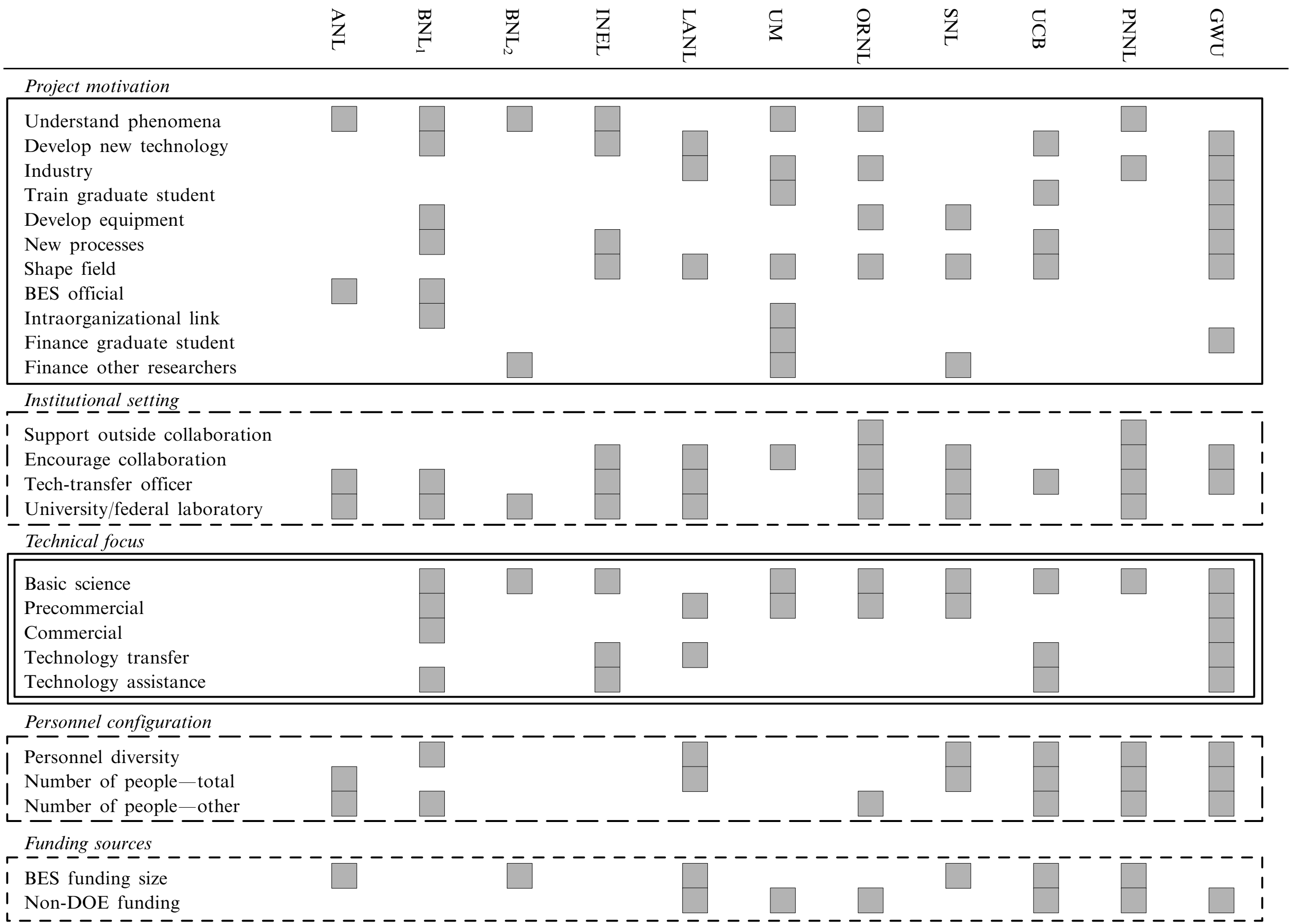

Figure 5. Algorithms.

two decades from around 1976 to the present. The research effort at Massachusetts Institute of Technology (MIT) was directed by Larry Evans until 1992, when Paul Barton succeeded him. Evans has since concentrated his full-time effort in the commercialization of software packages that result from the research.

The main concerns of the research are the flows of energy in industrial chemical plants. The aims of the research are to develop systematic methods to synthesize industrial chemical processes and to provide optimal solutions for the required energy flows. Even though the substantive applications are in chemical engineering, the research activities related to this case are truly interdisciplinary. They involve mathematical modeling techniques that are also applicable, with small modifications, to other large systems such as VLSI (very large-scale integration) integrated circuits.

The origins of this research program hark back to the synthetic fuels program implemented by DOE during the 1970s in response to the energy crisis. The initial funding under the synthetic fuels program was $5 million for five years, between 1976 and 1981, plus $1.5 million provided by industry. The goal of the research at this stage was to develop optimal solutions to the energy balance equations for large-scale, steady-state, commodity plants. The plant simulator would calculate the solutions for the entire flow sheet of a plant.

By the end of this funding period, the energy crisis emphasis began to wane at DOE and Evans decided to pursue the commercialization of the simulator and founded a spin-off company, Aspen Technologies Inc. The company developed the simulator for commercialization and also the plant control versions of the software that added control loops to the program for a plant in operation.

At this time, conversations began with the program manager for the division of engineering of BES, Oscar Manley, with the aim being to secure funding so that the research into simulators, optimizers, and controllers for other types of plants could continue. As a result, they implemented a collaborative arrangement with Idaho National Engineering Laboratory (INEL) and received a grant from BES that was funded in successive cycles until 1994, when the collaboration with INEL ceased. The MIT team has continued to receive BES funding for the research program. BES funding during this period was around $100 000 a year. The impact on industry in terms of energy savings by using the simulators and controllers is in the tens of millions of dollars per company, totaling around a billion dollars for all the companies using the technology.

The BES program manager encouraged Evans to maintain his close ties with industry while pursuing the research at MIT. Aspen Technologies developed products for the chemical industry while the MIT team pursued an intellectual agenda that was not driven by demands from industry partners, though it continuously worked on cases that Evans obtained through his industry contacts. During the 1980s, other companies obtained licenses to the Aspen technology and offered competing products. These industries constituted an intermediate industry sector that commercialized research results, both of their own R&D efforts and from the university, via the software products they offered to the chemical industry. Graduates from the university program have been in great demand both by the software companies and by the chemical industry. This has allowed Evans and Barton to build a circle of industry affiliates that provide grants of about $10 000 to $30 000 expecting that, in exchange, they will have access to recent graduates.

The career trajectories of these graduates have been remarkable. They have been hired by both types of industry and some have been hired back from the large chemical industry sector to the software industry or to academic research. In this way, even though there is no formal agreement to pursue proprietary research for industry, or to abide by timetables of industry problems, the intellectual agenda of the basic research effort is constantly enriched with a deep understanding of problems with potential impact on industry. The mobility of these professionals in and out of the research environment seems to be facilitated by the fact that the research results are encoded in the software packages and, therefore, they do not need to learn to function in an entirely new environment of work. The mediation of the software simulators and controllers creates roles that allow these professionals to fulfill production or research roles with low adaptation costs (Rogers, in press).

The production of publications has also been an important outcome, consistent with the researchers' contention that they pursue a basic research intellectual agenda. The team has had one or two PIs with one or two postdocs at any one time. The publication rate has been about ten papers a year. The number of doctoral students ranges from five to seven at any one time, yielding about two graduates every three years. In sum, the production of software with quite direct commercial implications is accompanied by a significant production of basic scientific knowledge and human-capital formation.

5 Portfolio 2: balanced portfolio

From the cases presented in the previous section, it should be clear that BES projects generally include multiple output types and, in that sense, provide one sort of balance. Indeed, this resembles the type of balance (Baker et al, 1986) urged for most private sector portfolios—balancing 'downstream' and 'upstream' projects, and balancing degree of technical risk. Even if it is not the explicit intent of project managers or researchers to provide a mix of basic research outputs and technology spin-offs from basic research, it occurs with surprising frequency.

Our concern in the balanced portfolio is to integrate a focus on discrete outputs with an emphasis on maintaining and extending the research community's capacity, especially its S&T human capital. This is a type of portfolio balance unique to the public sector. No private firm takes as its mission the nurturing of entire fields or subfields of science. Similarly, if private firms are involved at all in supporting higher education and graduate training it is with a view to benefiting either directly through cooperative R&D or through recruitment.

Science, technology, innovation, and the commercial and social value produced by these activities depend upon the conjoining of equipment, material resources (including funding), organizational and institutional arrangements for work, and the unique S&T human capital embodied in individuals. Arguably, a public sector R&D portfolio might well consider the supporting infrastructure for science and technology and not just output profiles.

We do not currently have data on all the capacity elements that could be considered in connection with a S&T human-capital model. But, as a starting point, we examine one obviously important factor, the contribution of projects to the training of graduate students. We begin by considering factors pertaining to the training of graduate students in BES-sponsored projects and then, in section 5.2, consider the joint impacts of student production and R&D outputs.

5.1 Balanced portfolio: graduate student training

The most obvious determinant of the training of students is the institutional setting; one expects that government laboratories would have a limited role in production of students and that university laboratories would typically have a larger role. As figure 6 (over) indicates, nineteen projects report student training as a significant output and this includes eight government laboratories. Thus, the interesting question

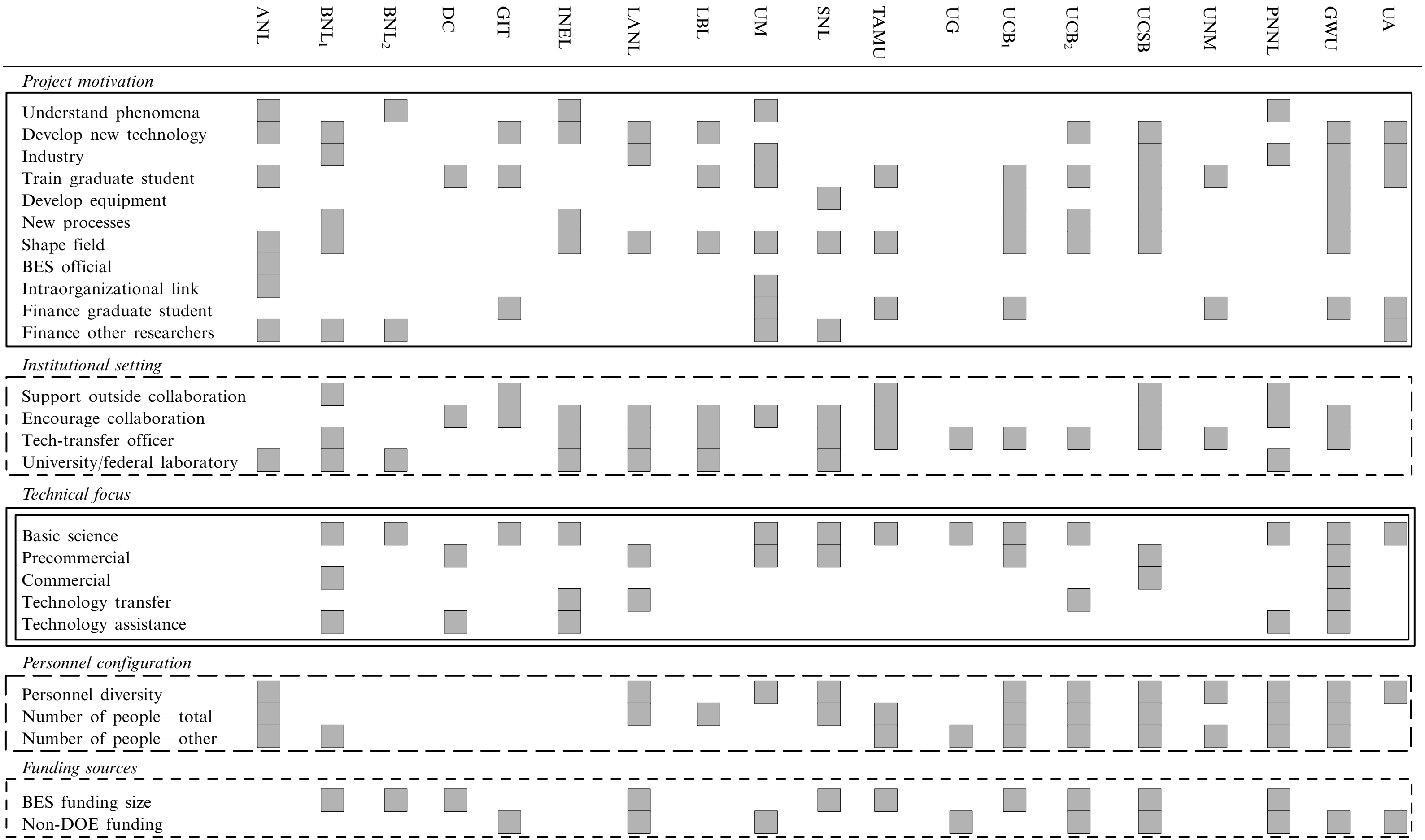

Figure 6. Training, instruction, and skill development of student project members.

is the characteristics of projects that do not have student training as a component. A first characteristic is that they are all government laboratories.

The *project motives* for those projects not having student training as an activity are not substantially different from those other projects that do. But the *research foci* are somewhat different. Projects not having student training as an activity are more likely to be oriented to precommercial applied work and, interestingly, are less likely to be involved in technology transfer (none of the five projects having no students has technology-transfer activity). The projects without students are more likely to be ones which produce 100 or more scientific articles, but are less likely to have patents, licenses, or to develop new technology processes. Generally, the BES projects having no students tend to be smaller scale and oriented almost exclusively towards production of fundamental scientific knowledge. All of the projects without students began before 1990, and they are generally less likely to be funded at the $1 million level and tend to include fewer than ten scientific and technical team members.

5.2 Balanced portfolio: causes and effects of balance

An opportunity-cost approach to R&D planning and evaluation begins with the assumption that achieving most R&D objectives will mean not achieving others. Thus, we consider what happens to R&D output maximization when we take into account the objective of increasing S&T human capital through the production of graduate students. We also consider the factors associated with a balanced portfolio, one which emphasizes various types of R&D output while, at the same time, valuing the development of the next generation of scientists and engineers.

In this analysis we classify the projects in terms of their ability to produce both discrete R&D outputs and S&T human capital. The approach here is simple. First, we examine the ranks of the projects with respect to the primary output variables examined in this paper and their ranks on support of graduate students. Then we create three new variables based on the joint ranks between, respectively, the three output variables and the student support variable. We then compare the difference in assessment between a 'maximization' perspective and a balanced-portfolio perspective. Finally, we seek to explain determinants of high-quality outcomes on a balanced portfolio by correlating the joint-rank variables and a number of project-attribute variables.

Table 1 (over) provides the ranks of each project on the various output variables and on student support. In order to facilitate subsequent analysis, the ranks are in descending order, that is, the ones with the highest absolute value also have the highest rank value. A quick scan of table 1 indicates that a high ranking for student support is sometimes associated with a low rank for output variables.

Table 2 (over) suggests that student support is not generally related, at least not significantly, to R&D output ranking. The Kendall's τ_b correlation results show that only one of the output variables is significantly correlated with the production of graduate students; there is a small (-0.278, $p < 0.10$) negative correlation with production of algorithms, perhaps related to the fact that algorithms tend to be a focus of federal laboratories. Generally, however, there seems to be little direct trade-off between providing maximum output in each of the three output categories and supporting students.

Next we rerank projects as a joint function of output and student support. This is accomplished by simply adding the rank for each output variable to the rank for student support.[(2)] Then we ask the key question in the balanced portfolio: 'which

(2) We note that, because of ties, the ranking metric produces some bias. An examination of other ranking approaches (including weighting and ranking by z-scores) indicated that the less sophisticated additive ranking produced little difference in actual results.

Table 1. Students supported and project outputs (ranked)[a].

Project	Student support	Article publication	Patent production	Algorithm production
ANL_1	1	20	1	1
ANL_2	2	2	1	1
BNL_1	9	1	6	1
BNL_2	1	1	1	1
BNL_3	4	1	1	4
DC	1	9	1	1
GIT	13	7	1	1
GWU	15	13	9	3
INEL	3	8	6	1
LANL	1	16	11	7
LBL	17	16	11	1
UM	7	6	1	6
NREL	1	5	3	1
$ORNL_2$	1	21	1	8
PNNL	13	16	1	1
SNL	1	17	9	6
SU	9	5	1	1
TAMU	14	18	1	1
UA	7	5	1	1
UNM	5	12	3	1
UG	10	11	1	1
UCB_1	18	22	6	1
UCB_2	16	20	9	7
UCSB	13	10	1	1

[a] The ranks are in descending order; the projects with the highest absolute numbers also have the highest rank number.

Table 2. Kendall's τ_b correlations between rankings for output variables and for student support ($N = 28$ for all cases).

Kendall's τ_b	Student support	Article publication	Patent production	Algorithm production
Student support				
correlation coefficient	1.000	0.056	0.000	−0.278
2-tailed probability	–	0.689	1.000	0.069
Article publication				
correlation coefficient	0.056	1.000	0.298*	0.300*
2-tailed probability	0.689	–	0.049	0.047
Patent production				
correlation coefficient	0.000	0.298*	1.000	0.259
2-tailed probability	1.000	0.049	–	0.117
Algorithm production				
correlation coefficient	−0.278	0.300*	0.259	1.000
2-tailed probability	0.069	0.047	0.117	–

*denotes a correlation which is significant at the 0.05 level (2-tailed).

Table 3. Kendall's τ_b correlations between rankings for output variables and for mixed output with project and management variables.

Project attribute	Article publication	Article publication and student support	Patent production	Patent production and student support	Algorithm production	Algorithm production and student support
University or government[a]	0.113	−0.183	0.169	−0.475****	0.237*	−0.470****
First year of BES funding	−0.355***	−0.209	−0.230	−0.078	−0.272*	0.117
Frequency of BES visits	0.246*	0.168	0.320**	−0.027	0.322**	0.082
Number of personnel on project (all years)	0.391***	0.388***	0.307**	0.146	0.189	0.154
Number of personnel now	0.285	0.364***	−0.208	0.095	0.147	0.232
CRADA[b]	0.348***	0.321**	0.230	0.123	0.401***	0.238
Amount of BES ($)	0.458****	0.416****	0.148	0.263	0.121	0.246
Amount of non-BES ($)	0.272*	0.005	0.200	0.229	0.306**	−0.286*
Interaction with technology-transfer office	0.420****	0.331**	0.591****	0.076	0.303**	0.016

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$, ****$p < 0.001$.
[a] Government laboratory project = 1, university laboratory project = 0.
[b] Project is a CRADA = 1, project is not a CRADA = 0.

factors are associated with high levels of output and how does this change when the portfolio is balanced to consider student support?' To examine this question we analyze Kendall's τ_b correlation coefficients both for the ranked output variables and for the mixed variable, which was based on ranks for both output and student support. The results are provided in table 3.

Table 3 indicates that a number of factors generally under the control of the program manager do, in fact, relate to R&D output. Projects ranking high in the production of algorithms tend to be located at government laboratories, they are older projects, they are especially likely to be CRADAs, and they are likely to have a significant amount of non-BES funding. Interestingly, more regular visits by BES program managers tend to be associated with production of algorithms. How does this 'portfolio' function change when we include ranking both by algorithm output and by human-capital production? The strongest (and not surprising) change is with respect to the locale: university or government. The sign changes and the magnitude of the coefficient increases. There is a mild tendency for algorithms to be produced at federal laboratories in greater number, but if we seek to maximize joint utility, the preferred locale for this activity is clearly the university. This tends to coincide with our case evidence in which algorithms are generally the focus of small, specialized groups, both at universities and in government laboratories, and the government settings almost never included students.

The findings for patent production follow a similar but not identical pattern. One of the factors related to patents is BES oversight in terms of frequent visits. These visits may be an indication of a program manager's interest in intellectual property or of the need for program managers to be involved in administrative procedures arising from patenting. Another factor related to patents is the number of people on the project; those involving patents also tend to have more project personnel during the life of the project. Interaction with the technology-transfer office is an unsurprising correlate. The picture changes little when we consider the joint effects of patents and human-capital production via student support. As before, the university setting plays an obvious role.

The most interesting and robust findings are with respect to the production of articles. Generally, the production of a high volume of articles is associated with (1) long-time BES funding; (2) a large number of project personnel during the life of the project; (3) a high likelihood of the project having a CRADA; (4) the amount of BES funding; and (5) interaction with the technology-transfer office. Minor factors include the presence of non-BES funds and frequent program-manager visits. When we pursue a mixed portfolio by adding student support to the equation, we find that the picture changes very little. The year of BES funding becomes less important (perhaps a surrogate for government laboratory funding because these projects tend to be older), but other relationships alter little, suggesting compatibility between the two basic elements of the mixed portfolio.

5.2.1 *Case 4: S&T human capital at University of California, Berkeley*
The 'Synthesis and Optimization of Chemical Processes' case at MIT is already an indication of the great impact of BES-funded research on human capital. Further illustration is provided by the case of research in 'Nuclear Magnetic Resonance (NMR) Spectroscopy' under the direction of Alex Pines at Berkeley. The main technical goal of this research is to extend the utility of NMR spectroscopy to materials other than liquids. NMR spectroscopy has inherent limitations when applied in the chemical analysis of solids and quasisolids. This research was able to increase the sensitivity and resolution of NMR spectroscopy when applied to these materials by several orders of magnitude, leading to numerous applications in materials sciences, electronics, biology, the oil industry, and general analytical chemistry instrumentation.

This research involved new discoveries in the realm of nuclear-particle physics that were translated into new experimental techniques applied in the investigation of systems in several disciplines. These characteristics of the knowledge led to a research strategy, pursued over a period of about two decades, which involved, simultaneously, the pursuit of problems in the core field of NMR and the use of NMR spectroscopy in several other fields. Pines was situated at the center of several collaborations that made contributions both to basic knowledge of nuclear-particle dynamics and to knowledge of protein systems in biology, semiconductor systems in electronics, and catalyst systems in physical chemistry.

The interdisciplinary nature of the research program and the multiple knowledge-front pursuit also led to a peculiar pattern in the formation of S&T human capital. Graduate students were included in research both at the core of NMR spectroscopy and in the application systems in other disciplines, always under the direction of Pines. Rather than following a direct reproduction pattern, that is, as new researchers pursuing the next stages of work in the area of their mentor, after graduation these students pursued careers as diverse as the research itself. With a rate of about two PhDs a year, a few went on to develop successful academic careers in top institutions and did so not only as chemists but also as biologists, physicists, and electronic engineers with specialties based on the systems to which they had applied NMR spectroscopy. Several others went on to pursue careers in industry with assignments related to the systems in which NMR spectroscopy may be fruitfully applied. The interdisciplinary nature of this research program, with a combination of research on fundamental principles which becomes instrumental knowledge in other fields, seems to be a very fertile setting for training scientists with a very diverse set of skills who are adaptable to many new settings of knowledge production.

The funding patterns for this research stream are important because they are directly related to the peculiar shape of the research agenda. Initially, the funding was not dominated by any single source and a combination of National Science Foundation and university grants allowed the program to start. The development of the experimental techniques required fixed amounts of funding at certain stages that could not withstand the negotiations over grant awards that often take place. Pines was successful in securing a funding base from BES about three years into the research program, which gave it the stability that this research strategy required. From that time on, BES funding has represented about 90% of the budget with a number of smaller grants from industry and other sources making up the rest. The yearly average operating costs of the program are very misleading because major equipment purchases can make the budget jump twenty times from one year to the next. Excluding major equipment purchases, yearly operating costs for this program have grown from around $100 000 to $500 000 over a twenty-year period. From the point of view of funding a research effort of these characteristics, it is clear that both meeting the fixed costs of the program and long-term stability of core funding are essential to its success.

The 'Surface Science and Catalysis' program at the Center for Applied Materials, UC Berkeley, under the direction of Gabor Somorjai is another example of a research program with high impact in the production of S&T human capital. The research focuses on the chemical reactions that take place on the surfaces or interfaces of various chemical compounds. They study the structure, molecular bonding, and reactivity at the surface of solids. The resulting knowledge is then used to explain macroscopic surface phenomena, heterogeneous catalysis, adhesion, and lubrication at the molecular level. The role of catalysts in accelerating or decelerating chemical reactions has important consequences for the energy efficiency of chemical processes as well as the synthesis of fuels. Therefore, Somorjai's program received DOE support consistently from the beginning, starting at low levels and growing over the years.

The research in this case is somewhat less interdisciplinary than in the NMR spectroscopy case. However, surface phenomena are important for a wide array of materials, allowing graduates from this research program, which have been numerous—around eighty over a twenty-five-year period, to pursue careers both in academia (about one third) and in industry (about two thirds). A similar number of postdoctoral students have been associated with the catalysis program but two thirds have pursued academic careers and one third careers in industry.

The Center is part of the Lawrence Berkeley Laboratory, which is closely associated with the university. This arrangement has been extremely important for Somorjai in enhancing the possibilities of his research program. The availability of specialized and expensive equipment together with opportunities for the exchange of ideas with colleagues working on other projects are both mentioned by the PI as especially helpful. Together with the high throughput of graduate students and postdocs, the program has been extremely productive in terms of publications, with a rate ranging from around ten papers a year during the first five years to around forty a year in the last five years. The stability of funding and the 'best of both worlds' arrangement through the association of the Lawrence Berkeley Laboratory with UC Berkeley have been key factors in the success of this research program.

6 Conclusions: outputs, impacts, and portfolio strategy

In this concluding section we consider the evidence from the case studies presented here in connection with the analysis of quantitative data from the entire set of cases. Then we consider the overall implications for strategic management of government R&D portfolios.

6.1 Case-study evidence in connection with aggregate data

6.1.1 *Funding stability*

One key to the success of the projects, as reflected both in the case studies presented here and in the data for all the cases, is the importance of funding stability. Unlike most US research agencies, BES tends to have longer term funding horizons, especially in its support for federal laboratories but for universities as well. The MIT case began in 1976 with a different PI than is now funded, the original PI having evolved into a commercial actor, setting up a company to use the results from the MIT project. The 'old' and the 'new' feed one another as the previous PI stays connected to the present one, increasing opportunities for each. The longevity of the core project-funding from BES has enabled this success. By one view, not ours, government funding might have been taken away years ago on the rationale that the project had developed a market. But it is the continuing of the fundamental work that has helped the economic development aspects of the project come to fruition.

The median beginning year from all the projects taken together is 1989. In other words, the projects have generally been running for a considerable length of time, and this longevity of funding has permitted useful twists and turns in projects. The PNNL project began in 1987 and later 'took off' after industrial collaboration and cooperative research were permitted. Thus, the project evolved from a relatively traditional fundamental research project to one that included fundamental research but, at the same time, examined issues of sufficient interest to industry to warrant their contributing research funds. The ORNL case is similar in some respects, moving from fundamental research on the superconducting wire to patents and commercial application. Stable funding and a multidisciplinary environment permitted ORNL the requisite time for transformation and elaboration of the project.

6.1.2 *CRADAs and cooperative research*

The aggregate data and the cases complement one another in their results on the beneficial impacts of CRADAs and cooperative research. In our interviews with scientists, we often found skepticism about the value of formal cooperative R&D, especially because of the increased administrative burden required. But the evidence shows that CRADAs are associated with a variety of concepts of productivity, not just technology development but also production of fundamental science. We can turn not only to the statistical evidence on this account but also to the case evidence, which underscores the role of CRADAs. The PNNL and ORNL cases relied strongly on CRADAs and cooperative R&D.

When we consider this finding along with the importance of interactions with the technology-transfer office (another area of skepticism among our interviewees), we can perhaps conclude that projects that have clear-cut means of linking with other organizations, researchers, and firms tend to be more effective in a variety of respects. The question, of course, is the direction of causality. Are CRADAs and technology-transfer activities truly helpful or are they simply the accoutrements of already successful projects?

6.1.3 *The basic/applied false dichotomy*

In this study and another using separate data sources (Rogers and Bozeman, 1997) the ability of basic research projects to generate wide-ranging accompanying outputs is impressive. Our software/algorithm, technology, and S&T human-capital cases all began as basic research, sustained the basic research, and spawned other valuable R&D products. In projects that are relatively long term and multidisciplinary, the usual dichotomy between basic and applied is often not relevant.

6.1.4 *Manager leverage*

Many of the project dimensions we focus on include aspects that are to some extent under the control of government R&D program managers. Among other factors, program managers have considerable control over the funding level, the number of years of project funding, degree of oversight and communication, and the treatment of funding from other sources, including other government agencies. We have already discussed the advantages of stable funding (which, of course, assumes an acceptable level of initial quality). Our evidence indicates that the amount of funding is not a primary determinant of output, but that managers should be prepared (when possible) to provide additional funding at various take-off periods, whether they be breakthrough scientific findings (PNNL), development of valuable software (MIT), or movement from research to commercial technology (ORNL). Interestingly, most of our indicators suggested that relatively frequent program-manager visits is associated with success. Again, there is the question of the direction of causality (our cases suggest it goes both ways).

6.2 Developing a balanced portfolio

We suggested above that the meaning of 'balance' in a government portfolio is in many ways quite different from that in a private sector portfolio. The notions of risk differ, the evaluative criteria differ, and the government agency often has responsibilities for sustaining scientific fields and S&T human capital. For BES, one part of the portfolio issue seems unproblematic. The agency's chief mission is to support fundamental research rather than to provide a wide variety of products at different stages in the basic–applied–development–commercialization spectrum. Moreover, many of its fundamental research projects seem to evolve into multioutput projects. All of the cases included a variety of R&D output types. Even the 'small science' university-based projects are diverse with respect to composition of R&D.

Given the uniquely public R&D objective of sustaining fields of research and the capacity of future generations, we considered portfolio balance of a certain sort—balancing output and support of graduate students. This is, of course, only one of several elements of S&T human capital embodied in BES projects (others include training and development of postdocs, technicians, and project researchers, education of the general public, and, in a few cases, teaching undergraduate students).

The primary portfolio split is, of course, whether the research project is based on a university site or a federal laboratory. But even this is not as clear as it might seem. The two UC Berkeley cases, NMR spectrometry and surface science, make use of the facilities of the Lawrence Berkeley Laboratory. It is no accident that two of the most valuable projects from the standpoint of scientific and technical human capital are university projects using, in part, federal laboratory facilities.

The empirical results from the entire set of cases seem to suggest that the opportunity costs of pursuing a balanced portfolio (output and S&T human capital) are typically modest. Moreover, in some cases there are 'opportunity net benefits'. The NMR case in particular shows that taking an S&T human-capital perspective can actually facilitate a focus on optimizing output. The industrial connections and funding for this fundamental research have largely come by way of the value of students produced. Government managers seeking to formulate a portfolio might do well to give consideration to a balance between output and human capital. Some agencies (especially the National Science Foundation) already provide explicit attention to projects' contributions to S&T human capital. Our interviews indicated that many BES managers also include human-capital calculations in the portfolio strategies, along with the other major element of capacity, large-scale and unique equipment. Our findings suggest that pursuit of a balanced strategy need not diminish the effectiveness of the research outputs from funded projects.

In our view, strategic management of government R&D portfolios is difficult to achieve but not impossible. A key is understanding the constrained environment of government R&D. Thus, strategically managing R&D at the level of the DOE is more than daunting, but at the level of programs within BES, the objective seems more reasonable. Our analysis seems to us to demonstrate the necessity of recognizing the diverse S&T goals of government R&D management, and the very real possibility that some of those goals conflict with others. We suggest an approach where the aims are relatively modest and where the methods employed are as simple as observing the presence or absence of attributes, as opposed to the more intricate mathematical portfolio-analysis methods appropriate to firms seeking to internalize the results of R&D.

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APPENDIX
Project details

ANL$_1$ Argonne National Laboratory, 'Artificial photosynthesis'
ANL$_2$ Argonne National Laboratory, 'Mineral – fluid interactions: experimental determination of atomic-scale processes using synchrotron radiation'
BNL$_1$ Brookhaven National Laboratory, 'Characterization of fatty acid desaturases and related lipid modifications enzymes'
BNL$_2$ Brookhaven National Laboratory, 'Measurement of fluid flow and fluid – rock interactions using synchrotron computed microtomography'
BNL$_3$ Brookhaven National Laboratory, 'First principles theory of high and low temperature phases'
DC Dartmouth College, 'Excitons and plasmas in semiconducting microstructures and ternary alloys'
GIT Georgia Institute of Technology, 'The organic chemistry of conducting polymers'
GWU George Washington University, 'He inductively coupled plasmas for emission and mass spectrometry'
INEL Idaho National Engineering Laboratory, 'Chemical materials and processes: SIMS'
LANL Los Alamos National Laboratory, 'Thermoacoustic engines'
LBL Lawrence Berkeley Laboratory, 'Enzymatic synthesis and biomolecular materials'
MIT Massachusetts Institute of Technology, 'Synthesis and optimization of chemical processes'
NREL National Renewable Energy Laboratory, 'Photoconversion processes in liquid crystal porphyrin films and other molecular semiconductors'
ORNL$_1$ Oak Ridge National Laboratory, 'Rolling-assisted biaxially textured substrates'
ORNL$_2$ Oak Ridge National Laboratory, 'Basic aqueous chemistry at high temperatures and pressures'
PNNL Pacific Northwest National Laboratory, 'Irradiation assisted stress corrosion cracking'
SNL Sandia National Laboratory, 'Chemical vapour deposition sciences'
SU Stanford University, 'Rock fracture networks and clusters and fluid flow properties in reservoirs and aquifers'
TAMU Texas A&M University, 'Correlations between surface structure and catalytic activity/selectivity'
UA University of Arizona, 'A model approach to hydrodenitrogenation catalysis'
UG University of Georgia, 'The metabolism of hydrogen by extremely thermophilic bacteria'
UM University of Maryland, 'Critical phenomena in fluids'
UNM University of New Mexico, 'Particle-induced amorphization of crystalline silicates'
UCB$_1$ University of California Berkeley, Center for Applied Materials, 'Surface science and catalysis program'
UCB$_2$ University of California, Berkeley, 'Nuclear magnetic resonance spectroscopy'
UCSB University of California, Santa Barbara, 'Molecular properties of thin organic interfacial films materials'