

Scientific Discoveries: An Institutional and Path-Dependent Perspective

J. Rogers Hollingsworth

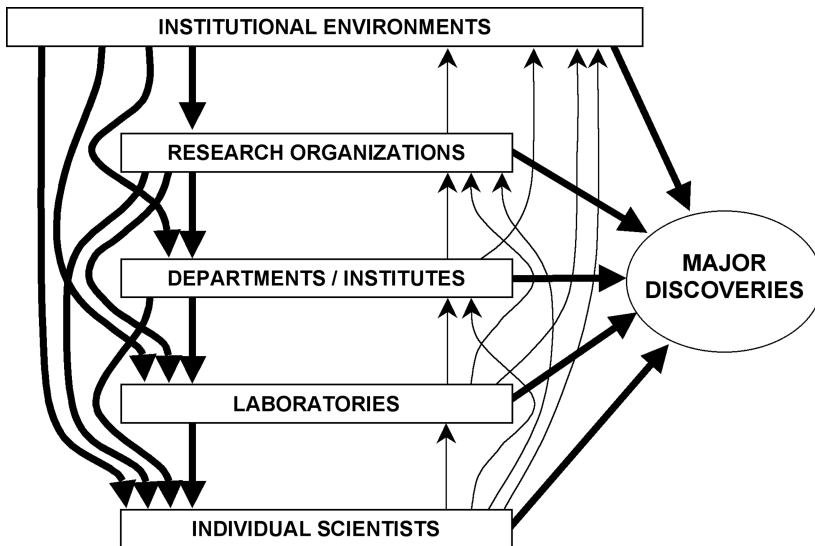
This paper is a part of a research program analyzing how institutional and organizational factors facilitate or hamper the making of major discoveries in basic biomedical science.¹ Most of the paper focuses on research organizations and institutions in the United States during the twentieth century, though there are occasional soft comparisons with the institutional environments and organizations of other societies. The research program as a whole examines research organizations in Britain, France, Germany, and the United States throughout the twentieth century.

Critical to this paper is the definition of a major discovery. A major breakthrough or discovery is a finding or process, often preceded by numerous small advances, which leads to a new way of thinking about a problem. This new way of thinking is highly useful to numerous scientists in addressing problems in diverse fields of science. Historically, a major breakthrough in biomedical science was a radical or new idea, the development of a new methodology, or a new instrument or invention.² It usually did not occur all at once, but involved a process of investigation taking place over a substantial period of time and required a great deal of tacit and/or local knowledge, if not both.³

The analysis is multilevel in nature. Research in most research organizations takes place in laboratories located in departments or divisions that are part of an organization; in turn such research organizations are embedded in a larger institutional environment. Figure One is a simplified perspective of the way each of these levels influences the process of making major discoveries. One of the major challenges

facing the scientific community is to understand how activities at one level of analysis are related to those at other levels.⁴ As the social science community lacks a good understanding of the way processes at multiple levels of societies operate, this paper is intended to make a modest contribution toward explaining how interactions occurring at multiple levels influence major scientific discoveries. The paper's perspective is nonlinear and co-evolutionary. The heavy downward arrows in Figure 1 indicate the dominant type of influence which institutions and organizations exert on laboratories and researchers. The direction is not one way, for activities at the level of the laboratory influence the behavior of entire organizations as well as institutional environments, and these in turn feed back on the activities of individual laboratories. Collectively, all of these factors help to explain why there is variation across laboratories in departments, across departments in organizations, across organizations in a society, and across societies themselves when it comes to the making of major discoveries. While each of these four levels is constantly changing, it is the institutional environment which is most enduring and resistant to change. Actors at lower levels are greatly constrained by the norms, rules, and systems of rules that by definition constitute the institutional makeup of a society.⁵

Figure 1. Factors at multiple levels influencing major discoveries.



Institutional Environments and Research Organizations

The institutional environments of research organizations consist of a variety of variables, all of which are treated equally here. Institutional environments range from weak to strong.⁶ Weak institutional environments exert only modest influence (1) over the appointment of scientific personnel of research organizations, (2) in determining whether a particular scientific discipline will exist in a research organization, (3) over the level of funding for research organizations, (4) in prescribing the level of training necessary for a scientific appointment (e.g., the habilitation), and (5) over scientific entrepreneurship (e.g., the existence of norms of individualism that socialize young people to undertake high-risk research projects).

Strong institutional environments are at the opposite end of the continuum on each of these characteristics. France is an example of a country that tended to have a strong institutional environment throughout the twentieth century, while research organizations in the United States have been embedded in a relatively weak institutional environment. However, the institutional environments of societies change over time, and the changes in the institutional environment may influence the capacity of a society to make major scientific discoveries. The data on the institutional environments of these four countries suggest that there is a high degree of complementarity among the five concepts constituting institutional environments: when one is weakly developed, the others tend to be weakly developed and vice versa. This perspective resonates with the concept of institutional complementarity, found in a variety of work within social science literature.⁷

The institutional environment in which research organizations are embedded has an impact on organizational behavior. The stronger the institutional environment is, the greater the organizational isomorphism—a factor that results in less diversity among the types and behavior of research organizations. When organizational isomorphism is high, there are strong pressures for organizations to converge in their behavior and culture. On the other hand, in weak institutional environments, diversity is greater with regard to types of research organizations and the structure and culture of the organizations. I have found that such a society possesses greater potential for multiple scientific breakthroughs.⁸

In societies in which external controls over organizations are highly institutionalized and strong, there has been less variation in the structure and behavior of research organizations. There, the connectedness⁹ between research organizations and their institutional or external environments has generally been so strong that research organizations have had relatively little autonomy with which to pursue independent strategies and goals. Conversely, the weaker the institutional environment in which research organizations have been embedded, the greater the variation in the structure and behavior of research organizations. When the institutional environments have been more weakly developed, organizations generally have had greater autonomy and flexibility to develop new knowledge and to be highly innovative. Hence, in societies where institutional environments have been the most developed, rigid, and strong, there has been less organizational autonomy and flexibility, and fewer radical innovations have occurred in basic and applied science.¹⁰

Heterogeneity in the types of research organizations has tended to be greater in weak institutional environments than in strong ones. Hence, in the United States, with a relatively weak institutional environment, there have long been many more types of universities than in Germany, where universities that have been embedded in a strong institutional environment resemble one another much more.¹¹ In the United States, we find small, elite, private universities (Rockefeller University, California Institute of Technology, Rice University); medium-sized private universities (Johns Hopkins University, University of Chicago, Vanderbilt University, Princeton); and large private universities (Harvard, Stanford, MIT, NYU). In addition, there are the large public universities in California (Berkeley, UCLA, UCSB, UCSD) and the Midwest (Michigan, Indiana, Wisconsin, Illinois, Minnesota). Historically, each type of university featured a distinct type of population, somewhat differentiated from other types of research organizations, in part because their dominant competencies were not easily learned or transmitted across organizational populations.¹²

Of course, in both strong and weak institutional environments every organization is unique, meaning that heterogeneity always exists among organizations. But organizations of the same type, and in the

same institutional environment, are likely to share many of the same attributes.¹³ Even if weak institutional environments led to more heterogeneity among types of organizations, forces were nevertheless at work that led to increasing organizational isomorphism both across and within organizational types.

The society likely to have had numerous breakthroughs was one with a weak institutional environment that permitted a high degree of nonconformity and high-risk research. My in-depth, cross-national, and cross-temporal organizational study of 291 major discoveries in the twentieth century demonstrates that major discoveries tended to occur more frequently in organizational contexts that were relatively small and had high degrees of autonomy, flexibility, and the capacity to adapt rapidly to the fast pace of change in the global environment of science. As Table 1 illustrates, such organizations tended to have moderately high levels of scientific diversity and internal structures that facilitated the communication and integration of ideas across diverse scientific fields.¹⁴ These organizations tended to have scientific leaders with a keen scientific vision of the direction in which new fields in science were heading and the capacity to develop a strategy for recruiting scientists capable of moving a research agenda in that direction.

Table 1. Characteristics of organizational contexts facilitating the making of major discoveries.*

- Moderately high scientific diversity
- Capacity to recruit scientists who internalize scientific diversity
- Communication and social integration of scientists from different fields through frequent and intense interaction
- Leaders who integrate scientific diversity, have the capacity to understand the direction in which scientific research is moving, provide rigorous criticism in a nurturing environment, have a strategic vision for integrating diverse areas, and have the ability to secure funding to achieve organizational goals
- Flexibility and autonomy associated with loose coupling with the institutional environment

* These characteristics were derived from intense, in-depth analysis of the organizational contexts in which major discoveries either occurred or did not occur through the twentieth century in Britain, France, Germany, and the United States (see J. Rogers Hollingsworth, Ellen Jane Hollingsworth, and Jerald Hage, eds., *The Search for Excellence: Organizations, Institutions, and Major Discoveries in Biomedical Science*, 2008).

Organizational contexts featuring such characteristics were Rockefeller University, the California Institute of Technology, the Salk Institute, and the Johns Hopkins University Medical School. Scientists at the relatively small Rockefeller University made more major discoveries in basic biomedical science than any other organization in the world during the twentieth century.¹⁵

Figure 2 portrays the kind of organizational context in which major discoveries are more likely to occur. These contexts possess a moderately high degree of scientific diversity and a high level of communication among scientists in diverse fields of science. Of course, as organizations acquire more and more diverse fields of science, they run up against limits to their ability to maintain communication across diverse fields.

Even in societies with relatively weak institutional environments, most organizational contexts hampered the making of major discoveries. Over time, most research organizations tended to become relatively large and more bureaucratic. They were divided into an increasing number of scientific disciplines, and communication diminished among scientists working in the various fields within the organization (see Table 2). Unlike Rockefeller University, most research universities were structured around

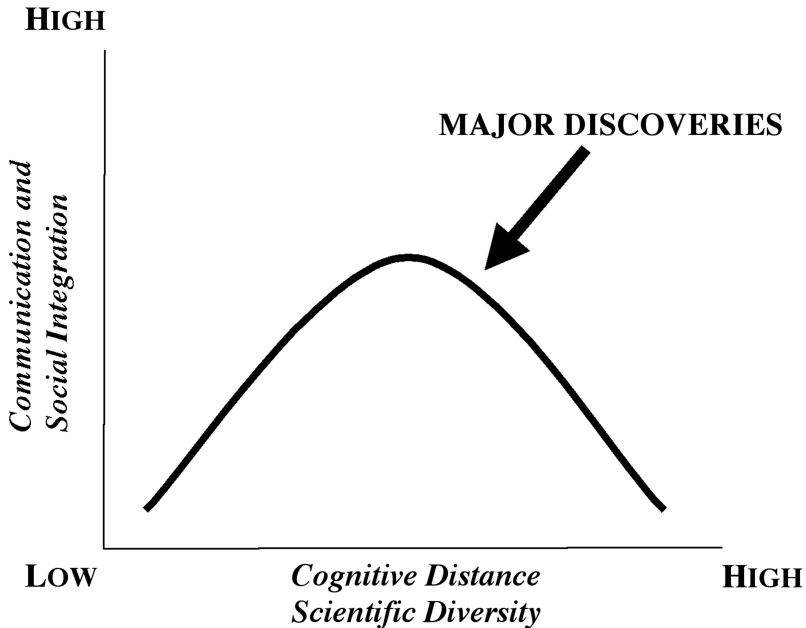
Table 2. Characteristics of organizational contexts constraining the making of major discoveries.*

- *Differentiation*: Organizations with sharp boundaries among subunits such as basic biomedical departments and other subunits, the delegation of recruitment exclusively to department or other subunit level, the delegation of responsibility for extramural funding to the department or other subunit level.
- *Hierarchical authority*: Organizations were very hierarchical when they experienced centralized (a) decisionmaking about research programs, (b) decisionmaking about number of personnel, (c) control over work conditions, (d) budgetary control.
- *Bureaucratic coordination*: Organizations with high levels of standardization for rules and procedures.
- *Hyperdiversity*: This was the presence of diversity to such a deleterious degree that there could not be effective communication among actors in different fields of science or even in similar fields.

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departments and academic disciplines: for that reason they lacked organizational flexibility and acquired a great deal of organizational inertia—since academic departments have had a tendency to continue working in the same general problem areas.

Figure 2. The Impact of communication and cognitive distance on major discoveries.



Institutional Environments and Isomorphism Within and Across Research Organizations

Societies vary in their capacity to produce major discoveries over time because they are influenced in various ways by several historical processes, notably organizational isomorphism and path dependency. Path dependency reminds us that the way things were previously organized influences the way they are organized today. Still, institutional environments, organizations, and individual actors are always changing. The stronger the institutional environment is, the greater the degree of organizational isomorphism and the greater the similarity of path-dependent

processes among organizations. Even in societies with weak institutional environments, there are forces which lead over time to greater degrees of homogeneous behavior (i.e., organizational isomorphism) across and within organizations. Different populations of organizations in the same society develop a set of competencies and routines that become institutionalized but remain societally specific. As a result of these competencies, actors in both different and similar organizations engage in a great deal of common learning and socialization. Scientists, technicians, and administrators from different types of organizations in the same society acquire a great deal of common organizational know-how that is transmitted across time and organizations. Some years ago, DiMaggio and Powell¹⁶ picked up on these ideas when they pointed out that organizations in the same society engage in many “mimetic processes.” Later, Hodgson¹⁷ developed the argument that routines are organizational metahabits which diffuse across populations of organizations within a particular institutional environment. To understand homogenizing forces across and within organizations in the same institutional environment, analysts have increasingly focused on control mechanisms of individuals in their socialization processes—although the control replicators are called many different things in the literature. Dawkins¹⁸ used the term memes, Lumsden and Wilson¹⁹ culturgens, Nelson and Winter²⁰ routines, and McKelvey²¹ comps. Whatever the term, social scientists have been focusing for some time now on the way competition among organizational actors within an institutional environment is suppressed by norms, rules, habits, and conventions at the group and organizational levels.²² Isomorphic pressures are especially strong when actors in highly saturated environments are competing for the same finite resources.²³

Isomorphism, no matter how powerful a force, certainly does not sweep through history unimpeded. It occurs at a very moderate rate, constrained by many forces. One factor retarding organizational isomorphism is the existence of diverse types of research organizations in a society. Many years ago, Stinchcombe²⁴ made the astute observation that organizations founded at different points in time, even those of the same type, are influenced in their behavior for long periods of time by many of the cultural attributes of the social technologies current at the time of their foundation. When Stinchcombe made his observation, historians

and social scientists had not explicitly developed the concepts of path dependency and organizational isomorphism, but his emphasis on how the history of organizations is permanently influenced by the moment of their creation is clearly suggestive of a path-dependency perspective. Stinchcombe was making the profound point that organizations do not necessarily closely track changes in their environment, but are somewhat inert, preserving certain nonadaptive qualities that often have deleterious effects on their capacity to be highly adaptive to their environments. Thus they resist isomorphic pressures; as a result, population heterogeneity persists despite forces leading to greater homogeneity.

There is a substantial body of literature suggesting that continuously high levels of radical innovation in modern societies require diversity in organizational forms and ideas, heterogeneity in organizational structures, and institutional environments with ample resources to nurture radical innovations.²⁵ Individual societies continuously confront contradictory pressures. On the one hand, they are subjected to processes that move organizational populations toward greater homogeneity and uniformity. On the other, homeostatic forces within populations of specific types of organizations constrain evolutionary change and preserve nonadaptive forms, facilitating organizational inertia.²⁶ If a society is to be continuously creative and make radical innovations, it must have sustained variation and diversity in organizational forms and ideas, which are more likely to flourish in weak institutional environments.

However great the forces of isomorphism among populations of organizations are, new organizational forms may continue to emerge from time to time. Unfortunately, we lack sufficient theoretical tools to specify when and where radically new organizational types will emerge. For theoretical insights into this problem, some of our best sources are the biological literature on the processes of speciation. It is useful to think of the emergence of new organizational forms as an organizational mutant. Mutations occur all the time, among both biological and organizational species. However, most do not “take hold” as they are outnumbered in their population environments, crowded out, and “rapidly dissipate through the normal intermixing process.”²⁷ Moreover, we know from numerous population-ecology studies that new organizations have low survival rates.²⁸

Thought of as a mutation, a new organizational form is more likely to survive if it occurs in organizational environments that are sparsely populated, have ample resources to support such new development, and if it is not crowded out by the normal process of intermingling with other types of organizations. New surviving forms tend initially to be relatively autonomous from their environments. In such environments, organizational speciation may occur, and in the short term, a new form may be immune to the pressures of organizational isomorphism. Environments with resources exceeding demand offer a greater opportunity for a new organizational form to survive than do more competitively saturated environments.²⁹

One example of the emergence of a new form of research organization is the establishment of several research institutions in the United States after 1960: the Salk Institute, the Scripps Research Institute (both in La Jolla, California), and the Fred Hutchinson Cancer Research Center (in Seattle, Washington). What was novel about these organizations compared with older ones (the Scripps Institute of Oceanography, the Rockefeller Institute, the various Carnegie Institutes, the Laboratory of Molecular Biology in the United Kingdom, the Institut Pasteur, the various Max Planck Institutes) was that this new form of research organization had no endowment, no permanent patron, and no assured source of financial support. These institutes, emerging in newly developing research environments in Southern California and Seattle, were managed by entrepreneurs skilled in raising money in the distinctive regional entrepreneurial landscape of the West Coast, where thousands of adventurous investors and philanthropists were in search of new, local investment niches. Traditional sources of capital—banks, the federal government, and more conservative philanthropists in the East who favored local organizations—tended to view these ventures with skepticism. Significantly, Silicon Valley emerged on the outskirts of Palo Alto, California, and the biotechnology industry also had much of its early success in the sparsely populated California landscape, not in the older centers of the United States where the industrial organizational density was quite high.

Since clusters of major discoveries tend to occur within relatively small organizations (see Table 1 and Figure 2), why is it that they may

also occur within a large organization that is separated internally into various departments? Such rare occurrences tend to take place where the following two conditions exist:

- The organization must be extremely decentralized (permitting the actors making major discoveries to enjoy a high degree of autonomy and flexibility).
- The actors within the organization must have access to sufficiently diverse types of resources so that their scientific practices and administrative routines are not crowded out by those already institutionalized within the larger environment of the host organization.

The subunits of organizations where these clusters occur tend to have most of the characteristics listed in Table 1. According to evolutionary logic, those making major discoveries in a new scientific area of research tend to be in a better position to escape the institutionalized, homogenizing pressures of the existing research organizations and to possess the autonomy to intermix, interbreed, and reproduce their own intellectual progeny within their particular subunit of the larger organization.

The occurrence of a cluster of major discoveries in an organization, especially in a single department over thirty or forty years, is extraordinarily rare. Such a cluster of discoveries occurred in the Faculty of Arts and Sciences of Harvard University between the mid-1950s and the mid-1970s following the establishment of two new departments: the Department of Biochemistry and Molecular Biology, and the Department of Organismic and Evolutionary Biology. From each of these departments came a number of major discoveries. Significantly, these discoveries occurred in new departments, not in older ones encumbered by the inertia of the past.³⁰

Over time, however, departments establish institutionalized routines, as do universities, and inertial processes set in, making it difficult for a highly creative subunit to continue being so innovative. At Harvard, as elsewhere, the level of innovativeness in a highly creative department eventually declined. Even organizations once highly decentralized, in which each subunit enjoyed a high degree of autonomy, tend to institutionalize a set of routines which diffuse across the organization, thereby establishing interlocking and conditional behaviors for all subunits of the organization. Eventually, a set of organizational routines becomes

institutionalized throughout the organization, thus establishing shared collective capabilities, capacities, and behavior. In short, the organization emerges with a distinctive culture.³¹

Initially these two Harvard departments were headed by outstanding leadership with visionary agendas and staffed by scientists researching areas that were moderately high in diversity and very highly integrated scientifically—characteristics listed in Table 1 above. Even though each scientist within the department tended to pursue a separate body of research, the work was highly complementary to the research program of the entire department, which possessed a distinctive culture that glued it together.

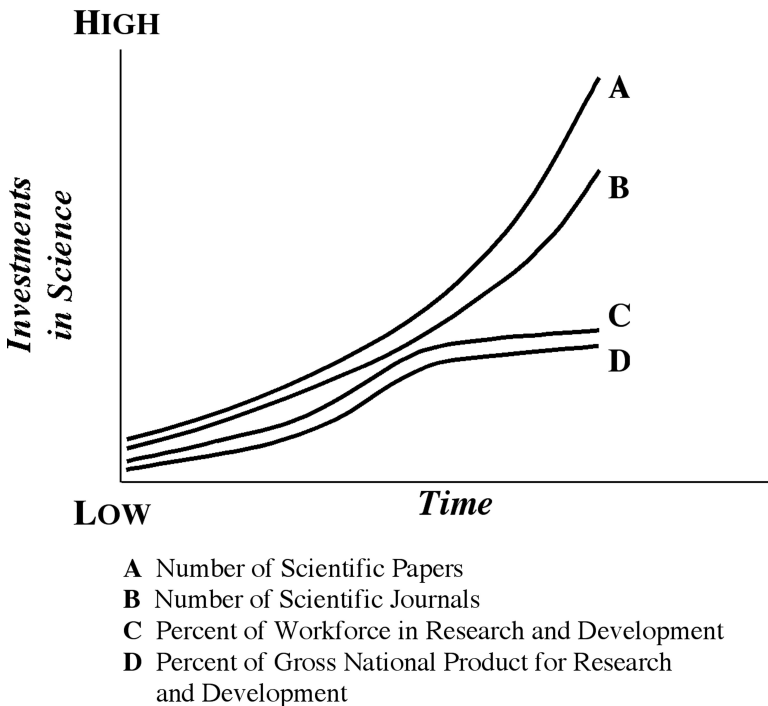
Eventually, the distinctive scientific excellence of these departments declined. The scientific agenda of the new departments diffused to other organizations throughout the world, and many of the original members of the departments retired, died, or left. As scientific practices became routinized, no other leader emerged with a radically new agenda, capable of transforming the departments again into those on the cutting edge of science. The routines of the larger organization in which the departments were embedded slowly penetrated the departments. For all of these reasons, it is difficult for a research department to remain on the cutting edge of research for more than two or three decades. It is possible for a new department with a new scientific agenda to emerge, but seldom does such a department then proceed to produce clusters of pioneering discoveries in science. Rare indeed are the equivalents of within-organization mutations that are able to “take hold.” Over the longer term, the distinctiveness of a “new departmental species” diminishes as it is constrained by the routines of the rest of the organization and other organizations in its institutional environment.

The Shift from a Weak to a Stronger Institutional Environment

Over time, the dynamics of the scientific enterprise embedded in a weak environment cause the institutional environment to become a much stronger one, and the institutional environment is transformed. In turn, the stronger institutional environment feeds back and alters the dynamics of the society’s social system of science.

What is it about the scientific enterprise that leads to such change in the institutional environment of research organizations? For some time, the world has been experiencing an enormous expansion of new information and knowledge, which in turn begets ever more information and knowledge. For well over a century, the number of scientific papers and journals has been increasing exponentially, fueled by increases in the number of scientists and financial resources for science. For many years, developed economies also witnessed an exponential increase in the number of scientists and in the percentages of gross domestic product devoted to scientific research.³² Of course, we have known since the time of Malthus that most forms of exponential growth must eventually come to a halt. No environment can continue to invest such extensive resources in scientific research: otherwise, at some point everyone would be a scientist and a society's entire gross national product (GNP) would be devoted to scientific research. Nevertheless, in all advanced industrial societies, the percentage of the population and of the GNP devoted to scientific research is continuing to increase, just not exponentially.³³

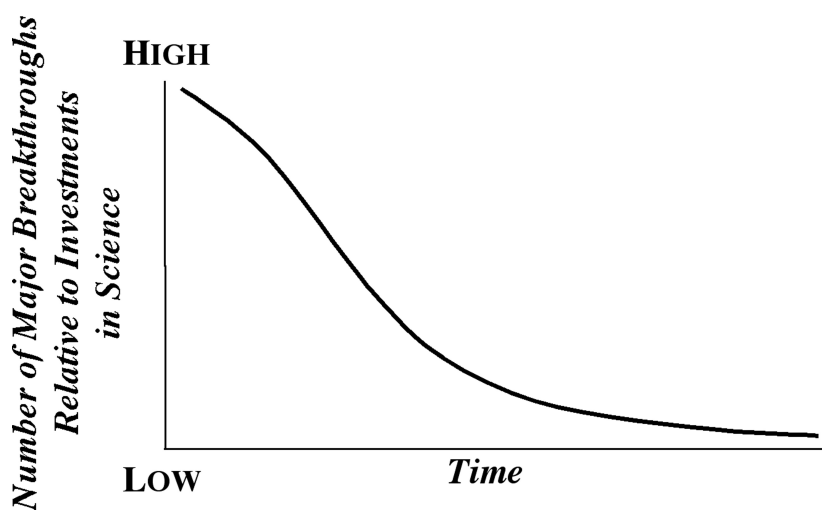
Figure 3. Historical growth of investments in science.³⁴



To understand these processes, we need to recall Max Planck's *Principle of Increasing Effort*: "with every advance in science, the difficulty of the task is increased; ever larger demands are made on the achievements of researchers, and the need for a suitable division of labor becomes more pressing."³⁵ With the expansion of knowledge comes increasing specialization, the development of new subspecialties, and the need for additional support staff. There are also increases in new instrumentation, leading to improved methods of measurement, which in turn lead to new fields of specialization and the need for even better instrumentation.

When new fields open up, the early investigators often make major breakthroughs: "the pickings are easier." As fields mature, the effort and resources required for significant advances increases continually. This constant "digging and searching" for significant findings as fields mature and broaden fuels an unending need for even more resources. As a result of this dynamic, the societal resources required to bring about a major discovery tend to increase exponentially. However, the number of major discoveries per annum increases very modestly—if at all. The idea that the number of important results stands as the square root of the total production of papers is frequently referred to as Rousseau's Law and is often attributed to Jean Jacques Rousseau.³⁶ Figure 4 illustrates the dramatic decline in the number of major discoveries relative to societal investments in science.

Figure 4. Number of major breakthroughs relative to scientific effort.³⁷



As the demand increased historically for more financial resources devoted to scientific research, central governments tended to become more involved in funding science and shoulder an increasing percentage of research expenditures, thus altering the institutional environment in which scientific research was embedded. This has had several consequences for the structure and culture of the social system of science. First, as central governments increased the proportion of their budgets spent on scientific research, politicians and government bureaucracies became more involved in making decisions about how funding should be allocated. Certain fields of science received an increasing proportion of investments while other areas were given only scant attention. Meanwhile, scientific communities have engaged in massive lobbying and public relations campaigns in an effort to influence the decisions of public officials. Second, as the amount of public sector money invested in science rose, governments increased their monitoring and auditing of research organizations in order to enhance research “efficiency and effectiveness” and to prevent fraud. Research organizations—like business firms—have increasingly become part of the “audit society.”³⁸ Third, governments have acquired a taste for assessing the social benefits of scientific research, and have increasingly expressed little interest in funding the pursuit of knowledge for its own sake.³⁹ They increasingly support research that promises payoffs “here and now,” in other words they prefer research with short-term societal benefits rather than high-risk research. Fourth, as central governments have become more involved in funding science and in making decisions about how the money should be used, research organizations have increasingly lost some of their autonomy. One consequence of these processes is an increasing convergence in the behavior of research organizations, a movement toward greater organizational isomorphism. Researchers have tended to gravitate toward scientific areas where there is funding. Since diversity in types of research organizations is associated with organizational autonomy and flexibility as well as scientific breakthroughs, an increase in the strength of the American institutional environment and the resulting greater organizational isomorphism has posed major problems for the capacity of American research organizations to continue making major breakthroughs.

Finally, the strengthening of the institutional environment of the American system of science has led to an increased commercialization of science. From a theoretical point of view, the strengthening of an institutional environment and an expanding role of the state in funding science has not necessarily led to the commercialization of science. This was quite contingent on a number of factors. In the United States case, the association between the strengthening of the institutional environment and the commercialization of science resulted primarily from the fact that the American scientific enterprise had historically been deeply embedded in a highly entrepreneurial culture.

To understand how this process has evolved in the American context, we must first recognize that social systems of science are somewhat bifurcated into public (i.e., communal) knowledge and private knowledge, and each of the two subsystems of knowledge had its own norms, incentives, and behavior.⁴⁰ Ironically, the increasing role of the government in funding science in the United States has led to a weakening in the development of public/communal knowledge/science. Historically, public knowledge has simply been knowledge owned by everyone in common. An example has been knowledge published in scientific journals to which everyone has had access: the reading of a scientific paper did not diminish its use for the next reader. This is very different from private knowledge, which is a private good, not available to all—in other words, if Jake eats his cake, no one else can eat it. If private knowledge has been patented or its use acquired by licensure, it is restricted to private use. These two systems of knowledge have had their own incentive structures. To most observers it has been relatively easy to understand the pecuniary motives of those who produce private knowledge for sale in the marketplace. But what have been the incentives that motivate those who produce knowledge owned in common by the community?

Merton⁴¹ and others⁴² observed that historically a major incentive to produce public knowledge was peer recognition. Societies bestowed rewards such as medals, prizes, and other forms of esteem on the discoverer. The scientist who was first in making a discovery received the credit, but unlike a sports tournament, there was little or no reward for being runner-up. Such an incentive system historically generated a great deal of competition among scientists and occasionally intense feuds as to who

deserved recognition for priority. To facilitate the working of an effective incentive system and to assist in adjudicating priority disputes, the international scientific community has relied on scientific elites to determine which contributions to knowledge were important ones. Table 3 briefly describes the public science sector which has existed in the United States during the twentieth century.

While modern societies have had a scientific sector that produces public goods (i.e., public sector science), during the process of modernization, a for-profit sector has also produced science and technology at an accelerating rate. In the for-profit sector, the incentives for research have been primarily monetary in nature. During the past fifty years, the particular process of American industrialization has tended to diminish the proportion of individual scientists motivated to pursue communal obligations and the production of public knowledge and to increase the proportion of scientists engaged in the pursuit of pecuniary gain.⁴⁴ This process became one of the most important forces leading to a transformation in the American system of science. Table 4 summarizes the characteristics of the for-profit science sector in the United States.

Table 3. Public sector science in the United States.⁴⁵

Property rights of scientific production: Science produced as a public good, belonging to the larger community.

Incentives to produce science: The reward was recognition of priority in discovery. Rewards came in the form of scientific awards, scientific citations, peer group esteem, salary increases.

Methods of funding: Sector funded by patrons, governments, grants, gifts, and contracts. If left to the market, the sector tended to be underfunded.

Locus of production: Heavy concentration in private non-profit and public sector organizations, although occasionally for-profit organizations produced public goods.

Vulnerability of sector: Sector tended to become bureaucratically funded over time. Funders tended to become less willing to finance high-risk projects. Research organizations became increasingly large and fragmented, hampering communication across diverse fields. As industrialization increased, an increasing proportion of scientists tended to seek monetary rewards rather than knowledge as an end in itself.

Long-term consequences of public sector science: Highly variable. Some knowledge had few or no effects on society; other knowledge often had great societal effects far in excess of the financial resources originally invested. Most major discoveries had significant payoffs only years later, though a few did have immediate benefits.

Table 4. For-profit sector science in the United States.⁴⁶

Property rights of scientific production: Most of the science produced in this sector is proprietary in nature: Patents, copyrights, and licensing agreements are widely used for defining and protecting intellectual property rights.

Incentives to produce science: The rewards tended to be primarily monetary in nature.

Methods of funding: Sector predominantly funded by market forces in the private sector. Increasingly, universities and other non-profit organizations have been licensing discoveries made with federal funds and establishing science parks for private firms with strong affiliations to universities.

Locus of production: Historically heavily concentrated in for-profit organizations but in recent years, this sector has also had increasing activity in non-profit and public-sector research organizations.

Vulnerability of sector: The sector has been heavily dependent on decision makers with short-term horizons. As a result, the sector has tended to emphasize incremental research, designed to maximize profits in the short term.

Long-term consequences of heavy dependence on for-profit sector of science: If the society becomes excessively dependent on this sector for the production of knowledge, there is not likely to be enough new, basic knowledge necessary for high technological and economic growth over the long run.

While the for-profit sector of science has been expanding in the United States throughout the twentieth century, its rate of growth dramatically increased during the past twenty-five years. Ironically, the increasing role of the government in funding science in the United States has led to a weakening in the development of public, communal knowledge and science. The passage of the Bayh-Dole Act by Congress in 1980 did much to accelerate the expansion of the for-profit sector of science. This act stipulated that intellectual property resulting from federally (i.e., communally) funded research in universities could be patented, with universities and their researchers to be the beneficiaries of resulting royalties. The act stated that universities, as a condition for receiving federal research funds, had an obligation to make a good-faith effort to transfer resulting technological knowledge to the marketplace or to make it available in some other form for use by society. As a result, university linkages with industry increased dramatically. The number of patents issued to American universities tripled in a single decade (1984-1994). Numerous universities established intellectual property transfer offices, developed adjacent science parks, and dramatically

increased their equity in firms located nearby and elsewhere. In 2000, American universities earned at least \$1 billion by conservative estimates, primarily in royalties.⁴⁶

Although an abundance of data is available on changes in patenting, the acquisition of patents by universities represents only the tip of the iceberg in the increasing commercialization of science in American research universities. My interviews with senior administrators and scientists in major research universities are consistent with other studies,⁴⁷ which indicate that patents amount to no more than 11 percent of the flow of knowledge with commercial value into the contemporary marketplace. The more important links between American universities and firms have been joint ventures between firms and individual university scientists, activities by university scientists in creating their own firms, efforts by universities and their affiliated foundations to act as venture capitalists and to become sole or part owners of new business ventures. Of course, a two-way interaction does take place between universities and firms, as firms in a number of sectors have substantially increased their investments in research conducted at universities. In recent years, major American universities have been at the forefront in developing new technologies that have spawned a transformation in the technology underlying a number of economic sectors: biotechnology, information technology, software, and computational biology. Because the knowledge created by American universities is so closely linked to these relatively new sectors, it is not surprising that the universities have been so intricately involved in their commercial development.⁴⁸

In a twenty-five-year period following the passage of the Bayh-Dole Act, the historical relationship between public sector science and for-profit sector science has been significantly altered, bringing about a transformation in the culture and behavior of American universities. Historically, universities were sites which were primarily concerned with producing science as a public good, while for-profit firms were primarily engaged in producing science and technology as private goods, although the practices distinguishing these two types of organizations were never very clear cut. While the two types of organizations had somewhat different goals and reward structures, some universities and their faculties had long engaged in producing both public and private knowledge, as

were also some for-profit organizations.⁴⁹ For example, the laboratories of AT&T and IBM had very enviable records for producing important basic scientific discoveries in the form of public science.⁵⁰ Nevertheless, in the aggregate the historical practices of American scientists in universities and in for-profit firms tended to be quite differentiated. In the past twenty-five years, the differences in the behavior of the two types of organizations have considerably narrowed.⁵¹

There is some evidence that in the short term the increasing commercialization of the American university is contributing to more technological innovations and to higher levels of economic productivity and growth. Clearly, a robust for-profit science sector has been an important stimulus to the American economy. However, sustainable increases in knowledge and technology are necessary in both public sector science and for-profit sector science. No one knows how to define the proper balance between the two sectors, but we have a great deal of evidence suggesting that, in the long run, public sector science will be underfunded should its financing be left to the market. Findings also indicate that the increasing commercialization of science and the bureaucratization of American research universities are beginning to discourage young investigators from conducting high-risk research.⁵²

It is extremely difficult—perhaps impossible—to predict what the economic payoff is likely to be from particular discoveries, even important ones. For many years there has been considerable debate about the way advances in technology influence the agenda for fundamental and basic science. Historically, a great deal of interaction and co-evolution has occurred in the development of both science and technology. Yet, for long-term economic growth, a sustainable abundance of underlying advances in fundamental or basic knowledge is necessary. Indeed, the consequences of many fundamental advances in basic knowledge were only realized in the marketplace after long periods of time.⁵³ Moreover, most of the consequences were unintended. William H. Bragg and his son Lawrence were awarded the Nobel Prize in Physics in 1915 for work in crystallography, a field of science which many decades later is now transforming the biotechnology and pharmaceutical industries. The discovery by Oswald Avery in 1944 about the importance of DNA and the later discovery of the structure of DNA by Francis Crick and James

Watson had no short-term economic impact, even though in the long run they are perceived as being two of the most revolutionary discoveries in twentieth-century biology. Decades later, these three discoveries are contributing to the development of genetic engineering and numerous other forms of biotechnology. The same argument can be made about many other basic scientific discoveries which had little economic payoff in the short term, but reaped considerable dividends decades later. No doubt our societies have yet to realize the economic rewards of many other past basic discoveries. Of course, the economic significance of a few major, basic biomedical science discoveries was quickly picked up by business firms and the discoveries soon thereafter began to yield returns in the marketplace (e.g., the 1980 Nobel Prize in Chemistry awarded to Frederick Sanger and Walter Gilbert for their work on the sequencing of DNA; the 1978 Nobel Prize in Physiology or Medicine awarded to Daniel Nathans and Hamilton Smith for their research on the role of restriction enzymes in cutting up DNA, and the 1993 Nobel Prize in Chemistry to Kary Mullis for his development of the polymerase chain reaction).

The realization of economic rewards from fundamental discoveries in the biomedical sciences is a very inefficient and unpredictable process. There is no way of knowing in advance which discoveries will make a significant contribution to the wellbeing of society or when the consequences might be realized. What is clear from the historical record is that high-risk research and fundamental basic research are necessary for the general good of a society in the long run. There is no empirical evidence to suggest that the future wellbeing of societies will be any less dependent on fundamental discoveries and high-risk research than has been the case in the past. As Wolfgang Streeck has often reminded us, institutional environments that provide strong incentives for underinvestment and overconsumption in the short term are likely to result in an under-supply of productive assets in the longer term.⁵⁴

As we consider the future of American science, it is helpful to engage in some historical perspective. In the late eighteenth and early nineteenth centuries, France was at the center of the global system of science.⁵⁵ Yet, by the middle of the nineteenth century, the center had already begun to shift to Germany, which retained its supremacy until the late 1920s.⁵⁶

The center then shifted to Britain, which retained its supremacy through World War II. Since then, the United States has been the major center of Western science. The distribution of major prizes and rankings on citation indices make it unmistakably clear that the United States has been the hegemon in world science for at least a half century.

When we reflect retrospectively on these various centers, it becomes obvious that their decline in performance relative to other countries had already started just as they were thought to be at the height of their superior performance. The elite in each of these countries were so engaged in celebrating the achievements of their system that they failed to understand that the dynamics, the structure, and the contradictions inherent in each of their systems were leading to its decline. Future analysts engaging in retrospective analysis of American science at the end of the twentieth century are likely to observe a system which by most indicators was performing extraordinarily well. Yet, in retrospect, they are likely to note that the increasing organizational isomorphism both within and across its research organizations, combined with the increasing commercialization of science, had begun to impose fundamental limits on the ability of the American system to sustain its level of excellence.

Appendix 1

Interviews with scientists who were recognized for making major discoveries in American research organizations or other research organizations discussed in this paper.*

David Baltimore, Professor of Biology, Massachusetts Institute of Technology and former President of Rockefeller University. Interview in his MIT office, 28 April 1995.

Derek H. E. Barton (Sir), Professor of Chemistry, Texas A and M University and Professor Emeritus, Imperial College, London. Interview at the Beckmann Center, Scripps Research Institute, La Jolla, California, 6 February 1998.

Alan Battersby (Sir), Emeritus Professor of Chemistry, University of Cambridge. Interview in his office, 20 March 2002.

Seymour Benzer, Professor of Biology, California Institute of Technology. Interview in his office, 30 March 1994; second interview at Cold Spring Harbor Laboratory, New York, 26 August 1995; third interview at Neurosciences Institute, San Diego, California, 17 March 1996; fourth interview in his office, 22 December 1999.

Paul Berg, Professor of Biochemistry, Stanford University School of Medicine. Interview in his office, 6 May 2003.

Michael Berridge, Senior Research Fellow, Trinity College, Cambridge and Head of Cell Signalling Programme, Babraham Institute (U.K.). Interviews at Trinity College, 9 June 1999, 24 January 2002. Interview at Babraham Institute, 29 March 2002.

J. Michael Bishop, Professor of Microbiology, Director of Hooper Research Laboratory, University of California, San Francisco. Interview in his office, 10 August 1994.

James Black, Professor, King's College London. Interview at McGill University, 23 September 2004.

Günter Blobel, Professor at Rockefeller University and HHMI investigator. Interview in his office, 12 April 1995. Subsequent interviews in his office, 16 March 2001, 18 March 2001, 21 December 2004, 12 and 14 March 2007.

Konrad Bloch, Higgins Professor Emeritus of Biochemistry, Harvard University. Interview in his office, 25 April 1995.

Bernard S. Blumberg, Professor, Fox Chase Cancer Center (Philadelphia). Interview at Rockefeller Foundation Study Center, Bellagio, Italy, 21 May 1984.

Sydney Brenner, Professor Salk Institute, and Former Director of Laboratory of Molecular Biology (Cambridge, U.K.). Interview in La Jolla, California, 7 April 2003.

Francis Crick, President Emeritus and Distinguished Professor, Salk Institute; former scientist at Cambridge University and at the Laboratory of Molecular Biology. Interviews in his office in San Diego, 6 March 1996 and 11 March 1998. Interview at UCSD, 6 June 2002.

- Renato Dulbecco, Emeritus President and Distinguished Professor, Salk Institute; Former Professor California Institute of Technology. Interview in his office in San Diego, 23 February 1996. Second interview in his office, 22 May 2000.
- Gerald Edelman, Research Director, The Neurosciences Institute, San Diego, California, and former Professor and Dean, Rockefeller University. Interviews in Klosters, Switzerland, 17 January 1995, and at Neurosciences Institute, San Diego, 13 January, 16 January, 19 January, 30 January, 14 February, 20 February, 22 February, 5 March, 16 March, 17 March 1996; 12 February 1998; 4 April, 11 April, 18 November 2000. Telephone interview, 3 April 2001.
- Manfred Eigen, Professor, Max-Planck Institut für Biophysikalische Chemie, Göttingen, Germany. Interview in Klosters, Switzerland, 16 January 1995.
- Gertrude Elion, Scientist Emeritus, Wellcome Research Laboratories, Research Triangle Park, North Carolina. Interview in her office, 17 March 1995.
- Martin Evans, Professor and Director of School of Biosciences, University of Cardiff. Telephone interview, 22 April 2002.
- Daniel Carleton Gajdusek, Chief of the Laboratory for Slow Latent and Temperate Virus Infections and Chief of the Laboratory for Central Nervous System Studies at the National Institute for Neurological Disorders and Stroke. Interview at Neurosciences Institute, San Diego, California, 11 March 1996.
- Robert Gallo, Chief of the Laboratory of Tumor Cell Biology, National Institutes of Health, Bethesda, Maryland. Interview at University of Wisconsin Union, 13 March 1994. Interview in Bethesda, Maryland, 29 June 1994. Interviews in his office, 31 August 1994, 4 September 1994, 4 March 1995, 17 November 1995.
- Walter Gilbert, Carl M. Loeb University Professor at Harvard University. Interview in Chicago, 14 October 1993. Interview in his office at Harvard University, 26 April 1995.
- Joseph Goldstein, Professor, Department of Molecular Genetics, University of Texas Southwestern Medical Center. Interview at Rockefeller University, 13 March 2007.
- Paul Greengard, Professor at Rockefeller University. Interview in his office, 16 May 2001.
- Roger Guillemin, Professor, Salk Institute. Interview in his office, May 8, 2000.
- Stephen C. Harrison, Higgins Professor of Biochemistry and Molecular Biology and HHMI Investigator, Harvard University. Interview in his office, 18 December 2002.
- Leroy Hood, Professor and Chairman, Department of Molecular Biotechnology, University of Washington (Seattle) and former Professor and Chair, Division of Biology at California Institute of Technology. Interview at his Seattle home, 29 July 1995. Telephone interview, 28 August 1996.
- David Hubel, Professor, Harvard Medical School. Interview in San Diego, California, 13 March 1998.
- Andrew Huxley (Sir), Emeritus Professor of Physiology, University of Cambridge, Former Master of Trinity College, University of Cambridge, and former President of the Royal Society. Interview in his room in Trinity College, 11 July 2000. Interviews 20 January and 4 March 2002.

- François Jacob, Senior Scientist, Institut Pasteur. Interview at Cold Spring Harbor Laboratory, New York, 24 August 1995.
- Eric R. Kandel, Director of Center for Neurophysiology and HHMI Investigator, Columbia University School of Physicians and Surgeons, member of Board of Trustees, Rockefeller University. Interview at Columbia University, 19 April 2001.
- Aaron Klug, former Director, Laboratory of Molecular Biology (LMB), Cambridge, England, President of the Royal Society, Honorary Fellow of Trinity College. Telephone interview, 24 May 1999. Interview in his office at LMB, 11 July 2000. Interview at Trinity College, Cambridge, 3 April 2002.
- Arthur Kornberg, Emeritus Professor of Biochemistry, Stanford University School of Medicine (Nobel laureate in Physiology or Medicine, 1959). Interview in his office, 5 May 2003.
- Edwin Krebs, Professor of Biochemistry and HHMI Investigator, University of Washington School of Medicine, Seattle. Interview in his office, 2 August 1995.
- Paul C. Lauterbur, Professor of Chemistry, University of Illinois. Interview in his office, 24 October 2005.
- Joshua Lederberg, President Emeritus, Rockefeller University, former Chair, Medical Genetics, Stanford University School of Medicine, and former Professor of Genetics, University of Wisconsin (Madison). Interviews at Rockefeller University, 16 September 1993, 13 April 1995; telephone interview, 27 August 1999; interviews in his office 25 January 2001, 4 April 2001.
- Rita Levi-Montalcini, Professor Emeritus of Biology, Washington University (St. Louis). Interview at her home in Rome, Italy, 15 June 1995.
- Arnold Levine, President, Rockefeller University. Interview in his office, 14 May 2001.
- Edward B. Lewis, Professor of Biology, California Institute of Technology. Interviews at Athenaeum and in his office, 25 March 1994, 21 December 1994.
- William N. Lipscomb, Jr., Professor Emeritus of Chemistry, Harvard University. Interview in his office, 16 December 2002.
- Roderick MacKinnon, Professor, Rockefeller University, and HHMI Investigator. Interview in his office, 1 March 2001.
- Bruce Merrifield, John D. Rockefeller, Jr., Emeritus Professor, Rockefeller University. Interview in his office, 11 February 2000.
- Vernon Mountcastle, Professor of Medicine, Johns Hopkins University. Interview, 7 March 1995. Second interview, 11 August 2000.
- Daniel Nathans, Professor, Department of Molecular Biology and Genetics, Johns Hopkins University, Baltimore. Interview in his office, 21 July 1997.
- Erwin Neher, Director of Department of Membrane Biophysics, Max Planck Institute for Biophysical Chemistry. Interview in his office, 15 April 2004.
- Paul Nurse, President, Rockefeller University. Interviews in his office, 24 December 2004, 13 March 2007.

- George Palade, Dean Medical School University of California, San Diego. Also former Professor at Yale University and Rockefeller University. Interviews in his office in San Diego, 7 March 1996, 13 March 1998.
- Max Perutz, former Director, Laboratory of Molecular Biology, Cambridge, England. Interview at Peterhouse College, 15 March 1997; interview at Laboratory of Molecular Biology, 11 June 1999.
- John Polanyi, Professor, University of Toronto. Interview at the Center for Advanced Cultural Studies, Essen, Germany, 5 September 2001.
- Mark Ptashne, Professor Memorial Sloan-Kettering Cancer Center and former Professor and Chair, Department of Biochemistry and Molecular Biology, Harvard University. Interview in his New York City apartment, 24 May 2001.
- Robert Roeder, Professor at Rockefeller University. Interviews at Rockefeller University, 24 April 2001, 8 May 2001.
- Harry Rubin, Professor of Molecular and Cell Biology, University of California, Berkeley. Interview in his office, 4 January 1995.
- William Rutter, Professor Emeritus of Biochemistry and Biophysics, University of California, San Francisco. Telephone interview, 11 August 1994.
- Fred Sanger, Emeritus Staff, Laboratory for Molecular Biology, Cambridge, United Kingdom. Interview at Emmanuel College, University of Cambridge, 7 June 1999.
- Philip Sharp, Chair and Professor of Biology, and former Director of Center for Cancer Research, Massachusetts Institute of Technology. Interview in his office, 3 May 1995.
- Hamilton O. Smith, Professor, Department of Molecular Biology and Genetics, Johns Hopkins University, Baltimore. Interview in his office, 21 July 1997.
- Michael Smith, former Director of Biotechnology Laboratory and Professor, University of British Columbia (Vancouver). Interview in his office, 5 February 1998.
- Oliver Smithies, Professor of Molecular Genetics and Pathology, University of North Carolina (Chapel Hill), former President of Genetics Society of America. Interview in his office in Chapel Hill, 30 March 1996.
- Solomon Snyder, Professor and Director of Neuroscience, Johns Hopkins University, Baltimore. Interview in his office, 18 July 1997.
- Jack Strominger, Professor of Biochemistry, Department of Molecular and Cellular Biology, Harvard University. Interview in his office, 16 December 2002.
- John Sulston (Sir). Former Director Sanger Institute (Hixton, U.K.), and former senior scientist, Laboratory of Molecular Biology (Cambridge, U.K.). Interview at St. John's College, Cambridge, 7 April 2006.
- Howard Temin, Professor in McArdle Cancer Laboratory, University of Wisconsin (Madison). Interview at McArdle Cancer Laboratory, 26 November 1993.
- Harold Varmus, Director of the National Institutes of Health and former Professor at University of California, San Francisco. Interview in his office, Bethesda, Maryland, 6 March 1995.

- Bert Vogelstein, Professor of Oncology and HHMI investigator, Johns Hopkins University, Baltimore. Interview in his office, 18 July 1997.
- James D. Watson, Director, Cold Spring Harbor Laboratory, New York. Interview at Cold Spring Harbor, 24 August 1995, and at Neurosciences Institute, San Diego, 20 February 1996.
- Torsten Wiesel, President, Rockefeller University. Interviews in his office, 14 April 1995, 14 July 1997, 7 February 2001, 4 May 2001, 25 May 2001.
- Don C. Wiley, John L. Loeb Professor of Biochemistry and Biophysics, Harvard University. Telephone interview, 4 November 1999.
- Edward O. Wilson, Pellegrino University Professor and Curator of Entomology, Museum of Comparative Zoology, Harvard University. Interviews in his office, 4 May 1995, 17 December 2002.
- Carl R. Woese, Professor of Microbiology, University of Illinois. Interview in his office, 26 October 2005.
- K. Wuthrich, Professor of Bio-physics, Swiss Federal Institute of Technology (ETH) (Switzerland). Interview in his office, 12 December 1994.

* Titles are listed as of the time of the interview.

HHMI = Howard Hughes Medical Institute.

Ellen Jane Hollingsworth participated in many of these interviews on both sides of the Atlantic.

Appendix 2

Oral histories and public interviews (only of individuals recognized as having made a major discovery).

- Paul Berg, Professor of Biochemistry, Stanford University. Interview available through Online Archive of California, UC Berkeley Regional Oral History Office. <http://ark.cdlib.org/ark:/13030/kt1c6001df>.
- Konrad Bloch, Professor of Biochemistry (Emeritus), Harvard University. Interview with James J. Bohning, 22 March 1993. Transcript on deposit at Beckman Center for the History of Chemistry, Philadelphia, Pennsylvania. Copy in possession of J. Rogers Hollingsworth.
- Herbert W. Boyer, former Professor, Department of Biochemistry, University of California San Francisco. Interview available through Online Archive of California, UC Berkeley Regional Oral History Office. <http://ark.cdlib.org/ark:/13030/kt5d5nb0zs>.
- British Broadcasting Corporation (BBC) audiovisual interview with Lawrence Bragg, Francis Crick, John Kendrew, Max Perutz, James Watson, and Maurice Wilkins. First broadcast 11 December 1962. Tape located in Wellcome Trust Library, London.

- Carl Cori, Professor of Biochemistry, Washington University Medical School. Interview with Harriet Zuckerman, 10 December 1963(CUL). Second interview with Paul G. Anderson, 18 October 1982 (WUA).
- André Cournand, Professor in the College of Physicians and Surgeons at Columbia University. Interview with Harriet Zuckerman, 27 September 1963 (CUL).
- E. E. Doisy, Professor of Biochemistry, University of St. Louis Medical School. Interview with Harriet Zuckerman, 12 December 1963 (CUL).
- Vincent Du Vigneaud, Professor of Chemistry, Cornell University Medical College. Interview with Harriet Zuckerman, 2 October 1963 (CUL).
- Joseph Erlanger, Professor of Physiology, Washington University Medical School. Interview with Harriet Zuckerman, 10 April 1964 (CUL).
- Martin Evans, Professor, University of Cardiff. Interview with Virginia Papaionnou, 2001. <http://www.laskerfoundation.org/awards/library/2001>. Accessed 03/10/02.
- Walter Gilbert, University Professor, Harvard University. "Autobiography." <http://www.nobel.se/laureates/chemistry-1980-2-autobio.html>.
- Steve Harrison, Professor, Department of Molecular and Cellular Biology, Harvard University. Oral History (two parts). Recorded and edited by Sondra Schlesinger, March 30, April 1, 1999. <http://medicine.wustl.edu/~virology/harrison.html>.
- Bertil Hille, Professor, University of Washington. Interview with Professor Eric Kandel, Columbia University, 1999 (HHMI).
- Leroy Edward Hood, founder and Chairman of the Department of Molecular Biotechnology, University of Washington and co-founder of the Institute for Systems Biology in Seattle, Washington. "My Life and Adventures Integrating Biology and Technology: A Commemorative Lecture for the 2002 Kyoto Prize in Advanced Technologies," [http://www.law.washington.edu/Casrip/classes/O'Connor/MyLifeandAdventures-KyotoP1%20\(Hood\).doc](http://www.law.washington.edu/Casrip/classes/O'Connor/MyLifeandAdventures-KyotoP1%20(Hood).doc). Accessed 8/18/2003.
- Gobind Khorana, Professor of Biology and Chemistry, Massachusetts Institute of Technology. Interview with H. S. Jones, July 1985 (BS).
- Aaron Klug, former director, Laboratory of Molecular Biology, UK. and former President of the Royal Society. Autobiography and Nobel Lecture from Nobel Museum website. www.nobel.se/chemistry/laureates/1982.
- Arthur Kornberg, Emeritus Professor, Department of Biochemistry, Stanford University "Biochemistry at Stanford, Biotechnology at DNAX." Oral interviews conducted by Sally Smith Hughes, Program in the History of the Biosciences and Biotechnology, Regional Oral History Office, Bancroft Library, University of California Berkeley.
- Roderick MacKinnon, Professor, Rockefeller University. Interview by Professor Christopher Miller of Brandeis University, 1999 (HHMI).

- Herman J. Muller, Professor of Genetics, Indiana University. Interview with Harriet Zuckerman, 12 December 1993 (CUL).
- Paul Nurse, Director Imperial Cancer Research Fund. Broadcast on BBC Radio, 10 February 2002.
- Paul Nurse, President Rockefeller University. Public Television interview with Charlie Rose, In New York City, December 2004 (Interview available through Office of Public Affairs, Rockefeller University).
- Linus Pauling, Professor of Chemistry, California Institute of Technology. Interview with Harriet Zuckerman, 26 March 1964 (CUL).
- Norman Pirie, Emeritus Professor, Rothamsted Experimental Research Station. Interview with W. S. Pierpoint. 27 June 1988 (BS).
- Mark Ptashne, Professor, Sloan-Kettering Cancer Center. Interview by James Watson, 1999 <http://www.laskerfoundation.org/library/ptashne/citation.html>.
- William Rutter, Former Professor of Biochemistry and Biophysics, University of California San Francisco. "The Department of Biochemistry and the Molecular Approach to Biomedicine at the University of California, San Francisco." 1998 interview. Online Archive of California, Regional Oral History Office, University of California, Berkeley.
- Frederick Sanger, Emeritus Professor Laboratory for Molecular Biology (Cambridge, England). Interview with Horace Judson, 13 November 1987 (BS).
- Frederick Sanger, Emeritus Professor Laboratory for Molecular Biology (Cambridge, England). Interview with Professor George G. Brownlee, 20 October 1992 (BS).
- Edward Tatum, Professor of Biochemistry, Yale University. Interview with Harriet Zuckerman, 23 September 1963 (CUL).
- Howard Temin, Professor, McArdle Laboratory for Cancer Research, University of Wisconsin. Interview with Barry Teicher and Margaret Andreasen, 26 July 1993 (OHAUW).
- Alexander Todd, Emeritus Professor of Chemistry, University of Cambridge. Interview with Sir Hans Kornberg, 26 June 1990 (BS).
- Harold Urey, Professor of Chemistry, University of California, San Diego. Interview with Harriet Zuckerman, 26 August 1963 (CUL).
- Don C. Wiley, Professor, Department of Molecular and Cellular Biology, Harvard University. Oral History recorded and edited by Sondra Schlesinger, 1 and 5 April 1999 <http://medicine.wustl.edu/~virology/wiley.htm>.

BS = Biochemical Society (London) Archive

CUL = Columbia University Library

CITA = California Institute of Technology Archive

OHAUW = Oral History Archive, University of Wisconsin

WUA = Washington University Archive

HHMI = web site of Howard Hughes Medical Institute. www.hhmi.org/science/neurosci

Notes

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1. To address these issues, I draw on the data from my study with Ellen Jane Hollingsworth and Jerald Hage of 291 major discoveries which occurred during the twentieth century in four countries: Britain, France, Germany, and the United States. See J. Rogers Hollingsworth, Ellen Jane Hollingsworth, and Jerald Hage, eds., *The Search for Excellence: Organizations, Institutions, and Major Discoveries in Biomedical Science* (New York: Cambridge University Press, 2008, forthcoming).
2. This way of thinking is very different from the rare paradigm shifts analyzed by Thomas S. Kuhn in *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962). Major breakthroughs about problems in basic biomedical science, as defined here, occur within the paradigms about which Kuhn wrote.
3. Depending on the scientific community to operationalize this definition, I consider major discoveries to be research that received one of the following forms of recognition: (1) the Copley Medal, awarded since 1901 by the Royal Society of London, insofar as the award was for basic biomedical research; (2) the Nobel Prize in Physiology or Medicine since the first award in 1901; (3) the Nobel Prize in Chemistry since the first award in 1901, insofar as the research had great relevance to biomedical science; (4) ten nominations in any three years prior to 1940 for a Nobel Prize

in Physiology or Medicine; (5) ten nominations in any three years prior to 1940 for a Nobel Prize in Chemistry if the research had great relevance to biomedical science; (6) prizeworthy designation for the Nobel Prize in Physiology or Medicine by the Karolinska Institute committee, which prepared a short list of possible prizewinners and recommended the winner(s); (7) prizeworthy designation for the Nobel Prize in Chemistry by the Royal Swedish Academy of Sciences committee, which prepared a short list of possible prizewinners and recommended the winner(s) (if the research had great relevance to biomedical science); (8) discoveries resulting in the Arthur and Mary Lasker Prize for basic biomedical science; (9) the Louisa Gross Horwitz Prize in basic biomedical science; (10) discoveries in biomedical science resulting in the Crafoord Prize, awarded by the Royal Swedish Academy of Sciences, if the discovery had high relevance to the biological sciences. I have had access to the Nobel Archives for the Physiology or Medicine Prize at the Karolinska Institute and to the Archives at the Royal Swedish Academy of Sciences in Stockholm for the period from 1901 to 1940. The archives are closed for the past fifty years for reasons of confidentiality, but I have used other prizes (Lasker, Horwitz, Crafoord) to identify major discoveries during the latter part of the twentieth century. I use prizes and other forms of recognition to identify major discoveries. My concern is not whether proper credit was assigned to individual scientists for major breakthroughs. Rather, I seek to understand the structure and culture of the organizational context where research did or did not result in a major discovery as described above. I have studied organizations, departments/institutes and laboratories, as well as the interactions among individuals. I am indebted to Professor Ragnar Björk of the University of Södertörn for conducting most of the research in the archives of the Royal Swedish Academy and the Karolinska Institute in Stockholm.

The research on major discoveries summarized here is based on a great deal of archival research, many interviews, and wide reading in many scientific fields. Archives have been used in the United States (e.g., Rockefeller Archive Center, American Philosophical Society, University of Wisconsin, Caltech, University of California Berkeley, University of California San Francisco, University of California San Diego, Harvard Medical School) and in Great Britain and Europe. I have conducted in-depth interviews with more than 500 scientists, administrators, and officers of major funding agencies on both sides of the Atlantic. Appendix 1 lists interviews that I conducted with scientists who were recognized for making major discoveries in American research organizations or other research organizations discussed in this paper. I also used as sources the oral histories and public interviews with individuals recognized as having made a major discovery listed in Appendix 2, as well as many others.

4. J. Rogers Hollingsworth and Robert Boyer, eds., *Contemporary Capitalism: The Embeddedness of Institutions* (Cambridge and New York: Cambridge University Press, 1997); J. Rogers Hollingsworth and Ellen Jane Hollingsworth, "Major Discoveries and Biomedical Research Organizations: Perspectives on Interdisciplinarity, Nurturing Leadership, and Integrated Structure and Cultures," in *Practicing Interdisciplinarity*, ed. Peter Weingart and Nico Stehr (Toronto: University of Toronto Press, 2000), pp. 215-44.
5. J. Rogers Hollingsworth, Karl Müller, and Ellen Jane Hollingsworth, eds., *Advancing Socio-Economics: An Institutional Perspective* (Lanham, Maryland: Rowman and Littlefield Publishers, 2002).
6. In writing this paper, I am especially indebted to a number of papers by Wolfgang Streeck who over the years has emphasized the important role of institutional environments in shaping the performance of organizations. For example, see the essays in Wolfgang Streeck, ed., *Social Institutions and Economic Performance: Studies of Industrial Relations in Advanced Capitalist Economies* (London and Beverly Hills, California: Sage Publications, 1992). Also see Richard Whitley, "Competition and Pluralism in the Public Sciences: The Impact of Institutional Frameworks on the Organisation of Academic Systems," *Research Policy*, 2003, 32: 1015-29.
7. On the concept of institutional complementarity, see Bruno Amable, "Institutional Complementarity and Diversity of Social Systems of Innovation and Production," *Review of International Political Economy*, 2000, 7(4): 645-87; Peter A. Hall and David Soskice, *Varieties of Capitalism: The Institutional Foundations of Comparative Advantage* (Oxford: Oxford University Press, 2001); Martin Höpner, "What Connects Industrial Relations and Corporate Governance? Explaining Institutional Complementarity," *Socio-Economic Review*, 2005, 3(2): 331-58; Colin Crouch, Wolfgang Streeck, Robert Boyer, Bruno Amable, Peter A. Hall and Gregory Jackson, "Dialogue on 'Institutional Complementarity and Political Economy,'" *Socio-Economic Review*, 2005, 3(2): 359-82.
8. Jerald Hage and J. Rogers Hollingsworth, "Idea Innovation Networks: A Strategy for Integrating Organizational and Institutional Analysis," *Organization Studies*, 2000, 21: 971-1004; J. Rogers Hollingsworth, "Institutionalizing Excellence in Biomedical Research: The Case of the Rockefeller University," in *Creating a Tradition of Biomedical Research: Contributions to the History of the Rockefeller University*, ed. Darwin H. Stapleton (New York: Rockefeller University Press, 2004), pp. 17-63; Hollingsworth, Hollingsworth, and Hage, eds., *Search for Excellence*.
9. On the concept of organizational and institutional connectedness, see Hage and Hollingsworth, "Idea Innovation Networks."
10. Hollingsworth, "Institutionalizing Excellence in Biomedical Research;" Hage and Hollingsworth, "Idea Innovation Networks;" Whitley, "Competition and Pluralism in the Public Sciences."

11. Abraham Flexner, *Universities: American, English, German* (New York: Oxford University Press, 1930); Joseph Ben-David, *The Scientist's Role in Society: A Comparative Study* (Englewood Cliffs, New Jersey: Prentice-Hall, 1971); Joseph Ben-David, *Centers of Learning: Britain, France, Germany, United States* (New York: McGraw Hill, 1977); Lothar Burchardt, *Wissenschaftspolitik im Wilhelminischen Deutschland* (Göttingen, Germany: Vandenhoeck and Ruprecht, 1975); Charles E. McClelland, *State, Society, and University in Germany, 1700-1914* (New York: Cambridge University Press, 1980); Charles E. McClelland, "Professionalization and Higher Education in Germany," in *The Transformation of Higher Learning, 1860-1930*, ed. Konrad H. Jarausch (Chicago: University of Chicago Press, 1983), pp. 306-320; Jonathan Harwood, *Styles of Scientific Thought: The German Genetics Community, 1900-1933* (Chicago: University of Chicago Press, 1993); Peter Lundgreen, ed., *Reformuniversität Bielefeld, 1969-1994: Zwischen Defensive und Innovation* (Bielefeld, Germany: Verlag für Regionalgeschichte, 1994); Lynn K. Nyhart, *Biology Takes Form: Animal Morphology and the German Universities* (Chicago: University of Chicago Press, 1995); Mitchell G. Ash, ed., *German Universities Past and Future* (Providence, Rhode Island: Berghahn, 1997).
12. Bill McKelvey, *Organizational Systematics: Taxonomy, Evolution, Classification* (Berkeley: University of California Press, 1982), p. 192; Howard Aldrich, Bill McKelvey and Dave Ulrich, "Design Strategy from the Population Perspective," *Journal of Management*, 1984, 10: 69.
13. J. Rogers Hollingsworth, Philippe Schmitter, and Wolfgang Streeck, eds., *Governing Capitalist Economies: Performance and Control of Economic Sectors* (New York: Oxford University Press, 1994).
14. The results reported in Tables 1 and 2 were derived from an analysis of major research universities across the twentieth century. In the United States, there were only about 15 such universities in 1920, and approximately 100 by 2000.
15. Hollingsworth, "Institutionalizing Excellence in Biomedical Research;" Hollingsworth and Hollingsworth, "Major Discoveries and Biomedical Research Organizations."
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