Hollingsworth, Joseph Rogers

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RESEARCH ORGANIZATIONS AND MAJOR DISCOVERIES IN TWENTIETH-CENTURY SCIENCE: A CASE STUDY OF EXCELLENCE IN BIOMEDICAL RESEARCH

J. ROGERS HOLLINGSWORTH

Wissenschaftszentrum Berlin für Sozialforschung gGmbH (WZB)
Reichpietschufer 50, D-10785 Berlin

J. Rogers Hollingsworth
Humanities Building, 4126
Department of History
University of Wisconsin
Madison Wisconsin 53706

Email hollingsjr@aol.com

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INTRODUCTION

This paper is a small part of a much larger historical and cross-national research agenda in which the author has been engaged for more than a decade. The agenda has confronted two major problems: (1) How does the institutional environment in which actors are embedded constrain their behavior (Hollingsworth, 1986; Hollingsworth, Hage, and Hanneman, 1990; Campbell, Hollingsworth, and Lindberg, 1991; Hollingsworth, Schmitter, and Streeck, 1994; Hollingsworth and Boyer, 1997; Hollingsworth, 2000), and (2) How do the structure and culture of organizations facilitate or hamper their innovativeness (Hollingsworth and Hollingsworth, 2000a, 2000b; Hage and Hollingsworth, 2000; Hollingsworth, Hollingsworth, and Hage, 2002 forthcoming; Hollingsworth, Müller, and Hollingsworth, 2002 forthcoming).

The paper addresses the problem of how the structure and culture of research organizations influence the creation of fundamental new knowledge. More specifically, the paper is part of a research project which is concerned with the question of why research organizations varied in their capacity to make major breakthroughs in biomedical science in the twentieth century. The perspectives that have been useful in shaping this project have come from diverse sources — the literatures on national systems of innovation, on organizational innovation, on evolutionary economics, on organizational capabilities, and literatures in the history and sociology of science. The ideas in these literatures have been refined and extended through many dozen historical case studies of major discoveries, which my colleagues and I have conducted in approximately 200 research organizations in twentieth-century Britain, France, Germany, and the United States (Hollingsworth, Hollingsworth, and Hage, 2002 forthcoming; also see unpublished sources noted at the end of this paper).

The theoretical framework of the paper is used to analyze the structure and culture of the one research organization which had more major breakthroughs in biomedical science than any other in the twentieth century: the relatively small Rockefeller University in New York City. Hopefully, this case study will shed light on the kinds of organizational strategies, structure and culture which facilitate the creation of fundamental new knowledge in very hybrid fields of science.

CONCEPTS, DATA, AND METHODS

THE CONCEPT "MAJOR DISCOVERY"

For purposes of this paper, and related research (Hollingsworth, Hollingsworth, and Hage, 2002 forthcoming), a major breakthrough in biomedical science is defined as a finding or process, often preceded by numerous "small" advances, which leads to a new way of thinking about one or more important problems (Ben-David, 1960: 828; Merton, 1961, 1973; Rosenberg, 1994: 15). This new way of thinking is highly useful for numerous scientists in diverse fields of science in addressing problems. This is very different from the rare paradigm shifts Thomas Kuhn analyzed in his classic The Structure of Scientific Revolutions (1972). Major breakthroughs about particular problems in biomedical science occur within the paradigms about which Kuhn was writing. 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. It has usually not occurred all at once, but has rather involved a process of investigation taking place over a substantial period of time and a great deal of tacit and/or local knowledge (Polanyi, 1966; Latour, 1987).

To implement the concept "major discovery," I rely heavily on the scientific community, drawing on criteria the scientific community has created to recognize major discoveries. Using a diverse set of strategies to operationalize the definition, I include discoveries which led to either the winning or near winning of a major prize. Though I rely heavily on discoveries associated with major prizes as a strategy for defining major discoveries, I am very careful to avoid a restriction to any single prize. For a discussion of the prizes (and near winning of prizes) used as indicators of major discoveries, see Note One below.[1]

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[1] The indicators of major discoveries are: (1) discoveries resulting in Copley Medal awarded since 1901 by the Royal Society of London, insofar as the award was for basic biomedical research, (2) discoveries resulting in a Nobel Prize in Physiology or Medicine since the first award in 1901, (3) discoveries resulting in a Nobel Prize in Chemistry, also since the first award in 1901, if the research had high relevance to biomedical science (this includes discoveries in biochemistry as well as an occasional breakthrough in several other areas of chemistry), (4) discoveries resulting in 10 nominations in any three years prior to 1940 for a Nobel Prize in Physiology or Medicine, or in Chemistry if the research had high relevance to biomedical science. The rationale is that this number of nominations suggests that broad support existed in the scientific community to the effect that the research represented a major scientific breakthrough even if it did not result in a Nobel Prize. Since the number of people who could make nominations for Nobel Prizes in the first half of the twentieth century was quite restricted by present day standards, this criterion of "10 and 3" represents broad recognition among nominators during those years), (5) Every year, the Royal Swedish Academy of Sciences and the Karolinska Institute each appointed a committee to study major discoveries and to propose Nobel Prize winners (in Chemistry, and in Physiology or Medicine, respectively). These two committees have made shortlists of discoveries considered to be "prize-worthy," and some of the discoveries were recognized for Nobel Prizes. I include in my population the discoveries on the shortlists through 1940 which were not recognized for Nobel prizes. My colleagues and I have had access to the Nobel Archives for the Physiology or

The emphasis on diverse fields of science is critical for my definition of a major breakthrough. Most biomedical research is highly incremental and specialized and is reported to highly specialized audiences. And, while I do not suggest that this kind of research is trivial or unimportant, its impact is more confined to specialized researchers, whereas the research which is described herein as major breakthroughs is widely recognized and highly appreciated in diverse fields of science. In short, major discoveries are types of knowledge absorbed by scientists across a number of different specialties, and major breakthroughs have tended to reflect a high degree of scientific diversity, as distinct from being highly specialized and narrow research.

Having identified a major discovery, my colleagues and I have then determined with a large number of case studies when and in what research organization(s), department(s), and lab(s) the discovery occurred. In some instances, the research organization did not have departments.

One goal of the case studies has been to identify organizations associated with major discoveries, and to allocate them "credit" for the major discoveries with which they were associated. In some cases, scientists made their major discoveries by conducting research first in one organization, before moving on to another with the same line of research. All the organizations in which scientists did work that was directly associated with major discoveries have been credited with the discovery.

The research takes note that all scientists who were engaged in making major discoveries were not always recognized by prize committees. However, this is a study about major discoveries and the properties of the research organizations where they occurred. Thus, the omission of "unrecognized" individuals by prize committees does not significantly bias our results. With one or two exceptions, unrecognized scientists were engaged in research at the same organizations as recognized scientists (Gura, 2001: 564; also see Howlett, 1998: 625–626). Our method of conducting in-depth studies of the organizations, departments, and/or labs where a major discovery occurred obviously permits us to identify those scientists who were involved but did not receive recognition by a prize committee.

This research is not a history of scientific ideas or a study of the creativity of individual scientists, although it acknowledges that discoveries were made by individuals

Medicine Prize at the Karolinska Institute and to the Archives at the Royal Swedish Academy of Sciences in Stockholm for this period, but for reasons of confidentiality, access to these archives is not permitted for the past 50 years. To capture the variety of major scientific discoveries during the later period, I also use several other criteria. I included (6) discoveries resulting in the Arthur and Mary Lasker Prize for basic biomedical science, (7) discoveries resulting in the Louisa Gross Horwitz Prize in basic biomedical science, and (8) discoveries in biomedical science resulting in the Crafoord Prize, awarded by the Royal Swedish Academy of Sciences. For purposes of this paper, I have included the above forms of recognition through the fall, 2000.

and that creativity occurs at the level of individuals (Hollingsworth, 2001). The concern of this paper is with the research laboratory and/or department and organization associated with the making of major discoveries. Major discoveries do not occur at random in organizations, labs, and departments. Rather, there are regularities in the characteristics of organizations and labs and/or departments where they occur, and these are the issues addressed in the broader research project as well as in this particular paper.

DATA AND METHODS

Once we have identified when and where a breakthrough occurred, we have then turned to a much more difficult and labor-intensive task—the determination of the properties of the organization and the laboratory where the discovery occurred. Informed both by our case studies and the theoretical literature on organizations, we structure our analyses around the following basic concepts: (1) organizational autonomy, (2) organizational flexibility/inertia, (3) scientific diversity, (4) differentiation of the organization and/or departments into subunits, (5) communication and integration across diverse fields of science, (6) leadership that has the capacity to develop strategies for integrating scientific diversity, and (7) hierarchical and bureaucratic coordination (i.e., standardization of rules and procedures). Because the key methodology is one of conducting case studies of the organizational context in which major discoveries occurred, it is possible to identify the key organizational concepts associated with major discoveries and to identify the causal ordering.

As a result of our case studies, we have identified the following organizational characteristics as most important in facilitating the making of major discoveries.

(1) **Organizational autonomy.** The capacity of an organization to make scientific appointments, engage in new lines of research, and organize new laboratories or departments according to the criteria which it develops independently of external disciplinary norms and governing authorities (for a discussion of this issue, see Hollingsworth, 2000).

(2) **Organizational flexibility**. The ability of an organization to shift rapidly to new and different research areas.

(3) **Moderate scientific diversity**. The existence of a variety of biomedical disciplines and subspecialties. For scientific diversity to exert maximal beneficial effect, there must be depth (e.g., individuals highly competent in the following task areas: theoretician, methodologist, scientist highly conversant with literature in various fields,

scientist highly competent in the latest instrumentation in diverse fields). The greater the proportion of the scientific staff who internalize scientific diversity, the greater the likelihood that scientific breakthroughs will occur. For additional details on diversity, see below.

(4) **Communication and social integration among the scientific community**. The bringing together of different cognitive perspectives through frequent and intense interaction in types of activities such as (a) joint publications, (b) journal clubs, (c) sharing meals and leisure time activities.

(5) **Leadership capacity to understand the direction in which scientific research is moving and to develop strategies for integrating scientific diversity.** Outstanding leaders have been able to engage in tasks which are both task oriented and socio-emotional in nature. At both the organizational and the laboratory level, they have been individuals with (a) strategic vision for integrating diverse areas and for providing focused research, (b) ability to secure funding for these activities, (c) ability to recruit sufficiently diverse personnel for research groups to be constantly aware of significant and "doable" problems, (d) ability to provide rigorous criticism in a nurturing environment, (e) capacity to orchestrate a diverse group of scientists in the present and at the same time to orient a scientific staff toward future directions.

While the diversity of perspectives creates problems of communication because of the cognitive distances among individuals, in organizations having recurrent major discoveries leadership plays a critical role in providing the means to overcome these difficulties. Our data demonstrate that one important function of the emotionally supportive leader is to encourage people to take intellectual risks and to participate in an open give-and-take climate of communication. Under these circumstances, hidden assumptions are often expressed, implicit knowledge may become codified, and radical ways of thinking about problems are more likely to slowly emerge.

(6) **Recruitment.** Organizations which have major discoveries time and time again tend to be ones where there is a moderately high level of scientific diversity which is well integrated. Organizations which have this kind of routine tend to recruit for permanent positions scientists who internalize moderately high levels of diversity at the time of their permanent appointments. These scientists tend to have very broad research interests.

If an organization is to make radical breakthroughs across time, it needs to be not only flexible but also ambidextrous in its internal operations—both taking the necessary incremental steps so that strategy, structure, leadership, and personnel are linked to one another in a fairly harmonious fashion on a day–to-day basis AND taking radical or revolutionary steps to look beyond the present so that the organization can quickly adapt to significant changes in the environment. The organization which can make major discoveries time and time again is one that can do both of these things (Hollingsworth, Hollingsworth, and Hage, 2000 forthcoming; Hage and Hollingsworth, 2000; Tushman and Romanelli, 1985; Tushman and O'Reilly, 1996; Romanelli and Tushman, 1994; Porter, 1991).

Science is very dynamic, constantly changing. Research areas that have been lively forums for research can quickly subside. Changes in technology and the development of new instrumentation often have the unintended consequences of permitting scientists to pose new questions and provide new solutions to older problems. As well, research organizations that make major discoveries time and time again must have the flexibility to make careful adjustment to fluctuations in governmental policies and sources of funding, and to volatility in the national and international economy. Coping successfully with constantly changing environments requires organizations to modify their strategies and their structures, encountering risks as they unbalance their existing systems. Such adjustments are necessary if research organizations are to do more than simply engage in incremental research addressed to highly specialized audiences.

An organization accustomed to the delicate balancing of strategy, structure, leadership, and personnel cannot lightly alter one of these components, lest its operating system be endangered. Within organizations, change in organizational strategy, structure, and leadership styles is often unwelcome, even resisted, inasmuch as it requires moving away from norms, habits, and conventions which had previously been effective. Organizations develop myths and cultural heroes, not easily set aside, and the development of new strategies and structures can be very stressful. Once such shifts are made, they can be perpetuated only if they are institutionalized, so that the mix of strategy, structure, leadership, and personnel enters a new balance (and eventually a new period of equilibrium).

With most research organizations, it is only after there has been substantial decline in organizational performance that there is a realization that fundamentally new strategy, structure, personnel, and leadership are needed if the organization is to have the potential to remain at the frontiers of science. An organization which can anticipate the need to make radical changes at the same time that it is vigorously continuing its productive present is an unusual organization. Rockefeller Institute/University—the subject of this paper—was highly successful in making numerous major breakthroughs over the last century precisely because of its ability to carry out this dual activity: to maintain its established practices at high levels of performance, and to look beyond the near future to make radical changes in its practices.

The achievements of the Rockefeller have been very uncharacteristic of research organizations across the globe. Most organizations tend to experience a great deal of inertia, generally failing to make radical changes in their structure, strategy, leadership, and personnel as environments change. Even organizations that have performed well have a tendency to congeal in self-congratulation, idealizing their own practices to such an extent that they do not adequately modify themselves in response to environmental change.

Leaders in research organizations are confronted with a dilemma. Over the short term they must strive to have congruence between the strategy, structure, and personnel of an organization. But over the long term, the leadership must be sufficiently able to reorient the strategy, structure, and personnel of the organization if it is to remain at the frontiers of science, the boundaries of which are always changing. In other words, the high performing organization is subjected to contradictory pressures—it must strive to keep strategy, structure, personnel, and leadership aligned, but in order to adapt to the fast pace of change in the global world of science, the leadership must at the same time constantly alter strategies, structure, and personnel.

In our research, we have also studied a control group of laboratories and research organizations (Hollingsworth, Hollingsworth, and Hage, 2002 forthcoming). These are research organizations which enjoy high reputations as places which conduct excellent science, but they have rarely had scientists who have made major breakthroughs in biomedical science. In these organizations, we have also done in-depth studies of the laboratories headed by distinguished scientists (e.g., scientists who are members of national academies but have not made a major breakthrough). Our research has revealed that the following organizational properties have hampered the making of major discoveries:

(1) **Differentiation**. Differentiation is concerned with sharp boundaries among scientific areas, that is, with formal, structural properties of units, such as (a) the number of biomedical departments and other kinds of units, (b) delegation of recruitment exclusively to the department or other subunit, (c) responsibility for extramural funding solely at department or other subunit level. Organizations which are highly differentiated into departments which, in turn, are fragmented into subspecialties tend to recruit scientists who are highly specialized and somewhat narrow in their research interests.

(2) **Hierarchical authority and bureaucratic coordination**. This involves (a) centralized budget controls, (b) centralized decision-making about research programs, (c) centralized decision-making about number of personnel, (d) standardization of rules/procedures.

(3) **Hyperdiversity**. The presence of scientific diversity to such a deleterious degree that there cannot be effective communication among actors across diverse fields of science.

Significantly, these properties have been more pronounced in very large research organizations. Larger organizations have tended to be highly differentiated and bureaucratic, and thus rarely to have major breakthroughs in biomedical science.

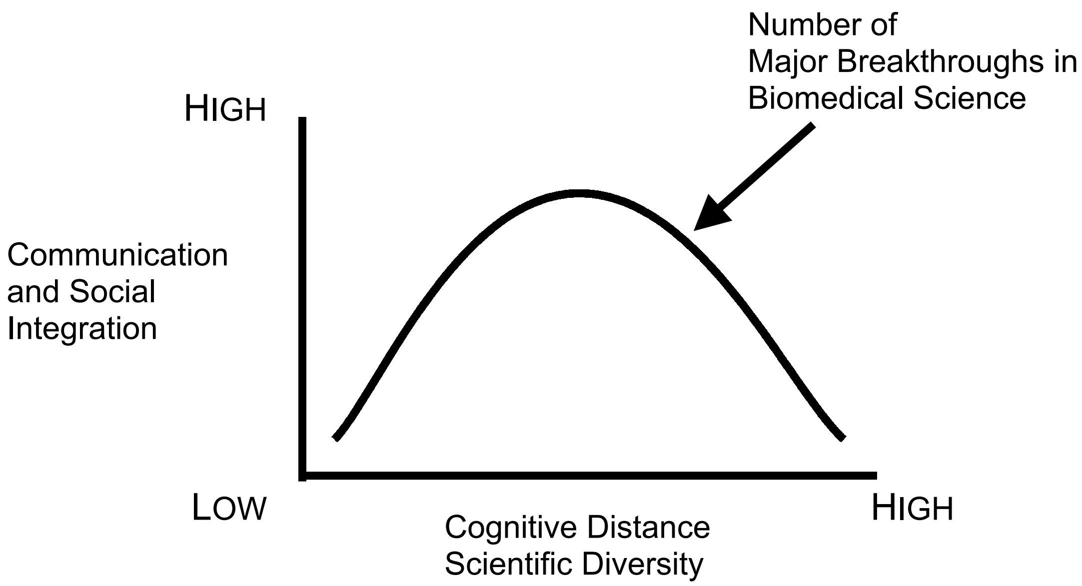
Figure One below portrays one of the most important processes relating to discovery in biomedical research organizations. It applies to research organizations having few or no major discoveries as well as to those having major discoveries time and time again. In organizations where there is little scientific diversity, there is unlikely to be radical or highly innovative thinking about existing problems. If there is little scientific diversity, there is little potential for novelty. Most scientific research is incremental in nature and tends to take place in organizational environments (laboratories) where researchers operate in a relatively narrow framework. In contrast, major breakthroughs, even when based on numerous experiments of an incremental nature, tend to take place in organizational environments in which there is intense and frequent communication across moderately high levels of scientific diversity (Hage and Hollingsworth, 2000; Hollingsworth and Hollingsworth, 2000; Hollingsworth, Hollingsworth, and Hage, 2002 forthcoming). Having scientists concentrated in the same mind-set is inhibiting to the making of major discoveries. What is needed for major breakthroughs is to have scientists bring together varied complementary skills—with intense and frequent communication—in order to see problems from new perspectives.

Our data have also revealed that the organizations with numerous major discoveries have tended to be sites where scientists from diverse backgrounds have been able to engage in intense and frequent communication with each other (see Figure One). Simply expressed, when scientific diversity remains unintegrated, the stimulation of varying perspectives is not realized. Therefore, the issue of how to integrate diverse ways of thinking and to facilitate the communication of tacit knowledge (i.e., to codify it) becomes a critical one in research organizations if they are to have major breakthroughs in biomedical science time and time again. This kind of communication is advanced by the social integration of scientific staffs.

FIGURE ONE

The Effect of Degree of Communication and Cognitive Distance

On Making Major Breakthroughs in Biomedical Science



In our research there have been only six organizations worldwide which have had large numbers of major breakthroughs in biomedical science in the twentieth century, so many breakthroughs that we refer to them as "national champions." These are the Institut Pasteur in France, the University of Cambridge in Britain, and in the United States, California Institute of Technology, the College of Arts and Sciences at Harvard University (as distinct from the Harvard Medical School), the Johns Hopkins University School of Medicine, and Rockefeller University. However, Rockefeller University led all other organizations in the number of major breakthroughs in biomedical science across the twentieth century. What follows is a case study of Rockefeller University, designed to shed some light on how it achieved and retained this level of excellence.

INSTITUTIONALIZING AND MAINTAINING A CULTURE OF EXCELLENCE

Understandably, leaders and scientists of the Rockefeller did not always correctly understand and respond to the changes in the global world of science. They did not always anticipate the major institutional and scientific patterns of change in America, nor did the organization consistently have the same level of excellence in its flow of ongoing work. Even so, why would Rockefeller University—a small organization in New York City not known by millions of Americans—have had more major discoveries in biomedical science throughout the twentieth century than such renowned organizations as Harvard, Yale, the University of California at Berkeley, and Stanford University in the United States; Cambridge and Oxford Universities in Britain; and the Pasteur Institute in Paris? And, in the biomedical sciences, why would it have had more major breakthroughs in biomedical science than all the Kaiser Wilhelm and Max Planck Institutes combined (see Appendix One for Rockefeller scientists who made major breakthroughs in biomedical science across the last century)? The key to this problem requires some comprehension of the development of a culture of excellence at Rockefeller shortly after its foundation.

To understand the origins of the Rockefeller organization, we need to recognize that in the latter part of the nineteenth century, the United States lagged far behind the frontiers of biomedical science as they existed in Europe—especially in Germany. Throughout the nineteenth century, the United States did not have high standards in biomedical science and did not provide high quality training for young scientists and physicians. Most medical schools had few if any laboratories, they were rarely affiliated with hospitals or universities, and their capital outlays were generally quite small (Flexner, 1910). Of course, some schools were better than others, and a few Americans made contributions to medical knowledge, but overall even the best medical schools and centers for research in the United States were inadequate. Among the best were those of the University of Pennsylvania, Harvard, Yale, the University of Michigan, and Columbia. Though by 1900 they were progressing in basic science research, the training and research at each of these schools fell far short of the best medical advances of the time. Since Germany had become the center for scientific medicine in the late nineteenth century, more than 15,000 Americans went there between 1870 and 1914 to study the medical sciences. In the history of American biomedical science, few things are of greater importance than the traveling of this generation of Americans to Germany, especially in the 1870s and 1880s. They attempted to bring back what they found, and it was these German institutions and ideas that ultimately revolutionized science and clinical medicine in the United States (Flexner and Flexner, 1941; Eliot, 1923: 28, 35; Ludmerer, 1986; Hollingsworth, 1986: 82–125).

All of this must be set in the context of the major institutional changes taking place in the United States during the second half of the nineteenth century. There was a transformation in the American economy, which produced unprecedented wealth in America. John D. Rockefeller, Sr., one of the most important entrepreneurs of American capitalism, believed that "the power to make money is a gift from God ... to be developed and used to the best of our ability for the good of mankind. I believe my duty is to make money and still more money and to use the money I make for the good of my fellow man according to the dictates of my conscience" (Collier and Horowitz, 1976: 48). To assist in his philanthropic activities, Rockefeller chose Frederick T. Gates, a Baptist minister, who quickly immersed himself in understanding Rockefeller's business and philanthropic affairs. Gates had a keen interest in medical matters. In an effort to learn more about the best medical practices of the day, he read the famous The Principles and Practice of Medicine by William Osler, one of the great medical men of the day but a "therapeutic nihilist" who believed that physicians knew very little about the specific cures of most diseases. Osler's pessimism about the capacity of physicians to cure illness led Gates to conclude that there was an urgent need to improve the state of biomedical knowledge.

Very much aware of the Institut Pasteur in Paris and Robert Koch's institute in Berlin, in 1897 Gates submitted a proposal to Rockefeller in which he argued that although various departments of natural sciences had been generously endowed in American universities, medicine, because of the commercial orientation of medical schools, had not been well supported. As a result, medical research was in a poor condition in America, usually conducted by practitioners who could at best steal a short time from their private practices. He argued that medicine could hardly hope to become a science until research was properly endowed and dedicated scientists were permitted to have uninterrupted study and investigation, completely independent of clinical practice. To achieve these ends, he proposed the establishment of a research institute. With Rockefeller's son, John D. Rockefeller, Jr., a new graduate of Brown University, Gates developed a wonderful working relationship in managing the Rockefeller philanthropic affairs in general, and more specifically, in the development of a biomedical research institute (Gates, 1977: 165, 179–188; Brown, 1979).

As both men quickly realized that they needed the advice of people with a rich knowledge of medical science, they sought out a distinguished group. Rockefeller Jr. consulted with his family physician, Dr. Emmett Holt, a renowned pediatrician and author of the leading textbook on pediatrics; William H. Welch, the Dean of the Johns Hopkins Medical School; Christian Herter, a well known scientific investigator who had established his own private laboratory in New York City for his research in biochemistry, pharmacology, and bacteriology; Hermann M. Biggs, head of the Division of Bacteriology of the New York City Department of Health; T. Mitchell Prudden, Professor of Pathology at the Columbia University College of Physicians and Surgeons (who had known Welch since their student days in Germany in the 1870s); and Theobald Smith of the Harvard Medical School, the leading American bacteriologist of the day and a former student of Welch's. Welch suggested the addition of his former student at Johns Hopkins, Simon Flexner, then Professor of Pathology at the University of Pennsylvania. Eventually these people became members of the Board of Directors of the Rockefeller Institute (Rous, 1949: 417; Corner, 1965: 35; Clark, 1959b, Zinsser, 1937).

Each had training in pathology, and each believed that the best strategy at the time for attacking disease was to use the tools of bacteriology. Each also thought that it was only a matter of time before physiology and chemistry would have to be integrated with bacteriology in order to advance knowledge of diseases. Despite common elements in their scientific backgrounds, the group represented diversity in biomedical science, which in the long term was to prove valuable to the Rockefeller Institute. Herter had extensive experience in research and in clinical practice; Holt in hospital clinical practice; Biggs and Smith had backgrounds in public laboratories, and Smith, Welch, Prudden, and Flexner were researchers in university medical schools. From the very beginning there was very high trust and camaraderie among these men, and this is extremely important in understanding the early success of the Institute.

One of the most important contributors to the success of the Rockefeller Institute was William H. Welch, who had already been instrumental in establishing Johns Hopkins as the premier medical school in North America. The more Welch reflected on the idea of an institute, the more he was impressed by its potential, so when he was pressed by Rockefeller Jr., Gates, and the small group of scientific advisors, he agreed to serve as president of the new institute's board, a position he held for thirty-two years. The choice of Welch as President of the Board is indicative of the capacity of the Rockefellers to recruit excellent people to implement their programs. Some have argued that Welch had more influence in developing the biomedical sciences in the United States than anyone else in American history. In addition to serving for more than three decades as President of the Board of the Rockefeller Institute, he served as President of the American Association for the Advancement of Science and was the first President of Pathologists and Bacteriologists, President of the American Medical Association, President of the Association of American Physicians, Member of the Board of Trustees of the Hooper Foundation for Medical Research at the University of California, President of the National Academy of Sciences, one of the key individuals in establishing the National Research Council, Member of the

Board of Trustees of the Carnegie Institution in Washington from 1906 to 1934, and Chairman of its Executive Committee from 1909 to 1916. As a key figure in these organizations and as President of the National Academy of Sciences, Welch had better contact with the elite in all fields of American science than anyone else. In his own field of pathology, virtually no important university post in America was filled without his approval in the first third of the twentieth century (Fleming, 1954: 131–137; Flexner and Flexner, 1941). The Rockefellers could not have made a better choice than Welch to steer the initial board of the Rockefeller Institute. He had an excellent understanding of the direction in which biomedical science was moving, high visibility and legitimacy in American science, and outstanding judgment about the type of scientists and the specific scientists whom the Institute should recruit.

One critical decision was what kind of organization the Rockefeller Institute should be. Because of their academic backgrounds, Welch, Prudden, Flexner, and the other scientific advisors tended to think the new organization should be linked to an existing university. However, the Rockefellers and Gates were insistent that there be no formal link with a university. The idea of the Institute, as conceived by the Rockefellers and Gates, laymen who had become sensitive to the shortcomings of medical knowledge, was that biomedical knowledge could not be substantially advanced until an organization existed with proper endowment, and with qualified scientists with adequate salaries, independent of private medical practice and university teaching, and devoting all its time to research (Flexner, 1930: 458; Flexner and Flexner, 1941; Gates, 1977).

THE DIRECTORSHIP OF SIMON FLEXNER

Welch proposed Simon Flexner as the first director. Here too, a better choice could hardly have been made. Flexner had many of the traits of other leaders who have served various Rockefeller family philanthropies during the past century—of humble origin, not having attended the nation’s most distinguished academic institutions, but ultimately a leader in the world of scholarship. Flexner, the fourth of five children, was born to Jewish parents in Louisville, Kentucky, and grew up in semi-poverty after his father died prematurely. Shortly after receiving his medical degree at an undistinguished medical school in Louisville, he went to Johns Hopkins in the early 1890s—though not as a regular medical student—where he became associated with Welch. Quickly, the breadth of his education increased. With the backing of Welch, Flexner also went to Europe and studied in some of its great laboratories. He worked in the laboratory of one of America’s most distinguished biomedical scientists, Jacques Loeb at Woods Hole. Slowly, Flexner was integrating medical and biological science, so that by the time he became the director of the Institute he had demonstrated that he was a creative and productive scientist, capable of absorbing and integrating new and complex ideas, that he had the capacity to develop and implement new research programs, and that he could operate on many fronts at once (Blumenthal, 1991).

Appointed as the first director in 1901, Simon Flexner left an indelible mark on the Institute. As a result of his efforts and achievements, the expectation developed among the Trustees of the Rockefeller that it should be headed by an individual who had a strategic vision for integrating scientific diversity, who could create an organizational environment which blended criticism and nurturing, who had the capacity to recruit personnel alert to significant problems and able to solve them, and who was able to secure funds. Over the past century, not all subsequent heads of Rockefeller have lived up to the leadership standards which Flexner epitomized, but these are the ideals by which successive leaders have been evaluated.

Whereas the Koch Institute in Berlin and the Institut Pasteur in Paris were founded around great scientists and their research, the Rockefeller Institute was a new kind of research organization which from the beginning emphasized diversity in the biomedical sciences. Instead of focusing on one specific area of science, the Institute pursued research in multiple areas of the biomedical sciences. Biomedical research was changing very rapidly by the time the Rockefeller Institute was established. Because bacteriology had become closely linked with pathology, and both fields were becoming more closely related to discoveries in organic and physical chemistry, as well as in physics, a broad conception of biomedical science was the guiding philosophy of the Institute from the beginning.

The recruiting of an initial staff proved to be very difficult. Most senior professors in leading American medical schools and universities viewed the Rockefeller Institute as a high-risk "upstart," and they were unwilling to leave the security of their permanent positions for it. Most young people, hoping for careers at established universities, also viewed the Institute with some suspicion. Moreover, many medical practitioners perceived the scientific agenda of the Institute as an assault on their legitimacy. Indeed, the dominant tone of the medical profession of New York City was quite hostile to basic biomedical research (Rous, 1949: 417).

For some years, there had been vicious attacks in the American press by Henry Demarest Lloyd, Ida Tarbell, and others against "Rockefeller money" and the Standard Oil Trust, and at the time of planning for the Institute, much of the nation's press was hostile to Rockefeller. Young scientists were frequently warned to steer clear of the Institute. Peyton

Rous, a future Nobel laureate from the Institute, and Jacques Loeb, one of the most important scientists in its history, had initial reservations about going there because they feared they might not have freedom to conduct their research (Pauly, 1987: 134–136; Rous, 1949: 418).

Flexner thought it necessary to recruit not only young people but also at least a few experienced scientists. But since senior American professors were unwilling to accept positions there, the initial members of the staff were a “motley group” (Rous, 1949: 418). The resulting senior staff had outstanding ability, but no involvement with established American universities and academic disciplines, which proved to be a blessing in disguise.

For the most part, the choices for initial appointments were opportunistic and personalistic in nature. Flexner placed considerable emphasis on the recruiting of scientists whose origins were in different cultural and scientific areas (e.g., Alexis Carrell from France; Karl Landsteiner from Austria; Hideyo Noguchi from Japan; Phoebus Levene and Samuel Meltzer from Russia; Jacques Loeb, Leonor Michaelis, and Max Bergmann from Germany). Several were of Jewish origin in an era of strong anti-Semitism. Almost every one of these scientists internalized cultural diversity in his own cognitive makeup, which increased the potential for crossing scientific disciplines. Feeling an affinity for others who crossed academic discipline boundaries, the early-appointed scientists established and reinforced a culture based on broad, interdisciplinary approaches to scientific problems.

From the beginning, the Institute did not organize the production of knowledge around academic disciplines, the usual practice in major universities. In organizations in which academic disciplines were dominant for organizing and coordinating the production of knowledge, there was a tendency to recruit specialists in disciplines, scientists who by definition internalized less scientific diversity (and, often, less cultural diversity). The distinctive Rockefeller recruitment of scientists socialized in several cultures, subsystems, disciplines, or working environments meant a staff with more potential to acquire new styles of thought and scientific competence. From the outset, Rockefeller was a place where scientists were willing to participate in multiple scientific worlds simultaneously, fostering the cross-fertilization of ideas and the opportunity for communication across diverse fields of research. These conditions facilitated the development of the hybridization of ideas which over time leads to scientific creativity, sudden insights, and the opening of novel pathways to difficult problems.

As a research organization, the Institute had several distinct advantages over most universities. Most teaching organizations attempt to present an entire field of knowledge to their students and find it awkward to neglect certain sub-fields. They tend to recruit people less because of their research excellence than because of the necessity to cover a particular area of knowledge. Unlike a university, a research institute has no obligation to cover an entire field of knowledge, and it can be very opportunistic in terms of the fields on which research is undertaken. It can neglect or pursue fields, can recruit scientists solely on the basis of their ability to attack selected problems, and has the flexibility to move into new areas with considerable rapidity. Moreover, the Rockefeller Institute had the luxury of being able to recruit scientists of excellence even if they had limited ability to speak English or could not teach (Flexner, 1930).

Generous financial endowment by the Rockefellers created excellent working conditions for scientific research. A number of other institutes founded at about the same time were also very well endowed: the Phipps Institute in Philadelphia, established by the steel magnate Henry Phipps; the Memorial Institute for Infectious Diseases in Chicago, funded by Harold McCormick, a son-in-law of John D. Rockefeller; and the Carnegie Institution in Washington, endowed by Andrew Carnegie. One could list more. However, the history of the Rockefeller Institute, viewed in a comparative perspective, suggests that while financial resources are necessary for outstanding research, they are less important than having the right strategy, structure, personnel, and leadership. An organization must be able to do excellent work on a continuing, day-to-day basis, but at the same time must be willing to reorient itself continually so that it anticipates scientific change. The Rockefeller Institute, with its laboratory structure, was ambidextrous enough to be able to adapt quickly to its environment, to move in new directions at the same time as it was carrying on more established lines of research. If a research organization is to have major discoveries time and time again, one of its most important resources is the quality of its leadership, a variable to which most organizational sociologists give scant attention. Over the years, the Rockefeller has had several directors/presidents who were capable of interacting in a meaningful way with its scientists and who personally knew the leading biomedical scientists of the world. Of the eight directors or presidents since the founding of the Rockefeller Institute, six made major discoveries in biomedical science and the two who made no major biomedical discoveries (Detlev Bronk and Fred Seitz) were distinguished scientists who had been President of the National Academy of Sciences. Four of the eight were Nobel laureates in physiology or medicine.[2]

Originally, a single Board of Directors designed and implemented plans for the development of the new institute, but in 1910 Welch, who had been President of the Board

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2 Simon Flexner served as Director until 1935 and Herbert Gasser from 1935 until 1953. After 1953, the title Director was dropped in favor of President. Detlev Bronk served as President 1953–1968, Fred Seitz 1968–1978, Joshua Lederberg 1978–1990, David Baltimore 1990–1991, Torsten Wiesel 1991–1998, and Arnold Levene 1998 to the present.

of Directors, became President of the Board of Scientific Directors, and due in part to the prestige he lent the Institute, some of America's leading biomedical scientists subsequently agreed to serve on the Board. Over time Board members included future Nobel laureates Vincent du Vigneaud, Herbert Gasser, and George Whipple, alongside three other scientists who made major breakthroughs in biomedical science: Walter B. Cannon of Harvard, Ross Harrison of Yale, and Theobald Smith of Harvard (mentioned above). In addition, Detlev W. Bronk, President of Johns Hopkins University, and James B. Conant, President of Harvard, served on the Board.

The Board of Scientific Directors was responsible for the appointment of the scientific staff and for the establishment of general policies concerning the scientific investigations, and it did much to keep the research organization performing at a very high level. The Director of the Institute (Flexner) was appointed by the Scientific Directors and was in intimate contact with the scientific staff. Beginning in 1910, there was also a Board of Trustees which had oversight over the financial affairs of the Institute. The distinctive role of the Board of Scientific Directors, combined with the skills of Flexner and his successor Herbert Gasser, facilitated the recruiting of some of the best scientists assembled at that time. Permanent appointments had to be approved by both Boards, though the Director appointed non-permanent members of the staff, determined their rank, and fixed their salaries. Flexner personally interviewed every scientist, even those who were at the Institute for only a single year, before any appointment was made.

The original Board of Trustees consisted of Rockefeller Jr., Gates, and their lawyer Starr J. Murphy. Welch and Flexner served on both the Board of Trustees and the Board of Scientific Directors, providing communication between the two boards. The existence of two boards lasted until 1953, when the two were merged into a single Board of Trustees. Since then, the Rockefeller organization has not had a separate board of world-class scientists making the final decisions about personnel. The quality of recruitment, while continuing to be of high quality, has not had the same degree of extraordinary consistency as during the time when the Institute had a Board of Scientific Directors with distinguished scientists intimately involved in making staff appointments and rigorously overseeing the scientific research of the Institute. The Board of Scientific Directors met three or four times a year at the Institute and focused in great detail on the quality of appointments at the Rockefeller. Since 1953 there have been scientists on the Board of Trustees, but they have never exercised the same degree of rigorous oversight over the organization's appointments as was the case when there was a separate board for scientific affairs.

At the Institute, permanent scientists were called Members and had indefinite appointments corresponding to professorial appointments in American universities. The next highest title was Associate Member, a three-year appointment renewable for three more years. In general, after a second three-year term, Associate Members either were appointed as Members or were expected to resign. Associates were appointed for two years, and assistants and fellows for one. Eligible for reappointment, scientists in the lower ranks left the Institute or rose to higher rank after three to five years. Like the Kaiser Wilhelm or Max Planck Institutes, which later had similar promotion policies, the Rockefeller provided advanced training for what became the elite of biomedical scientists in America. The process of appointment as a Member was extremely rigorous. Not all senior scientists received their initial appointments at that level. For example, in 1934, 46 percent of senior scientists had initially been appointed to the rank of Member, while 54 percent had been promoted from a lower rank. Members rarely left: only one had resigned by the mid-1930s, and even he returned.

Although the Board of Scientific Directors was responsible for the scientific policies of the Institute, the research problems under investigation were chosen by individual scientists. The Members and their associates and assistants made up groups which were called laboratories, and which were grouped into divisions. Funds for scientific investigations were made to laboratories and administered by the head of the laboratory, though budget allocations were, of course, modified from time to time by special action of the Board of Scientific Directors.

The meetings of the Board of Scientific Directors were held quarterly, and for these meetings, the head of each laboratory or division submitted a technical scientific report. Discussion of the reports became the most important item of business at the meetings. The reports played an important role in communicating to the Board what was going on in the Institute. Members of the Board of Scientific Directors generally attended all meetings, and they read the reports carefully. Some members of the Board made a habit of consistently visiting various laboratories, not in the spirit of monitoring but because of their genuine interest in the Institute's research. An important incentive to be a Board member was the opportunity to be informed about the Institute's research activities (Rivers, 1967: 198). In short, the Board of Scientific Advisors played an important role in rigorously evaluating the quality of research at the Institute.[3]

During the first three decades of the Institute, its overall governance and operating procedures were reflections of Flexner's philosophy, ideals, personal mannerisms, and scientific style. The insistence after 1910 that every laboratory throughout each calendar

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[3] Unpublished and untitled lecture by Simon Flexner, Bronk Papers, Record Group 303.1, Box 6, Simon Flexner folder, Rockefeller Archive Center [hereafter RAC].

year prepare periodic and detailed scientific reports for the Scientific Directors was due in part to Flexner's insatiable appetite for learning. As the Institute developed, these reports reflected some of the latest trends in science, and provided opportunities for Flexner and the Directors to reflect upon what the Institute might do next. Whereas Flexner had been primarily interested in infectious diseases when he was at Johns Hopkins during the 1890s, by the time of Jacques Loeb's appointment to the Institute in 1909, Flexner's cognitive framework had significantly expanded to encompass a very broad view of biomedical science. He increasingly believed that biomedical sciences must rely primarily on the basic sciences of chemistry, physics, and biology and that investigations had to be based on the methods of those fields. This change was due in part to the fact that the Institute provided a rich learning environment. Flexner held the view that most scientific knowledge is interrelated. For him, all forms of life were "related organically and ... united physiologically and pathologically." In the pursuit of knowledge, there should be no disciplinary boundaries separating the study of different forms of life, though there had to be separate laboratories for purposes of "economy of action" (Clark, 1961: 172).[4]

Flexner was very much aware that knowledge changes very rapidly in the global world of science, and he believed that if a biomedical Institute was continuously to absorb and integrate new knowledge, there had to be an intimate commingling of investigators in various fields of science, even if in the short term some fields of biomedical science appeared unrelated to others (Flexner, 1930: 461). He believed that the range and scope of the Institute should provide considerable scientific diversity. Moreover, the internal organization should be highly flexible so that it could quickly adapt to new knowledge. For Flexner, the culture within the Rockefeller should be such that the scientific staff would be willing to communicate and cooperate with each other. Such a strategy meant that every scientist should internalize extensive scientific diversity, and should be able to communicate with every other scientist. Indeed, scientific diversity and ability to communicate with others on the scientific staff were prerequisites for recruitment. Because so many of the scientists were fluent in multiple languages, foreign scientific publications were frequently discussed by the staff. And because there were staff fluent in German, French, and Russian, foreign scientists were eager to visit the Institute and to discuss research of mutual interest.

Flexner was aware that there was a short supply of scientists who had the kind of scientific diversity he wanted to recruit. His strategy was that the Institute over time should

[4] See also Simon Flexner to Abraham Flexner, July 4, 1924, Flexner Letters, Flexner Papers, American Philosophical Society [hereafter APS].

train a number of its own staff, and during his directorship, the Rockefeller became one of the world's leading centers of training for young postdoctoral biomedical scientists. This meant carefully recruiting able young people and providing them with excellent opportunities to grow scientifically. He was prepared for the overwhelming majority to depart after a few years of training, but the most exceptional were retained. This obviously meant that Members were constantly attentive to the strengths and weaknesses of younger staff. Although only one in twenty was retained, by 1950 almost half of those with permanent Institute appointments had risen through the ranks.

Flexner's correspondence demonstrates that he was enormously attentive to the well-being of young investigators. Even if they failed in particular investigations, he would encourage and console them and give them new opportunities. When a depressed young investigator told him that he had accomplished nothing worthy of publication, Flexner warmly consoled him by remarking, "Nothing? ... you don't seem to realize that to have nothing is to have something" (Rous, 1949: 427). Flexner was wise enough to know that for these young people, coming to the Institute was one of the great experiences of their lives, and he frequently encouraged his senior colleagues to make certain that this was the case. Even if Flexner did not retain a young scientist, he frequently paid the investigator a salary for a year after his departure as an incentive for another academic institution to recruit him. This kind of practice let young people know that if they went to the Rockefeller, they would have excellent opportunities to find a position upon exiting, and this did much to keep a steady stream of able young scientists going to the Rockefeller.

Over time, hundreds of young people passed through the Institute and went on to be prominent biomedical scientists. Flexner was highly committed to upgrading the quality of America's universities and medical schools, and believed that one of the major contributions of the Institute was its development of young investigators and teachers for universities and medical schools. He was very proud of the fact that by the early 1920s, the leading American universities in the country eagerly sought the Institute's young investigators (Rous, 1949: 425).

With so many scientists passing through the Institute, some errors in judgment were made. Not everyone retained was of world-class distinction, but probably no other research organization in the world had such a high proportion of the scientific staff eminent in biomedical science. Moreover, the Institute encouraged a few scientists to depart whom it should have kept. One such scientist was Michael Heidelberger, who because of his exceptional qualities was at the Institute for a very long period (1912 to 1927, except for a short period during World War I), but was eventually encouraged by Flexner to leave for fear that he was not getting sufficient recognition for his many achievements, which were invariably coauthored with a senior scientist. In the words of Elvin Kabat, Heidelberger was a "Leonardo da Vinci-type Renaissance man ... who became the father of quantitative immunochemistry" (Kabat, 1992).

As Director, Flexner also worked to maintain close research communication with members of the original Board of Directors and the subsequent Board of Scientific Directors and the Board of Trustees, and this was important in promoting trust among those governing the Institute. This was especially important in the development of a cohesive culture in the history of a new organization. Flexner and Christian Herter became intimate friends, and their families visited one another's homes. The Flexners and the Herters had summer houses at Seal Harbour and one of Flexner's sons married one of Herter's daughters. With Hermann Biggs, Theobald Smith, and Emmett Holt, Flexner's relations were somewhat formal, though they became closer over time. He was especially respectful of Smith's scientific knowledge, and he wrote a book with Holt on dysentery in infants (Rous, 1949: 421). Among the trustees, Flexner developed a very warm relationship with Gates, and the Flexners frequently visited the Gates home in New Jersey and their summer house on Lake George. Over time, he also developed a close relationship with Rockefeller Jr. As Mrs. Flexner and Mrs. Rockefeller liked each other, the Flexners frequently joined the Rockefellers at their New York City residence and were invited from time to time to the family estate at Pocantico Hills.

More than anyone else, Flexner institutionalized a culture of excellence within the Institute. Throughout his directorship, he did not become a full time administrator but continued to work in the laboratory (Rous, 1948: 613). Within the Institute, he was usually too busy to engage in small talk. He set very high standards not only for himself but for everyone around him. In some respects, he was a bundle of contradictions. He could be extraordinarily charming, but if he believed the situation called for it, he could be coldly blunt. He tended to be extremely instrumental in his dealings with the scientific staff. He was willing to be very harsh with individuals if he believed this was the most effective way of getting "the most out of a man and was best for his development" (Clark, 1961: 172). On the other hand, he worked tirelessly to establish a nurturing environment in the Institute. Overall, he was a very considerate and affectionate individual. He kept in touch with the lives of most everyone in the Institute, and all staff—scientific or otherwise—usually received birthday or wedding anniversary cards. He was solicitous of the needs of the nurses, secretaries, porters, laboratory helpers, keepers of animals—all of the unheralded people essential for a high quality research organization. When they encountered family emergencies, he would help them cope with the problem—financially if appropriate (Rous, 1949: 427; Cohn, 1948: 228; Rivers, 1967:123–127).

On the other hand, there is evidence that during the Flexner years, the Institute was often conservative—if not outright "stingy"—with regard to salaries. In part, this practice stemmed from Flexner's fear that universities would accuse the Rockefeller Institute of stealing faculty by paying inflated salaries. This did not happen: Rather, it was the universities which from time to time attempted to lure Rockefeller Members away with extremely high salaries. Because he was concerned for the welfare of American universities, Flexner was determined to keep Institute salaries within the range of American salaries at high quality universities (Rivers, 1967: 124–125; Rous, 1949: 425). For research expenses within the Institute, however, the situation was different. In comparison to the leading universities in America, Institute scientists had generous research funding from the Director.

Funding for research was administered by Flexner in a somewhat personalistic style. Even so, the decision-making process about funding research within the Institute was in some respects more effective than that which currently exists in most contemporary American funding agencies. Investors the world over seek information about the quality and safety of their investments. Flexner had a great deal of information about the research potential of almost everyone in the Institute, and he and his colleagues on the Board of Scientific Directors were in an excellent position to make well-informed decisions about what was high-risk and low-risk research, who had the abilities to conduct a high-risk, long-term project and who did not. Moreover, they were well positioned to monitor investments in scientific research, as a result of periodic scientific reports and their ability to discuss and advise on the progress of specific research projects. This, of course, was sound investment strategy: It meant well informed investors very knowledgeable about the level of risk represented by those in whom they were investing and with high capacity to monitor their investments. The fact that the Journal of Experimental Medicine and other high-quality journals were edited at the Rockefeller provided the Director with a great deal of information about papers produced at the Institute. In sum, the Institute strategy for funding science was highly flexible because of the large amount of information possessed by those making scientific investment decisions.[5]

From the very beginning, the culture of the Institute was entrepreneurial and high-risk oriented. Flexner regarded trivial and unimportant research with contempt. He had no

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[5] The process of funding science at the Rockefeller was very different from the process of funding science by the National Science Foundation and the National Institutes of Health in contemporary America. With numerous proposals arriving before study groups and program officers, these two organizations are less well positioned to assess the degree of risk involving a researcher and a research project. The NSF and NIH grant proposal submission process and the decision-making process about research funding have become more standardized and bureaucratized, with the consequence that much high-risk research tends to be discouraged.

objection if labs were unproductive for lengthy periods of time, as long as they were addressing important problems. He would wait a long time before concluding that a young scientist was not suitable for the Institute. Mindful of the weakness of his own early education, he took the view that one of the missions of the Institute was to provide research training for young postdocs. And because of the Institute's rich scientific diversity and the excellent internal communication among the scientific staff, he frequently encouraged scientists to move into totally new fields of research. On one such occasion, he informed a young scientist who was moving to a new problem that it would take at least two years to begin to understand its parameters: "I will not expect anything of you until after that." Flexner's ability to identify and fund new areas of research—as with the work of Peyton Rous—was emblematic of the Institute's capability to anticipate new directions, even radically different directions, and move into them rapidly (Rous, 1948: 612; Jones, 1946: 295; Rous, 1949: 424).

On almost all occasions, Flexner was available as both an intellectual and scientific resource. In a number of cases, he went out of his way to inform young investigators that they needed an assistant and that he would provide the funding (Rous, 1949: 424). At the height of the depression, when Tom Rivers told Flexner that he needed extra funding for his research (though he did not specify the amount), "without batting an eye" Flexner provided $10,000, a great deal of money at the time (Rivers, 1967:121). On the other hand, Flexner did not hesitate to be stern—even with a senior Member—if he believed the person was not being prudent with the resources provided by the Institute.[6]

Gates and the two Rockefellers did much to set the tone for the research strategy of the Institute. From the beginning they had informed the original Board of Directors that they expected no short-term utilitarianism or results. Indeed, Gates and the Rockefellers had early concluded that no important discoveries were likely to result from the Institute. Their major hope was that the Institute would conduct high quality research, be a training ground for young investigators, and serve as an example for other philanthropists. Research on important problems, even if long periods of incubation were necessary, was encouraged from the outset (Gates, 1977; Fleming, 1954: 157). Thus, without pressure to produce results in the short term, Flexner could encourage his staff to "think big," to take risks, but to be aware that rigorous standards of excellence would be the criteria by which all results would be assessed.

From the beginning of the Institute, Welch, Flexner, and others on the Board of Scientific Directors had advocated a research program on animal pathology, in part because

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[6] Simon Flexner to Phoebus A. Levene, November 30, 1931, Simon Flexner Papers, APS.

certain diseases are transmissible from animals to humans. Unanticipated events in 1913—a widespread outbreak of hog cholera—brought the issue to the fore. After studying the matter, Flexner and Theobald Smith of the Harvard Medical School proposed that the Institute create a full-scale department of animal pathology, to which the Board agreed. Because of his distinguished international reputation, Smith was asked to head the new department, and the decision was made to locate the department in the Princeton, New Jersey area. In 1917, the Institute's Department of Animal Pathology opened with a state-of-the-art facility, and with a faculty that grew to be an integrated scientific community with research and social life tightly intertwined (Kay, 1986: 454; Corner, 1965: 134).

The new department at Princeton was almost the equivalent of establishing a new institute. Organizationally under the direction of Flexner, the department's day-to-day operations, most staff appointments, and local policies were under the direction of the local head (Smith). The Princeton department had scientific leadership with a vision of how to address "doable" major problems, with the capacity to identify scientific talent from diverse fields, and able to integrate this scientific diversity in a rigorous but nurturing environment. As well, the Princeton site had scientific diversity, scientific depth, scientific integration, and high-quality staff. Significantly, a number of major discoveries did occur in the Princeton branch of the Rockefeller Institute, breakthroughs which resulted in Theobald Smith's being awarded a Copley Medal and John Northrop and Wendell Stanley receiving Nobel Prizes in Chemistry for separate research conducted in the Princeton laboratories between 1917 and 1937. However, these recognitions are only a part of the story of the Princeton department.

Smith was a very cautious director of research and, unlike Flexner, risk-averse. Thus, he added relatively few staff who did not complement his immediate research interests. However, because Flexner exercised scientific oversight over the Princeton department, he was able to counterbalance Smith's conservative tendencies, with fortuitous consequences for the Princeton program. This occurred most dramatically in the career of John Northrop, one of the most innovative scientists during the first forty years of the Institute. Though the Princeton department focused on animal diseases, Flexner arranged for Northrop to move to the Princeton site in 1926, even though he clearly did not focus on animal diseases.

Flexner's moving Northrop to Princeton contributed to a creative research program which culminated in Northrop's being awarded a Nobel Prize for Chemistry in 1946. At the Rockefeller Institute, Northrop had worked closely with Jacques Loeb, who taught him how to design complex experiments in order to answer important, well-focused questions. He also influenced Northrop to be interested in the colloidal properties of proteins and to work on pepsin and trypsin. Northrop was a creative and highly productive scientist with a magnetic personality, and over the years he attracted a number of first-rate scientists to his lab (de Kruif, 1962: 3–35). Using solubility measurement, ultracentrifuge analysis, and electrophoresis, Northrop and his colleagues demonstrated that pepsin is pure enzyme, thus challenging the then dominant views of Richard Willstätter about enzymes. As a result of the work of Northrop's lab as well as that in the Institute's New York labs, Rockefeller investigators "led the world" in enzymology by the early 1930s (Olby, 1974; Herriott, 1962: 4–5).

By the time of Smith's retirement (1930), Flexner had decided that the Institute should have a program in plant pathology, also located at Princeton. He believed that a program on plant disease—especially plant viral diseases—would be of enormous benefit to the Institute (Flexner and Flexner, 1941: 295–296; Corner, 1965: 313). One of the nation's leading plant pathologists, Louis O. Kunkel, was appointed to head a program in plant pathology, with a mandate to study plant diseases caused by bacteria, fungi, and viruses. Kunkel decided to concentrate on tobacco mosaic virus (TMV), and to tackle his research program he recruited a diverse group of talented young scientists—including Wendell Stanley—who could work on most aspects of the biology of TMV.

Stanley's training had made him a promising investigator in physiological chemistry, but he was devoid of training on viruses. At the time Stanley began his work, no one had previously isolated a virus. Stanley assumed that the virus was protein, and in the small Princeton environment, he quickly became very familiar with the work of Northrop's lab, which had demonstrated that enzymes were crystallizable and were proteins. Thus, Stanley set out to discover if viruses could be purified by methods similar to those used by the Northrop group (Pirie, 1969: 232–233; Edsall, 1971; Stanley, 1964). Several years of hard work and more than one hundred chemical reagents later, Stanley reported that he had isolated a "protein" virus, and after further purification he obtained highly infectious needle-like crystals. In a 1935 paper, he reported that he had "strong evidence that the crystalline protein herein described is either pure or is a solid solution of proteins" (Stanley, 1935). "No discovery made at the Rockefeller Institute, before or since, created such astonishment throughout the scientific world as this" (Corner, 1965: 320). Immediately, Stanley became a national celebrity. For many, he was raising fundamental problems about life, just as Jacques Loeb had earlier done at the Institute. Because Stanley's 1935 paper attracted so much attention in the popular media, it naturally stimulated great interest in the scientific community, and criticism was not long in coming. From Britain, researchers reported that TMV was not a pure protein, as reported by Stanley, but contained approximately six percent nucleic acid (RNA), and that it was a nucleoprotein. Why had Stanley missed the RNA?

Over the years, Stanley was subjected to severe criticism both for missing the RNA in his paper and for not recognizing that the "crystals" were paracrystalline in nature (Kay, 1986; Pirie, 1969; 1986). Yet when Stanley began his research in 1932, the true nature of viruses was still a mystery. It had not been established whether they were inorganic, hydrocarbon, carbohydrate, lipid, or protein. No one had isolated them as such. Though Stanley's 1935 paper had its shortcomings, it was a major step forward in the field of virology. Later, with input from the Swedish scientist Thé Svedberg, Stanley was able to extend his work and publish a portrait of the rod-shaped TMV. By 1946, largely due to Stanley's initiatives, work on the concentration and purification of different viruses was proceeding in several laboratories, and over a dozen viruses had been obtained in highly purified form, primarily by the techniques of high-speed centrifugation.

Having a distinguished program established at Princeton—a program initially focused on animals and then incorporating research on plants—not only brought new kudos to Rockefeller scientists, but brought to the fore whole new lines of inquiry. To some extent, the local circumstances at Princeton mimicked the New York example of intense and frequent communication among scientists of very diverse interests, although internal disagreements were also a part of the Princeton locale. In establishing an off-site program, the Rockefeller Institute took a gamble, acting somewhat precipitately in choosing the Princeton location.

The Institute also took the lead in enhancing scientific excellence in America by editing and publishing journals. By developing journals of the highest quality and encouraging the staff to publish in them, the leadership of the Institute further emphasized its independence from the existing institutional environment. The Journal of Experimental Medicine (founded by Welch) was the first journal to be edited at the Institute. Flexner became the editor in 1904, and in addition to all his other responsibilities he was chief editor for fifteen years, though assisted at the outset (1904–1910) by Eugene Opie. Eventually, he asked Peyton Rous in 1921 to be coeditor, and Rous essentially functioned as the editor for the next thirty-six years, though Flexner continued to be listed as coeditor until his retirement as the Institute Director. Flexner's successor Herbert Gasser served as coeditor from 1935 to 1953, the entire period for which he was Director (Corner, 1965: 62–63; Flexner and Flexner, 1941: 243–250; Dulbecco, 1976: 275; Andrews, 1971: 653; Robertson, 1953: 25).

In 1918, Jacques Loeb, with John Osterhout of Harvard, launched the Journal of General Physiology, which was subsidized by the Rockefeller. With Institute support, Loeb also launched a monograph series designed to advance the development of experimental biology from a quantitative and physiochemical point of view. Through these publications, Loeb helped to unite various fields of biology by breaking down barriers among different specialties (Pauly, 1987: 150; Blinks, 1974: 213). The Institute also published numerous other monographs, sometimes at irregular intervals. There was a series entitled Studies from the Rockefeller Institute for Medical Research, which by 1930 had seventy volumes of 600 pages each. The Journal of Biological Chemistry also was published by the Institute for many years, and Phoebus Levene, Donald Van Slyke, and others at the Institute published a high proportion of their papers there (Tipson, 1957; Wolfrom, 1961: 1319; Herter, 1910).

During the more than thirty years that he served as Director of the Institute, Flexner played a major role in improving the quality of the Institute's publications. These aided the productivity and creativity at the Institute and were important in drawing attention to the Institute's accomplishments. Over the longer term, probably no journal has had greater impact in establishing new trends in biology than the Journal of Biophysical and Biochemical Cytology, later renamed the Journal of Cell Biology. This was another journal which was published at the Institute, and its founding was due to the inspiration of Keith Porter and the strong support of Herbert Gasser (Palade, 1977).

The experience of the Rockefeller Institute demonstrates that diversity and depth of knowledge in a well-integrated research organization have the potential to change the way people view problems and to minimize their tendency to make mistakes and/or to work on trivial problems. Frequent and intense interaction among people with low levels of diversity tends not to lead to major breakthroughs, but if scientists work in environments where there is moderately high scientific diversity and depth, and have frequent and intense interaction with those having complementary interests, they increase the probability that the quality of their work will improve. It is the diversity of disciplines and paradigms to which individuals are exposed in frequent and intense interactions that increases the tendency to develop new ways of thinking about fundamental problems. For such a process to continue over the longer term, the organization must not only provide the stimulation, resources, and environment for today, but also, in a sense, anticipate the future by undertaking whole new lines of research.

Intellectual and social integration were maintained at the Rockefeller Institute by a variety of devices. Eating meals together while conversing about serious scientific matters was an important part of the Rockefeller culture and an important means of integrating the scientific diversity and depth of the Institute. There was good food at lunch, served at tables for eight. The idea was that a single conversation could take place at such a table, but not at a larger one.

The degree of intellectual and scientific diversity was much lower at the Rockefeller Institute than that at the colleges of Oxford and Cambridge, where eating at “high table” was also an important part of the culture. At the English colleges, diversity ranged all across the board (e.g., from archaeology and ancient and modern languages to chemistry, physics, biology, and mathematics). With so much diversity, it was considered poor etiquette to talk about one’s work at the “high” table of the colleges, as many of those present would be unable to comprehend the line of discussion. At Rockefeller, in contrast, diversity was only within the biomedical and related sciences, and the norm was to carry on lively lunchtime discussions about these fields. The lunch table was a great learning experience where people had intense discussions about new approaches to research. Indeed, these luncheon experiences led not only to new factual information and changes in philosophical viewpoints, but also to collaborative research projects across fields (Dubos, 1976: 31). Without the kind of culture exemplified in the lunch experiences at the Institute, some of the major discoveries made at the Institute would not have occurred.

For many, the lunch table at the Institute was the high point of the day. Paul de Kruif, a scientist at the Institute who later became a strong critic, nevertheless thought the dining room was one of its most stimulating characteristics.

> ... at the lunch break there was balm for my discouragement. Here I could listen to the scintillating talk of my betters. ... I never tired of listening to the philosophy of Alexis Carrel, who had won the Nobel Prize in medicine ... Carrel, who had been in America a long time, had carefully preserved his French accent, which made him sound to me even more learned than he was ... Then at the luncheon table there might be Dr. Peyton Rous, refined, gentle, exquisitely cultured ... In this refectory there was an air of solemnity to be expected and appropriate to the unveiling of mysteries (de Kruif, 1962:16–17).

And in the words of Dubos:

> There never was a symposium—in the etymological sense of the word ... that was more scientifically productive and intellectually pleasurable than those held daily in the lunch-room of the Rockefeller Institute, though coffee and ideas were the only intoxicants (Dubos, 1976: 31).

Apart from lunch, scientific integration was also facilitated by the Friday afternoon lecture which everyone was expected to attend, when Institute investigators or distinguished scientists from all over the world reported about their work. In the Hospital Department, there were afternoon tea times which most scientific staff attended.

One of the most important integrating devices was the journal club, especially the Hospital Journal Club which generally met twice a month. Everyone was expected to attend, and to be prepared to report on a paper outside their own research field, but of general interest to everyone. No one knew in advance who would be called upon to present materials to the journal club. Why would world-class scientists agree to participate in such activity? They did so because they knew they were members of an extraordinarily distinguished organization, and they believed that one reason it was so outstanding was that they were continuously learning from each other. This kind of regular reading outside one's own specialization and in areas of interest to others in the organization was one way of integrating scientific diversity. In short, the Institute had developed a culture of continuous learning for its scientists.

Its learning environment was supported by being located in New York City. Before World War II, most distinguished foreign scientists traveling to America arrived in New York and invariably visited the Rockefeller Institute. Certainly no other biomedical research organization in America was so favored with foreign scientists. It was in the neighborhood of the New York Hospital-Cornell Medical Center and across town from Columbia University's medical center. By 1930, New York City had become a leading center of biomedical science, and this environment provided an opportunity for access to many of the latest ways of thinking about biomedical science. If this small institute could not have all the diverse ways of approaching biomedical science represented on its permanent staff, it was in the fortunate position of having many of the world's leading scientists passing through with reports about their work. Moreover, Rockefeller Foundation grants to young British and European scholars brought to New York the cream of the crop of young scientists, for stays of varying lengths.[7]

Institute scientists took great pride in being associated with what many viewed as a temple of science (de Kruif, 1962: 13). Even today, as soon as one crosses from York Avenue on the upper east side of Manhattan onto the grounds of the Rockefeller, one is aware of being in a sanctuary, separated from the hustle and bustle, the grime and dirt, the

[7] The Rockefeller Foundation was a completely separate organization from the Rockefeller Institute. Over the years, only a very insignificant amount of funding went to the Institute from the Rockefeller Foundation, and during the Flexner years there was often a fair amount of tension between Rockefeller Foundation officers and Flexner (Kohler, 1991).

shouting and cursing of the city. The original idea of the Institute had been conceived in part by a religious minister, Gates, and throughout the century, some observers referred to the Institute in religious terms. Not only for Gates, but also for the Rockefellers, Flexner, and Welch, biomedical science acquired almost religious proportions.

How did this happen? Gates spoke for the two John D. Rockefellers when he wrote that "disease is the supreme ill of human life and is the main source of almost all other human ills: poverty, crime, ignorance, vice, inefficiency." He went on to explain that these ills could not be cured by the economic and social restructuring of society, as many socialists believed. Rather, society's problems were technical in nature and required technological solutions, which medical science could provide (Berliner, 1977: 7, 1986). For Gates, these views had a religious quality to them, for scientific medicine was the means of building a new and better world, of uplifting and civilizing one's fellow men. The Rockefeller Institute should become the functional equivalent of a temple for the new religion, and medical investigators were to be the equivalent of priests who would reduce suffering and poverty. Gates referred to the Institute as a theological seminary and Flexner as a scientific "Doctor of Divinity." Indeed, de Kruif thought "Flexner looked as if he might be high in the College of Cardinals" (de Kruif, 1962: 12; Brown, 1979: 125; Pauly, 1987: 136). And in writing about the Institute, de Kruif reported that for Flexner, the greatest achievement of the Institute was not "the scientific output, its undoubted saving of lives, its contributions to basic knowledge," but rather "it was the harmony, the serenity, the brotherhood with which the staff had always worked together" (de Kruif, 1962: 56).

Welch had early on advised Flexner to recruit scientists "who agree well together" (Rous, 1949: 425). Both believed that new ways of thinking about scientific problems were most likely to emerge in a scientific environment in which scientists had intense and frequent interaction with one another, so that a fundamentally new way of thinking about a problem could occur. For them, an environment of harmony, trust, and dignity was essential, for whether one was in a religious or scientific monastery, the priesthood must be passionate in its quest for the truth. Thus, in recruiting staff for such a sanctuary of science, it was important to select individuals who were imbued with a spirit of cooperation and to avoid the prima donnas.

To recruit staff who would pursue problems on a large scale, Flexner thought it was important to find scientists concerned with deep philosophical problems. During the first half century of its existence, the Institute had a number of scientists who not only internalized vast scientific diversity but whose scientific thinking was well integrated with a rich philosophical framework. This kind of scientific staff generated enormous excitement at the Institute lunch tables, which became famous over much of the world. As in most "monasteries," there was some conflict. In fact, some Members cared little for others. But relative to most research organizations, there was a high degree of communication and cooperation among the scientific staff, and this contributed to its rich learning environment (interviews with Edelman, Hotchkiss, and Ris).

In 1935, Flexner at age seventy-two retired as Director of the Institute after more than thirty years of service. Though he had been able to recruit only a "motley" group of scientists at the outset, by the time he stepped down as Director, the Institute had emerged as one of the world's leading centers of biomedical science. Whereas in the first decade of the century, most scientists with established reputations would not have considered accepting an appointment there, the Institute had now long eclipsed the Pasteur Institute, the Lister Institute in London, and others. During the years that Flexner served as director, ten scientists had done research at the Institute which resulted in their receiving recognition for major discoveries in biomedical science. Five (Alexis Carrel, Karl Landsteiner, John Northrop, Peyton Rous, and Wendell Stanley) had done work at the Institute which resulted in their receiving a Nobel Prize either during or after Flexner's tenure. Theobald Smith received the Copley Medal for research which he did at both the Institute and at the Harvard Medical School. Oswald Avery, Simon Flexner, Jacques Loeb, Theobald Smith, and Hideyo Noguchi made breakthroughs which resulted in their receiving numerous nominations in multiple years for a Nobel Prize in Physiology or Medicine, enough to meet the criteria established in Appendix One as major discoveries. <u>During the Flexner years, no other research organization in the world had more major breakthroughs in biomedical science than the Rockefeller Institute, and no research organization in the world since then has had so many major breakthroughs in a comparable period of time (thirty-four years).</u>

During the Flexner years, the Institute had distinctive advantages in identifying both junior and senior talents throughout the world. Although the Rockefeller Foundation was not a major funding agency for the Rockefeller Institute, the Rockefeller Foundation had become a major funding source for biomedical science throughout Europe and North America. To gather information about the quality of scientists and their labs, the Foundation obtained extensive reports about research in every major biomedical research organization in Europe and North America. Because he was a trustee of the Foundation and because his brother Abraham had a long affiliation with the Foundation, Simon Flexner had extensive knowledge about "who was up and coming" on both sides of the Atlantic, and was in a strategic position to invite outstanding young scientists to the Institute. Similarly, his prominence in the National Research Council provided him with inside information about many promising young scientists (Kohler, 1991).

During the 1920s, aspiring biomedical scientists in America took the view that they needed to spend at least two to three years at the Institute for postdoctoral training. By 1935, more than 150 Rockefeller-trained scientists had become professors in America's leading universities and were beginning to establish their own training programs, a process which in the longer term would narrow the gap in quality between the Rockefeller Institute and other organizations in biomedical science. But until the National Institutes of Health became the leading training center for young biomedical scientists during the 1950s and 1960s, the Rockefeller Institute was clearly the major American center for postdoctoral training in the biomedical sciences, with younger people also from Europe increasingly going to the Institute for advanced training. By the time of Flexner's retirement, the Institute had become not only a world-class center for research, but also a major center for advanced training. Without obligations to train either undergraduates or graduate students, it was a very serious center for advanced training for only the best of the best. When future Nobel laureate Alan Hodgkin went there in 1938 from the University of Cambridge, he was struck by the very high quality of its scientists, its instrumentation, and its greater formality and seriousness as compared to Cambridge (Hodgkin, 1977, 1992). And when the distinguished bacterial physiologist Marjory Stephenson of the University of Cambridge completed her visit to the Rockefeller Institute in 1931, she wrote the Secretary of the Medical Research Council, Walter Fletcher, that research in bacterial chemistry at Cambridge and Oxford was years behind. She was especially impressed with the way that enzymology and other areas of biochemistry had become integrated with bacteriology at Rockefeller: "It saddens me to think that there is no work at all of this kind even beginning in either of the two great places for bacteriology in Cambridge and Oxford" (quoted in Kohler, 1985: 180). This contrast between science at the Rockefeller and Cambridge was also very disappointing to Fletcher, who had worked for years to break down disciplinary boundaries at Oxford and Cambridge (Kohler, 1978, 1985, 1991).

THE DIRECTORSHIP OF HERBERT GASSER

Among the reasons for the continued excellence of the Rockefeller Institute were its flexibility, derived in large part from its autonomy, and its independence from the institutional environment in which it was embedded. Unlike many research organizations at the time in both Europe and America, it was not dependent on external funding, and was therefore immune to the kinds of constraints which funding sources place on organizations (Hollingsworth and Hollingsworth, 1988). Until the mid-1950s, the Rockefeller Institute never accepted grants or any funding from sources other than the Rockefeller family. Moreover, it was relatively autonomous from the norms and rules imposed by most scientific disciplines. Most universities, organized around academic disciplines and their academic departments, are constrained in their behavior by the norms and rules of existing academic disciplines. But because the Rockefeller Institute was organized around laboratories rather than around scientific disciplines and fields, it had a greater capacity to be flexible and to adapt quickly to new research strategies. And in contrast to the institutional environment in Germany and France, the Rockefeller Institute had a high degree of autonomy to appoint anyone to its staff. In Germany, in contrast, most scientists who became professors first had to qualify by having the German habilitation. But the Rockefeller could and did appoint as Members scientists who were trained in many parts of the world.

The Institute was still performing well when Flexner retired, but it was in need of a new director with different scientific perspectives. Even high performing research organizations undergo demographic changes and need new leadership from time to time in order to become better adapted to the fast pace of scientific change. And in the case of the Rockefeller, a number of the original members of the Scientific Board of Directors had died (Biggs in 1923, Holt in 1924, and Prudden in 1924), as had some of its most distinguished scientific investigators (Meltzer in 1920, Loeb in 1924, Noguchi in 1928, and Theobald Smith in 1934).

During the 1930s, the Institute was one of the world's leading centers in enzymology, virology, bacteriology, and pathology. Indeed, there have been suggestions that Herbert Gasser, the next director, was appointed to succeed Flexner in an effort to prevent the Institute from becoming too narrowly concerned with virology and bacteriology (Rivers, 1967: 195–196; Olby, 1974: 183). Even though the Institute had attained a high level of excellence in a number of areas of biomedical science, it had clearly made no effort to cover all fields of biomedical science. Flexner believed that an effort to work in too many fields would lead to uneven quality, and that research programs of low quality would bring down standards of excellence in the entire organization (Flexner, 1939). Hence, during the Flexner years the Institute had made few or only modest efforts to conduct research in a number of areas in which other research organizations in either Europe or America had already become quite prominent (e.g., research in vitamins and other areas of nutrition, endocrinology, genetics, neurophysiology). Flexner's strategy had been to have areas of core competence which were complementary and to avoid excessive scientific scope and diversity, lest the organization become less integrated and have less communication among its various component parts.

The decision to bring in an outsider, Gasser, as the new director was a wise one. Many organizations—because they have a distinctive culture—tend to choose their new leaders from within, but such choices may well enhance organizational inertia and hamper the organization's capacity to adapt to the fast-changing environment. Of the eight directors/presidents since the founding of the Institute, seven were recruited from the outside, and the sole exception, Torsten Wiesel, spent most of his career elsewhere, moved to Rockefeller University at the age of sixty, and had been at Rockefeller for less than ten years when he was appointed as President. The Rockefeller organization has been much more flexible in adapting to new strategies and different styles of science than would have been the case had it recruited its directors and presidents from among people who had spent most of their careers there.

Gasser, like Flexner, had grown up in America's heartland. He was born in 1888 in the small town of Platteville, Wisconsin, attended the University of Wisconsin in Madison and studied physiology there with Joseph Erlanger, and later was a student at the Johns Hopkins Medical School. He then became a colleague of Erlanger, who had moved to Washington University in St. Louis, and by 1921 Gasser had been appointed professor of pharmacology. Gasser went to London (1923–1924) to work with A.V. Hill, the neurophysiologist who was awarded the Nobel Prize in Physiology or Medicine in 1922. He also developed a close collegial relationship with Henry Dale and Edgar Adrian, both of whom were awarded Nobel Prizes in the 1930s. In the longer term, these relationships were important in establishing a network between British neurophysiologists and the Rockefeller Institute. Gasser later moved from St. Louis to New York to be Professor of Physiology at the Cornell Medical College, and as a result of being in the neighborhood of the Institute, he became quite familiar with it.

Like all subsequent directors or presidents of the Rockefeller, Gasser worked to preserve the culture which Flexner and his colleagues had developed. However, Gasser had his own distinctive style of doing science, and this was enormously beneficial in facilitating the Institute's adaptation to changes in the world of science. Like Flexner, Gasser made continuous efforts to extend the scope and depth of his knowledge. The more he observed increasing specialization in science—especially in American universities—the more he was motivated to contain the centrifugal forces of differentiation within research organizations by promoting the unity of the biomedical sciences. His ideal of an outstanding scientist was someone who internalized considerable intellectual diversity, and who was a theorist, an experimenter, a builder of instruments, and an artist. Indeed, he believed that at the Institute there should be serious efforts to blend humanistic and scientific knowledge, and that great scientists tended to internalize and integrate both humanistic and scientific knowledge (Adrian, 1963, 1964; Bronk, 1963).

A major difference between Gasser and Flexner was Gasser's greater concern with methods of measurement. By building on his awareness of the new initiatives in measurement in the field of neurophysiology, Gasser extended the kinds of problems pursued in existing kinds of research and steadily pushed the organization into new lines of work. This was an example of a major change at the Institute, folded into ongoing programs. Reaching maturity during the early days of electronics, he attempted to master areas of physics with implications for biomedical sciences. In his own work, he was at the frontier of using electronic amplifiers and recording for observing the most minute signs of nerve action. As director, he demonstrated that he was attuned to the latest methods and ideas in the biological sciences.

When Gasser became Director, the United States was experiencing the worst depression in its history, and this had some adverse effects on the Institute. Even before Gasser arrived, Flexner had written to Rockefeller Jr. assuring him that over the next several years the Institute would be especially frugal and would not ask for additional funds. As a result of the fiscal problems brought by the depression, Gasser was severely limited in terms of the new fields of science which he attempted to introduce at the Institute. Salaries were tight, and when young scientists left the Institute, most were not replaced. Between 1935 and 1941, the total scientific staff of the Institute declined from 134 to 105. During the depression, the income from the Institute's endowment fell dramatically, with the result that in 1939 the Institute's expenses exceeded its income by 23 percent. The Institute was able to cover its deficit only because it had accumulated a surplus during the Flexner years (Rivers, 1967: 580–581; Corner, 1965: 329–332).

Partly due to the economic problems of the depression followed by the disruption of wartime, Gasser modified the Institute's strategy with regard to younger scientists. During the Flexner years, when a Member left the Institute his lab had been closed, but on several occasions Gasser was willing to permit a remaining Associate Member to head a lab and to develop a research program. Both Flexner and Gasser were willing to take risks with younger scientists, but Gasser was more inclined to grant some young scientists a high degree of autonomy in developing their own research programs. The strategy paid off handsomely in the cases of several younger scientists who later made major discoveries which began at the Rockefeller organization (e.g., Henry Kunkel, Stanford A. Moore, George Palade, Keith Porter, William H. Stein). Among these scientists, there were subsequently three Nobel Prizes, two Lasker Prizes for Basic Biomedical Science, and two Horwitz Prizes for research conducted at Rockefeller (interview with Palade).

Despite the budget constraints, Gasser provided leadership for altering the research strategies of the Institute. As retirements occurred, the research emphasis on bacteriology and infectious diseases, paramount during the Flexner years, decreased. Gradually, Gasser was facilitating the development of different research programs. One of the most significant, with long-term consequences, was neuroscience. Rather quickly, Gasser developed a program in neurophysiology which focused on the fundamental properties of nerve cells, dendrites, and the primary synaptic endings of nerve fibers. He surrounded himself with several competent foreign investigators—Harry Grundfest from Russia, Rafael Lorente de Nó from Spain, David Lloyd from Oxford, and Jan Friedrich Toennies from the Berlin Technische Hochschule. By the late 1940s, three scientists from the neurophysiology group held the rank of Member: Gasser, Lloyd, and de Nó (Hodgkin, 1977: 7–8; Corner, 1965: 332–340).

While the work for which Gasser was to win the Nobel Prize had been done at Washington University before he arrived at the Rockefeller, he did lay the foundations for work in neurophysiology which over the decades would evolve into a rich neuroscience program at the Rockefeller. His successor as Director of the Institute—Detlev Bronk—was also a neurophysiologist who built on the foundations laid by Gasser. This tradition led to H.K. Hartline's work in the neurophysiology of vision, which was awarded a Nobel Prize for Physiology or Medicine in 1967. By the mid-1970s, Rockefeller had a major program in three areas of neuroscience, and the scientists in them made up one of the largest groups in the organization. These areas were (1) Molecular and Cellular Mechanisms, (2) Neurophysiology and Behavioral Physiology, and (3) Information Processing, Communication Behavior, Cognition, Memory, and Brain Function. Eventually, Nobel laureate Gerald Edelman of Rockefeller University would develop one of the most prominent theories in the broad field of neurobiology (Edelman, 1987, 1989, 1992; and Edelman and Tononi, 2000), and would be instrumental in arranging for the Neurosciences Institute to be located at Rockefeller University, where it remained until 1991. In addition, Edelman was instrumental in recruiting to the Rockefeller Torsten Wiesel, who had received the Nobel Prize for his work in the area of neurophysiology of vision. He became a Professor at Rockefeller in 1983 and President of the University in 1991. Edelman was also the key person involved in recruiting Paul Greengard, who in 2000 was awarded a Nobel Prize for his neuroscience research (interviews with Torsten Wiesel, Paul Greengard, and Gerald Edelman). From the time of Gasser to the present, these and numerous other distinguished scientists have placed Rockefeller in the vanguard of neuroscience.

Research organizations may stay at the leading edge of science by bringing in senior scientists who have already been the pathbreakers in a field as well as by having investigators already in the organization develop new frontiers. The capacity to do both is a result of past and present strategy choices and good leadership. This is most evident with regard to the role of biological chemistry at the Rockefeller. In this case, Emil Fischer's school of organic chemistry was partially transplanted to the Institute in the early part of the century, and the transplantation subsequently branched in numerous directions, one of which was protein chemistry, giving rise to a number of major discoveries in the process. Indeed, Fischer's prophecy in his 1902 Nobel lecture was very much realized at Rockefeller: "Since the proteins participate in one way or another in all chemical processes in the living organism, one may expect highly significant information for biological chemistry from the elucidation of their structures and their transformations" (Smith, 1979: 116).

At the Institute there were a number of scientists who had studied with Fischer and who, in turn, had inspired a number of luminaries of protein chemistry (e.g., John Northrop, Moses Kunitz, Wendell Stanley, Stanford Moore, William H. Stein, Lyman Craig, Alfred Mirsky). Loeb, while not trained as a chemist, also had a major influence in developing the tradition of protein chemistry at Rockefeller (Pauly, 1987: 152–153, 170–171). This lineage was responsible for the Rockefeller's becoming a world-class center for major discoveries within the broad outlines of protein chemistry. In several respects, the critical period for the development of protein chemistry was between 1925 and 1960, when there was increasing evidence in both Europe and America that many important biological activities were due to specific proteins. While there was already vast information supporting this view, it was only during this period that protein chemistry was put on a firm experimental basis, largely as the result of the development of new methods and instruments that substantially increased the knowledge of the function and structure of proteins.

One of the great protein chemists of the twentieth century, Max Bergmann, was appointed to Rockefeller in 1934. He had left Germany following the Nazis' ascent to power. A former student of Emil Fischer, at the Rockefeller Bergmann attracted a group of able young scientists who would later develop into some of the leading protein chemists in the postwar world: William Stein, Stanford Moore, Joseph Fruton, Klaus Hoffman, Emil L. Smith, Paul Zamecnik—among others. Work on proteins at the Institute was largely suspended during World War II. However, when Bergmann died of cancer at the height of his career in 1944, the lab was left without a chief. Gasser demonstrated that he had an excellent ability not only to discern the direction in which science was moving but also to identify talent: He asked Moore and Stein—two relatively young scientists—to stay on at the Institute and gave them the freedom to do almost anything they pleased in the biochemical field. In 1949, they were appointed as Associate Members and they received permanent appointments at the Institute in 1952. They won the Nobel Prize in Chemistry in 1972 for their collaboration leading to the development of quantitative chromatographic methods for amino acid analysis, especially their automation techniques, which led to the entire sequence for ribonuclease A being described, the first complete description of the chemical structure of an enzyme.

Gasser also demonstrated imaginative leadership with his support of other young scientists engaged in fundamentally new kinds of research. One example was his support of Lyman Craig—long associated with Walter Jacobs—who also worked independently in developing other techniques for purifying some proteins. For this work Craig was later awarded the Lasker Prize for Basic Biomedical Science.

But nowhere did Gasser demonstrate better insight in judging the direction in which science might move than in his support of a group of young cell biologists. Albert Claude had arrived at the Rockefeller in 1929 and worked in the laboratory of James B. Murphy in an effort to purify and characterize the agent that caused a transmissible form of cancer in chickens, the Rous sarcoma virus. Working in the area of virology, Claude used a high-speed centrifuge to spin fractionated cells infected with viruses in an effort to isolate and purify their agents. His investigations led to the determination that cells contained tiny bodies, which he labeled "microsomes." The critical problem was to understand their structure and function. To address this problem, Claude used an electron microscope, a new laboratory tool. Under the pioneering influence of Claude's younger colleague, Keith Porter, the Institute then led the way in the development of electron microscopy for the study of cell biology. Aside from Porter, Claude also had the assistance of a number of other young scientists—George Palade, Roland Hotchkiss, George Hogeboom, and Walter Schneider (Palade, 1971; interviews with Palade and Hotchkiss).

It was the interdisciplinary environment of the Rockefeller Institute which facilitated Claude's imaginative investigations. He brought to the study of cells deep insights from the fields of oncology, virology, biochemistry, histology, and cytology. His ability to internalize so much scientific diversity enhanced his ability to see things in fundamentally new ways, and so to lay the foundation for the development of cell biology at Rockefeller.

Claude left the Institute in 1949 to return to his native Belgium, and Murphy—the head of the lab—retired in 1950. But Gasser's keen eye for promising research, along with his willingness (as in the case of Stein and Moore) to keep a lab open when its head disappeared from the scene, set the stage for major new advances. Gasser chose to invest Institute resources in acquiring an electron microscope. Technically, Gasser became the head of the Porter-Palade group so that it might continue to function, though for several years Porter actually provided the day-to-day leadership of the newly developed electron microscopy laboratory. It is no exaggeration to suggest that modern cell biology was born at the Rockefeller under the leadership of a diverse group of "junior" scientists (Palade, 1971, 1977, 1998).

Without the nurturing role of Gasser in deviating from the precedent of closing down laboratories when their heads departed, these developments would not have occurred. Significantly, it was Gasser's support for the development of new technologies needed for new fields of research which facilitated Rockefeller's becoming one of the world's leading centers for protein chemistry and cell biology (interview with Palade).

But perhaps the single most notable achievement in the entire history of the Rockefeller was the paper by Oswald Avery and his younger colleagues Colin MacLeod and Maclyn McCarty in 1944, which offered evidence that genetic specificity is embedded in the chemical structure of DNA (McCarty, 1985; Lederberg, 1985). The story of this work, which had little to do with Gasser's leadership, has been recounted many times, and there is no need to go into great detail about it here. However, it is worthwhile to note certain aspects of the process of this discovery in order to illustrate how the structure and culture of Rockefeller facilitated scientific creativity.

Today, there is universal recognition that the paper from Avery's group represents one of the most important discoveries in the history of the biomedical science. Nobel laureate Joshua Lederberg has observed that between twenty and twenty-five Nobel Prizes were subsequently awarded for research dependent on the 1944 paper. Similarly, Peter Medawar, the Nobel laureate immunologist, called the discovery "the most interesting and portentous biological experiment of the twentieth century" (Lederberg, n.d.; Bearn, 1996: 550–554; Rockefeller University, 1993: 2).

From the viewpoint of my research, there are several intriguing problems related to the Avery paper. Since the Rockefeller Institute was the dominant center of the Protein Central Dogma, how was it that the challenge to this paradigm came from within the Institute and was made by a bacteriologist and physician, Avery, who lacked formal training in chemistry and genetics? Could this discovery have been made anywhere else at approximately the same time?

As a trained physician, Avery spent much of his career concerned with understanding pneumonia and attempting to devise strategies for either treating or preventing the disease. It is no exaggeration to say that by 1940 he was one of the world's foremost authorities—if not the foremost authority—on various types of pneumococcus. Over the years, his research had shifted from an interest in the development of an effective serum for the treatment of pneumonia to an immunochemical understanding of the chemical basis of the biological specificity of different types of pneumonia. For more than two decades, a dialectic was operating in Avery's mind: the applied scientist's concern with treating and preventing pneumonia, and a basic scientist's concern with the underlying chemical and biological processes. It was the tension between these two forms of inquiry and the effort to integrate them which was the key to Avery's scientific creativity. In our own day, as those trained as either basic scientists or physicians tend to drift further and further apart, the story of how Avery integrated the concerns of both the clinician and the basic scientist needs to be carefully noted.

Avery spent his entire Rockefeller career in the pneumonia section of the Hospital Department. While the leading investigators in the Hospital (e.g., Cole, Cohn, Dochez, Horsfall, Rivers, Van Slyke) were clearly distinguished investigators by national standards, there was a tendency after World War I among many in the Laboratory Department to perceive the Hospital investigators as applied investigators and therefore somewhat inferior. Avery was very much aware that while he was highly specialized and knowledgeable about pneumonia, he was an outsider as far as fundamental knowledge of physics, chemistry, and biology was concerned. But like all his colleagues in the Hospital Department, he was obsessed with learning, and he constantly tried to absorb basic knowledge from his Laboratory colleagues in an effort to expand the scope of his specialty—pneumonia.

During the 1930s, Avery had lunch at the tables for which the Institute was famous with such distinguished organic and biological chemists as Phoebus Levene, Karl Landsteiner, Donald Van Slyke, Alfred Mirsky, Max Bergmann and his brilliant young associates William Stein, Stanford Moore, Joseph Fruton, Emil L. Smith, and Paul Zamecnik. Moreover, he had a high degree of familiarity with the work of John Northrop and Wendell Stanley, who would be awarded Nobel Prizes in 1946. Never has one organization had such a stellar collection of biological chemists all in one place and all clearly working at the frontiers of science. It is no wonder that on two separate occasions Linus Pauling, one of the century's greatest chemists and at the other end of the continent, collaborated with members of this group (e.g., Landsteiner and Mirsky). For Avery, as a non-chemist, the ability to lunch on a daily basis with such a collection of scientists was exhilarating, but at the same time somewhat intimidating.

Once he set out to identify the substance responsible for transforming one type of pneumococcus to another, Avery was determined to seek information from any available source. He frequently sought the counsel of the great scientists at the Rockefeller. Revealing his ignorance about the latest methods in chemical analysis that had been developed at Rockefeller and elsewhere, he was not shy in approaching his scientific colleagues in the privacy of their labs. Had he been a microbiologist in a large university in America, he would never have had the opportunity to learn over the lunch table so much about the recent advances which had taken place in protein chemistry during the previous twenty years.

While Avery recognized the need to borrow from other fields in order to address his particular research problems, he generally preferred to work with young scientists who had been trained as physicians rather than as basic scientists. However, on several occasions he did work with young basic scientists, including Michael Heidelberg and René Dubos, with notable success, and those collaborations helped to prepare the way for his 1944 paper. It is doubtful whether Avery could have acquired the same degree of scientific diversity in any other organizational environment to do the experiments which resulted in his 1944 paper (Amsterdamska, 1993: 25–27; Dubos, 1976; Olby, 1974: 183–185).

There is another aspect of work on the 1944 paper which deserves attention, a consideration which reveals much about distinguished research organizations. Major discoveries are often rooted in specific local cultures, and that was indeed the case with Avery and the Rockefeller institution. Jacques Loeb—one of the most influential scientists in the history of the Rockefeller—played an important role in establishing a physico-chemical viewpoint on biological processes, a perspective which early became dominant at the Institute. As Dubos (1976: 46) has observed, because of Loeb's influence "ways of thinking about life ... that do not involve a physico-chemical approach have never found a congenial home within the walls of the Rockefeller Institute." Significantly, protein chemist John Northrop, whose work had aided Avery, had worked under Loeb. Indeed, Loeb's interest in protein chemistry had a profound influence on Northrop's decision to consider the protein nature of enzymes, which culminated in his Nobel Prize. Northrop, in turn, had a profound influence on Wendell Stanley, Moses Kunitz, and other distinguished enzymologists and virologists at the Princeton Laboratory of the Institute. And it was the enzymology work of the Princeton group—operating as a legacy of Loeb—that led Avery to develop his most convincing evidence that the pneumococcal transforming substance contained DNA as an essential factor of the genetic determinant (Cohen, 1979). Anticipating modern genetics, as early as 1912 Loeb had written that one of the most important tasks for the biological sciences was to determine the chemical substances in the chromosomes responsible for heredity. More than one observer has emphasized the importance of Loeb in developing a tradition of a physico-chemical viewpoint of biology which ran through Simon Flexner to John Northrop and Wendell Stanley to Avery and ultimately to Jim Watson's decision to work on the structure of DNA (Fuerst, 1982; Wyatt, 1972; Rasmussen and Tilman, 1998; Loeb, 1964; Watson, 1968: 29–30).

In his years at the Institute, Gasser had the assistance of a number of the country's leading scientists on the Board of Scientific Directors: Detlev Bronk, who during his tenure became President of Johns Hopkins University, President of the National Academy of Sciences, and Chairman of the Board of the National Science Foundation; James B. Conant, President of Harvard University; Vincent du Vigneaud of Cornell University (a future Nobel laureate); Warfield Longcope of Johns Hopkins University, one of the nation's leading professors of medicine; and Nobel laureate George H. Whipple of Rochester University. With their guidance and the participation of the scientific staff—some of them mentioned above—new pathways were opened not only in such broad areas as protein chemistry and cell biology, but also in the mechanisms of heredity and virology.

During the depression and World War II, the Board of Trustees and the Board of Scientific Advisors were troubled by the declining rate of income from the Institute's investments and by rising expenditures. Partly for this reason, the Trustees decided in 1947 to close the Institute's research site in Princeton, New Jersey, facilities established approximately three decades earlier. In addition to financial considerations, the two governance boards were very much concerned about the physical separation of the two facilities. The decision was very controversial, for two members of the Princeton staff (Northrop and Stanley) had recently been awarded the Nobel Prize in Chemistry. With the closure of the Princeton department some Princeton staff returned to New York City, and others parted from the Rockefeller Institute.

THE PRESIDENCY OF DETLEV BRONK

The Institute had never been content to rest on its laurels, and had continuously been willing to make serious reassessments of its performance. Accordingly, the Trustees—in anticipation of Gasser's retirement—appointed a committee in the early 1950s to make recommendations about the Institute's future. Given that by the early 1950s no research organization in the world had made so many major discoveries in biomedical science during the twentieth century, it is remarkable that the committee was prepared to look at a series of radical options for the future. The committee seriously considered whether the Institute should be closed down and its assets distributed to other research organizations, whether it should move from New York to a less costly environment, or whether it should merge with an existing university.

By the early 1950s, it was clear to the Board of Trustees that the Rockefeller Institute had more than achieved its original purpose of providing a model of a high quality research organization in biomedical science, one which would set the standard of excellence in biomedical science. Partly because of the existence of the Rockefeller Institute, there were now numerous research organizations making important discoveries and training very good scientists. By 1953, more than 200 former members of the scientific staff held the rank of full professor (or the equivalent) in research centers throughout the country. Thus, if the Institute had attained its goal of helping to develop centers of biomedical research excellence in the country, why should its existence be continued? Perhaps Rockefeller wealth should be used for some other societal purpose not yet being addressed?

The committee consulted with a number of leading scientists throughout the country. Detlev Bronk, one of the most influential members of the committee, was adamantly opposed to closing the Institute. Recognizing that the Institute had achieved its initial goals, Bronk took the view that the Institute should revise its goals and purposes. In the future, it should not only be a center of research excellence, but should also have as a major goal the providing of graduate education for the future scientific leaders of the country. Because it would be a new kind of training center, the Rockefeller organization would continue to do something distinctively different from all other educational centers in America.

Impressed with Bronk's ideals, the Trustees prevailed upon him to become Gasser's successor. Thus, in 1953, the Trustees voted to change the Institute into a graduate university with the authority to grant the degree of Doctor of Philosophy and Doctor of Medical Science. The Board of Scientific Directors was abolished and the Board of Trustees became the sole governing authority of the organization. The Rockefeller Institute became a small university, the only graduate university in the United States. However, it was not until 1965 that the name was changed from Rockefeller Institute for Medical Research to Rockefeller University.

Bronk, who served as President from 1953 until 1968, was very troubled that the nation's leading centers of training were producing such highly specialized people, whom he pejoratively characterized as technicians. In contrast, he wanted the Rockefeller to produce scientists who had broad knowledge in a variety of fields. He recognized that scientific excellence required a mastery of subject, but thought it was important that scientists understand how their own research was intricately related to other fields of science. A recurring theme in his public speeches was the desirability of the unity of knowledge, and for him the ideal university was one in which there would be intense and frequent interaction among scientists from diverse fields. Ideally, each scientist would have a broad competence in many areas of science. Moreover, Bronk was very humanistically oriented, and frequently spoke of the importance of scientists' understanding how they were inextricably connected to the past, present, and future of their society. For Bronk, the importance of scientific research was to be measured by its larger humanitarian contributions to society. The transformation which he proposed for the Rockefeller organization was that it would be a training ground for the future leaders of science, scientists who would aspire to break down artificially segregated areas of knowledge and to struggle for the realization of the unity of knowledge for the benefit of society (Chance, 1978; Adrian, 1976; Brink, 1976, 1979). From time to time, Bronk would preach to the Board of Trustees about the kind of university he had in mind, as he did at the Trustees' meeting of May 22, 1961:

> It is unwholesome, and indeed dangerous for the future welfare of mankind, for scientists to live and work, to study and teach, in an environment in which they are not in close contact with creative, critical scholars in the humanities and arts. Such associations are a valuable quality of a university. Lack of such contact with those who help determine the future of civilization makes specialized research institutes barren intellectual environments.

But how might a small university attain such goals? Paradoxically, a small university such as Rockefeller has more potential to produce broadly trained scientists than larger research universities which teach almost everything. Bronk believed that it was inappropriate to include all fields of science in the research program. In the largest research universities with their increasing fragmentation and specialization, such a goal had become increasingly difficult to achieve. When universities attempt to encompass all fields of science, some areas are invariably mediocre, and when mediocrity develops in some fields, it leads to mediocrity elsewhere. Because of the small size of the Rockefeller University, Bronk recognized that it could not offer programs in the humanities. As a result, early in his presidency the university offered each student a fund of $1,000, to take advantage of the rich cultural offerings in New York (such as concerts, opera, theater, ballet, museums, or galleries). Moreover, he arranged for humanists and social scientists to present frequent public lectures at the University and for concerts to be held in its facilities.

Because it was not feasible to encompass all specialized fields of science, the university would focus on the "most significant, most fundamental and most broadly relevant areas of mathematics, physics, chemistry, and biology." Since the university was organized around individuals rather than around departments, its structure should encourage cooperation in teaching and research. Accordingly, in recruiting faculty to the university, emphasis was placed on the choice of people who had broad interests and who by inclination enjoyed collaboration with others in diverse fields of science. However, the emphasis on breadth of scholarly interests and interdisciplinary cooperation did not mean that certain individuals or groups would be recruited simply to provide service for more fundamental areas of science. Indeed, the philosophy at Rockefeller was that all scientists and groups were encouraged to be autonomous, with their own identity and dignity.

The program which Bronk espoused had implications for the growth of the university, however. When Bronk became President in 1953, there were 21 Members, whereas at the end of his tenure as President, there were 51 Professors. In the same period, there was a tripling in the size of the scientific staff from 99 to 303.[8] Whereas in 1953, there were biological scientists, medical doctors, biophysicists, and physical chemists, the faculty was quickly expanded to include scientists in psychology and animal behavior, physics, mathematics, and philosophy. Moreover, the fields of biology, chemistry, and biophysics were extended. Within three years Rockefeller appointed as professors several scientists who had either already been awarded or would later be awarded Nobel Prizes: Keffer Hartline from Johns Hopkins, Fritz Lippman from the Harvard Medical School, and Edward Tatum from Stanford. Other distinguished, senior faculty appointed during the Bronk years were the philosopher Ludwig Edelstein, the biologist Paul Weiss, the mathematician Marc Kac, physicists George Uhlenbeck and Abraham Pais, behavioral scientists Carl Pfaffmann, Neal Miller, and Floyd Ratliff, the geneticist Theodosius Dobzhansky, the cell biologist Christian de Duve, and the chemist Theodore Shedlovsky. Bronk also arranged for a number of the world's leading scientists to spend a week or two, over a period of several years, with the students: Lord Adrian of Cambridge, Isidor Rabi of Columbia, David Goddard of Pennsylvania, Ragnar Granit of Stockholm, John Kirkwood of Yale, Kaj Ulrik Linderstrom-Lang of Copenhagen, Louis Marie Monnier of Paris, Alex von Muralt of Bern. By the end of Bronk's term as President, Rockefeller University had one of the most distinguished collections of biomedical scientists in the world.

For Bronk, the University was to be a genuine community of scholars with graduate students, postdocs, and faculty having intense and frequent interaction—all engaged in learning from one another. An important key to such a university was the selection of students, and he frequently argued that it was important that students be selected with the same care as the faculty. Hence, during his years as President, Bronk personally

8 Academic Affairs, Bronk Papers, Record Group 303, Box 7, File 9, RAC; Report of Six Faculty Committees, September 9, 1968, Bronk Papers, Record Group 303, Box 17, File 4, RAC.

interviewed most of the students before they were admitted. The only students selected for admission were those whose commitment to advanced study and research was believed to be equal to that of the faculty. All students were considered to be intellectually mature, highly motivated, and capable of self-study. As a result, there was to be no core curriculum. Each student was to have his/her own program of study based primarily on learning from advanced textbooks, professional journals, tutorials and seminars on special subjects, and work in the labs of some of the world's most distinguished and experienced scientists. In a sense, there was a separate curriculum for each student. Even though Rockefeller technically had been transformed into a university, it remained more of an institute than a research university as normally known in American society. Rarely would more than twenty-five graduate students be admitted a year, and by 1966 there were only 128 students (Brink, 1976; Lyon, 1964). Because there were so few students relative to the number of faculty, close associations developed among students and faculty (Brink, 1979: 67; interviews with Hotchkiss, Gally, Edelman, Zinder).

Normally, admitted students had completed an undergraduate baccalaureate or held a Doctor of Medicine degree. Students were usually candidates for the degree of Doctor of Philosophy, but those who were Doctors of Medicine could pursue a degree of Doctor of Medical Science. Thus, one of the major goals in moving from an institute to a university was to prepare young people to be scientific leaders of the nation—as David Rockefeller expressed it, "a reaffirmation and an expansion of prior objectives, a reaching out to new opportunities for the pursuit of excellence" (Rockefeller, 1976: 88; interview with Rockefeller).

During the years of Bronk's presidency and ever since, the Rockefeller organization has been confronted with somewhat paradoxical goals. It aspires to have considerable breadth in its scientific interests, yet is determined to remain small and to permit each lab to be autonomous. It has been difficult to maintain strength in chemistry, physics, mathematics, and different fields of biology, and at the same time to promote interdisciplinary cooperation. For many years, the University attempted to address this paradox by promoting the unity of science through the interpretation of biological phenomena in physical and chemical terms. Perhaps unsurprisingly, a number of scientists who were recruited to Rockefeller as organic or physical chemists over time became biochemists. Some physicists became biophysicists, and some mathematicians emigrated to other fields as a result of their interests in computer science and various technologies. Thus, at the Rockefeller there has long been a strategy of recruiting scientists who have broad interests and who by temperament have been eager to collaborate with and learn from those in other fields. Because of its small size, the Rockefeller has generally been reluctant to recruit those who by temperament prefer to work in isolation—though over time there have been exceptions.

Another aspect of the continuity between the Bronk years and the culture of excellence institutionalized during the Flexner years was the historical emphasis on providing services for the scientific staff: the design and construction of instruments, illustration services, glass blowing, a spectroscope laboratory, etc. To keep these and other services at a high level of excellence, over the years the organization has recruited an extraordinary staff trained in the techniques of mechanical, electrical, and chemical engineering.[9] Having state-of-the-art support services has been important in keeping the Rockefeller at the cutting edges of multiple fields of science.

Accompanying the expansion of faculty during the Bronk years was a building program that reshaped the physical setting of the University. This included a residence for graduate students and a building with two wings: the Abby Aldrich Rockefeller Hall (containing a dining room, lounges, library, and accommodations for visiting scholars) and the Alfred H. Caspary Hall (with administration offices). Connected to Caspary Hall was a new auditorium. A modern nine-story laboratory and several smaller buildings were also constructed (From Institute to University, 1985: 14). Bronk's aesthetic aspirations for the University were revealed by his arranging for one of the world's leading landscape architects, Daniel Urban Kiley, to design the University's grounds in the late 1950s. The result was the development of some of the most beautiful grounds in the city.

Bronk was a dreamer, a person whose ambitions for the University were virtually without limit. Hence, it is not surprising that it soon became obvious that the University could not survive primarily on its endowment, as in the past. Once extramural funding from the National Institutes of Health became available, the Rockefeller organization began to turn to the federal government for research funding. This marked a critical turning point in the history of the organization: Labs began to grow in size, turned somewhat inward, and became somewhat more independent from the rest of the University. No longer did everyone have lunch together. There were too many senior scientists and students for the early twentieth-century type of communication and integration to exist. Many labs began to have their own journal clubs, and attendance at the weekly Friday afternoon scientific presentations for the entire organization dropped off dramatically. There was no longer the same degree of horizontal communication across labs as during the first half of the century. Of course, Rockefeller University was still quite small relative to most American research universities, but as size increased, the degree of integration and communication among the

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9 Memo of September 18, 1968, Bronk Papers, Record Group 303, Box 17, File 4, RAC.

scientific staff diminished (interviews with Seitz, Lederberg, McCarty, Edelman, Hotchkiss, Zinder, Archibald).

THE PRESIDENCIES OF FRED SEITZ, JOSHUA LEDERBERG, AND DAVID BALTIMORE

It is not unusual that an organization which undergoes rapid expansion and reorganization under one leader later finds that the rate of change is not sustainable, and therefore the next leader faces a period of major readjustment and stabilization. This is precisely what happened under the University's next president, Fred Seitz (1968–1978), a physicist. Like Bronk, he had been President of the National Academy of Sciences.

With hindsight, it is obvious that the University had been extravagant in its expenditures during the Bronk years. After Seitz became President, the economic environment of the University quickly changed. By 1970 the University, like many others throughout the country, found itself confronting serious financial problems, exacerbated by inflation—rising salaries as well as construction and energy issues. There were cuts in some federal grants, causing the University to draw money from its endowment to support programs to which it was committed. Moreover, the recession that began in 1968 led to a drop in income from the endowment. Because non-academic pay scales had fallen below city standards during the Bronk years, Seitz and his staff now had to address this problem as well. For the first time in its history, the organization was confronted with not only a deficit, but a mounting one. This was the shape of things as Seitz completed his first year as President. As during the depression of the 1930s, the University was forced to undertake a serious assessment of its operations, the result of which was the recognition that the institution could not sustain the rate of growth of the Bronk years. Overall, the 1970s and 1980s were difficult years. For financial reasons, if for no other, Seitz had no alternative but to undo some of the decisions of his predecessor. In contrast to years of fanfare and ceremony under the presidency of Bronk, the tone during that of Seitz was more restrained, due to the financial difficulties which the University faced. Recruiting continued, but less aggressively. On-site staff were carefully reviewed, and laboratories were warned that long-term prospects for funding their researchers were very limited for financial reasons (interviews with Seitz, Nichols, McCarty, Lyons).

Even so, the 1970s and the 1980s were decades of outstanding achievement and recognition. In 1969, Bruce Merrifield was awarded the Lasker Prize for Basic Biomedical Research for his work in developing a method for the synthesis of polypeptides and proteins, and in 1984 the Nobel Prize in Chemistry for the same line of work. In 1972, Gerald Edelman was awarded the Nobel Prize in Physiology or Medicine (shared with Rodney Porter of Oxford University), while William Stein and Stanford Moore (with Christian Anfinsen of the National Institutes of Health) received the Nobel Prize in Chemistry (Edelman, 1994). Two years later (1974) Albert Claude, George Palade, and Christian de Duve shared the Nobel Prize in Physiology or Medicine for work they had conducted at Rockefeller, and two years earlier (1970) Keith Porter and George Palade had shared the Louisa Gross Horwitz Prize for their investigations involving electron microscopy of biological materials.

The University Trustees became increasingly concerned about their dependency on federal funding for research. Cuts at the federal level could imperil ongoing research, just as, to the contrary, a War on Cancer could cause coffers to open. Fearful that in the longer term, federal funding would prove unstable and would reduce the University's autonomy and flexibility, the Trustees instituted campaigns to raise funding from the private sector (such as individuals, foundations, and business firms). The Trustees intended that federal grant money would provide about half of the University budget, and the other half of the budget would be covered by private fund-raising and income from the endowment. Private sector funding was absolutely necessary if the University was to remain at the frontiers of biomedical science, and especially to expand its research program in cell biology and molecular biology.

By the end of the Bronk years, the Trustees had approved the creation of a University Senate and an Academic Council, which was the Executive Committee of the Senate. As a result, a number of senior faculty increasingly insisted on being involved in decision-making in university affairs, very much in contrast to the years when the organization was a relatively small institute. During the Flexner and Gasser years, even senior Members rarely knew what major decisions were being made by the Director and the two Boards. Indeed, during the Gasser years, the Institute offered senior positions to Linus Pauling, Carl Cori, Vincent du Vigneaud, and Francis Schmitt—all of whom declined—but the author of this paper has been unable to find any evidence that any Member of the Institute's staff was ever informed of these efforts.[10]

During the Bronk years, the scientific staff had continued to be relatively uninvolved in the governance of the Rockefeller. But once the University experienced the financial deficits of the 1970s and 1980s and difficult choices had to be made about how to

[10] Summary of W. F. Loomis interview with Herbert Gasser. Rockefeller Family Papers, Record Group 3, Box 51, Folder 534, p. 26, RAC.

reduce the deficits, the faculty attempted to become much more involved in the governance of the University. Whereas there had been virtually no discussion among the scientific staff in the early 1950s as to whether graduate students should be trained at the Rockefeller, during the Seitz years the faculty began to discuss what proportion of funds should be devoted to teaching as distinct from research, the degree to which heads of laboratories would have complete autonomy in appointing scientists in their own labs, how many Rockefeller-funded scientists might be in a single lab, and how decisions would be made about the appointment and promotion of laboratory staff. Clearly, changes in the economic environment in which the University was embedded had suddenly made the governance of the University much more complex than had been the case during the Bronk years, when the American economy was much more expansive. In sum, the process of institutionalizing a new mode of decision-making in an era of fiscal constraints created stress within the organization. Fortunately for the University, Seitz was a very stable and unflappable administrator, who realized that if the University was to continue being a distinguished scientific center, long-term financial planning would have to be integrated into the day-to-day management of science. As research became much more expensive and the structure of research organizations more complex, many more factors entered into the management of an organization. In short, the complex demands of administering the organization between 1970 and 1990 made necessary a marked departure from the more personalistic style of management which had existed prior to Seitz's presidency.

Meantime, Seitz and his successor, Nobel laureate Joshua Lederberg (1978–1990), were also very attentive to the infrastructure of the university, keeping it as a world-class center in instrumentation design and construction, and attempting to add new laboratory space (From Institute to University, 1985: 18). During the Seitz presidency, Hidesaburo Hanafusa, James Darnell, and several other senior faculty were recruited, but most efforts to add other senior staff came to nil. Even so, during the early years of Lederberg's presidency the University continued its efforts to make distinguished appointments, appointments which resulted in great distinction for the University: Jan Breslow, Paul Greengard, Torsten Wiesel being the most high-profile (interviews with Breslow, Greengard, Wiesel, Nichols, Seitz, Lyons, J. Darnell, Hanafusa).

Throughout the presidency of Lederberg, the University's financial condition continued to be constrained by the poor performance of the American economy. Believing in the desirability of Rockefeller University remaining small, Lederberg reduced some layers of administration and attempted to make himself more directly available to faculty. Many of his plans—whether very general or more programmatic (e.g., parasitology, toxicology)—were hard to realize without more discretionary funds. A great deal of energy—administrative, trustee, and faculty—was invested simply in addressing fiscal problems of daily living. Issues such as fund-raising and problems attendant upon life in the city (e.g., lack of affordable housing for both junior and senior faculty) were never-ending.

Throughout the century, the two John D. Rockefellers and David Rockefeller were always available to lend support to the Rockefeller during times of stress. David Rockefeller joined the Rockefeller Institute Board of Trustees in 1940 and served for more than 55 years. In 1950, he succeeded his father as President of the Board. It is difficult to imagine how the Rockefeller could have remained a center of scientific excellence without his counsel, his financial assistance, and his ability to be a bridge between the University and other sectors of American society. Rockefeller's political connections with local, state, and national governments and with elites in many sectors of American society were very valuable assets for the University. David Rockefeller's associates and members of his family report that his involvement with the University's affairs was the most important commitment of his public life, even more important than his role as Chairman of the Board of the Chase Manhattan Bank and his other activities involving local, national, and international affairs. For almost forty years, he hardly ever missed a meeting of the University's Board of Trustees, and was generally available to University administrators and scientists who wanted to consult with him about University affairs (interviews with Goodwin, Pillsbury, Lederberg, Nichols, Bowen, Rockefeller, Edelman, Seitz, Wiesel).

Several perspectives concerning the behavior of organizations need to be kept in mind in order to understand some of the internal turmoil which occurred at the Rockefeller between 1989 and 1991. By the late 1980s, there was an increased perception both within and outside of Rockefeller University that it was not performing as well as it had some decades earlier. Changes in the environment—especially the availability of funding from the National Institutes of Health (NIH)—had a major effect on the university. The center of gravity of basic science research had shifted westward, partly as a function of the existence of jet aircraft. Increasing numbers of scientists were willing to have careers on the West Coast—in Berkeley, Seattle, Los Angeles, San Diego, and elsewhere. And as the cost of living in the New York environment escalated, it became increasingly difficult for the Rockefeller to recruit scientists of their first choice, a constraint that had been quite rare in earlier years (interviews with J. Darnell, Seitz, Lederberg, Zinder, Nichols).

In the early part of the century, ambitious young biomedical scientists had been eager to spend a few years at Rockefeller Institute. By the latter part of the century, there were many centers of excellence throughout American society, and the best of the nation's young scientists were increasingly unwilling to go to the Rockefeller, for there most young scientists were not able to be heads of their own labs until they attained the rank of full professor. By the late 1980s, it was well known that many of the nation's best biomedical scientists had their own labs when they were quite young (e.g., Joshua Lederberg and Howard Temin at Wisconsin, Paul Berg and Dale Kaiser at Stanford, Jim Watson and Walter Gilbert at Harvard, Mike Bishop and Harold Varmus at the University of California, San Francisco). With numerous research centers throughout the country, most good young scientists increasingly expected to have more autonomy and their own laboratories before becoming senior professors. This was a problem which was widely discussed within the Rockefeller, but by the late 1980s, more often than not, young scientists were still not becoming heads of labs. As senior scientists retired, it became increasingly difficult to recruit distinguished replacements. At the same time that these problems arose, the Rockefeller, like most of the nation's leading research universities, was required to institute salary and wage freezes in response to the poor performance of the American economy (interviews with Temin, Lederberg, Kaiser, Gilbert, Bishop, Varmus, Watson, Baltimore, Zinder).

Apart from the fiscal problems and the junior staff problems, the University faced the persisting tasks of developing new labs and renovating old ones—a constant at all serious research organizations. With changes occurring at an ever-increasing rate in the larger world of science, there were unrelenting pressures to recruit new faculty in molecular biology, neurobiology, and cancer research, as well as in other areas. By 1990, however, the University had recruited only one senior scientist from the outside in the previous five years. In 1990 the average age of the tenured faculty was fifty-eight, meaning that within another decade, a number of distinguished faculty would retire.

In the midst of these changes in the larger environment, David Baltimore of MIT, a Nobel laureate, became President of Rockefeller in July 1990. A former graduate student at Rockefeller, Baltimore was one of the nation's most productive scientists and was widely perceived to have been a very successful administrator of the Whitehead Institute for Biomedical Research, an affiliate of MIT. His appointment was strongly opposed by a sizable minority of the Rockefeller faculty, primarily because of the complex and unresolved investigations of a scientific paper which he and several of his MIT collaborators had published in Cell in 1986 (Boston Globe, October 5, 1989: 43). There were allegations that some of the experiments in the paper were misreported by the senior author, Thereza Imanishi-Kari. While there were no suggestions that Baltimore personally misrepresented the data in the paper, he was widely criticized for failure to conduct a proper investigation into the allegations about the paper and for defending the "flaws" in the paper too long and too vigorously. Because of this cloud over Baltimore, his tenure as President of Rockefeller became very controversial (Kevles, 1998, 1999). His most vocal critics—both inside and outside of the University—charged that he had been extraordinarily arrogant in his handling of many of the issues related to the disputed paper (interviews with Zinder, Blöbel, J. Darnell).

By the time Baltimore arrived in the summer of 1990, the University had made efforts to address some of its outstanding problems. A new laboratory building was under construction, and in 1989 several junior faculty had been appointed as heads of laboratories. Still, a number of procedures for dealing with the retention, dismissal, and promotion of junior faculty had not been resolved, and the University's budget was seriously unbalanced, with a deficit of $12.3 million during Baltimore's first year.

Baltimore immediately initiated a $250 million fund-raising drive and instituted a wage freeze. At the same time, he established a number of search committees to recruit junior and senior faculty, and proposed major changes with regard to the status of junior faculty. In short, although Baltimore addressed problems already on the University's agenda, he was clearly a catalyst, dramatizing the University's problems and acting decisively with regard to some of them.

This was not an auspicious time. With so many major issues crowding his agenda, it was especially difficult for Baltimore, as a new President, to establish his authority and legitimacy. He was attempting not only to manage the University but at the same time to operate his laboratory. Relocating and setting up his lab was quite time-consuming. The tensions created by the 1986 Cell paper continued, casting a pall over his Presidency. Within the University, turmoil over the paper was compounded by the fact that a few of the senior faculty had known Baltimore since his graduate student days at Rockefeller, and intensely disliked him. Some felt that he had a long history of being arrogant and recalled that as a student, he was frequently referred to as "Little Lord Baltimore" (interviews with Norton Zinder). Moreover, some senior faculty resented the way that Baltimore arrived as President: They felt that he viewed himself as the man on horseback who was coming to save the University from a group of entrenched oligarchs. Some faculty were embittered by rumors heard around the country that he had long badmouthed the University. The sentiment of one senior faculty member captured the views of many others. "To portray this faculty as a bunch of old dying swans who have lost touch with modern biology is complete and utter nonsense" (Science, December 13, 1991. 254: 1577).

Almost everything Baltimore proposed quickly became controversial in one quarter or another. Although the senior faculty were essentially in agreement about the desirability of changing the role of the junior faculty with regard to heading labs (as indicated by the virtually unanimous approval of the Academic Senate in the fall of 1990 for Baltimore's proposals regarding junior faculty), controversy continued about even this issue. Despite the agreement on the principle that some scientists who were not professors might head labs, issues about which junior faculty would head labs were unresolved. In May 1991, five assistant and associate professors were selected to be heads of labs, bringing to sixteen the number of non-tenured heads of laboratories (News and Notes, May 17, 1991). As Baltimore rewarded some junior faculty with their own labs, cleavages emerged within the junior ranks. It appeared as though a two-tiered system of junior faculty was emerging. Some junior faculty received internal promotions, while others realized that their long-term chances of heading labs were no better than before Baltimore's arrival.

The issue of the Cell paper, too, simply would not disappear. In early 1991, there were leaks in the press to the effect that the U.S. Secret Service had uncovered evidence of fraud by Imanishi-Kari. In the spring, Baltimore's public comments about the Cell paper led to an intensification of criticism of him and the University, both in the press and among some of the nation's most respected scientists. As these criticisms reverberated within the University, Baltimore turned to junior faculty for support, leading to criticism that he "was using the junior faculty at some risk to their own careers" (New York Times, December 4, 1991, B11; interviews with De Lange, Friedman, J. Darnell, Kuryian).

Finally, in the fall of 1991, in the midst of this turmoil, the overwhelming sentiment of the senior faculty was conveyed to the Trustees that Baltimore was incapable of being an effective President, and shortly thereafter he resigned (Science, December 13, 1991, 254: 1578). With hindsight it is obvious that the Baltimore Presidency was unfortunate for the University as well as for Baltimore himself. In July 1996, a three-person board appointed by the Department of Health and Human Services concluded that the "preponderance of evidence" indicated that the allegations about misconduct in producing the data in the Cell paper could not be proven (Kevles, 1998).

There was a great deal of ill-chosen rhetoric on the part of both supporters and critics of Baltimore during the affair. Some of the rhetoric of Baltimore and his supporters early on suggested that the Rockefeller faculty was depleted and that the quality of science was no longer distinguished. The validity of these claims requires some perspective. It is true that there was a time when it was believed at Rockefeller that anyone receiving a permanent appointment at the organization should do work of a caliber to make the person a serious contender for a Nobel Prize. But with increases in size and the emergence of several dozen major centers of biomedical scientific research throughout the nation, there was a clear realization that such goals were unrealistic and that Rockefeller no longer exercised hegemonic influence over biomedical science as in the past. However, in 1990 the organization was still a distinguished and major center for biomedical research. Within a decade, several of its most creative scientists would be singled out for major recognition—with Nobel Prizes, the Lasker Prize for Basic Biomedical Science, and the Louisa Gross Horwitz Prize. Just a few years earlier, Hidesaburo Hanafusa had received the Lasker Prize for Basic Biomedical Science. Also, Gerald Edelman had emerged as one of the world's most creative theoreticians in neuroscience, though his work, like any other work that is so theoretical, was controversial. While Bruce Merrifield had done the research for which he was awarded a Nobel Prize some years earlier, his lab was still very active in work involving ever more difficult synthetic challenges and much higher molecular numbers. And there were numerous other scientists doing excellent science at the University.

THE PRESIDENCIES OF TORSTEN WIESEL AND ARNOLD LEVINE

Upon Baltimore's resignation, the Trustees appointed as President Torsten Wiesel, a world renowned neurobiologist who had shared a Nobel Prize in Physiology or Medicine with David Hubel in 1981, and who had moved to Rockefeller from the Harvard Medical School in 1983. The Trustees could not have found a more suitable person for the task. Wiesel, who served until 1998, was an excellent President, one of the very best in the history of the organization.

Wiesel, born and educated in Sweden, had moved to America in 1955. When he became President of Rockefeller, he was sixty-seven years old and immediately became completely focused on serving the University. Wiesel had many of the traits of an outstanding president—a good sense of the direction in which science was moving, a keen ability to identify talent and to raise money to support good science, and a capacity to create a nurturing environment for both senior and junior scientists. After the intense cleavages of the Baltimore Presidency, the Rockefeller faculty were eager to have stability and harmony and to rebuild their damaged reputation. Wiesel, an eminent scientist with considerable modesty and humility, was highly successful in bringing people together. Almost intuitively he was able to integrate the traditions of Rockefeller University with the directions in which science was changing as it moved toward the twenty-first century. Early in his presidency, he encouraged open faculty participation in discussions of both the weaknesses and strengths of the University. He then coordinated the development of an Academic Plan, an effort involving unprecedented collaboration between faculty, administration, and trustees. His strategy of bringing all parties to the table was typical of a rather low-key management style, very different from the bombastic style of Bronk. Very quickly, a consensus emerged that the University would build on its current seventy laboratories, and slowly expand to between eighty and a hundred labs as resources became available and appropriate talent was identified (Rockefeller University, 1994; interviews with Baker, Blöbel, Bowen, Furland, Imhoff, Kandel, Montgomery, Pillsbury, Rockefeller, Wiesel, Zinder).

Since the University was structured around laboratories rather than departments, a critical problem was how to keep the centrifugal forces created by so many different labs from becoming unmanageable. It was almost as though Wiesel had an innate ability to solve the problem. He understood that with so much diversity, it was necessary to have an ambidextrous style of management. A major key for coordinating so much diversity was to emphasize the tradition and culture of excellence which had been institutionalized at Rockefeller. Through the use of a broad array of rituals, the organization throughout the 1990s highlighted its strong, widely shared culture, a process which helped to promote harmony and communication between the different laboratories. Without a strongly shared culture, it would have been much more difficult to generate the trust and predictability necessary for a well-functioning organization.

In addition to the emphasis on the common culture, the University also promoted the coordination of labs by grouping them around seven research centers, in recognition of the natural affinities among the interests of scientists. These centers were financed in part by successful University fund-raising in the private sector during the 1990s. The seven centers were (1) the Center for Biochemistry and Structural Biology, (2) the Center for Sensory Neuroscience, (3) the Center for Human Genetics, (4) the Center for Research on Alzheimer's Disease, (5) the Center for Studies in Physics and Biology, (6) the Center for Immunology and Immune Diseases, and (7) the Center for Mind, Brain, and Behavior (Rockefeller University, 1994; New Research Programs, 1997; Brennan, 1999). The centers were intended to enhance communication and strengthen collaboration across labs, and to provide coherence for fund-raising from both federal and private sector sources. Depending on their interests, scientists could be affiliated with more than one center. In the process of developing the centers, some faculty gained a much better understanding of the complementary interests of different labs. The centers have varied in their performance, as well as in size and resources, but they have provided a meaningful strategy for promoting communication across laboratories. The functioning of the centers has been complemented by the fact that in recent years, labs at Rockefeller have become somewhat smaller, reducing their tendency to be so internally focused.

While governance of the University is shared by the labs, the research centers, the Academic Council, and the University Senate, the office of the president plays an extremely important role in governance by making financial and personnel decisions, and in determining the allocation of laboratory space and laboratory budgets (if non-grant funds are being used). During Wiesel's first four and a half years as President, the University raised approximately $135 million in private gifts and pledges, much of which was used to support the recruitment of new faculty and to create state-of-the-art laboratories. The University increased the number of laboratories and recruited a number of junior and senior scientists. During these same years, twenty new laboratories were headed by non-tenured scientists.

When Wiesel left the presidency in 1998, there was a widespread perception both within and beyond the University that Rockefeller was once more in a very sound condition, both scientifically and financially. The University had restored stability to its budgets and operations, and had increased its endowment by over 50 percent in a five-year period. Part of this increase resulted from the performance of the American economy, but much of the improvement was due to the management team Wiesel and the Trustees had put in place (Rockefeller University Annual Reports, 1997, 1998; interviews with Bowen, Imhoff, Baker, Furland).

Very mindful of the turmoil and embarrassments of the early 1990s, by the turn of the new century the University was anything but complacent. There was a tone of rebuilding, but with modesty as the University attempted to learn from its competitors. In retrospect, perhaps the previous turbulence had been healthy for the University, as one of its effects had been to create more of a community among its faculty than had existed at any time for decades.

To lead Rockefeller into its second century, in 1998 the Trustees appointed Arnold Levine as President, a distinguished cancer researcher and former Chair of the Department of Molecular Biology at Princeton University. Levine epitomizes the Rockefeller scientist, as he internalizes a great deal of scientific diversity (with broad knowledge of molecular biology, cell biology, and genetics) and has a good awareness of the history of various fields of science. Rockefeller has come full circle. As was the case with the first director, Simon Flexner, Levine thinks strategically. He has a good sense of where science is moving, is very conscious of where he wants to direct the University, is keen to promote the training of young scientists, and heads an organization which for the moment has substantial funding and relatively high morale.

At the same time, the Rockefeller organization has undergone profound changes over the past century. Even in its recent past, there has been exponential growth in its infrastructure, with hundreds of research associates, postdocs, technicians, and other support staff. However, it is still very small relative to other American universities, with around fifty full professors, twenty-seven associate professors, and fifty-three assistant professors (Brennan, 1999: 47). Because of its small faculty, there is far more frequent and intense communication among a larger proportion of its scientific staff across diverse fields of science than at any other American research university. True, Rockefeller is more fragmented than it was forty years ago because it has so many labs, but it is much less differentiated internally than every other American university. The fact that there are still no departments and that a lab is likely to close down when the head of the lab departs means that the organization has an extraordinary flexibility to adapt to the fast pace of change in the global environment of science.

This flexibility and adaptiveness explain why Rockefeller University, despite its small size, still towers over most research organizations in America. It has a higher proportion of its faculty as either members of the National Academy of Sciences or Howard Hughes investigators than any other research organization in America. Moreover, in an intensely competitive funding environment, its scientists receive more funding for biomedical research per scientist from the National Institutes of Health than scientists in any other research organization in America. In 1999 and 2000, Rockefeller University demonstrated that its extraordinary excellence as a center for major discoveries is still very much alive when members of its faculty received four of the most coveted prizes in biomedical science: Robert Roeder received the Louisa Gross Horwitz Prize in basic biomedical science, Roderick MacKinnon received the Lasker Prize for Basic Biomedical Science, and Günter Blobel in 1999 and Paul Greengard in 2000 received the Nobel Prize in Physiology or Medicine. These four prizes in a two-year period represent more recognition for major discoveries than most of the leading American research organizations received in the whole of the twentieth century (Hollingsworth, Hollingsworth, and Hage, 2002 forthcoming). In short, at the turn of the millennium there was excellent evidence that Rockefeller University is still one of the world's premier biomedical research organizations and is still receiving more recognition for major discoveries in biomedical science than any other organization.

CONCLUDING OBSERVATIONS

This case study of the Rockefeller suggests that the following factors are associated with organizations which have fundamental breakthroughs time and time again across a number of decades:

Organizational flexibility: Knowledge changes rapidly, and if an organization is to be continuously at the frontiers of fundamental new knowledge, it must be highly flexible so that it can frequently move into new areas of research. Most research organizations, hampered by organizational inertia, have great difficulty being flexible enough to develop fundamental new knowledge or to operate continuously at the frontiers of knowledge. Most organizations experience a great deal of organizational inertia, tending to reproduce research units or traditions when scientists retire or resign, rather than moving into new research areas.

Scientific diversity and integration: Organizations which make major breakthroughs time and time again are those with a moderately high degree of scientific diversity. For fundamental breakthroughs to occur time and time again in an organization, scientists in diverse fields must have intense and frequent interactions with one another. In short, there must be a good degree of scientific integration. How this is attained varies from organization to organization, but the integration of scientific diversity is facilitated by scientists' socializing with each other. Examples are the sharing of lunch and/or tea, scientific retreats, journal clubs held jointly by different research units, or lectures which all the scientific staff are expected to attend. For intense and frequent interaction to occur on a scale comparable to that at the Rockefeller, an organization's scientific staff must avoid absences of weeks or months at a time.

Leadership: If an organization is to have major breakthroughs over a long period of time, a particular kind of leadership is needed—leaders with a good sense of the direction in which science is moving, the ability to identify talent, to skill to facilitate the movement of the organization in the desired direction, the ability to generate funding to move into new fields of knowledge, and, finally, the capacity to provide a nurturing environment (an environment in which there is rigorous criticism, meted out with a high degree of sensitivity).

At the Rockefeller, the president has always been directly involved in the recruitment and promotion of all permanent staff. All presidents not only provided scientific leadership, but were also involved in the administration of the organization. The philosophy of the Rockefeller was (and still is) that the scientific agenda had to be well integrated with administrative services, and the best way for this to occur was for the same person to direct the scientific and administrative activities.

Recruitment: The Rockefeller had extremely high standards for making permanent appointments. For many years, for every young scientist who was promoted internally to a permanent appointment, there were between twenty and twenty-five scientific staff who were not retained. Indeed, the Rockefeller historically has treated every permanent staff position as its most precious asset. Not only has the president been intimately involved in every permanent appointment, but the president historically has had the right to veto an appointment, and over the years has exercised this veto power frequently. In addition, the Rockefeller has always depended on external advisors in the making of permanent appointments. During the first fifty years, there was a board of scientific directors made up of some of the most distinguished scientists in America, and not a single appointment was made without their extensive participation. During the last half century, the board of trustees has included a group of distinguished scientists and they, too, have been intimately involved in exercising oversight over permanent scientific appointments. They have frequently exercised veto over appointments which they have considered of insufficiently high quality.

Historically, most senior scientists at the Rockefeller have been promoted through the ranks. Indeed most, though not all, of the Nobel prizes awarded to the Rockefeller staff went to scientists who arrived at the Rockefeller as very young scientists and who rose to be world-class scientists at the institution. Very few of its most distinguished scientists were recruited from the outside as senior scientists.

Organizational autonomy: The Rockefeller University has been embedded in a weak institutional environment during most of its history. As a result, it has had the capacity to appoint scientists to senior positions without being constrained by externally imposed norms of credentialing (e.g., habilitation, formal training in the fields of appointment), and the organization has been relatively independent of control by state bureaucracies and their rules. Because of its autonomy, it has had a high degree of flexibility to carry out almost any line of activity which its leaders have wanted to conduct.

Finally, the excellence of the Rockefeller has been due to the fact that it has had a very rich learning environment. Its staff have long been engaged in educating one another across fields. A great research organization is one in which most of the participants have a high degree of curiosity about, and familiarity with, what all the permanent staff in the organization are doing. At the same time, the Rockefeller has been a rich training environment for young people recruited primarily as young postdoctoral scholars, who have been expected to move on after three or four years at the most. Part of the Rockefeller's great learning excitement has resulted from the fact that its senior and junior staff have been recruited from many parts of the world, and this cultural diversity, added to its scientific diversity, has enhanced very high levels of creativity within the organization.

ACKNOWLEDGEMENTS

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An early draft of this paper greatly benefited from the comments of Richard Whitley of the University of Manchester, and of Darwin Stapleton, Director of the Rockefeller Archive Center—who has been very supportive of my research at the Center for many years. I also wish to acknowledge the indispensable help of David Gear, Research Associate, University of Wisconsin, whose aid has been invaluable for many aspects of this paper and the larger research of which it is a part. I especially thank Ellen Jane Hollingsworth who has participated in many aspects of the larger project. I regret that she has not permitted me to list her as co-author of this paper, which her contributions clearly merit!

APPENDIX ONE

SCIENTISTS WHOSE MAJOR DISCOVERIES, ALL OR PART, WERE MADE AT ROCKEFELLER INSTITUTE/UNIVERSITY AND THE FORMS OF RECOGNITION FOR THEM

Name of Scientist	Forms of Recognition
Oswald Avery [1]	Nominated 10 times in three different years for Nobel Prize in Physiology or Medicine 1931.[2] For the discovery that the immunological specificity of type II pneumococcus is due to a polysaccharide. Copley Medal 1945. For recognizing the transforming principle of DNA.
Günter Blobel	Horwitz Prize 1987; Lasker Prize 1993; Nobel Prize in Physiology or Medicine 1999. For his discovery that proteins have intrinsic signals that govern their transport and localization in the cell.
Alexis Carrell	Nobel Prize in Physiology or Medicine 1912. For his work in suturing blood vessels and in the transplantation of organs.
Albert Claude	Horwitz Prize 1970; Nobel Prize in Physiology or Medicine 1974. For applying the techniques of centrifugation and electron microscopy to the isolation and identification of subcellular structures.
Lyman Craig	Lasker Prize 1963. For his countercurrent distribution technique as a method for the separation of biologically significant compounds, and for the isolation and structure studies of important antibiotics.
Christian de Duve	Nobel Prize in Physiology or Medicine 1974. For combining subcellular fractionation with biochemical analysis in order to discover the cell organelles of lysosome and peroxisome and for identifying their functions.
Gerald Edelman	Nobel Prize in Physiology or Medicine 1972. For determining for the first time the complete chemical structure of immunoglobulins (antibodies), the key molecules of immunity.
Simon Flexner	Nominated 10 times in three different years for Nobel Prize in Physiology or Medicine 1911.[2] For developing serum treatment of cerebrospinal meningitis.
Paul Greengard	Nobel Prize in Physiology or Medicine 2000. For his discoveries concerning signal transduction in the nervous system.
Hidesaburo Hanafusa	Lasker Prize 1982. For demonstrating how RNA tumor viruses cause cancer, and elucidating their role in combining, rescuing and maintaining oncogenes in the viral genome.

H.K. Hartline	Nobel Prize in Physiology or Medicine 1967. For work on the physiology and chemistry of vision.
Bertil Hille [3]	Horwitz Prize 1976; Lasker Prize 1999. For elucidating the functional and structural architecture of ion channel proteins, which govern the electrical potential of membranes throughout nature, thereby generating nerve impulses and controlling muscle contraction, cardiac rhythm, and hormone secretion.
Henry Kunkel	Lasker Prize 1975; Horwitz Prize 1977. For his discoveries in immuno-pathology.
Karl Landsteiner	Nobel Prize in Physiology or Medicine 1930. For his classification of blood groups and for his further discoveries over the years of subgroups within the original groups which he identified.
Jacques Loeb	Nominated 10 times in three different years for Nobel Prize in Physiology or Medicine 1911.[2] For his research on the colloidal behavior of proteins.
Roderick MacKinnon	Lasker Prize 1999. For elucidating the functional and structural architecture of ion channel proteins, which govern the electrical potential of membranes throughout nature, thereby generating nerve impulses and controlling muscle contraction, cardiac rhythm, and hormone secretion.
Bruce Merrifield	Lasker Prize 1969; Nobel Prize in Chemistry 1984. For his development of a simple and ingenious method for synthesizing peptides and proteins.
Stanford Moore	Nobel Prize in Chemistry 1972. For research on enzymes, body proteins central to life; particularly for working out for the first time the chemical structure of pancreatic ribonuclease, an enzyme that breaks down ribonucleic acid (RNA).
Hideyo Noguchi	Nominated 10 times in three different years for Nobel Prize in Physiology or Medicine 1921.[2] For his research in demonstrating the relationship between Oroya Fever and verruca peruviana and cultivation of the causative agent.
John Northrop	Nobel Prize in Chemistry 1946. For the preparation of enzyme and virus proteins in pure form.
George Palade	Lasker Prize 1966; Horwitz Prize 1970; Nobel Prize in Physiology of Medicine 1974. For contributing important techniques of centrifugation and electron microscopy and using them to define how cells synthesize proteins and how they package proteins for secretion.
Keith Porter	Horwitz Prize 1970. For his fundamental contributions to the electron microscopy of biological materials.

Robert Roeder	Horwitz Prize 1999. For his research on the processes of gene activation.
Peyton Rous	Nobel Prize in Physiology or Medicine 1966. For establishing a virus as the cause of chicken sarcoma.
Theobald Smith	Nominated 10 times in three different years for Nobel Prize in Physiology or Medicine 1921;[2] Copley Medal 1933. For his research on host-parasite interrelationships.
Wendell Stanley	Nobel Prize in Chemistry 1946. For the preparation of enzyme and virus proteins in pure form.
William Stein	Nobel Prize in Chemistry 1972. For research on enzymes, body proteins central to life; particularly for working out for the first time the chemical structure of pancreatic ribonuclease, an enzyme that breaks down ribonucleic acid (RNA).

1 Oswald Avery received recognition for two major discoveries. One was his work on polysaccharides in the 1920s and the other was his work on DNA in the 1940s (Dubos, 1976; McCarty 1985; Amsterdamska, 1993; Bearn, 1996; Dale, 1946).

2 Scientists who are included in this study as a result of having 10 nominations in three different years are identified as "10 in 3" and the date listed for them is the first odd-numbered year in the decade in which they received the largest number of nominations. Thus, a scientist receiving two nominations in 1911, three in 1913, and five in 1921 would be listed with the year 1921.

3 The body of work for which Hille was recognized was begun while he was a student at Rockefeller University.

APPENDIX TWO
SOURCES

INTERVIEWS*

Henry Abarbanel, Professor of Physics and Director of Institute for Nonlinear Science, University of California San Diego. Interviews at UCSD Faculty Club, 5 February, 24 February 1998, 13 July 2001.

Pnina Abir-Am, Scholar in Residence at Rockefeller Archive Center. Interview at Rockefeller Archive Center, 10 May 2001.

Richard Adamson, former Chief of Cancer Etiology, National Cancer Institute of Health. Interview in his office in Washington, D.C., 2 March 1995.

Michael Aiken, Provost, University of Pennsylvania. Interviews in his office and in his home, 18, 19 February 1993.

Jens Alber, Professor of Administrative Sciences, Konstanz University (Germany). Interviews 11 October 1994, 23 November 1994.

Bruce Alberts, President, National Academy of Sciences. Former Professor of Biochemistry and Biophysics, University of California, San Francisco; former Professor of Biochemistry, Princeton University. Interview in his office in Washington, D.C., 20 November 1995.

Robert Alberty, Dean Emeritus of Science and Professor Emeritus of Chemistry, MIT. Interview in his office, 2 May 1995.

Vince Allfrey, Emeritus Professor Rockefeller University. Interview in his office, 22 February 2000.

Jutta Allmendinger, Professor of Sociology, University of Munich. Interview in her office, 16 June 1998.

Fred Appelbaum, Director, Division of Clinical Research, Fred Hutchinson Cancer Research Center (Seattle). Interview in his office, 31 July 1995.

Reginald M. Archibald, Professor Emeritus, Rockefeller University. Interview at Rockefeller University, 2 February 2001.

Ruth Arnon, Professor, Weizmann Institute, Israel. Interview in San Diego, CA, 17 February 1998.

Michael Ashburner, Professor of Genetics, University of Cambridge. Interview in his office, 9 June 1999.

Alan Attie, Professor of Biochemistry, University of Wisconsin (Madison). Interview at author's home, 15 November 1996.

Stratis Avrameas, former Director, Unité d'Immunocytochimie, Institut Pasteur, Paris, France. Interview in French in his office 12 May 2000.

W. Bachtold, President of Research Council, Swiss Federal Institute of Technology (ETH), Switzerland. Interview in his office, 12 December 1994.

William Baker, Chairman of the Board of Trustees Emeritus, Rockefeller University, and former head of Bell Labs. Interview at Rockefeller University, 30 January 2001; telephone interview 2 February 2001.

Evan Balaban, Fellow at Neurosciences Institute, San Diego, CA. Former student, Rockefeller University. Interviews at Neurosciences Institute, 6 February 1996, 7 January 1998, 12 April 2000.

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

Michel Barne, former Director of Unité de virologie et vaccins viraux at Institut Pasteur. Interview in English in his apartment, 16 March 2000.

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, CA, 6 February 1998.

Steve Bass, Biological Scientist, Genentech Inc. Interview at Cold Spring Harbor Laboratory, New York, 26 August 1995.

Bob Bauer, Manager, Advanced Systems Development Laboratory, Xerox Palo Alto Research Center. Interview at Scripps Institute of Oceanography, 9 July 2001.

Alexander Bearn, Executive Director American Philosophical Society; Life Trustee and former Professor, Rockefeller University. Interviews in his office, 15 February and 23 August 2000.

William Beers, Vice President for Facilities and Research Support, Rockefeller University. Interview in his office, 18 May 2001.

Artur Benz, Professor of Administrative Sciences, Konstanz University (Germany), 30 November 1994.

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, CA, 17 March 1996; fourth interview in his office, 22 December 1999.

Jean Bernard, former Professor of Medicine, Hôpital Saint-Louis, Paris, France. Interview in French in his apartment 17 April 1998.

Michael Berridge, Professor of Zoology, University of Cambridge. Interview at Trinity College, 9 June 1999.

Howard Birnbaum, Professor of Material Science and Engineering, University of Illinois. Interview at Scripps Institute of Oceanography, 9 July 2001.

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

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.

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

Helmut Blocker, Head of Department, Genome Research, Protein Design, Gesellschaft für Biotechnologische Forschung mbH in Braunschweig. Interview in Klosters, Switzerland, 17 January 1995.

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

Michel Boiron, former Professor of Medicine and former Director, Unité de virologie des leucémies, Hôpital Saint-Louis, Paris, France. Interviews in his office in French 17 June 1997 and 12 April 1998.

Derek Bok, President Emeritus, Harvard University. Interview in his office, 24 April 1995.

Sir Christopher Booth, Royal College of Physicians. Interview in his office, London, 3 May 1997.

Guy Bordenave, Director, Unité d'Immunophysiologie moléculaire, Institut Pasteur, Paris, France. Interview in French in a laboratory 10 May 2000.

Michel Bornens, Director Laboratory de Cells, Unité mixe de recherche, compartimentation et dynamique cellulaire at Institut Curie, Paris. Interview in English in his office on 27 May 2000.

David Botstein, Professor, Department of Genetics, Stanford University School of Medicine. former Professor of Biology, MIT. Interview at Cold Spring Harbor Laboratory, 25 August 1995.

Fred Bowen, former Executive Vice President, Rockefeller University. Interview at Rockefeller University, 14 March 2001.

Eugene Braumwald, Professor of Medicine, Chief of Medicine, Brigham and Women's Hospital. Interview in his office, 27 April 1995.

Jan Breslow, Professor, Rockefeller University. Interview in his laboratory, 14 April 2001. Second interview, 18 April 2001.

Gerth Brieger, William H. Welch Professor of History of Medicine, Science, and Technology, Johns Hopkins University, Baltimore. Interview in his office, 21 July 1997.

Henri Buc, Director, Unité de physicochimie des macromolécules biologiques, Institut Pasteur, Paris, France. Interview in French in his office, 6 March 2000.

Martin J. Bukovac, University Distinguished Professor, Michigan State University, East Lansing, MI. Interview in Bonn, Germany, 9 July 1996.

Stephen K. Burley, Professor and Deputy to the President for Academic Affairs and HHMI Investigator, Rockefeller University. Interview in his office, 10 April 2001.

Beth Burnside, Vice Chancellor for Research, University of California, Berkeley. Interview at Scripps Institute of Oceanography, 9 July 2001.

Robert H. Burris, Professor Emeritus of Biochemistry, University of Wisconsin (Madison). Interview in his office, 16 October 1995.

William J. Butterfield (Baron Butterfield), former Vice-Chancellor, University of Cambridge. Interview in his home, 12 July 2000.

Martin Cadwallader, Associate Dean of Graduate School, University of Wisconsin (Madison). Interview in his office, 7 and 8 March 1994.

Anthony Cerami, Director, Kenneth S. Warren Laboratories and former Professor and Dean of Graduate Studies, Rockefeller University. Interviews at Rockefeller Archive Center, 12 March 2001, 10 May 2001.

Henry Chadwick (Sir), former Master, Peterhouse College, University of Cambridge. Former Regius Professor, University of Oxford; former Regius Professor, University of Cambridge. Many interviews at Rockefeller Foundation Study Center, Bellagio, Italy, June 1994. Interview at his home in Oxford, 13 April 1997.

Margaret Chadwick (Lady). Numerous interviews at Rockefeller Foundation Study Center, Bellagio, Italy, June 1994.

Marsha Chandler, Senior Vice Chancellor for Academic Affairs, University of California, San Diego. Interview in her office, 9 March 1998. Other interviews in La Jolla, California, 7 December 2000, 12 July 2001.

Jean-Pierre Changeux, Director, Unité de neurobiologie moléculaire, Institut Pasteur, Paris, France. Interview in French in his office, 25 April 2000.

Erwin Chargaff, Professor Emeritus of Biochemistry, Columbia University. Interview at his home in New York City, 19 November 1993.

John Child, Professor of Judge School of Management, University of Cambridge. Interview in Stresa, Italy, 11 September 1997.

Purnell Choppin, President Emeritus, HHMI, and former Professor, Rockefeller University. Interview at Rockefeller University, 14 November 2000.

Philippa Claude, Senior Scientist, Department of Zoology, University of Wisconsin, Madison, and daughter of Albert Claude. Interview in her office, 13 March 2000.

George Cohen, former Director, Unité de biochimie cellulaire, Institut Pasteur, Paris, France, and former Director, Laboratoire d'Enzymologie, CNRS, Gif-sur-Yvette, France. Both interviews in his office, the first in French on 9 March 2000 and the second in English on 9 June 2000.

Joel Cohen, Professor, Rockefeller University. Interview in his office, 8 February 2001.

Mildred Cohen, Professor Emeritus of Bio-physics, University of Pennsylvania. Interview in her office, 31 August 1993.

Melvin Cohn, Professor Emeritus, Salk Institute. Interview in his office, 5 December 2000.

Randall Collins, Professor, Department of Sociology, University of California, Riverside. Interview at Royal Swedish Academy of Science, Stockholm, Sweden, 26–27 September 1996.

Karen Cook, James B. Duke Professor, Duke University. Numerous interviews, Rockefeller Foundation Study Center, Bellagio, Italy, May and June 1996.

Patrice Courvalin, Director of Unité des antibactériens at Institut Pasteur, Paris, France. Interview in his office in English, 25 February 1998.

Andrew Crawford, Professor of Physiology, University of Cambridge. Interview at University of Wisconsin (Madison), 26 June 2000.

Angela Creager, Professor of History, Princeton University. Interview at Bancroft Library, University of California, Berkeley, 12 August 1994.

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

Odile Croissant, maître de recherche, CNRS, Institut Pasteur, Paris, France. Interview in conference room in French, 18 April 2000.

E. David Cronon, Dean Emeritus of College of Letters and Sciences and Professor Emeritus of History. Interview at the University Club, University of Wisconsin, 17 March 1994.

Kathryn Crossin, Section head of Neurobiology Lab, Scripps Research Institute. Former postdoc at Rockefeller University. Interview at Neurosciences Institute, San Diego, 19 January 1996.

James Crow, Professor Emeritus of Genetics, University of Wisconsin (Madison). Interview in his office, 10 November, 1993.

Bruce Cunningham, Professor Scripps Research Institute, San Diego, CA. Former Professor, Rockefeller University. Interviews at Neurosciences Institute, 7 February 1996 and 11 March 1998.

James E. Darnell, Jr., Professor, Rockefeller University. Interview in his office, 10 April 1995. Other interviews in his office, 8 March 2001, 18 April 2001, 29 May 2001.

Robert Darnell, Professor, Rockefeller University. Interview in his office, 25 January 2001.

Eric Davidson, Professor of Biology, California Institute of Technology, and former Associate Professor at Rockefeller University. Interview in his office, 5 May 2000.

Lance Davis, Professor of Economics, California Institute of Technology. Interview in his office, 28 March 1994.

Scott Davis, Associate Director, Division of Public Health, Fred Hutchinson Cancer Center (Seattle). Interview in his office, 1 August 1995.

Robert Day, Director, Fred Hutchinson Cancer Research Center. Telephone interview, 20 July 1995.

Soraya de Chadarevian, Department of Philosophy and History of Science, Cambridge University. Interview, 4 November 1998.

Titia De Lange, Professor at Rockefeller University. Interviews in her office, 6 April 2001, 10 April 2001.

Manny Delbruck, (wife of Max Delbruck), Pasadena, CA. Interview at Cold Spring Harbor Laboratory, New York. Interview, 26 August 1995.

Hector F. DeLuca, Harry Steenbock Professor of Biochemistry, University of Wisconsin (Madison). Interview in his office, 17 March 1994.

Robert De Mars, Tracy Sonneborn Professor of Genetics, University of Wisconsin (Madison). Interview 14 December 1997 by telephone.

Pierre Devillers, former Member of the Directorate of INSERM, Institut Curie, Paris, France. Interview in his office in French, 15 May 2000.

Carl Djerassi, Professor of Chemistry, Stanford University. Interview in Madison, WI, 18 May 1995. Second interview October 7, 1997 in Department of Chemistry, University of Wisconsin (Madison).

Paul Doty, Professor Emeritus of Biochemistry, Harvard University. Interview in his office, 3 May 1995.

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.

Isabelle Dusanter, Research Director at INSERM, Paris, France. Interview at lunch, in English, 27 March 2000.

Walter Eckhart, Professor Salk Institute. Interview in his laboratory, 23 May 2000.

Gerald Edelmann, Research Director, Neuroscience Institute, San Diego, CA and former Professor and Dean, Rockefeller University. Interviews in Klosters, Switzerland, 17 January 1995, and at Neurosciences Institute, 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. Interview by phone 3 April 2001.

Mark Edinger, Professor of Chemistry, University of Wisconsin. Interview at O'Hare Airport, Chicago, 19 August 1995.

John Edsall, Professor Emeritus of Biochemistry, Harvard University. Interview in his office, 4 May 1995.

Manfred Eigen, Professor, Max-Planck Institut für biophysikalische Chemie, Göttingen, Germany. Interview in Klosters, Switzerland, 16 January 1995.

Gertrude Elion, Scientist Emeritus, The Wellcome Research Laboratories, Research Triangle Park, NC. Interview in her office, 17 March 1995.

Franklin Epstein, Professor of Medicine, Harvard Medical School, and former Chief of Medicine, Beth Israel Hospital. Also former Professor of Medicine, Yale University. Interview in his office at Beth Israel Hospital, 26 April 1995.

Tom Everhart, President, California Institute of Technology. Interview at the Athenaeum at the California Institute of Technology, 21 December 1994.

Marilyn G. Farquhar, Professor of Cellular and Molecular Medicine, University of California San Diego, and former Professor, Rockefeller University. Interview in her office, 25 May 2000.

Eugenio Ferrari, Scientist, Genentech Inc. Interview at Cold Spring Harbor Laboratory, New York, 26 August 1995.

Gerald R. Fink, Director, Whitehead Institute for Biomedical Research and Professor of Biology, MIT. Interview in his office, 4 May 1995.

Roberto Franzosi, Fellow of Trinity College, University of Oxford, UK. Interview at his home, 12 April 1997.

Hans-Joachim Freund, Heinrich Heine University, Düsseldorf, Germany. Interview at University of California, San Diego Faculty Club, 20 February 1998.

Jock Friedly, Freelance Journalist. Telephone interview, 4 September 1994; second interview in Washington, D.C., 22 August 1995.

Jeffrey Friedman, Professor at Rockefeller University and HHMI Investigator. Interview in his office, 16 March 2001. Second interview 18 April 2001.

Richard Furlaud, former Chair, Board of Trustees, Rockefeller University (1990–1998), Member of Board of Trustees (1976–1998), President of Rockefeller Council (2001). Interview at Rockefeller University, 17 May 2001.

Edward Furtek, Associate Vice Chancellor for Science and Technology Policy and Projects, University of California, San Diego. Interview at his office, 9 July 2001.

Daniel Carleton Gajdusek, Chief of the Laboratory for Slow Latent and Temperate Virus Infections and Chief of the Laboratory for Control Nervous System Studies at the National Institute for Neurological Disorders and Stroke. Interview at Neurosciences Institute, San Diego, CA, 11 March 1996.

Robert Galambos, Professor Emeritus of Neuroscience, University of California, San Diego. Interview at UCSD Faculty Club, 13 March 1998.

Einar Gall, Director, Neurosciences Institute, San Diego, CA. Interviews at the Neurosciences Institute, 15 March, 1996, 7 January 1998, 6 February and 17 February 1998.

Joe Gally, Senior Fellow, Neurosciences Institute, San Diego, CA, and former graduate student at Rockefeller University. Multiple interviews at the Neurosciences Institute, during visits at the Institute in 1996, 1998, and 2000.

Robert Gallo, Chief of the Laboratory of Tumor Cell Biology, National Institutes of Health, Bethesda, MD. Interview at University of Wisconsin Union, 13 March 1994. Interview in Bethesda, MD, 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.

Alex Glazer, Professor and Chair, Department of Molecular and Cell Biology, University of California, Berkeley. Interview in his office, 4 January 1995.

Ian M. Glynn, Emeritus Professor and Head, Department of Physiology, University of Cambridge. Interview in his room at Trinity College, 12 July 2000.

David Goodstein, Professor of Physics, and Vice Provost, California Institute of Technology. Interview in his office, 29 March 1994.

Judith Goodstein, Registrar, Archivist, and Historian, California Institute of Technology. Interview in Archives at California Institute of Technology, 24 March 1995.

Neva Goodwin, Co-Founder and Co-director of the Global Development and Environment Institute (G-DAE) at Tufts University and Trustee of Rockefeller University. Interview in her home, in Cambridge, MA, 16 February 2001.

Ellen Gordon, Dermatologist, Department of Medicine, University of Wisconsin (Madison). Interview in Dermatology Clinic, University of Wisconsin, 27 September 1999.

Jane Gorchevsky, Archivist, March of Dimes. Interview in White Plains, NY, 18, 28 May 2001.

Edgar Grande, Professor of Political Science, Technical University of Munich. Interview at Max-Planck-Institut für Gesellschaftsforschung in Cologne, Germany, 7 July 1996.

Harry Gray, Professor of Chemistry and Director of the Beckman Institute, California Institute of Technology. Interview in his office, 22 December 1999.

John Gray, Professor of Plant Sciences, University of Cambridge. Interview in his office, 8 June 1999.

Paul Greengard, Professor at Rockefeller University. Interview in his office, 16 May 2001.

Ralph Greenspan, Professor of Biology, New York University (1996); Senior Fellow, Neurosciences Institute (1998). Interview at Neurosciences Institute, San Diego, CA. Interviews 16 January 1996, 7 January 1998, 13 March 1998, 11 April 2000, 14 April 2000.

Joel Grossman, Professor of Political Science, Johns Hopkins University, Baltimore. Interviews 18, 19 and 20 July 1997; 28 and 29 December 1999.

Roger Guillemin, Professor, Salk Institute. Interview in his office, 8 May 2000.

Robert Gunn, Professor and Chairman of Department of Physiology, Emory University, Atlanta, GA. Interview in his office, 13 May 1997.

John Gurdon, John Humphrey Plummer Professor of Cell Biology; Chairman, Wellcome CRC Institute, University of Cambridge. Interview at Wellcome CRC Institute, 9 June 1999.

Charles M. Haar, former member, Board of Trustees, Massachusetts General Hospital and Professor, Harvard Law School, Harvard University. Numerous interviews at Rockefeller Foundation Study Center, Bellagio, Italy, May–June 1996.

Ed Hackett, Program Officer, National Science Foundation. Interviews at Royal Swedish Academy of Science, Stockholm, Sweden, 26–27 September 1996.

Peter Hall, Professor of Government and Associate Dean, College of Letters and Sciences, Harvard University. Interview at Harvard University, 3 May 1995.

Geoffrey H. Hartman, Sterling Professor, Yale University. Several Interviews, Rockefeller Foundation Study Center, Bellagio, Italy, June 1996.

Colleen Hayes, Professor of Biochemistry, University of Wisconsin. Interview at University of Wisconsin, 18 December 1997.

Bernadine Healy, former Director of National Institutes of Health. Telephone interview, 4 September 1994.

Norman G. Heatley, Biochemist and pathologist (retired), Oxford University. Interviews at St. John's College, University of Cambridge, 14 March 1997, and at his home, 13 April 1997.

John Heilbron, Professor Emeritus of History and History of Sciences, former Vice Chancellor, University of California, Berkeley. Telephone interview, 5 January 1995.

Nathaniel Heintz, Professor of Rockefeller University and HHMI Investigator. Interview in his office, 28 March 2001.

Virginia Hinshaw, Dean of the Graduate School, University of Wisconsin (Madison). Interview in her office, 11 November 1995.

Jules Hirsch, Professor, Rockefeller University. Interview at Rockefeller University, 14 March 2001.

Maurice Hofnung, Director, Unité de programmation moléculaire et toxicologie génétique, Institut Pasteur, Paris, France. First interview in French in his office on 14 March 2000 and second interview in English during lunch, 18 April 2000.

William Hollingsworth, Professor Emeritus of Medicine and former Associate Dean, School of Medicine, University of California, San Diego. Telephone interview, 18 February 1995 and interview at his home in San Diego, 14 March 1996.

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.

Norman Horowitz, Professor of Biology, California Institute of Technology. Interviews in his office, 28 March 1994, 20 December 1994.

David Horvath, Associate Dean, Letters and Sciences, University of Wisconsin (Madison). Interview in his office, 19 March 1998.

Rollin C. Hotchkiss, Professor Emeritus, Rockefeller University. Interview in Chicago, 14 October 1993. Interview by telephone, 18 March 2000. Interview in his residence in Lenox, MA, 17 February 2001.

Yoshiki Hotta, Director General of National Institute of Genetics. Interview in his office in Mishima, Japan. 27 October 1997.

Arthur Hove, former Assistant to the Chancellor, University of Wisconsin (Madison). Telephone interview, 25 February 1993. Second interview in the Library, University of Wisconsin (Madison), 12 December 1997.

Alice Huang, former Professor, Harvard Medical School and Biologist at California Institute of Technology. Interview at her home, Pasadena, CA. 22 December 1999.

David Hubel, Professor, Harvard Medical School. Interview in San Diego, CA. 13 March 1998.

James Hudspeth, Professor, Rockefeller University and HHMI investigator. Interview in his laboratory, 17 April 2001.

Sally Hughes, Professor of History of Medicine, University of California, San Francisco. Interview in her office, 10 August 1994.

Donald Humphrey, Associate Dean for Research in the Medical School and Professor of Physiology, Emory University, Atlanta, GA. Interview in his office, 13 May 1997.

Neen Hunt, Executive Vice President, Albert and Mary Lasker Foundation. Interview in her office in New York City, 26 March 2001.

Lee Huntsman, Associate Dean for Scientific Affairs, University of Washington School of Medicine. Interview in his office, 1 August 1995.

Andrew Huxley (Sir), Emeritus Professor of Physiology, University of Cambridge, and former President of the Royal Society. Interview in his room in Trinity College, 11 July 2000.

Hugh H. Iltis, Professor Emeritus of Botany, University of Wisconsin (Madison). Interview in his office, 14 October 1995.

Maren E. Imhoff, Vice President for Development, Rockefeller University. Multiple interviews in her office, January–June 2001.

Thomas R. Insel, Director, Yerkes Regional Primate Research Center of Emory University, Atlanta. Interview in his office, 11 May 1997.

François Jacob, Senior Scientist, Institut Pasteur. Interview at Cold Spring Harbor Laboratory, New York, 24 August 1995.

Kristina Johnson, Dean, School of Engineering and Professor of Bio-Engineering, Duke University. Interview at Scripps Institute of Oceanography, 8 July 2001.

Fred Jones, Associate Professor, Scripps Research Institute, San Diego, and former student at Rockefeller University. Interview at Scripps Institute, 16 January 1996.

Dale Kaiser, Professor, Department of Biochemistry, Stanford University School of Medicine. Interview at Cold Spring Harbor Laboratory, New York, 25 August 1995.

Harmke Kamminga, Director, Wellcome Unit for the History of Medicine, University of Cambridge, UK. Interview in her office, 14 March 1997.

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.

William Kelley, Chief Executive Officer and Dean, School of Medicine, Robert G. Dunlop Professor of Medicine, Biochemistry and Biophysics, University of Pennsylvania. Interview in his office, 18 February 1993.

Patrick Kenis, Professor of Administrative Science, Konstanz University (Germany), 24 November 1994.

Clark Kerr, Emeritus President, University of California. Interview at his home in Berkeley, 12 August 1994.

Daniel Kevles, Professor of History and Provost, California Institute of Technology. Interview at Athenaeum and his office, 29 March 1994; second interview at CalTech, 27 December 1999.

Marc Kirschner, Professor of Cell Biology, Harvard Medical School. Interview in his office, 1 May 1995.

Joseph Kirsner, Professor Emeritus of Medicine, University of Chicago, former Deputy Dean of Medical School, Head of Gastroenterology, and Chief of Staff, University of Chicago Medical School. Interview at his home, 7 May 1994.

Al Kirwin, Vice Provost for Research, University of Washington (Seattle). Interview in his office, 3 August 1995.

Irving Klotz, Professor of Chemistry, Northwestern University. Interview at Northwestern University, 29 October 1993.

Aaron Klug, former Director, Laboratory of Molecular Biology (LMB), Cambridge, UK, President of the Royal Society. Interview by telephone, 24 May 1999. Second interview in his office at LMB, 11 July 2000.

Robert E. Kohler, Professor of History and Sociology of Science, University of Pennsylvania. Multiple interviews in Philadelphia, July and August 1993, 14 February 2000.

Hans Kornberg (Sir), University Professor, Boston University. 1975–1995 holder of Dunn Chair in Biochemistry, University of Cambridge. Interview in his office at Boston University, 16 February 2001.

Dan Koshland, Professor of Biology, University of California, Berkeley, former Editor of Science. Interview in his Berkeley office, 5 January 1995.

Morgan Kousser, Professor of Social Science, California Institute of Technology. Interview in his office, 24 March 1995.

Edwin Krebs, Professor of Biochemistry and HHMI Investigator, University of Washington School of Medicine, Seattle. Interview in his office, 2 August 1995.

Julius Krevans, former Dean, School of Medicine, University of California, San Francisco. Telephone interview, 8 August 1994.

Gilbert Kukielka, Research Fellow in Cardiology, Johns Hopkins University, Baltimore. Interview at his home, 19 July 1997.

John Kuryian, Professor, Rockefeller University and HHMI Investigator. Interview in his laboratory, 20 April 2001. Telephone interview, 5 July 2001.

Trevor Lamb, Professor of Physiology, University of Cambridge. Interview in his office, 12 July 2000.

Peter Lange, Associate Provost (and later Provost), Duke University. Interview in Durham, NC, 16 March 1995, 20 September 2000.

Steve Langman, Scientist at Salk Institute. Interview at University of California, San Diego, 6 March 1998.

Todd LaPorte, Professor of Political Science, University of California, Berkeley. Interview at Scripps Institute of Oceanography, 8 July 2001.

Henry Lardy, Professor of Biochemistry and Enzyme Institute, University of Wisconsin (Madison). Interview in his office, 8 November 1993.

Ron Laskey, Professor of Zoology, Wellcome CRC Institute, University of Cambridge. Interview in his office, 8 June 1999.

Philippe Lazar, former Director of INSERM, Paris, France, Interview in his office at ORSTOM, in French, 30 March 2000.

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; interview by telephone, 27 August 1999, interviews in his office, 25 January 2001, 4 April 2001.

Elisabeth Leedham-Green, Archivist, Cambridge University Library, Cambridge, UK 2 November 1998, 6 November 1998, 10 June 1999.

Gerard Lehmbruch, Professor of Administrative Science, Konstanz University (Germany). Interviews 15 November 1994, 22 November 1994, 29 November 1994.

Richard Lerner, President of Scripps Research Institute, San Diego. Interviews in his office, 12 March 1996, 12 March 1998.

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.

Myron Levine, Department of Human Genetics, University of Michigan Medical School. Interview at Cold Spring Harbor Laboratory, New York, 24 August 1995.

Edward B. Lewis, Professor of Biology, California Institute of Technology. Interviews at Athenaeum and his office, 25 March 1994, 21 December 1994.

Albert Libchaber, Professor, Rockefeller University. Interview in his office, 11 May 2001.

Katja Lindenberg, Professor and former Chair, Department of Chemistry and Biochemistry, University of California, San Diego. Interviews at UCSD Faculty Club, 5 February and 24 February 1998.

Alice Lustig, Senior Vice-President, Rockefeller University. Interview in her office, 29 May 2001.

David Lyons, former Vice-President, Rockefeller University. Interview in his office, 30 January 2001.

Roderick MacKinnon, Professor, Rockefeller University and HHMI Investigator. Interview in his office, 1 March 2001.

Enid A.D. MacRobbie, Professor of Plant Sciences, University of Cambridge. Interview in her office, 11 June 1999.

Boris Magasanik, Jacques Monod Professor Emeritus of Biology at MIT, former Chairman, Department of Biology, MIT. Interview at Cold Spring Harbor Laboratory, New York, 26 August 1995.

Hubert Markl, President, Max-Planck-Gesellschaft zur Förderung der Wissenschaften, Munich, Germany. Interview in Bonn, Germany, 9 July 1996; second interview in his office in Munich, 15 June 1998.

Laurence J. Marton, former Professor at University of California, San Francisco School of Medicine, and former Dean, School of Medicine, University of Wisconsin (Madison). Interviews in his Madison office, 2 August 1994, 13 October 1995.

Charles Massey, former Senior Vice President, National Foundation. Interview by telephone, 29 October 2001.

Karl Ulrich Mayer, Director, Max-Planck-Institut für Bildungsforschung, Berlin. Interview in his office, 5 July 1996.

Maclyn McCarty, Professor Emeritus and Vice President Emeritus, Rockefeller University. Interview in his office, 6 February 2001.

Michael McCrum, former Vice-Chancellor, University of Cambridge. Interview in his home, Cambridge, UK. 10 June 1999.

Sheila McMichael (Lady) (Dr. Sheila Howarth), former Executive Officer of Medical Research Council of Great Britain. Interview at Royal Society of Medicine (London), 9 May 1997.

Peggy Means, Chief Operating Officer, Fred Hutchinson Cancer Research Center (Seattle). Interview at the Cancer Center, 1 August 1995.

Fritz Melchers, Director, Basel Institute of Immunology (Switzerland). Interviews in his office, 15 December 1994, 10 January 1995.

Bruce Merrifield, John D. Rockefeller, Jr. Emeritus Professor, Rockefeller University. Interview in his office, 11 February 2000.

Robert Merton, University Professor, Columbia University. Interview at Russell Sage Foundation, New York City, 19 April 1995.

Robert Metzenberg, Bascom Professor Emeritus of Biomedical Chemistry, University of Wisconsin (Madison). Interview in his office, 28 February 1995.

Pierre Meystre, Professor, Max-Planck-Institut für Quantenoptik, Garching, Germany; Optical Sciences Center, University of Arizona. Interview in Bonn, Germany, 8 July 1996.

George Miklos, Senior Fellow, Neurosciences Institute, San Diego, California. Interview at Neurosciences Institute, 16 January 1996.

Carol Moberg, Senior Scientist, Rockefeller University. Interview in her office, 11 February 2000. Interview by telephone, 15 March 2000.

Peter Model, Professor of Rockefeller University. Interviews at Rockefeller University, 26 March 2001, 8 May 2001.

Ian Molineux, Professor of Biochemistry, University of Texas (Austin). Interview at Cold Spring Harbor Laboratory, New York, 25 August 1995.

Maurice Montal, Professor of Biology, University of California, San Diego. Interview at Neurosciences Institute, 7 January 1998.

Geoffrey Montgomery, Special Assistant to the President, Rockefeller University. Interviews at Rockefeller University, 13 April 1995 and 14 July 1997.

Neil Morgan, Columnist, San Diego Union Tribune. Interview in his office, 19 May 2000.

Jack Morrell, Emeritus Reader, Bradford University. Interview at his home, 12 June 1999.

Anne Marie Moulin, Medical doctor and historian of science at INSERM Unit 158, Hôpital Necker, Paris. Interviews in Paris, 2 August 1996 and at Royal Swedish Academy of Sciences, Stockholm, Sweden, 27 September 1996.

Vernon Mountcastle, Professor of Medicine, Johns Hopkins University. Interview 7 March 1995. Second interview 11 August 2000.

Tom Muir, Head of Laboratory of Synthetic Protein Chemistry, Rockefeller University. Interview in his office, 28 February 2001.

Shigetada Nakanishi, Professor, University of Kyoto Medical School, Kyoto, Japan. Interview in his office, 30 October 1997.

Dan Nathans, Professor, Department of Molecular Biology and Genetics, Johns Hopkins University, Baltimore. Interview in his office, 21 July 1997.

William Neaves, Chief Executive Officer, Stowers Institute for Medical Research, Kansas City, MO. Interview in his office, 22 March 2001.

Paul Neiman, Director, Division of Basic Sciences and Program in Molecular Medicine, Fred Hutchinson Cancer Research Center (Seattle). Interview in his office, 31 July 1995.

Richard Nelson, Professor of Economics and Political Science, Columbia University. Interview at Columbia University, 23 February 2001.

Hans Neurath, Professor of Biochemistry, University of Washington School of Medicine. Interview in his office, 3 August 1995.

Rodney Nichols, Executive Director, New York Academy of Science, and former Executive Vice-President, Rockefeller University. Interview in his office, 26 February 2001.

Erling Norrby, Professor of Medicine and former Dean of The Karolinska Institute (Stockholm). Interviews at Scripps Research Institute, San Diego, 26 February 1996; Royal Swedish Academy of Sciences, Stockholm, Sweden, 26–27 September 1996, 18 September 2001; Neurosciences Institute, 3 February 1998.

Leslie Orgel, Professor, Salk Institute of Biological Studies. Interview in his office, 18 May 2000.

Ray Owen, Professor of Biology, California Institute of Technology. Interview in his office, 22 December 1994.

George Palade, Dean of Medical School, University of California, San Diego; 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, UK. Interview at Peterhouse College, 15 March 1997; interview at Laboratory of Molecular Biology, 11 June 1999.

David Pendlebury, Editor of The Scientist. Interview at his office in the Institute of Scientific Information (Philadelphia), 31 August 1993.

Joseph Perpich, Vice President for Grants and Special Projects, HHMI, Chevy Chase, MD. Interview in his office, 6 March 1995.

Robert Petersdorf, former Chair, Department of Medicine, University of Washington School of Medicine; former Dean of School of Medicine, University of California, San Diego; former President of American Association of Medical Colleges. Interview at his office in Veterans Hospital, Seattle, WA, 2 August 1995.

Harry L. Peterson, Vice Chancellor, University of Minnesota. Telephone interview, 17 March 1994.

Marnie Pillsbury, Trustee of Rockefeller University. Interview in her office at Rockefeller Center, 30 May 2001.

Van Potter, Hilldale Professor Emeritus of Oncology, University of Wisconsin (Madison). Interviews at McArdle Cancer Laboratory, University of Wisconsin (Madison), 26 November 1993, 13 April 1995.

Hans-Wolfgang Presber, Vice President for Medicine, Humboldt University, Berlin, Germany. Interview in his office, 25 June 1998.

Peter Preuss, President of the Preuss Foundation and Regent of the University of California. Interview at Scripps Institute of Oceanography, 8 July 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.

Beverly Purnell, Max-Planck-Institut für biophysikalische Chemie in Göttingen, Germany, and Department of Biochemistry and Molecular Biology, Pennsylvania State University. Interview in Bonn, Germany, 9 July 1996.

Jeffrey W. Purnell, Max-Planck-Institut für biophysikalische Chemie in Göttingen, Germany, and Department of Chemistry, Pennsylvania State University. Interview in Bonn, Germany, 9 July 1996.

Ronald Raines, Professor of Biochemistry, University of Wisconsin (Madison). Interview at author's home, 15 November 1996.

Hans-Jörg Rheinberger, Director of the Max-Planck-Institut für Wissenschaftsgeschichte, Berlin. Interview in his office, 29 June 1998.

Alex Rich, William Thompson Sedgewick Professor of Biology, MIT. Interview in his office, 28 April 1995.

George Rieke, Associate Professor, Rockefeller University. Interview 25 April 2001.

Hans Ris, Professor of Zoology, University of Wisconsin (Madison), and former postdoc at Rockefeller University. Interview in his office, 15 March 2000.

Nancy Rockafeller, Professor of History of Medicine, University of California, San Francisco. Interview in her office, 10 August 1994.

David Rockefeller. Interview in his office, New York, 24 July 1997.

Robert Roeder, Professor at Rockefeller University. Interviews at Rockefeller University 24 April 2001, 8 May 2001.

John Ross, former Chair, Departments of Chemistry, Massachusetts Institute of Technology and Stanford University. Interview at Neurosciences Institute, 18 February 1998.

Sheldon Rothblatt, Director of the Center for Studies of Higher Education, University of California, Berkeley. Interview in his office, 5 January 1995.

Henry Rozovsky, former Dean, College of Arts and Sciences, Harvard University. Telephone interview, 2 May 1995.

Harry Rubin, Professor of Molecular and Cell Biology, University of California, Berkeley. Interview in his office, 4 January 1995.

Martin Rudwick, Geologist and Professor of History, University of California, San Diego. Interview at University of California San Diego Faculty Club, 10 February 1998.

Marjorie Russell, Associate Professor, Rockefeller University. Interview at Rockefeller University, 8 May 2001.

William Rutter, Professor Emeritus of Biochemistry and Biophysics, University of California, San Francisco. Telephone interview, 11 August 1994.

David Sabatini, Professor and Chair of Department of Cell Biology, New York University Medical School. Interview at Rockefeller University, 28 March 2001.

Darrell Salk, Biomedical Scientist. Interview in Seattle, WA, 18 September 1996. Interviews in San Diego, 11 April 2000, 4 June 2001.

Fred Sanger, Emeritus Staff, Laboratory for Molecular Biology, Cambridge, UK. Interview at Emmanuel College, University of Cambridge, 7 June 1999.

Rudi Schmidt, Professor Emeritus of Medicine, former Dean of University of California Medical School, San Francisco. Interview in his office, 10 August 1994.

David Schubert, Professor, Salk Institute of Biological Studies. Interview in his office, 20 May 2000.

John Searle, Professor of Biological Philosophy, University of California, Berkeley. Interview at Neurosciences Institute, 17 March 1996.

Fred Seitz, President Emeritus of Rockefeller University. Former President, National Academy of Sciences and former Professor of Physics, University of Illinois at Urbana-Champaign. Interviews in his office in New York, 15 April 1995, 24 February 2000, 4 May 2001.

Maxime Seligman, former director of Unité d'Immunologie et immunopathologie. Interview in his office at Hôpital Saint-Louis in English, 21 April 2000 and 30 May 2000.

Irving Shain, former Vice Chancellor and Chancellor, University of Wisconsin (Madison), and former Provost, University of Washington (Seattle). Telephone interview, 24 July 1995. Second interview in his Wisconsin Alumni Research Foundation office, 20 December 1997.

Steven Shapin, Professor of Sociology of Science, University of California, San Diego. Interview in his home, 11 March 1996. Second interview, UCSD Faculty Club, 6 February 1998.

Lucy Shapiro, Professor and Chair, Department of Developmental Biology, Stanford University School of Medicine. Numerous Interviews, Rockefeller Foundation Study Center, Bellagio, Italy, May and June 1996.

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.

Fritz Sharpf, Director, Max-Planck-Institut für Gesellschaftsforschung (Cologne), Konstanz University (Germany), 23 November 1994.

Philip Siekevitz, Professor Emeritus of Rockefeller University. Interview in his office, 11 February 2000.

John Simpson, Dean of the College of Arts and Sciences, University of Washington (Seattle). Telephone interview 5 September 1996, interview in his office 19 September 1996.

Piotr Slonimski, former Director, Laboratoire de Génétique physiologique, Gif-sur-Yvette, France. Interview in English in his office 15 June 2000.

Henry Small, Editor, The Scientist. Interview in his office at the Institute of Scientific Information (Philadelphia), 31 August 1993.

Hamilton O. Smith, Professor, Department of Molecular Biology and Genetics, Johns Hopkins University, Baltimore. Interview in his office, 21 July 1997.

Lloyd (Holly) Smith, Professor Emeritus of Medicine, University of California, San Francisco. Interview in his office, 10 August 1994, telephone interview, 17 February 1995.

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. Interview in his office, 18 July 1997.

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

HHMI = Howard Hughes Medical Institute.

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[**] The Rockefeller Foundation Archives contain extensive materials on every center of biomedical research prior to 1960.

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Abbreviations used:
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

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