Introduction

The collaborating scientists, both Japanese and American, were ecstatic. Their historic discovery of neutrino mass was about to be announced at an international meeting to an audience of 300 physicists. The demonstration, convincing beyond a reasonable doubt to those present, showed that neutrinos, created from the impact of cosmic rays on the upper atmosphere, change form as they pass through the Earth. Although the mass of the neutrino itself had not been measured, neutrinos could now be held to account for a significant portion of the mass in the universe.

But even as participants argued about the number of Nobel Prizes that would result, a pall fell over the conference. The day after the discovery, major cuts in funding for the Kamioka Neutrino Observatory were announced by the Japanese government. Owing to budget shortfalls, an immediate 15 percent reduction would be followed by another 15 percent the following year. The Super-Kamiokande neutrino detector, costing $100 million to build, would have to be shut down for an extended period.[1]

"Big Science"

The aforementioned episode in the recent history of science illustrates both the intimate connection of modern science with political decision making and a blind spot in the way we interpret that connection. The Kamioka project is plainly "Big Science" in the sense originally intended by Derek Price and Alvin Weinberg. Kamioka absorbs the professional efforts of 120 scientists, and its large scale requires significant government funding. These resource requirements brought the project within the purview of Japanese national politics, and its decision makers chose to cut funding to cope with budgetary difficulties. The result is a morality tale for our times: Virtuous scientists with dreams of enlightening humanity, beware the power of bureaucrats who live in fear of unbalanced books, irregular procedures, and low approval ratings—they will cut you off even when Nobel-caliber results are within reach.

We, however, find the story of Kamioka telling for reasons that involve the composition and the organization of large scientific collaborations. The prospect of scientific discovery requires significant technological apparatus—in this case a ten-story water tank built under a mountain range—that serves as a focal point for the work of several organizations. Such a federation is in one sense robust—it is no mean feat to generate $100 million in government funding, design and build the tank, fill it with ultra-pure water, and surround it with electronic components that function around the clock to record the effects of subatomic particles passing through the tank. How, we wonder, can it be simultaneously so vulnerable to a downturn in the economy of a wealthy nation? It will not do to suggest that the project personnel are scientific geniuses with no aptitude for politics. There may be scientific genius in the project, but there are boatloads of political savvy and organizational skill as well. Kamioka would not have come this far without an effective organization of people and talented managers of resources.[2]

The standard starting point for coping intellectually with a phenomenon such as Kamioka is the phrase "Big Science," which is more than a label but less than a concept. Since its introduction by Price (1963) and Weinberg (1961, 1967) the term has become a fen of vagueness and ambiguity through overuse. It points to the exponential growth of science in the West, the increasing proportion of social resources devoted to scientific activities, and the large scientific projects established by government laboratories. Now studies of "Big Science" include university-industry collaborations in power production, the allocation of credit in collective efforts, the functioning of space programs, the effects of deadlines and scientific administration, and even the spatial distribution of firms in science parks.[3] "Big Science" also connotes the cooperation of researchers in teams, the massive infusion of funds from the state, the involvement of industry in directing R&D projects, the politicization and militarization of the research process, and the creation of international science and technology networks to address global problems. Our conclusion is that the notion is simply too broad to be analytically useful.

We believe it is essential to set aside our amazement at the scale of such undertakings as Kamioka and focus instead on their organization and on their pursuit of instrumentation. One of the most significant developments in late-twentieth-century science is the frequency with which the appetites of researchers for instrumentation have outstripped the resources of the organizations that employ them. Multi-organizational collaborations have come to populate many areas of science that require complex instrumentation or contributions from distinct disciplines or sectors. Whether the objective is to produce experiments in particle accelerator centers, to build and equip spacecraft or telescopes, or to obtain data from Earth's inhospitable places, the co-presence of scientists, engineers, project managers, technicians, graduate students, and staff from a variety of organizations constitutes a novel development in the social organization of science. In Karin Knorr Cetina's words, they embody new forms of social organization, an "eruption of structures . . . at once global and seemingly communitarian in nature . . . that represent possible alternatives to the small-scale conduct of expertise on the one hand and to other global structures" (1999: 201).

This work is an empirical study of an increasingly important structure for the production of knowledge in the modern world: the multi-organizational collaboration.[4] Whenever new organizational forms proliferate, they raise a variety of questions. How do such collaborations form? Are larger collaborations more bureaucratic than smaller ones? What are their principal means of organization? Are organizational forms related to technology? To what degree are collaborations dependent on trust and other factors? These questions form the skeleton of the chapters to follow.

We have built a database of 53 collaborations by analyzing the transcripts of oral histories conducted for a series of documentation research projects. The American Institute of Physics' Center for History of Physics carried out these interviews during the 1990s. In thematic terms, AIP's concern with documentation was similar in many respects to historical concerns with scientific change and sociological concerns with organizational structure and function. Archivists concerned with how collaboration records are produced, how they are used, and where they end up need to understand the collaboration's evolution, structure, and function. AIP's selection of cases reflects an interest in the policies of archives in the United States. Though some international and European collaborations were included for comparative perspective, our sample largely represents North American science. AIP's efforts have generated material for the first statistical, "meso-scale" examination of multi-organizational collaborations. The scope of the AIP sample is a pragmatic constraint on our research. Insights gained from the qualitative analysis of interviews AIP conducted with participants and administrators enabled us to develop sensible measures of the characteristics of collaborations. Using standard statistical analyses and classic organizational concepts, we identify relationships among the significant structural dimensions of the collaborations. We interpret these relationships by means of specific historical illustrations. These relationships are a map through the maze of possible pathways that scientists pursue to satisfy their curiosities and career ambitions; the histories reveal the mechanisms of modern collaboration. By discerning and then explicating these relationships on the basis of the material at hand, we believe we can both advance sociological understanding of these new organizational forms and enrich policy discussions of their utility and impact. Our interpretation is based on the assumption that *the structure of collaborations is best viewed in terms of the practices through which data are acquired.*[5] Without data, scientists cannot make claims of empirical knowledge and cannot build research careers. What matters most are the forms of organization that integrate or segregate structural elements of collaborations; these forms shape the autonomy of member scientists in producing knowledge.[6] The principle that may be said to emerge is that bureaucracy is the protector of freedom: collaborations use particular organizational forms to define their participant's rights to acquire and use data. We will not argue this point now, but we hope to demonstrate it in the chapters that follow.

In the next section, we distinguish our sense of "collaboration" from a variety of closely related senses. Next, current perspectives on scientific collaboration are reviewed. Because it is somewhat unusual, our specific method of combining historical and sociological techniques is treated in some detail. In the final section we introduce the twin themes of technology and bureaucratic organization, summarizing the line of argument in the five substantive chapters that follow.

Teamwork and Collaboration in Modern Physics

An empirically informed understanding of multi-organizational collaborations as organizations is essential for the sociology of science.[7] For the purposes of this study, an "inter-organizational" or "multi-organizational collaboration" is a research project involving at least three independent organizations.[8] This definition focuses attention on collaborations that are predicated on a federalist organization of power. The projects undertaken by *all* the collaborations in our sample could have been performed by single organizations had the political will existed to provide a single organization with the necessary resources or to empower a single organization to command the resources of other organizations. Although some nations are more inclined than others to create self-sufficient scientific organizations, even the large, territorial institutes of the Academy of Sciences in the former Soviet Union participated in some of the collaborations in our sample.[9] Today, with a single remaining superpower, science is entering a prolonged period in which voluntary cooperation among independent, often competitive organizations will be the primary means of pursuing large-scale research.

Several important modes of collaboration do *not* meet our criterion for multi-organizational collaboration. We are not concerned with cases whereby a single organization develops its research program partly by contracting out for the services of other organizations; the contractors in this scenario are selling their services rather than agreeing to dedicate resources to a common set of objectives. We are not concerned with collections of competing research teams that periodically swap findings or materials. Such groups often view themselves as "collaborating," but they are not coordinating their resources or setting a common agenda.[10] We are not concerned with cases in which one or two organizations are able to assemble the resources to carry out a large research project. Finally, we are not concerned with collaboration in the sense of the informal, unmanaged communication that has linked scientists in networks across organizational lines since the seventeenth century, if not longer.

Our study re-conceptualizes, in contemporary organizational terms, the theme of "teamwork" in scientific research. This staple of sociological analysis became prominent in the study of the sciences when sociologists and physicists recognized that aspects of collective, coordinated research, so effective in the mission-oriented Manhattan Project (Perry 1993: 957), were becoming routine in branches of peacetime research aimed at academic audiences. A. M. Thorndike of Brookhaven National Laboratory, which served as an important postwar organizational model for research both in the United States and in Europe, observed in 1967 that the "experimenter" in physics was rarely, if ever, a single individual:

> [He] may be the leader of a group of younger scientists . . . the organizer of a group of colleagues . . . a group banded together to carry out the work with no clear internal hierarchy . . . a collaboration of individuals or subgroups brought together by a common interest, perhaps even an amalgamation of previous competitors whose similar proposals have been merged by higher authority. . . . The experimenter, then, is not one person, but a composite . . . a social phenomenon, varied in form and impossible to define precisely. One thing, however, he certainly is not. He is not the traditional image of a cloistered scientist working in isolation at his laboratory bench.[11]

Such "teamwork," now held to be an indispensable feature of modern science, became an object of sociological research in the 1960s. The analysis of stratification in science examined phenomena such as the Matthew Effect, an aspect of which was the differential accrual of recognition by renowned senior scientists over their junior colleagues for collaborative work (Merton 1973). Warren Hagstrom, who in his classic 1964 work on the "scientific community" devoted a great deal of attention to group work, argued that the mutual dependence of researchers rendered collaboration necessary.[12] Hagstrom distinguished two forms of collaborative research: the traditional and the modern. The former had as its paradigm the professor-student association. The latter was embodied in a more complex form of organization, involving an intricate division of labor and greater centralization of authority.[13]

Absent from the considerations of sociologists in the 1960s was that continuing growth in the numbers of scientists working on an experiment would require increasing the number of organizations involved in an experiment. The history of particle physics after the 1950s illustrates that increasing the number of researchers on a project increases the number of organizations that are involved. Thus, a typical bubble-chamber collaboration at the European Center for Nuclear Research (CERN) in the mid 1960s consisted of about 15 physicists. One decade later the number of researchers working cooperatively on CERN's largest bubble chamber, Gargamelle, was about 50, from seven organizations. In 1985 the Delphi collaboration, working with the Large Electron-Positron Collider at CERN involved more than 350 high-energy physicists from 37 organizations in 17 countries (AIP 1992d).[14] Particle physics was not alone in this postwar development.[15]

What bear and reward sustained study today are cases in which organizations find their individual interests overlap sufficiently to merit joint pursuit of their ambitions, the competition among the individual participants is internalized and managed within the collaboration, and the contributions of three or more organizations make a project feasible. These larger, more complex, and relatively formal affairs are named and socially recognized nodes of organization within science. Their novelty and prominence revive questions that were posed in the 1960s, when both sociologists and physicists realized that meaningful experimental work could not be done by isolated and self-sufficient scientists: What is the basic unit of scientific research if not the individual? How do individuals build reputations and careers in view of this new organizational framework? How do permanent organizations build reputations and strengths when they must combine resources with competing organizations? What strictures do collaborations impose on their members? What forms of governance do they adopt, and what are the jurisdictions of their governors?

Approaches to Scientific Collaboration

To date, no comprehensive theory of scientific collaboration exists. Yet scholarly studies of collaboration are neither novel nor scarce. However, they are overwhelmingly either statistical studies of authorship patterns in the published scientific literature (bibliometric studies) or detailed narratives of individual projects, organizations, or sites. Though valuable, such studies are limited. To compare collaborations across specialties and to reach empirically based conclusions about their characteristics, evolutions, and outcomes requires the analysis of collaborations from several areas of the sciences. This is the first such study.

Bibliometric studies use the public evidence of research activity—most often papers and reports, patents and agreements—as indicators of trends and processes. Co-authorship is typically employed as an indicator of collaboration. Because scientific journals are specialized by field and because journal articles include the organizational affiliations of the authors, bibliometric studies can determine trends in collaboration across nations and across areas of science. Studies have generally shown that multi-institutional co-authorship has increased over time. For example, the percentage of such articles in the Science Citation Index[16] rose from one-third of all articles worldwide in 1981 to one-half in 1995.[17] This trend is evident for every country and every scientific field measured.[18] While worldwide multi-authorship in physics is actually lower than rates for all fields, the incidence for U.S. physicists is higher.[19] International collaboration is increasing too. From the early 1970s until the early 1980s the proportion of internationally co-authored papers doubled (Luukkonen et al. 1993). Measured as a percentage of all co-authored articles, international co-authorship increased from 17 percent in 1981 to 29 percent in 1995 across all countries and fields. Not only is international collaboration increasing; inter-sectoral collaboration also has grown. About 25 percent of all papers published by academic authors involved co-authors in another sector, compared with 20 percent in 1981.[20] Such results were one motivation for our study. But the available data for bibliometric studies set severe limits on the interpretability of results and on the character of questions that can be asked. Co-authorship stems from several types of social relationships—between colleagues, between teachers and pupils, between supervisors and assistants, and between junior and senior researchers. Yet the nature of these ties is not generally identifiable from the published record, and not all such relationships imply the existence of a formalized collaboration. Nevertheless, the trends are consistent with the view that collaboration among organizations is increasing in frequency and importance. In combination with the high public profiles of collaborations and the anecdotal impressions of eminent scientists regarding the increasing importance of collaborations, the case for sustained effort toward improving our understanding of collaborations is compelling.

The more serious limit on co-authorship data is that such data cannot generate insights into the internal dynamics of collaborations. Bibliometric studies group specific collaborative projects in static snapshots, without any indication of the underlying processes of formation, organization, and outcomes. The published result is the only evidence of a collaboration, divorced from social organization and context. Why did these scientists collaborate? How important was the distinction between leaders and followers? How often did they meet? How did they resolve their difficulties? In short, what happened during the process of collaborative work?

At the other end of the spectrum are theoretically informed case studies of particular collaborations or sites. Historians of science have provided extensively documented narratives of the development of accelerator laboratories that host particle-physics collaborations[21] and have recounted the stories of individual collaborative experiments (Krige 1993; Galison 1997). Anthropologists and sociologists have observed collaborative research projects and have provided important interpretive tools (Traweek 1988; Zabusky 1995; Collins 1998; Knorr Cetina 1999).

Case studies do overcome the main limits on bibliometric research, but they represent a beginning rather than an end. They share a microsociological focus, a qualitative methodology, a cultural-anthropological or narrative orientation, and (owing to the research intensity required by the approach) an emphasis on single organizations, centers, or projects. Their strengths are in providing theoretical guidance, identifying social processes, and raising questions about important organizational and cultural dimensions. But when the findings of case studies are contrasted, they display such diversity as to defy generalization. Three examples may suffice.

(1) Carlo Rubbia's success at lobbying the management of CERN and its political overseers to develop the accelerator needed to search for theoretically predicted heavy particles put him in position to form the collaboration credited for discovering the W boson (Krige 1993). In contrast, Lyman Spitzer, the main scientist to lobby the National Aeronautics and Space Administration (NASA) and Congress to develop an orbiting optical telescope was unable to secure any role in the design and construction of the telescope (Smith 1989: 248–258).

(2) The particle physicists who were building the first time projection chamber reluctantly and resentfully conceded that they had to abandon their role as patriarchal masters of their engineers for a power-sharing arrangement (Galison 1997: 574–640). In contrast, geophysicists planning to make *in situ* measurements in polar regions craved power-sharing arrangements with logistics experts and resented having to take time away from what they considered proper scientific work to master the intricacies of managing an expedition (Schild 1997). In further contrast, the European Space Agency has placed engineers in charge of space-science missions. The scientists contributing instrumentation do not even appear on mission organization charts and must compete with each other to acquire the resources that mission engineers control (Zabusky 1995: 70–102).

(3) Leon Lederman, who led the string of Fermilab experiments that included the discovery of the bottom quark, used his position to encourage his collaborators to tackle the topics he considered significant and to forge a consensus among participants about the quality of individual findings (Nebeker 1994). In contrast, Albert Silverman, the first spokesperson for the CLEO collaboration, was a coalition builder who sought out common and complementary interests among collaboration members who generally preferred to keep their own counsel on the direction and quality of research (Genuth 1992: 99–125).

Qualitative case studies, while illuminating both structural and cultural aspects, are unable to provide a systematic assessment of the relative importance of one process over another. Dimensions such as communication, the division of labor, work as a process, technology, negotiation, and size are all "crucial" to the scholar who discovers their importance, but little attempt is made to show why some may be more important than others. Further, it is not clear whether the collaborations that have been studied are representative. To what extent are the findings of case studies generalizable? Is an observed relationship unique to a particular collaboration, or is it a pattern characteristic of most? Our approach, a "meso-level" comparison of 53 collaborations in the physical sciences, enables us to characterize types of collaborations and to assess the importance of structures on the basis of their connections with processes or outcomes of interest. Far from "everything" being related to "everything else," there are relatively few patterns that emerge.

Method

Debating methods has been one of our favorite pastimes over the course of this study. Reflexivity is inherent in a project that has mobilized historians, archivists, and sociologists at a variety of institutional locations to examine multi-institutional collaborations in physics. That we required a multi-institutional collaboration to study such collaborations seemed to strike at the heart of questions regarding the "how" of the study. The contrast between historical and sociological approaches could always be counted on to arouse passion: the historical imperative of diving into primary materials for a nuanced assessment of particular scientific developments, versus the sociological necessity of understanding events as *kinds* of events. It seemed as if interdisciplinarity could only be achieved by placing two distinct studies side by side, with nothing but proximity to connect them.

But historical methods are located in historians, and sociological methods in sociologists. We began this project working for several years in two distant locations—historians and archivists at one, sociologists at the other. The turning point came when one of the sociologists moved to the other location, cross-fertilizing the teams and accounting for the method eventually adopted. Intellectually, the significance of this move lay in the realization that a relational understanding was an opportunity for all and a hindrance to none. More fundamental than the idea of case study versus statistical analysis, of archival immersion versus reliable coding, of qualitative detail versus quantitative precision, was the notion that our common questions centered on *comparisons* between these new kinds of scientific organization. Were there any commonalities in form and process? How best could we characterize the patterned diversity that was apparent in rudimentary comparisons of these collaborations? We rejected the notion that information about a single collaboration, however elaborately studied, however fully described, would be sufficient to make any claims about the nature of collaboration. As the number of cases mounted, each case became an illustration, but not one that could serve as representative until we could be convinced it manifested a repeated pattern. Arguments about representativeness were our most important source of understanding.

Galison describes the “image” and “logic” traditions as fundamental to the history of twentieth-century physics (1997: 19–31). We found the contrast an apt metaphor for our own methodological difficulties over the kinds of information we should collect, the ways it could and should be examined, and the format in which it should be presented. We came to see it as a window on the struggles in which the science and technology studies (STS) community has been increasingly disinclined to engage. As the field has drawn strength and numbers from outside its 1970s core of scientists and sociologists, the *case study* that describes a particular controversy, technological development, or scientific event, has become the common denominator, the common language of the field. In microphysics, the “image” tradition rests on a “deep-seated commitment to the production of the ‘golden event’: the single picture of such clarity and distinctness that it commands acceptance” (Galison 997: 22). Such an image would be, in its way, so complete and well defined, so free of distortion and background, that no further data need be brought to bear for the demonstration. In physics, these images were pictures of objects or particles, taken to be the constituents and characteristics of the world, such as Anderson’s picture of the positron in 1932 or the bubble-chamber picture of the single-electron neutral-current event in the 1970s. Even when the image presented was the result of a compilation of thousands of other images, the *demonstration* took the characteristic form of the selected picture. In the field of science and technology studies, there are such demonstrations, or golden events: the failure of formal means to transfer the skill of laser building or the indigenous knowledge of Cumbrian sheep farmers in their interactions with radiation scientists (Collins 1974; Wynne 1989).

In the “logic” tradition, a single picture, no matter how free of distortion and background, could never provide evidence of a fundamental kind. That kind of evidence rested on statistical demonstration. In the cosmic-ray experiments of the 1930s, Geiger-Müller counters were placed above and below a gold brick. The particle penetration of the gold was determined by the excess of joint firings of the two counters over the calculated accidental rate. Statistical significance was the important matter, since particular co-incident firings could well be accidental. In STS, too, there have been demonstrations in the logic tradition: the Matthew Effect, showing that rewards tended to accumulate over time to those with early career productivity; the increasing density of social ties with the development of problem areas (Mullins 1972; Cole and Cole 1973).

To reap the advantages of both quantitative and qualitative methodologies, we shifted our approach to the comparative, organizational properties of scientific collaborations. Many of these properties may be viewed as structural, but they are features generated by underlying social processes, and ultimately they result in scientific results, reputations, and success or failure. Because our goal was to understand the structure of scientific collaborations, we sought to create conditions under which simple forms of qualitative and quantitative analysis could be combined systematically. The analysis shifted during the course of the project from the micro to the meso level. The focus changed from interaction and everyday practice to the examination of multi-institutional collaborations as inter-organizational formations. The units of analysis became the collaborations themselves. Instead of studying how *people* interact in scientific projects, we examine how *organizations* work jointly in a collaboration, the structural features of these combinations, their variations, and their patterned social consequences.

What we sought was a combination of these two traditions of Image and Logic, of "Casing" and "Counting." Specific, historical details should illuminate the particular case in the context of a range of like cases. The problem, then, became how to select such cases and how to identify their relevant dimensions. The solution we adopted was to begin and end with the individual case, filtered through a quantitative examination of features that we could examine for all of the collaborations in the study. This involved five steps: (1) selecting a variety of collaborations in the physical sciences, (2) identifying a set of common dimensions, (3) categorizing the collaborations according to their values on these dimensions, (4) cross-classifying dimensions in order to discover significant associations between dimensions, and (5) identifying collaborations that represented these associations. The fifth step was crucial: we sought to determine which associations lent insight into common collaborative processes and which ones were spurious, uninterpretable, or meaningless.

The history of our own collaboration is a fair summary of these steps, involving the evolution of three main data-collection efforts combined in the analysis.[22] These phases move from high descriptive detail but less focused comparisons in a relatively narrow scientific area to more focused comparisons in a broader range of areas with less descriptive detail. One major finding that emerged from the sequencing of phases is that collaborations in high-energy physics—our first field of study—should not be taken as typical of collaborations in general. Subsequent phases impressed upon us the range of variability in the phenomenon of scientific collaboration.

AIP's Center for History of Physics provided the data for our analysis. The AIP Study of Multi-Institutional Collaborations in Physics and Allied Sciences was initiated in 1989 by Joan Warnow-Blewett and Spencer Weart to establish documentation strategies for the identification and preservation of records. The first phase, through the early 1990s, was devoted to the study of particle physics. The second phase examined space science, geophysics, and oceanography—field sciences with long traditions of teamwork. In both phases, we interviewed between five and fifteen members of particular collaborations, selected to cover a range of scientific styles and conditions. Through 1995, approximately 500 interviews had been conducted, focusing on the history and organization of these collaborations, their technologies, their management, and their outcomes. The third phase, which continued through the late 1990s, included 23 additional projects in five new specialties: ground-based astronomy, materials science, heavy-ion and nuclear physics, medical physics, and computer-mediated collaborations.

AIP started with particle physics because it has become both paradigmatic of collaborative work and a leading example of the scientific ascendancy of the United States after World War II. In the 1930s, atomic physicists, most notably E. O. Lawrence at the University of California at Berkeley, developed particle accelerators that required unusually large machinery and buildings, but great European experimentalists such as Enrico Fermi, the Joliot-Curies, and Otto Hahn used the charged particles flung out by radioactive materials either to probe atoms directly or to make neutrons, which could probe atoms without having to overcome the forces of electrical repulsion. However, as a starting point for experimentation, particle accelerators eventually won out over radioactive sources and bigger accelerators over smaller accelerators for physicists determined to investigate atomic substructures. American physicists after World War II used their new-found abilities to raise and manage government funding to create national laboratories that took over the development of accelerators too large, too expensive, or too specialized for universities or corporations to undertake. But since many physicists preferred to work in universities, and since one purpose of particle-physics experiments was to train new physicists, collaborations became standard for particle-physics experiments, with university groups typically contributing instrumentation to detect particles and national-laboratory groups providing instrumentation to customize the accelerator beam for the experiment. European nations appropriated this arrangement when they funded and built the European Center for Nuclear Research.

Around the time the AIP study began, Sharon Traweek estimated that there were approximately 1,000 active researchers in particle physics in the world, with another 2,000 abreast of the most recent developments (1988). In this first phase our selection of collaborations was facilitated by the existence of reliable databases on projects at most accelerator sites, as well as funding agencies such as the Department of Energy and the National Science Foundation. We concentrated on collaborative experiments approved between 1973 and 1984 at five of the major accelerator centers: Brookhaven National Laboratory, the Cornell Electron Storage Ring, CERN, the Fermi National Accelerator Laboratory, and the Stanford Linear Accelerator Center.[23] The experiments varied by detector type, target type, accelerator type, and scientific objective (e.g., a search for a rare process, or a crucial test of a theory). The final selection of collaborations was based on a list of 72 experiments from these sites. The list was first restricted to 27 experiments considered to be most important, and later reduced to 19 on the basis of the availability of participants for interviewing. Since inter-organizational collaborations are defined as those involving three or more organizations, we sought to interview at least one individual from each participating organization, including physicists, graduate students, engineers, postdocs, computer specialists, and technicians.[24]

In particle physics, most collaborations formed around accelerators in one of two ways. In some, a few leaders were committed to the exploration and use of all facets of a particle, a process, or an experimental technique. Such collaborations performed strings of experiments. In others, coalitions of physicists with diverse scientific interests, but with a common interest in a detector or an accelerator, performed free-standing experiments. Although every collaboration had a spokesperson to serve as a link between the collaboration and the accelerator laboratory, the responsibilities and duties of those individuals varied considerably. In string collaborations, the spokespersons were typically physicists who had conceived of measurements that could be accomplished with incremental changes to previous instrumentation designs; in free-standing collaborations, the spokespersons were those who initially suggested novel detector designs that could attract physicists with diverse interests. In the latter, administrative burdens were much greater. These collaborations were more likely to change spokespersons, either to improve their organization or to spread the onerous work among more participants. Hence, collaborations differed in levels of organization, in leadership, in joint planning, and in interdependence, as well as in the technologies of fixed-target and colliding-beam experiments.

The second phase of our study broadened the sample of collaborations to include space science, geophysics, and oceanography. Would these collaborations differ from those in high-energy physics? In space science and geophysics the role of the state is extremely significant; the formation of collaborations is lengthy and political. Industrial contractors were needed to develop instruments that would operate in the field for long periods. A wider range of sectors are involved in these collaborations than in particle physics, but again, collaborations were focused on research sites. In particle physics these sites were accelerators. In second-phase specialties, sites were either research vehicles such as spacecraft and ocean-going vessels or systems of data gathering. In geophysics and oceanography an attempt was made to include seismographical, climatological, and oceanographic research, both internationally and nationally recognized projects, and both smaller and larger collaborations. Fourteen projects in space science and geophysics were selected, most dating from the same period as the experiments in high-energy physics. The final sample consisted of six collaborations in space science and eight in geophysics and oceanography.[25]

As we began the interviews in the second phase, the unique qualities of high-energy physics became apparent: the dimensions of interest had to be generalized or we would not be able to answer the question raised earlier: Is an observed relationship unique to a particular collaboration or is it a pattern characteristic of many? We conducted 219 interviews with academic and government scientists, modifying our instruments owing to the complexity and variety of institutional settings. As in the first phase, transcripts of the interview tapes were obtained and indexed by historical and organizational themes. As an understanding of these fields grew, it became clear that other actors had important roles in space science and geophysics, so interviews were conducted with policy makers and program officers of funding agencies.

In space science, collaborations were managed by government agencies in space-flight centers; in geophysics, collaborations formed with multiple funding agencies and interests. Projects in these areas were field-oriented, with important structural contrasts to particle physics. Often experimental techniques were borrowed from other branches of physics and detection techniques were developed by the military. The

(U.S.) National Aeronautics and Space Administration and the European Space Agency provided management authority for engineers in flight centers, while teams of senior and junior scientists built instruments to meet the engineering constraints of a spacecraft. Because they could deal individually with project managers, the autonomy of individual science teams was high, with a Project Scientist designated to coordinate issues among a group of principal investigators. In some cases a new subcommunity of scientists was created, but only after political campaigns to marshal support within the space agency and the scientific community.

In geophysics, numerous funding agencies exist with diverse goals and structures, such that no standard organizational template is possible. Projects differentiate according to whether they "imported" or "aggregated" techniques. In the "importing" model, capabilities or techniques that had proven useful in other scientific areas or in industrial work were introduced into academic geophysics. Scientists formed consortia and hired executives to manage the development and deployment of instruments with the input of standing committees. In the "aggregating" model, a diversity of experimental specialists was mobilized to investigate a site or a process. A Science Management Office (SMO) was then organized to oversee the collection of data. The director of the SMO, who was usually one of the specialists deploying instrumentation, attempted to balance the specialists' common needs against their desire for individual autonomy. The scale, importance, or complexity of the instrumentation usually constrained the SMO director's options.

The third and final phase of the AIP project shifted away from the collection of exhaustive data on particular cases toward a systematic methodology that would allow the incorporation of a greater variety of fields along a limited number of dimensions. As a project team, we felt we had engaged with a significant number of collaborations and that the dimensions identified would be useful in the examination of collaborations outside these core areas. We sought to describe these new projects using a small number of informants, and after the second phase we were prepared for diversity.

The third phase involved a sample of 23 collaborations in five fields. The first field, which we labeled "uses of accelerators," was chosen to compare how scientists other than particle physicists organize themselves to use particle accelerators ($n = 6$). Ground-based astronomy ($n = 7$) was included because inter-organizational collaborations are prominent in the construction of telescopes such as the Wisconsin-Yale-Kitt Peak Project and the Columbus and Magellan telescopes, and in very-long-baseline interferometry. Materials research illustrates collaborations between universities, industrial, and governmental laboratories, such as the Superconductivity Center, microchip consortia, and the Polymer Interface Center ($n = 4$). In medical physics and clinical medicine ($n = 3$), multi-institutional collaborations are formed to pursue a new technique or therapy (e.g., digital mammography, image-guided needle biopsy). The final field, which we labeled "computer-centered collaborations," is not a traditional field of science, but it seemed clear that this type of collaboration would become more significant in the future because of the increasing ease with which data can be shared electronically. In addition to the Upper Atmospheric Research Collaboratory, which attempted to merge data streams from multiple remote instruments in real time, we included collaborations in parallel computation and cosmology ($n = 3$). What these projects share is a focus on a special mode of work centering on computers.[26]

The development of the survey instrument assumed increased importance as we sought to increase the number of cases by interviewing fewer scientists in each collaboration. The interviews from high-energy physics, space science, and geophysics were thematized in order to determine dimensions of collaboration that cut across all fields we had studied to this point.[27] We eliminated features that, however important in a specific case, seemed idiosyncratic or unique to the particular field of study. The dimensions that formed the basis for the final group of interviews were those that emerged as significant—or at least relevant—for the collaborations previously examined.[28]

Once the data from these interviews were collected and coded, individual records were aggregated to produce the data source used in what follows. Our method required collaborations as the units of analysis. First, information on each of the variables was averaged over the several informants for each project. Next, each aggregated variable was examined in relation to the individual component scores to determine the most reasonable aggregate score. Aggregate file variables were recoded to reflect the closest approximation to this "best summary" of the opinions of those involved in a particular collaboration.[29]

After we were satisfied with the coding and analysis of the third phase, we incorporated a group of collaborations from the first two phases by using the same set of dimensions. Clearly, our ability to do this was predicated on the fact that the third-phase instrument was developed from the dimensions initially identified from the interviews from the first two phases.[30] We revisited the original interviews and coded 110 interviews on

30 collaborations from the first two phases.[31] Owing to missing data, an attempt was made to follow up with 30 of those interviewed earlier.[32] Hence, the empirical analysis is based on information from participants in 53 scientific collaborations.

We began by exploring these thematic dimensions in the interview transcripts. Next, we examined univariate and bivariate relationships between these factors and other measured dimensions in this group of collaborations. Although such a procedure is guided by preliminary and revisable notions of likely associations, in the end it is largely inductive. It is not worth wasting too much time on ideas that do not pan out. The challenge arises when a relationship is evident between two dimensions that might plausibly be related. Many associations do not represent large differences, either statistically or substantively. Others can be interpreted, but not without an imaginative stretch. Since the historical understanding of cases *precedes* the search for statistical relationships, many associations were rejected as uninteresting.

The pivot point of our approach is the relation between the specific and the general, the illumination of a particular case in the context of a range of "like" cases and the illumination of a relationship in the context of a specific set of historical details from particular cases. Bar charts are used to display evidence for certain kinds of relationships. These, however, have another important function as a *selection criterion* for our interview extracts. How do most scholars and ethnographers select illustrative quotations? One hopes they are selected for some representative purpose. Yet with a large number of interviews it is no exaggeration to say that *interview extracts could be marshaled to illustrate almost any relationship.* To take one example from our analysis, it would be inappropriate to select materials that speak to the importance of trust from collaborations that saw themselves as successful. Why? Because such material can equally often be found in others that do not view themselves as successful. That, indeed, is at the root of what it means to say that trust is not related to assessed performance, as chapter 5 shows.

After identifying those associations that seemed promising or revealing, we returned to the case histories of individual collaborations that *constituted* the association. Take, for example, a positive relationship between conflict and interdependence. Such a relationship is positive because many conflictual collaborations are highly interdependent. Identifying these collaborations and examining the reasons for the association in particular cases lends specificity to the analysis and renders it less likely that the correlation is spurious. For example, in the discussion of the relationship between conflict and interdependence in chapter 5 we identify an experiment at the Stanford Linear Accelerator Center that helped us to understand the projects in which conflict between scientists and engineers proved more likely to occur.[33]

Implementation of the method across the five main substantive chapters involves the identification and analysis of specific cases that represent particular types of collaboration (chapters 1 and 3), or particular relationships (chapters 1, 2, 4, and 5) in order to understand the processes that characterize these social organizations in a more concrete fashion. Each chapter contains at least five illustrations of varying lengths. The main fields (particle physics, space science, geophysics, ground-based astronomy) are used at least twice. Seventeen cases, about one-third of the total employed in the quantitative analysis, illustrate such features as formation, magnitude, organization, technology, and conflict. About half of these illustrations begin in one chapter but recur in others, though in deference to the reader these are adjacent in all but two cases. The collaborations we frequently use as exemplary are Fermilab 715 (particle physics), Voyager (space science), DND-CAT (materials science), AMPTE (space science), and Keck (ground-based astronomy).[34] Other cases, including IUE, CRPC, and STCS, are used to illustrate a simple relationship. Every chapter includes at least one brief illustration from particle physics—one of our main themes is that this field is relatively unusual.[35] The case studies provide some insight into common collaborative processes, and they help to determine which patterns to interpret as meaningful and which to reject as uninterpretable or as beyond our imaginative powers.[36]

The weakness of our method is the selection bias inherent in a sample of collaborations that actually came to fruition. The results presented here must be viewed in the light of the kind of collaborations included in the study, a sample of 53 relatively successful collaborations, at least in the sense that they persisted long enough to receive resources. If a collaboration began and collapsed, or was discussed but never funded, it was not included in our range of cases. Simply put, we are unable to say anything about collaborations that were not collaborative for long. Quite possibly, such beginnings of organization constitute a larger number of scientific occasions than those that persist. Much remains to be said about collaborations from the time an idea is put forth until the day of funding or rejection.

Overview

Why collaborate? Put so starkly, the question implies a direct answer: Collaboration is viewed as the best or perhaps the only way of achieving one's objectives. Why have collaborations become more common and prominent in scientific communities? Because individual organizations cannot command the money, facilities, and expertise needed to acquire the kinds of data their scientists find meaningful. Thus, collaborations are appropriately described as "technoscientific," a term that gained currency in the 1990s as a way of emphasizing the fuzzy boundaries between equipment, practices, inscriptions, and claims in the local contexts where knowledge is created (Bijker et al. 1989). Collaborations are technoscientific in the specific sense that concept, design, and organization all revolve around the technological practices required to collect data.[37] Our central argument is that an understanding of modern scientific collaborations requires close consideration of the shaping roles of technology and bureaucracy. In theoretical terms, knowledge of nature is the "moral object" of collaboration, but the design and acquisition of instrumentation is the "real program," the proximate goal (Wuthnow 1987).

Just as large scientific collaborations must develop technological practices for acquiring data, they are also predicated on developing an organizational structure. There would be no point in discussing collaborations without some characterization of their organizational properties, whether from a scientific, a humanistic, or a managerial point of view.[38] Unfortunately, characterizing organizations for readers outside the social sciences is greatly complicated by the connotations of the term "bureaucracy," which many natural scientists consider an epithet or pejorative. Its use condemns a certain type of organization without argument. We will nevertheless employ the term "bureaucracy" in its traditional, descriptive sense to mean a hierarchical organization with a well-defined division of labor and with formal rules or procedures for achieving goals (Weber 1946). There is no better term that comports with both scholarly and ordinary language.[39] To say that one collaboration is more bureaucratic than another is not to criticize the first and praise the second; criticism and praise begin by asking whether a collaboration has the right level of bureaucracy for achieving its goals under its circumstances.[40]

This book is organized in five substantive chapters concerning the formation, the magnitude, the organization, the technological practices, and the experiences of scientific collaboration. How are collaborations formed? What differences are associated with larger size or longer duration? How are collaborations organized? Is there a relationship between organization and technology? What features, if any, are associated with the success of collaborations in the eyes of their participants? In the pages that follow, these questions are addressed through a combination of statistical analyses and case histories that illustrate a variety of structural relationships.

We begin in chapter 1 with the process of forming large projects. Using cluster analysis, we identify five basic ways collaborations form. These forms do *not* correlate closely with research specialty but with the level of complexity required by the collaboration. We introduce the term "encumbered" for collaborations that faced resource uncertainty, pressure from parent organizations, structural change at the funding agency, or the involvement of an external authority in selecting participants. The degree of encumbrance affects the organization and the interdependence of collaborations, with lasting consequences for their development. Collaborations tend to be larger when they form in the context of pressure from funding agencies and parent organizations. Owing to the importance of size in earlier discussions of "Big Science," chapter 2 focuses specifically on the magnitude of inter-organizational collaborations. We use "magnitude" in preference to "size" because the temporal dimension of collaborations, particularly duration from idea to funding, is as important as the number of participants.[41] Whatever the particular arrangement of members, the magnitude of collaboration has implications for its organization and management. Larger collaborations, not surprisingly, tend to involve greater formalization and more hierarchical structures. But by limiting the scope of activities subject to formal procedures and hierarchical decision making, even bureaucratic collaborations manage not to violate their members' sense of the quintessential nature of science. For example, where matters directly concern the production of results, deliberations are often widely participatory and decision making is often overtly democratic.

Chapter 3 focuses on the variety of ways in which collaborations organize themselves to accommodate the sensibilities of their individual and organizational members, to comply with the requirements of their funding agencies, and to satisfy the managerial and administrative perquisites for acquiring meaningful scientific data. The principal finding is that multi-organizational collaborations display patterned diversity, but the patterns do not generally coincide with scientific specialty. There is no "geophysics style of collaborating" or "materials science style of collaborating." Particle physics is an exception to this generalization, but even these collaborations can be used to illustrate contrasting categories and relationships. *Bureaucratic* collaborations possess many of the classical Weberian features: a hierarchy of authority, reliance on written rules and regulations, formalized responsibilities, and a specialized division of labor. *Leaderless* collaborations, like bureaucratic ones, are formally organized, highly differentiated structures. Yet in contrast to the more bureaucratic collaborations, these projects did not designate a single scientific leader to represent the interests of scientists or to decide scientific issues. The third principal form is *non-specialized* collaborations, which possess lower levels of formalization and differentiation than bureaucratic and leaderless collaborations. But because non-specialized collaborations, like bureaucratic collaborations, have a hierarchical structure for scientific decision making, we consider both non-specialized and leaderless collaborations "semi-bureaucratic." The remaining collaborations are *participatory*. Their members describe decision making as wide open and consensual, define organizational structure through verbally shared understandings or legally non-binding memoranda, and have few levels of authority and little use for formal contracts. Because particle-physics collaborations dominate this category, we speak of "particle physics exceptionalism."

Scientific collaborations are fundamentally dependent on equipment. Technology, broadly defined as the set of instruments and practices that scientists employ in the acquisition and manipulation of information, embodies interdependence and autonomy within collaborations. Chapter 4 considers how technological practices are related to organization. In general terms, the level of bureaucracy in collaboration is inversely related to the level of interdependence in the acquisition and analysis of data. The purpose of creating bureaucracies in these collaborations was to ensure that the organizations or teams would be autonomous in acquiring or analyzing data, whereas the purpose of participatory organization was to ensure that data streams would be collective property.[42] Most collaborations pursue partially interdependent technological practices, adopting aspects of bureaucracy that ensure their members can operate autonomously in some areas but must reach consensus in others.

These issues are crucially important for the scientists that participate in collaborations because they lead directly to credit and conflict that affects careers. In research where one's time, instrumentation, and ultimately data are one's own, there are no such issues. But no work on the sociology of recent collaborations should be considered complete without an effort to understand how collaborations are experienced by participants. Chapter 5 delves into perceptions of success or failure, with particular emphasis on their relation to trust. Trust has been a persistent theme in the literature on both science and organizational process. Social studies of science have ascribed a central role to trust in the constitution of knowledge since the beginning of modern science (Shapin 1994), yet there are few studies of its operation in collaborations where actors must coordinate their efforts toward a common goal. We find that foundational trust—trust that other scientists can contribute to the joint enterprise—*is* important for multi-organizational collaborations. But this foundational trust is so widespread that its presence does not discriminate between more or less successful projects. Assessing other forms of trust, we found no relationship between trust and the success of collaborations. Nor is there a relationship between complex trust and the extent to which projects are built from pre-existing relationships.

Why is trust thought to be so important? We find that trust is inversely related to conflict—which is, in turn, positively associated with bureaucracy. This explains why trust is generally viewed as positive. As much as scientists might like to conduct research in collaborations with colleagues they know and trust, the reality is that they are often in collaborations with strangers. They have become adept at developing mechanisms for overcoming deficits of trust and managing conflict. What we call the "paradox of trust" is that bureaucratic organization segments work to impose a structure for interaction that is in some sense non-collaborative. By elaborating formal structures for social practices, collaborations minimize mutual dependencies and reduce the need for high levels of trust. Some interdependence is characteristic of all inter-organizational collaborations, but the close interdependencies, low bureaucracy, and fluid organization that characterize particle physics are atypical.

Most of the large-scale projects analyzed here have technological motivations, are difficult to initiate, require massive efforts from their principals, and involve decision making with imposing career consequences. In all the other fields we examined, scientists in collaborations were more independent than particle physicists in the generation and dissemination of scientific results. They were also more autonomous in the activities that constitute the groundwork of collaboration. Technology, broadly conceived, is the basis for collaboration. The independence so valued by scientists is resistant to fracture *because* of bureaucracy, not in spite of it.