**How Experiments Begin: The Formation of Scientific Collaborations***

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Multi-organizational collaborations are increasingly important in contemporary science, but their formative processes have been neglected by scholars in the social studies of science. Based on an examination of 53 collaborations in physics and related disciplines, we have found five types of formations. Collaborations that encountered greater difficulties in forming became more formal in their organization and management. [9797 words]

## Introduction

How do large-scale, multi-organizational research projects form? What are the circumstances that impel and permit scientists to group themselves in collaborations that temporarily combine pieces and parts of their employing organizations? Are stories of their initiations similar within or across scientific specialties, nations, and science funding agencies? What effects do their origins have on their evolution? Such questions are no longer just for students of particle physics and the occasional investigator of a large field study. Materials scientists and biologists in various combinations now flock to accelerator laboratories that provide synchrotron radiation beams. Medical scientists conduct clinical research in collaborations involving dozens of medical centers. The home page of the United States National Science Foundation (NSF) includes in its list of disciplinary programs a link to A Crosscutting and Interdisciplinary Programs, which encourage scientists to draft multi-organizational proposals. Its Materials Research Division now supports Materials Research Science and Engineering Centers, which are often multi-organizational, rather than Materials Research Laboratories.

In forming a multi-organizational collaboration, researchers create a context they hope will serve them better than others. Science studies scholars generally, and historians especially, seek to identify the contexts in which knowledge is created, and use such findings in constructing characterizations of particular times and places. Addressing the formation of collaborations is thus an excellent starting point for identifying the features that characterize the pursuit of knowledge in the United States and western Europe at the end of the twentieth century.

To date, however, there have been few case studies of individual collaborations. The literature usually takes particle physics as its subject matter over other specialties, and addresses epistemology over social theory. Also, the literature reveals such a diversity as to defy generalization about the character of multi-organizational collaborations. Carlo Rubbia's success at lobbying the management of the Organization Européenne pour la Recherche Nucléaire (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. By contrast, Lyman Spitzer, after successfully lobbying the United States National Aeronautics and Space Administration (NASA) and Congress to develop an orbiting optical telescope, ended up with no role in the design and construction of the telescope and its suite of scientific instruments. Particle physicists building the first time projection chamber reluctantly and resentfully conceded that they had to abandon their role as patriarchal masters of their engineers. Instead, they created a power-sharing arrangement in which engineers managed construction and were entitled to veto physicists' ideas when they threatened the budget or schedule. By 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. By 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. The leadership of Leon Lederman in the string of Fermilab experiments that included the discovery of the bottom quark consisted of encouraging collaboration members to tackle significant topics and in forging a consensus among participants over the quality of individual findings. By contrast, the role of 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.

Given the paucity of case studies and their diversity of features, the cause of history is perhaps best advanced through sociology. A classification scheme based on research into collaborations from several specialties may provide a framework into which historians can fit case studies. Classifications can provide concepts to help characterize the diversity of research that otherwise remains lumped under 'Big Science'. Fortunately, the Center for History of Physics at the American Institute of Physics made multi-organizational collaborations the principal focus of its efforts in documentation research in the 1990s, and conducted hundreds of interviews with participants in selected collaborations. From this base, we have assembled a data set of 53 collaborations in particle physics, space science, geophysics, ground-based astronomy, materials science, and medical physics. (see Appendix A.) For each collaboration, we coded over 100 dimensions pertaining to formation, magnitude, organization and management, instrumentation and data, and outcomes. The application of 'cluster analysis' generates a taxonomy that reveals general patterns of formation. Analysis of relationships between project formation factors and other dimensions provides a basis for discussing the influence of a collaboration's formation on its evolution.

In the first section, we describe the dimensions used to characterize the formation of the multi-organizational collaborations in our data set. Cluster analysis yields an interpretable typology with five types of formations. We next interpret this typology, using an extended description of a single collaboration to illustrate each type. In the third section, we examine the attributes of a collaboration's formation and evolution, focusing on resource uncertainty, organizational sectors, and the role of participating organizations. To interpret these associations we refer to our chosen examples. We conclude by reflecting on the (ir)relevance of scientific specialty and patron to the formation of collaborations.

**Dimensions of Project Formation**

The formation of collaborations may be construed as the interplay of four factors: (1) the interpersonal context, or the relations among independent scientists who are responsible for research directions; (2) the donor context, or the availability of patrons and the fiscal and political climate in which such patrons operate; (3) the sectoral context, by which we mean the relations existing among academic, industrial, and governmental sectors, which traditionally have different goals and cultures; and (4) the home-organization context, in which we include permanent organizations like university departments, national research laboratories, government research laboratories, and corporate research laboratories. Table 1 summarizes the distribution of project formation characteristics of multi-organizational collaborations.

[Table 1 about here.]

We addressed the interpersonal context by probing the breadth and depth of collaborators's prior acquaintance and the extent to which they sought each other out. We described the degree to which a collaboration was built upon pre-existing relationships as 'high' when several participants reported they had previously worked together. We described the degree to which a collaboration was built on brokered relationships as 'high' when participants reported that a an external authority, such as a funding agency, actively determined who became members of the collaboration. While it was usually the case that collaborations built on pre-existing relationships had low brokering and vice-versa, these variables are not mutually exclusive. In some cases, both measures were high, e.g., when an authority insisted a collaboration be formed from two sets of independent proposers who had each previously worked together.

Donor (funding) context focuses on the entrepreneurship required -- either by the would-be collaboration or donor -- in order to launch the collaboration. Resource uncertainty indicates whether a collaboration had an ‘obvious’ source for the resources it needed, where ‘obvious’ indicates a funding agency with a program that fit the collaboration’s plans or a national research facility with the capabilities to support the investigations the collaboration sought to undertake. The variable distinguishes collaborations that justified themselves by claiming to be superior within a recognized research niche, from collaborations that went hunting for donors with arguments of novelty. The question about funding-agency reorganization enabled us to make distinctions among collaborations with obvious funding sources. When support came from a newly created funding category, or when a collaboration had to obtain more resources than the obvious source could provide, we coded the collaboration as relying on a full or partial reorganization of the funding agency. International collaborations were frequently based on partly reorganized funding arrangements.

To address the sectoral composition in the origin of a collaboration, we distinguished four types of organization: (1) university departments, (2) university institutes not affiliated with a single department (e.g., oceanographic institutes), (3) research institutes that are independent of a university, and (4) corporate research laboratories. By separating the first and second, we can distinguish collaborations in fields with strong links to traditional educational curricula from those in fields with weak links. We use an undifferentiated ‘research institutes’ for all manner of government and government-funded research organizations because their various legal arrangements were generally irrelevant to their roles in collaborations. We judged a collaboration to have had a dominant instigating sector when its original membership was overwhelmingly from one sector, or when participants from one sector pointed to institutions in another sector as having taken the lead in designing the collaborative project. We judged a collaboration as not having a dominant instigating sector when informants indicated that project planning was diffused among institutions from multiple sectors.

All the collaborations in our sample included participants employed by three or more organizations. However, the extent to which organizational interests figured in the formation of the collaborations ranged from crucial to nil. In some cases, participants needed approval to participate. Where, for example, corporate scientists had to convince their supervisors that collaboration was consistent with protecting intellectual property, or when university physicists had to convince their deans, we coded the collaboration as involving home-institution pressure. By contrast, collaborations of university physicists whose activities were covered by ongoing contracts between their organizations and a funding agency were coded as <u>not</u> involving pressure from home institutions.

**Types of Collaboration Formations**

By performing cluster analysis on selected project formation variables, we obtain a graph (known as a dendrogram) that groups collaborations on the resemblance between their formations (Figure 1). The further to the left on the distance axis, the larger the number of clusters, the smaller the number of each cluster's members, and the more similar are the members within each cluster. The further out to the right on the distance axis, the smaller the number of clusters, the larger the number of each cluster's members, and the less similar are the members within each cluster. One must always compromise between wanting the intellectual economy of a small number of clusters (at the far right of the figure) and the substantive cohesion of highly differentiated clusters (at the far left). Following social scientific convention, we use distance 10 as the cut-off point. This five-cluster solution is also the model we selected that best balances generality and interpretability. The combinations of variables that distinguish these five clusters are our basis for characterizing the contexts that give rise to collaborations.

[Figure 1 about here.]

**Type 1: The Dominant Sector/Conventional collaboration, or Wake Up and Smell the Coffee.**

Suppose you are an established scientist with good connections with comparable scientists who work for the same kind of organization. And further suppose you realize that there is an excellent opportunity to launch a research project that would <u>not</u> stretch the norms of what your home organization expects of you. But to take advantage of the opportunity, a collaboration must be formed. What do you do? The obvious answer is you find an appropriate time to tell your colleagues at these other organizations to WAKE UP AND SMELL THE COFFEE! Once you have enough such colleagues to appear credible as a collaboration, then you can take steps to attract the additional participants you need.

That, in the abstract, is the story of collaborations in this group. They form because members of similar organizations agree that they have an excellent shot at world-class research if they propose a project that requires working together. The impetus varied -- a boldly imaginative idea, changes in the availability of facilities, the need to learn new skills, expanding horizons of research, or simply the creation of new government programs. But in all cases, someone spread the word among peers while parlaying the opportunity into a dominant position. In terms of specific variables, these collaborations came about without pressure from the home organizations, formed with a dominant sector, with an obvious source of funds to solicit, and including a mixture of scientists, some of whom had, and others who had not worked together before.

Take as a concrete example the Active Magnetospheric Particle Tracer Experiment (AMPTE). Since 1958, when James Van Allen and his University of Iowa colleagues discovered the radiation belts that bear his name, the source and dynamics of these belts have had obvious bearing on the operation of satellites. In 1971, Tom Krimiges, a Van Allen protégée, who worked at the Applied Physics Laboratory (APL), realized that if the solar wind 'blew' charged particles into the magnetosphere, the process could be investigated by releasing tracer ions outside the magnetosphere and searching for those ions inside the magnetosphere. Initial calculations convinced him that detection and release capabilities were adequate to demonstrate the existence of such a process at levels where it could plausibly be assumed to occur.

Following this epiphany, Krimiges ran his ideas past Erwin Schmerling, the NASA Headquarters scientist with responsibility for magnetospheric physics. Krimiges also promoted the idea at the Max Planck Institute for Extra-Terrestrial Physics (MPIET) in Germany. Like APL, MPIET designed and built spacecraft to investigate near-Earth space; unlike APL, ion releases were a specialty of MPIET. The wisdom of pursuing this collaboratively was obvious: 'I remember Hermann Föppl, who was their chemical expert on how to do this [release ions], and when I met him, ... I noticed that his eyebrows had been burned off in one of those experiments'. Krimiges' counterpart, Gerhard Haerendel, was receptive and supportive. That news was sufficient for Schmerling to begin a trickle of funds to study designs for mechanically coupled spacecrafts for ion identification and ion release. Krimiges and Haerendel also lined up fellow scientists, inside and outside their organizations, whose instrumentation they wanted to include in a full-blown proposal to build the spacecrafts --making sure that the spacecrafts gave scientists chances to use their instruments for less risky explorations of the natural environment. Notably, the German scientists successfully argued for the inclusion of British colleagues, who turned the inert mechanical adaptor between the German and US spacecrafts into a third spacecraft.

The collaboration never wondered *where* and *how* to obtain funding; only, *when* it might come about. While MPIET committed itself to providing the ion-release spacecraft, all participants understood that APL needed dedicated NASA funds greatly beyond those Schmerling controlled. Matters languished until NASA asked outsiders to propose projects. But when this happened, Krimiges and Haerendel were ready – and drafted a proposal that balanced the risky active experiment against more secure goals. The external panel that NASA assembled to review the proposals recommended AMPTE, and the collaboration was in business.

Because AMPTE was inconceivable without the Applied Physics Laboratory, the Max Planck Institute for Extra-terrestrial Physics, and the Rutherford-Appleton Laboratory (which built the British spacecraft), research institutes were clearly the dominant partners. Because AMPTE's means and ends fitted readily into these organizations' traditions, they did not seek to influence the project. Because AMPTE was space-based research into theories of magnetospheric dynamics, not earth-based research nor research into environmental effects on military satellites, it was appropriate for NASA rather than for either the National Science Foundation (NSF) or the Department of Defense (DOD). And because the collaboration expanded to diversify its goals and broaden its appeal, it soon included scientists who had not worked together, as well as scientists who had.

**Cluster 2:** ***Dominant Sector / Unconventional Collaboration, or Damn the Torpedoes--Full Speed Ahead***.

Now imagine you are a scientist who senses that s/he is at a crossroads in her/his career. Maybe you have recently been hired and need a project that will make an impact on your new employer. Maybe you believe that you are near the end of what you can do with your current resources and skills, and have your eye on techniques that you do not have the means to acquire. Maybe the political economy of research is squeezing your discipline. And further suppose that you learn that a collaborative venture might secure you a new niche. Do you dismiss the opportunity because you don't know, and may not be compatible with the other people or organizations? Do you dismiss the opportunity because it might require you to spend time fighting to change rules or modify traditions in your home organization? Or fighting to change the rules or modify the traditions of someone else's organization? Or compromise your personal or organizational interests for the greater good? The people in these collaborations did not. They ignored the torpedoes, and managed to form collaborations – at least, before explosions sank their prospects.

These collaborations formed because scientists and their organizations had to work together to revive their scientific prospects. In most cases, there was no one individual whose ideas were clearly responsible for the project. In other cases, an individual's ability to control resources proved more important than his/her intellectual creativity. In most cases, conflicts among the organizations or their main participants were the source of potential explosions. In others, change was required in a single organization of strategic importance to the collaboration. Like the first type, these collaborations had a dominant instigating sector and an obvious source of funds. Unlike the first type, their members had generally not worked together before, and their home organizations was influential, because the collaboration promised (or threatened) to alter relationships among the home organizations.

The Incorporated Research Institutes for Seismology (IRIS) is a particularly dramatic example. The political economics of research was disheartening to many university seismologists in the early 1980s. Those interested in monitoring the Earth's vibrations at frequencies relevant to discerning large-scale, internal structures of the Earth, were chafing at their dependence on government observatories for detecting underground nuclear tests. Those interested in the regional structure of the Earth envied oil-exploration geophysicists, who acquired huge data sets by recording the vibrations of large numbers of intricately planned explosions, and then determined whether there were oil deposits at accessible depths. Every seismologist could walk into any number of other scientific laboratories, and see how new computers and more sophisticated programming had generated unprecedented capabilities for acquiring and analyzing digital data. Though a fortunate few had the social and technical abilities to work with national security or industrial data, or had the resources to develop instrumentation, most American university seismologists did not feel capable of building and analyzing their own large data sets.

At various conferences, American seismologists lamented their state and built momentum for a pan-university initiative. Independent workshops held in late 1983 and early 1984 laid out an NSF proposal for a digital global seismic network and for digital portable seismometers. When word filtered down from NSF that two large seismology proposals could undercut each other, the two groups agreed to form a single consortium and drafted a joint proposal, which NSF agreed to fund.

The workshops also demonstrated the conflicting interests within the collaboration. Some seismologists wanted to base the collaboration at a university with its own seismic network. Others feared that the collaboration would not treat members equitably if managed from a member organization. Those from universities with good government relations wanted to build upon what was in place. Others sought autonomy from government. Seismologists from universities with strong computing centers wanted the collaboration to concentrate on instrumentation. Others wanted the collaboration to support the processing of large data sets. Seismologists from universities with instrumentation laboratories wanted the collaboration to support them, while others wanted to tap industrial suppliers of instrumentation to the government and oil industry. How IRIS was structured and functioned was bound to affect the standing of the universities whose earth sciences departments had seismologists.

Significantly, several of the scientists who took on the development of an administrative structure for the collaboration believed they were entrusted with the task because they had *not* been among the instigators and were not partisan in the workshops' conflicts. They consulted university counsels, who made sure the consortium's articles of incorporation and by-laws ensured its tax-exempt status, but who did not offer advice on issues of governance. The scientists adopted, with modest incremental changes, the by-laws of an extant earth sciences consortium, and hoped for the best.

At one of the first meetings of the Board of Directors, the worst nearly came to pass. Disgruntled members planned to move for an immediate change in the by-laws in order to force a reconsideration of the collaboration's balancing of interests. The Chairman of the Board, having heard of the plan, brought a corporate lawyer who 'explained the facts of life of this corporation' to the directors. The move was defeated, and nobody has since threatened outright revolt over an intra-collaboration controversy.

IRIS plausibly had to represent academic seismology as a whole to justify its ambition to obtain large quantities of digital instrumentation, and to develop data-management techniques for the enormous data sets that such instrumentation would create. The homogeneity of its sectoral composition did not make its formation easy. The instigators could not restrict membership and remain credible; consequently, most participants had not previously worked together. The collaboration could not discuss the characteristics of the instrumentation and techniques that it hoped to develop without addressing the resources for seismology among American universities. Thus IRIS's member organizations, unlike AMPTE's, were suspicious of each other and tried to use collaboration procedures to their advantage.

**Type 3: *Entrepreneurial Funding Collaboration, or, Get Out of your Business-as-usual Rut***

Now imagine that you are an accomplished scientist who, while not exactly at a crossroads, is nonetheless uneasy about his/her career. Perhaps you have a position at an organization with aging facilities and fear that you will have difficulties remaining competitive. But when you think about government funding for recapitalizing your organization's facilities, you conclude that no government agency will underwrite a non-national facility. And when you think about broaching the subject with your organization's executives, you conclude you will just give them a good case of sticker shock. Or perhaps you have a secure position at an amply equipped but insecure organization that is under pressure to justify its continued existence. Or perhaps you are at a secure, amply equipped organization but have a structurally insecure position (e.g., a non-teaching faculty position at a university). In all these instances, ‘business-as-usual’ will not serve your interests. It will not lead to the acquisition of new facilities, a better justification for your organization’s continued existence, or a better justification for your existence within your organization.

Forming a collaboration in this category was a way to get out of the business-as-usual rut. If neither the government nor your home organization will recapitalize your facilities, cobble together a few institutions to share expenses and benefits. If you need to diversify the areas your organization covers with its first-rate facilities, form a collaboration with those struggling to tackle a promising area with second-rate facilities. If you need to justify your value because you do not perform or directly support one of your organization’s basic revenue-generating tasks, form a collaboration that links your research (and the research of your disciplinary colleagues at other organizations) with the interests of other organizational colleagues who are engaged in revenue-generating tasks.

Doing business unusually carries the burden of cultivating patrons. None of the collaborations in this group had an obvious source of funds. Some managed to fund themselves; some combined self-funding with government funding; some raised money from private philanthropies; some prodded multiple program offices to negotiate joint arrangements for providing funding; and some combined funding from several governments. Doing business unusually also requires developing a new set of procedures, which can best be done with people with whom you work well. Most of the instigators of these collaborations had previously worked together. These collaborations did resemble the previous type in that instigators faced pressures from their home institutions. Some had to overcome the opposition of colleagues with competing ideas for recapitalising facilities; some had to renegotiate their jobs within their organizations; some faced conflicts over the merits of individuals or organizations for intra-collaboration leadership.. Overcoming such difficulties invariably created a residue of bruised feelings.

The Berkeley-Illinois-Maryland Array (BIMA), a radio-astronomy facility built by the three universities in Hat Creek, California, is a good example. The National Radio Astronomy Observatory was founded in 1956 against the arguments of astronomers who believed their field would be better served by funding individual researchers than constructing large instruments. Yet, it succeeded in carving a niche, creating facilities that university astronomers could use. Given the competition between optical and radio astronomers, a drive to recapitalize an aging university observatory could founder before it left an Astronomy Department. Appeals for national funding had to demonstrate that a particular university’s facility was also in the national interest. So when Berkeley’s radio astronomers heard that University of Illinois astronomers were interested in jointly upgrading and expanding Berkeley’s array of radio antennae, they jumped: 'Even if we gave up half the use to another group, we'd still be miles ahead .... It was very much a win-win situation'.

However, a Berkeley-Illinois proposal needed more than the National Science Foundation (NSF) was willing to provide. Thus when Leo Blitz of the University of Maryland, who had held a post-doc under Jack Welch, the director of Berkeley's Radio Astronomy Laboratory, asked whether Maryland could join the collaboration, Welch responded, 'Sure, it just takes money'. So, with Maryland's radio facilities nearly obsolete, its optical astronomers lacking concrete plans to compete with BIMA, and a new Dean of Science and Engineering controlling discretionary capital, Blitz parlayed his relationship with Welch into an institutional commitment from Maryland.

Maryland's joining required the three organizations to revise their division of labor and rights of use. Welch suggested that Maryland just needed money to join, and assumed that this would not be contentious. Indeed, it was not, though some awkwardness was involved in the division of labor. As the builder of the original Hat Creek antennae, Berkeley was well equipped to provide additional antennae and hardware, while Illinois, as the host of a supercomputer center, could provide software and data storage. Maryland took responsibility for the cables connecting the antennae into an interferometer -- the hardware that Berkeley was least versed in -- and the interferometer operations and calibration programming --the software having the most intimate interfaces with the hardware. Because of the contrasting rules and regulations of three state universities, it took university counsels a year to work out an agreement that fit their administrative practices and embodied the understandings reached by the radio astronomers. Nevertheless, with the three universities contributing the capital, the NSF was willing to provide funds to operate and maintain a Hat Creek array for interferometry at millimeter wavelengths.

Forming a collaboration was the price that Berkeley, Illinois, and Maryland's astronomers paid to administer a competitive facility in radio astronomy. The collaboration was university-dominated because the universities wished to run an observatory. The three had in common aging facilities and modest capital. However, the scientists relied on 'know-who' rather than an impersonal search for similarly inclined organizations. Because they entered into the collaboration confident of each other's reasonableness, they took in stride the difficulties of dividing labor and coping with differences in policy. The gravest threat to the collaboration came from the competition for capital within each organizations. During the year of negotiations that led to BIMA's charter, one of the institutions lost a prestigious optical astronomer who was displeased with the expanding emphasis on radio astronomy. Changing business-as-usual had a cost, but the radio astronomers did not bear much of it.

**Type 4: Business-As-Usual Collaboration - or, Collaborators with Resources Wanted**

Now suppose you are a scientist in a field where it is difficult for anyone to do much of anything *except* in collaboration, and you have a bright idea for a project. You would contact professional friends, but probably find that most were already committed to other projects. Then what? Or perhaps you are a scientist who has spent your research career with a small knot of colleagues, and you have a bright idea for a project that can only be carried out through a sizable collaboration. What do you do? For both cases, the answer is <u>advertise</u>: COLLABORATORS WANTED; prior experience preferred, access to needed resources (e.g., time, money, students, engineers) required. While such ads have never, to our knowledge, been placed in a scientific journal, they are effectively spread by word-of-mouth.

Collaborations in this group form because scientists with a vision succeed in selling their vision to enough colleagues with the needed competencies. They differ from 'wake-up-and-smell-the-coffee' collaborations in that no one sector dominates the collaboration, and the collaborators are less likely to have worked together previously. These factors imply that the instigators routinely go beyond their circle of professional friends to acquire participants. They differ from 'damn-the-torpedoes' and the 'new-business' collaborations in that they do not have to address conflicts with home organizations. Particle physics, which is almost always pursued in collaboration, is disproportionately represented in this category. Individual particle physicists, their home organizations, and their funding agencies have all learned to welcome collaborative research.

Take, as an example, Fermilab 715, one of a string of fixed-target experiments that used a beam of hyperon particles. At Fermilab's inception, Joseph Lach moved from Yale to join the Fermilab research staff and began discussing hyperon experiments with his former colleagues. Soon they were personnel-limited. What they could do --potentially even their right to deploy instrumentation at Fermilab --depended on what they could sell others on doing. The clearest indicator that the collaboration was selling a high-quality product was that it could attract physicists performing similar experiments at other laboratories. Providing an idea appealing enough to convince such physicists to come to Fermilab was a junior physicists' route for upward mobility, both within the collaboration and also, potentially, within their home organizations.

Peter Cooper, a junior professor with hopes of winning tenure at Yale, came up with such an idea. He realized that the Yale-Fermilab detector could be economically reconfigured to perform a more elegant, definitive version of an experiment that Roland Winston of the University of Chicago had led at Argonne National Laboratory. Winston's experiment had produced results that contradicted accepted theory. Cooper's idea hooked Winston, who did not consider himself wedded to his Argonne results. The crowning coup was that Cooper's idea hooked the University of Leningrad physicist Alexei Vorobyov, who had been working at CERN on experiments that directly competed with Fermilab.

Winston and Vorobyov had no prior experience of Lach, Cooper, and their circle of colleagues. Incorporating them into the collaboration was nevertheless straightforward. They both had financial support that was sufficient to cover their contributions. Each pursued a strategy for an essential task -- electron detection -- that was not already covered in the collaboration. Once Cooper and Lach were able to convince Fermilab's director that they were avoiding all the pitfalls the director's advisory committee could spot, they had permission to build the experiment. The hoped-for results were doctoral dissertations for Yale and Chicago students, tenure for Cooper, and international acclaim for Lach, Winston, and Vorobyov should the anomalous results hold. Their choice to pursue hyperon physics would be justified as a route to fundamentally new physics.

This collaboration, unlike the entrepreneurial, relied upon a core of multi-sectoral expertise: accelerator laboratory physicists to provide a customized beam of particles and university physicists to provide detector components that, in combination, could characterize the processes the beam particles caused or underwent. Forming a collaboration was a necessity for the success of Fermilab 715's participants. Each assumed s/he could cope with any competent physicist whose scientific skills and interests were compatible. Except for the uncertainties surrounding Soviet-American relations, visas, and travel rights in the 1980s, nobody worried about the policies of each other's organizations. A memorandum of understanding was sufficient guarantee of everyone's sense of purpose and dedication.

**Type 5: Externally Brokered Collaborations, or Come Together, Right Now, Over Me**

Finally, imagine you are a scientist enjoying a satisfying career with a secure position in a secure institution. Thanks to forces largely beyond your control, you and your disciplinary colleagues are all contemplating a stellar research opportunity that begs for a collaboration. Perhaps a new facility is coming on line and needs people to make use of it, or a funding agency has started a new budget line and is soliciting proposals, or a rarely occurring natural phenomenon is soon to occur. You may have some misgivings about pursuing such opportunities, because so many other scientists will be interested in participating that it may seem well nigh impossible to field a winning proposal by organizing or joining a collaboration of professional friends in similar organizations. But if you and prospective collaborators do not come together in a timely fashion over this opportunity, you personally -- your scientific community generally --will be the poorer for not having your ideas and abilities in the mix of possibilities. Two possible routes to participation confront you: (1) prepare as self-sufficient an individual proposal as possible, hope a central authority decides to include you in its mixing and matching of individual proposals, and accept working with whomever is selected; or (2) seek out different kinds of scientists from those you would not normally work with, convince them that collaboration will serve everyone's interests, and hope that the socially unconventional character of your collaboration will make you appealingly different.

As a group, collaborations in this category are less likely to be built on prior relations because of the impersonal competition involved in selecting participants. Such collaborations are less certain of finding funding niches because of the unconventional combination of organizations with different traditions and restrictions. Such collaborations never have a dominant sector, reflecting the breadth of prospective participants. Finally, these collaborations are more likely to be pressured by their member organizations because of the difficulty of justifying unconventional combinations of organizations.

An excellent example of a collaboration that came about when a central authority 'mixed and matched' self-sufficient proposals was the Voyager project. Scientists at the Jet Propulsion Laboratory, who made a point of seeking opportunities for planetary science projects, alerted NASA that there would be a rare planetary alignment that would make it possible, in the winter of 1976-77, to fly spacecraft past several outer planets by using gravitational boosts to accelerate spacecrafts from one planet to the next. JPL engineers and panels of the National Academy of Sciences debated how to upgrade planetary spacecraft for a longer, riskier mission while leaving sufficient money, weight, and power for a suite of scientific instruments. When the Academy, JPL, and NASA reached an understanding of the spacecraft's engineering parameters and the project's overall budget, virtually every planetary scientist in the world had ideas for what measurements it should make. NASA organized numerous 'working groups' to investigate various instrumentation possibilities and to set specifications within which NASA could conduct competitions to design and build the desired instruments. In some cases, working groups became fraught, as members sized up each other as potential competitors, and fretted over how their recommendations would affect chances in the coming competition. However, by including some competitors in the working groups, NASA insured the consensus recommendations would be sufficiently generic to conduct an open competition.

The competition was held, and the outcome created enough awkwardness to frustrate a conspiracy theorist. A co-investigator on an underdog proposal recalled, 'I would say you could have knocked me over with a feather when it was selected.' He attributed the selection less to the merits of the proposed instrument's design than to the track record of the principal investigator's organization for turning out well-built space instrumentation, and to the willingness of other experienced scientists to serve as co-investigators on the experiment. A JPL science team's proposal was rejected in favor of a similar instrument proposed by outsiders, who then had to work with JPL's engineers and managers to integrate their instrument with the rest of the spacecraft. Though no interviewee gave any indication of expecting or receiving anything but appropriate professional behavior from other participants, the collaboration was sealed not by a memorandum of understanding, but by a set of legally enforceable contracts among NASA Headquarters, JPL, and the principal investigators and their organizations.

**The Legacy of Project Formation**

The five general ways in which multi-organizational collaborations form are not distinctively associated with other aspects of collaborations. As much as historians may want to believe in the utility of knowing the fine structure of the contexts that give rise to collaborations, our data yield no interesting correlations between formative types and other structural and processual characteristics of collaborations. This is not to say that the creation of a collaboration is irrelevant to its evolution. Rather, it is that the variables that are strategic in discriminating among the ways in which collaborations form, are not significantly correlated with the ways in which they later evolve. Some variables, however, are so correlated, and this information can assist scientists and policy-makers to anticipate how a collaboration will evolve.

In the remainder of this essay, we examine the role of resource uncertainty; the organizational sectors that instigate the project; and the context of the participating organizations. Important consequences follow if funding agencies or other central authorities apply pressure to form collaborations, if a collaboration has to search for patrons, if a collaboration's source of funds is a new funding program, if organizations outside academia instigate a collaboration, if no sector dominated a collaboration's formation, or if home organizations influence a collaboration's formation. In each case, complexity and regulation affected their structure. Our term for this property is 'encumbered.' We suggest that when a collaboration's formation is encumbered, there are consequences that collaborators should prepare to assume. To illustrate, we display the most significant relationships between three project-formation variables and other aspects of collaborations: viz., resource uncertainty, the instigating sector, and pressure from the home organization.

**1) Resource Uncertainty**

Resource uncertainty --whether there is a well-known, well-established program that regularly provides resources for collaborations -- showed the greatest discriminatory power in project formation. When collaborations have an obvious source of funds and do not face having to search for fresh support, they are unlikely to have formal contracts and centralized decision-making over collaborative functions. But they also are more likely to subject topics for data analysis to collaboration control and to share data widely within the collaboration. This combination of collaboration self-governance and a broad domain of activities is a distinctive type of organizational style that is typical of particle physics. Obvious funding sources for collaborations are indicative of a community in which multi-organizational collaborations are so common that participants are comfortable with allowing collaborations to regulate many activities without the formal specification of rules.

[Figure 2 about here.]

Fermilab 715 and BIMA illustrate opposite ends of this spectrum. Fermilab 715 had obvious sources of funding. Participants in particle physics typically need to work with data from multiple components to reconstruct events from the variety of particles into which unstable, heavy particles decay. This condition and its consequences were so commonly accepted that the Fermilab collaboration working with hyperon beams added the University of Leningrad group -- with whom none of the Americans had worked -- because of Leningrad's unique mastery of Transition radiation detectors,' which were then the most efficient and capable means of detecting electrons and ascertaining their properties. Even when Fermilab's director told an American physicist on the experiment, to 'Get rid of the TRD's, it will never work,' the physicist felt entitled to respond 'I think it's an important part of the experiment' --and to ignore 'one of the few times that he told me to do something directly.'

By itself, electron detection was meaningless -- it had to be combined with information that components acquired about other particles created in the experiment. The collaboration could not operate meaningfully except by sharing data; and each member wanted the experts on the various components to examine how the data from each was analyzed and used. Participatory and consensual decision-making over data acquisition issues were thus the norm. Any decision the experiment's titular leader made in the absence of consensus was open to reconsideration:

We had some great fights over just how should we set the trigger [to filter out unwanted

> signals before they were recorded]....[One physicist] and the spokesperson had a great fight in public, where the spokesperson finally had to say, ‘No, we don't change the trigger the way you suggested’. The other physicist blew up. The next week we changed it his way. So, what else is new? He was right as usual. But he had to make the case to the satisfaction of the collaboration. This was a week of running. We only took data for six weeks during this experiment. It was a fairly contentious issue.

Communal data acquisition and shared data streams also imposed on the collaboration the need to manage data analyses to insure due recognition for the organizations. In one instance, involving dissertation students at two different organizations, the collaboration carefully carved differentiated topics for their dissertations without making its most important results hostage to student inexperience:

> Basically no graduate student really published the first paper ... that was the primary focus ... where the major thrust of the analysis was. In general you really like students to publish things that they really did the analysis on. The way we broke it up was [one student] took the magnetic moments ... [which] was a completely orthogonal topic [to the primary focus]. So, that was easy. [Another] student took the asymmetry parameters, having measured all the particles ....That had never been done before. That was ... a transition to [the third student] who had to do the full blown analysis ... and all that complicated stuff. There was reasonable differentiation. We only had three theses to make. Had we had to invent the fourth, we could have had one guy publish the electron asymmetry and one guy publish the neutron and neutrino asymmetry. We didn't have to do that, but we could have.

A formal agreement would have at best been useless and at worst an impediment for this collaboration. There was no need to codify a division of labor for the construction of detector components because the members were content to proceed on the basis of their prior expertise and the availability of previously used instrumentation. Any advance specification on the acquisition of data or division of topics among students would have interfered with arguments on data acquisition strategies and the use of students’ talents as factors in decisions about dissertation topics. In particle physics, the routinized source of funds is associated with a well-known culture of collaborating, that obviates the need for formalities and specifies the need for regulating data acquisition and analyses on a collaboration-wide basis.

As a contrast to a particle physics collaboration, BIMA (from the 'get-out-of-your-business-as-usual' group) originated in a context of uncertain resources. BIMA's formation required that radio astronomers at three state universities obtain capital funds from their universities *and* draft a successful proposal to NSF. With three universities spending their own money for BIMA, it was a foregone conclusion that there would be a formal contract stipulating fiscal obligations. A new organization was required to insure that each university's interests were served. Overseeing the project were two committees:

> the BIMA board, which consists of three people from Berkeley, two people from Illinois, and two people from Maryland .... [T]hat group decides the major thrust. The day-to-day decisions are made by the executive committee, which is the directors of the three institutions, and that's why we have phone calls every two weeks, and at certain times more frequently.

Thus decision-making was centralized and hierarchical on matters that were deemed appropriate for the collaboration as a whole. At the same time, BIMA was organized in order to limit the scope of decisions and leave some power within the member organizations. This was reflected in its budgetary operations:

> All the budgets of each [organization] are discussed with one another, although one of the things we did in setting it up ... is that we agreed that the individual universities would put up money for the project, but for the most part, the money is spent on themselves. Okay? So that, for example, there are programmers at Illinois and programmers at Maryland, and they're hired out of their own money. The money is spent there. The support of the students is spent there .... But the key thing here is that for the most part, the money is spent locally. That's the kind of arrangement that the institutions like.

Most importantly, in place of particle-physics style collective debate over the best scientific strategy, in BIMA the participating organizations control the scientific use of the collaboration's instrumentation. Individual scientists were made responsible for generating and carrying out research plans.

> Everybody who wants to do a project writes a scientific proposal and then we have referees, one from each university and two people from outside, to evaluate what goes on. There is a formal guarantee that a minimum of 30 percent [of use for science] goes to Berkeley, a minimum of 20 percent to Maryland, and 20 percent to Illinois, and then there is publicly available time [reflecting the financial contribution of NSF], but we can compete for that as well.

Data taken with BIMA instrumentation were not shared within BIMA (unless successful proposers struck their own agreements to do so), but they have been archived for future use by interested researchers.

By making the uncertainty of resources a challenge to be overcome rather than a limit on their ambitions, the radio astronomers at Berkeley, Illinois, and Maryland got out of their 'business-as-usual' ruts. Through comparison with collaborations like Fermilab 715, with its obvious source of funding, it is clear that BIMA's participants assumed a distinctive set of burdens and constraints. Extraordinary business could only be conducted if certain prerogatives of the home organizations were respected. There was to be little or no redistribution of resources across the established organizations. No matter how much better one university's proposals for BIMA's use, the quantity of time each university had was written into their formal contract and protected by its representatives on the proposal review panel. There was not to be muddling of lines of affiliation of BIMA users. Each university was to have clear grounds on which to judge and reward its users. The scientific leaders of member universities had to assume responsibility for a centralized, hierarchical organization that did not control very much. Indeed, BIMA was more centralized and hierarchical than most collaborations, to insure that it would not control any more than was necessary to build the array of radio antennae, to make them operational, and to generate good publicity for the member universities.

2) **Sectoral Instigation**

Universities were the most important sector in our sample, and were almost never insignificant to the multi-organizational collaborations we studied. At our level of analysis, differences among universities seem far less important than their commonalities. The primary distinction in the collaborations we studied was between those that were instigated exclusively by universities and all others. We expected collaborations with multi-sectoral instigation to face greater formative complexities than university-instigated collaborations. We expected collaborations with corporate instigators to be encumbered in their formation because of corporate needs to secure intellectual property.

Figure 3 displays the relationships of sectoral instigation with four factors. Collaborations with multi-sectoral (or non-university) instigation were usually larger (defined as seven or more organizations or teams), were more likely to have a communications center, and more likely to have an agreement about sharing data.

[Figure 3 about here.]

The source of these relationships was the need for instigators to make a collaboration ‘university-friendly,’ even where a non-university sector was to dominate. Take the AMPTE as an example. Research institutes, and not universities, were the organizational instigators because functionally differentiated spacecraft were needed to carry out the ‘active experiment’ –i.e., to assess the power of the solar wind to ‘blow’ ions into the magnetosphere. But the Applied Physics Laboratory, the Max Planck Institute for Extra-terrestrial Physics, and the Rutherford-Appleton Laboratory were not pre-eminent in all the relevant fields. The more uses to which spacecraft could be put, the better the project would look to NASA, its advisory committees, and to science policy-makers in Germany and the United Kingdom. Consequently, the instigators sought universities and corporate groups with relevant expertise. The number of participating teams and organizations ballooned.

A rough *quid pro quo* governed their arrangements. University teams made two major concessions to the research institutes and their engineers. First, they allowed the research-institute engineers in charge of spacecraft design and construction to be communications centers with responsibility for identifying interface problems. The university teams knew the engineers had the confidence of the instigating scientists.

> SCIENTIST: To get the resources, this was ultimately negotiating, in this case, with APL [Applied Physics Laboratory]. If you came to a point where you said, ‘Absolutely need additional weight,’ you make your case strong enough .…
>
> INTERVIEWER: To whom are you making your case: Krimiges [the scientific leader] or Dassoulas [the engineering leader]?
>
> SCIENTIST: It really depends.... Early on, of course, it's Krimiges. Before you get to detailed design. By the time you are talking about detailed interfaces with spacecraft, then, at that point--weight, power, etc.--it was with Dassoulas.

The engineers, as a matter of training and experience, knew how much things cost. Their empowerment led to the completion of design and construction within budget.

The second concession made by the university teams was to share data during the time the active experiment was being run –that is, when the spacecraft outside the magnetosphere released tracer ions for the spacecraft inside the magnetosphere to detect. In return, the university scientists received two privileges. First, they were recognized as participants in the active experiment (all understood that Krimiges was the author of the active experiment). Second, and more important, they had autonomous use of their instruments to pursue their individual interests when the active experiment was not being run. As the results of the active experiment were disappointing, the individual teams' measurements of the natural environment assumed greater importance.

**3) Home-Organization Context**

In about 40% of the cases we studied, individual scientists who wished to participate in a multi-organizational collaboration had to secure approval from their home organizations. Such collaborations usually had large numbers of teams, irregular communications, formal contracts, and many levels of authority. These features are to be expected when organizations are concerned with the way in which the collaboration will affect their reputations and interests.

[Figure 4 about here.]

IRIS's history embodies this dynamic. The uncoordinated pioneering efforts of those few seismologists whose universities provided enjoyed superior computation facilities, research and development laboratories, or good connections to industry or government agencies were responsible for raising the general community's awareness of the virtues of digital instrumentation and large data sets. IRIS's proponents among these fortunate pioneers assumed that IRIS had to be large to impress the NSF. To get universities without seismological facilities to join IRIS, and to put up money for start-up costs, IRIS had to develop an elaborate a formal structure. Otherwise, seismologists at other universities would have had trouble convincing their administrators that IRIS would be fairly and equitably governed.

The formal structure at which they arrived, included the three distinct programs that IRIS would perform. It would build up (1) a digital global seismic network; (2) a stable of digital portable seismometers, and (3) a data management center to support the handling of large data sets. Each program would have its own budget, its own officer in charge, and its own advisory panel of seismologists from IRIS organizations. The three officers reported to a consortium president who was accountable to the Board of Directors, which had an active executive committee to monitor collaboration-wide issues.  No elaborate inter-program  technical integration was needed.  The member organizations had only to communicate at annual meetings of the Board of Directors, where the consortium's budget was set.

Initially, this communication was more meaningful than anyone expected, because the directors made the IRIS budget meeting an occasion for discussing historical inequities in the distribution of resources. Thus:

> When those things [specifying IRIS's functions] started surfacing, they didn't sit too well with some folks who had something to lose, some autonomy to lose.... Some of the people who started the whole thing had these big, eclectic meetings, with all the seismologists ... where this was laid out as they were going to save every little university, and 'You've got to get on board because there's no self-serving thing here, this is all for you.  We need your backing.'  Then, suddenly, it turned out, 'Well, it was all for you, but it was all for me, too.'  [T]here was a subset of membership that didn't want to spend this [IRIS] money on [a Data Management Center] .... Those who didn't need it, and in fact would benefit without it, because it would keep their position relatively high in such facilities, and it would put more IRIS money into things that they couldn't provide for themselves.  So, it was a fight every year to distribute the budget, and make sure that DMC got something.  It was the ugly aunt that we had to keep dragging out of the closet and say, 'She's going to eat with us.'

When the process of collaboration requires that scientists secure the approval of their home organizations, it is obvious that the operation can become an arena in which broader conflicts are played out.

**Conclusion**

That multi-organizational scientific collaborations form in several different ways should come as no great surprise.  The multiplicity of funding sources in the United States, the complexities of combining support from multiple governments in international collaborations, or from multiple agencies in the same national government, and the range of scientific specialties that spawn collaborations all introduce a large element of diversity into the process.  However, it does seem extraordinary that patterns in the formation of multi-organizational collaborations do not coincide with scientific specialty or patronage, with the exception of particle physics.  Even particle physics collaborations crop up in most of the categories created in cluster analysis.  For every type of project formation, we found a broad choice of specialties and patrons from which to choose an illustrative case.

At least in terms of the ways they form collaborations, scientific specialties exhibit internal heterogeneity, and their patrons exhibit flexibility. **[On reflection, I don't see any need for that opaque sentence.]** A nifty idea for measuring a parameter will serve as a wake-up call to scientists in any specialty, as will a new funding agency program that invites scientists to try new approaches to making measurements. Envy of the facilities enjoyed by scientists in government, industry, or abroad will energize scientists in any specialty to brave the politics of forming a collaboration to obtain such facilities. Aging facilities that cost too much for an organization to recapitalize ,or other sources of long-term professional insecurity, will prompt scientists to become entrepreneurial. Inadequate human or technical resources will inspire scientists in any specialty to advertise for collaborators. Research opportunities will lead all funding agencies to organize impersonal competitions among scientists in relevant specialties.

The relationship between project formation and other facets of multi-organizational collaborations demonstrates the utility and limitations of research into 'origins' as a form of applied social science. Where funding is from uncertain sources, or where the home organizations of participants are a factor, scientists will need to create collaborations with relatively strong formal organizations. Often the pursuit of new funding sources forces participants into relationships they have never met. In such cases, collaborations formalize management and centralize organization to insure fairness and accountability; and they limit their scope to insure autonomy for their members. When organizations other than universities are involved, engineers from corporate or governmental backgrounds may be managers, but the need for university participation will insure that such collaborations accommodate academic sensibilities.

Multi-organizational collaborations are predicated on a federal basis. Where researchers work for independent organizations with the power and will to maintain their independence, large-scale research will require formalized collaboration. Federalism has long been the governing norm in the United States, and the nations and institutions of Europe on both sides of the former Cold War are experimenting with federalist frameworks for many activities. One of the shibboleths of American politics --that the various policies of local jurisdictions serve as 'laboratories for democracy' -- should be paraphrased for the sciences -- that the organizations that employ or fund scientists are 'laboratories for science policy'. Until such time as the political will exists to standardize scientific organizations and specialties, 'big science' will remain a term which thinly veils our ignorance of the dynamics of research that our society spawns.

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